Lionel Robbins’s essay on the nature and significance of economic science

75th anniversary conference proceedings

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In 1932 Robbins set out to inquire about that which defines the subject matter of economic analysis: what economists can and cannot say and the method by which they can reach their conclusions. But this was far more a complex struggle than ‘merely’ to identify the methodology of economic analysis. At that time, he also had to cross swords with other approaches concerning the delineation of the subject of economics. On the one hand, he had to fend off historicism or the claim that social phenomena cannot be subdivided into separate spheres of examination. On the other hand, he also had to position himself vis-à-vis the empirical drive emanating from institutional economics of the time.

In many respects today we face a similar situation to the one that Robbins faced. Economics is under attack for lack of relevance, for inappropriate premises regarding human behaviour, for failing to confirm many of its propositions and for ignoring other aspects of social relations. This is particularly poignant as today’s economics, in many ways, is based on Robbins’s conclusions from his Essay on the Nature and Significance of Economic Science.

Robbins’s analysis did not lead to identifying the agenda of economics with any of the prevailing doctrines of the time. In fact, through a careful study of the existing alternatives both in substance and methods, Robbins forms an entirely new economics which, to some extent, is an amalgamation of Austrian and Lausanne themes. However, the economics he defined and, which still lives in contemporary textbooks, was new and brave and suggested yet another school of thought (the LSE School).

Both the apparent difficulties that economics seems to be facing and the fact that its foundation can be traced back to the Essay suggested that the 75th anniversary of the Essay was a good opportunity both to repeat Robbins’s exercise and to examine its historical significance. So we invited scholars to do both. The conference, which ran over two days and included 30 papers, was divided broadly into the following themes:

1. Reflections on Definitions and Boundaries of Economic Analysis;
2. Ethics-Economics relationship;
3. Methodology;
4. The Role of Policy.

Most papers in the conference were focused on topics 1 and 3 but topics 2 and 4 are, of course, no less significant.

Naturally, there was no single voice coming out of the conference. However, the breadth of issues which were tackled suggests to us that the exercise was worth pursuing. We hope that this volume will stimulate a discussion and a debate about the nature and significance of economic science but, most of all, we hope that it would stimulate a greater deal of reflection by those who are at the forefront of economic analysis.

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Lionel Robbins, Economising and Innovating:  
A Business School Perspective*  

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Abstract

We revisit and critically evaluate Lionel Robbins’s famous definition of economics from a business-school perspective, in the light of post-Robbins developments in (1) neoclassical economic theory and (2) evolutionary economics and management theory. We argue that while the economising approach to economics captured by Robbins’s definition has an important place in business school curricula, there are various economics-related topics of significant interest to business-school audiences – not least those relating to technological change and its impact on resource creation and intertemporal economic performance – that can only be addressed by moving beyond the strictures of Robbins’s conception of the subject.

1. Introduction

The aim of this paper is to revisit Lionel Robbins’s famous definition of economics from a business school perspective and in the light of post-Robbins developments in neoclassical economic theory, evolutionary economics and management scholarship.

The main thrust of our argument is that while economics in its Robbinsian “economizing” guise contains important lessons for business school audiences, his insistence on economic analysis proceeding by taking means-resources - what he calls the “ultimate data” of “technique” and institutions (such as property rights) - as given, may actually divert attention from or even obscure various other issues of central importance from a business school perspective. The reason for this is that while business leaders and managers are certainly interested in questions of economizing, they are also interested in questions of innovation and strategy. And many of the issues involved here are ones that have less to do with the efficient allocation of existing resources than with questions of how resource constraints might be reduced, i.e. with technological change, increasing returns, intertemporal efficiencies and the productivity-enhancing effects of the co-evolutionary character of market structures, organisations and technological change. These factors are vital determinants of intertemporal efficiency and economic performance, and therefore

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cannot be treated simply as parameters that are only interesting insofar as they affect relative scarcities.

The paper is structured as follows. We begin in the next section by revisiting the definition of economics proposed by Robbins in his 1935 *The Nature and Significance of Economic Science* (henceforth NSES) and argue that his particular view of economics as being purely about economizing is of a piece with his view that economics is not about the causes of wealth or welfare. Section 3 then looks at the influence of the Robbinsian view on business school economics via Post Robbinsian economic theory. Section 4 discusses recent developments in neoclassical economic theory, evolutionary economics and management scholarship that focus on the role of technological change and its relationship to market structures, organisations and institutions. We argue that these developments put into question Robbins’s view that the economist should treat “technique” and “institutions” as “ultimate data”. In particular, we argue that economising cannot always be treated as separable from innovating, and that in the business world it is mainly through innovation and technological change that long-term economizing can be effected. Section 5 closes with some concluding remarks.

2. Robbins’s Definition of Economic Science

According to Robbins’s famous definition, “Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” (NSES, p. 16). By “ends” Robbins means human objectives, possible states of affairs that can be can be ranked in terms of their importance or desirability. By “means” he has in mind the available time and other resources that could be deployed to achieve those ends. Economic problems, as he conceives them, arise in situations where there are competing ends of different levels of importance, and where the available means could be put to more than one use and are scarce relative to those ends. In situations of this kind economic choices have to be made:

“… when time and the means for achieving ends are limited and capable of alternative application, and the ends are distinguishable in order of importance, then behaviour necessarily assumes the form of choice. Every act which involves time and scarce means for the achievement of one end involves the relinquishment of their use for the achievement of another. It has an economic aspect” (NSES, p. 14).

Robbins thus characterizes economics in terms of what we will call an economizing orientation, namely a concern with analyzing how scarce resources may be put to their best use. He contrasts this conception with what he calls the “materialist” conception that he associates with scholars such as Cannan, Marshall, and even Pareto and J.B. Clark, and according to which economics is about the “causes of material welfare” (NSES, p. 4). Robbins is sharply critical of this conception and insists that whatever economics may be about, it is not about the causes of material welfare (NSES, pp. 4-23). However, the target of Robbins’s criticism is not so much the emphasis on the causes of welfare per se, but that a focus on material welfare would render economics unable to accommodate certain activities such as enjoyment of leisure and the services of an opera singer on the grounds that these are not instances of material wealth (NSES, pp. 4-23).
Robbins is surely right that leisure activities and the provision of services should fall under the purview of economics. As he puts it:

“… is true that the scarcity of materials is one of the limitations of conduct. But the scarcity of our own time and the services of others is just as important. The scarcity of the services of the schoolmaster and the sewage man have each their economic aspect … it is not the materiality of even material means of gratification which gives them their status as economic goods; it is their relation to valuations. It is their relationship to given wants rather than their technical substance which is significant” (NSES, pp. 21-22).

However, accepting that the scope of economics extends to “non-material” welfare and the implication that the materialist conception of economics should be rejected for excluding them, does not by itself imply that economists should not be concerned with the causes of welfare in some more general sense. That is to say, there is no logical barrier to allowing that economics should be concerned with “economising” in the Robbinsian sense, at least in part, and also extend to the analysis of causes of welfare. As far as Robbins himself is concerned, and while he is unequivocal in his insistence that economics is not about the causes of material welfare in Chapter I of NSES, he there does not explicitly deny that it might be about the causes of welfare in a more general sense.

In Chapter III Robbins goes on to claim that instead of dividing economics into the theory of production and the theory of distribution – whether the former is concerned with explaining “the causes determining the size of the ‘total product’” and the latter with “the causes determining the proportions in which it is distributed between different factors of production and different persons” (NSES, p. 64) – as Adam Smith and others economists did, we now have “a theory of equilibrium, a theory of comparative statics and a theory of dynamic change” (NSES, p. 68). Robbins leaves his reader in little doubt about his position on the theory of production:

“We have all felt, with Professor Schumpeter, a sense almost of shame at the incredible banalities of much of the so-called theory of production – the tedious discussions of the various forms of peasant proprietorship, factory organization, industrial psychology, technical education, etc., which are apt to occur in even the best treatises on general theory”… (NSES, p. 65).

And commenting on Adam Smith’s own excursions on this topic:

“… although Adam Smith’s great work professed to deal with the causes of the wealth of the nations, and did in fact make many remarks on the general question of the conditions of opulence which are of great importance in any history of applied Economics, yet, from the point of

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1 Indeed one may be forgiven in thinking that Robbins would not disagree with this when he states that “The services of the opera dancer are wealth. Economics deals with the pricing of these services, equally with the pricing of the services of a cook.” (NSES, p.9, emphasis added). Robbins, however, goes on to say that economics should not, nevertheless, be concerned with the determinants of the wealth of nations. This may strike his readers as somewhat inconsistent.
view of the history of theoretical Economics, the central achievement of his book was his demonstration of the mode in which the division of labour tended to be kept in equilibrium by the mechanism of relative prices” … (NSES, p. 68)

The upshot of all this is that, for Robbins, it is never the means-resources in their own right that are of significance for the economist, only the relationship between ends and means that is of significance to the economist, and not the ends and means in their own right. That is to say, means (and ends) should be treated as givens by economists, as “ultimate data” that it is not their business to enquire into:

“… the subject matter of Economics is essentially a set of relationships – relationships between ends conceived as the possible objectives of conduct on the one hand, and the technical and social environment [means] on the other. Ends as such do not form part of this subject matter. Nor does the technical and social environment. It is the relationship between these things and not the things in themselves which are important for the economist” (NSES, p. 38).

And again:

“Economists are not interested in technique as such. They are interested in it solely as one of the influences determining relative scarcity. Conditions of technique “show” themselves in the productivity functions just as conditions of taste “show” themselves in the scales of relative valuations. But there the connection ceases. Economics is a study of the disposal of scarce commodities. The technical arts of production study the “intrinsic” properties of objects or things” (NSES, pp. 37-38).

3. The Economising Conception and Business School Economics

Robbins’s view had a revolutionary impact on economics (see for example Baumol, 1984), as taught both in economics departments and in business schools. In the case of business schools, much of the economics taught zeroes on what we will call “core” microeconomics delivered in the familiar neoclassical style of standard introductory and intermediate textbooks. While the level and extent of provision varies quite significantly across schools, as well as across different programmes offered within the same schools, the kind of topics that tend to be covered invariably include some elementary price theory, the theory of the consumer, the theory of the firm (costs, revenues and profit maximisation), market structure, some managerial economics, welfare economics and market failure (market power, externalities and public goods).

Neoclassical microeconomics is widely regarded as the paradigm example of economics in its Robbinsian guise, and this is true of economics texts directed at a business school audience. Thus UK-based authors Nellis and Parker (2006) in their Principles of Business Economics declare that:

“Economics is concerned with the efficient allocation of scarce resources. When purchasing raw materials, employing labour and undertaking investment decisions, the manager is involved in resource allocation” (Nellis and Parker, 2006, p. 3, emphasis in the original).
Similarly, US-based authors McKenzie and Lee (2006), in what is described on the cover as “the first microeconomics text written exclusively for MBA students”, distinguish the economist’s work from that of other social scientists as follows:

“Economists take a distinctive approach to the study of human behaviour, and they employ a mode of analysis based on certain presuppositions. For example, much of economic analysis starts with the general proposition that people prefer more to fewer of those things they value and that they seek to maximize their welfare by making, reasonable, consistent choices in the things they buy and sell” (McKenzie and Lee, 2006. 10).

While neither book mentions Robbins specifically and McKenzie and Lee display a preference for American over British authorities, the spirit of the Robbins view clearly shines through in the passages quoted above. Further, it seems to us right that business schools student be exposed to the economizing perspective in the Robbinsian sense, since business leaders, managers and entrepreneurs are often engaged in allocating resources, in having to make difficult choices between competing ends under conditions of scarcity and attempting to find more efficient and cost-effective and efficient ways to perform already-existing functions. Basic lessons about resource allocation, opportunity costs, diminishing returns, marginal analysis and so on, are central to all this kind of activity and therefore valuable to the students.

There are of course other reasons for teaching core microeconomics to a business school audience. First amongst these is that it provides a theory of price and insights into the operation of the price mechanism, a characterisation of the firm and different forms of market structure and their effects – especially useful with respect to more mature and relatively more stable sectors (Pitelis, 2007) – and the effects of market failure (the last of which is becoming increasingly important because of increasing concerns about the environment and relevant policy responses). Second, microeconomics is in many ways a fundamental discipline that provides the theoretical underpinning of parts of other subjects that students will encounter on their courses, such as business strategy. Michael Porter’s (1980) approach to competitive strategy, for example, derives from the microeconomic market structure analysis. Third, a grounding in microeconomics puts students in a better position to receive, interpret and evaluate the many messages they will be receiving about the ‘economy’ during their working lives.

However, to say that the core microeconomics taught in business schools is useful is of course not to say that there aren’t limitations to the material and its potential relevance. Some of these limitations are directly related to aspects of the economizing orientation articulated by Robbins, but others have do with features of the discipline that have crystallized in ways that he might not have imagined. Here are three features we regard as characteristic of modern microeconomics and which we shall focus on below:

1. The assumption that actors are motivated purely by self-interest and pursue this aim in a perfectly-informed and perfectly-consistent way, maximising utility if they are consumers or maximising profits if they are firms. In general, the methodology of core microeconomics is to analyse economic phenomena
as the outcome of the actions (and interactions of) such rational agents. This approach is consistent with Robbins’s conception of economics (NSES, p. 78).

2. An emphasis on static allocative efficiency and the idea that this is most likely to be effected by the desirable properties of certain “optimal” market (industry) structures such as perfect competition, perfect contestability or Bertrand competition (see for example Varian, 1992). This emphasis is largely a post-Robbins development.

3. A failure to explore the full ramifications of the possibility that industry structures which are optimal from the point of view of static efficiency, may well be sub-optimal from the point of view of dynamic or intertemporal efficiency. This failure reflects Robbins’s insistence on treating “technique” as a datum, thereby discouraging the analysis of the determinants of technological change.²

To appreciate these issues better, is worth remembering that one of the major achievements of this approach has been to prove that under conditions of perfect competition, a market economy, will allocate (scarce) resources in an efficient way. Economic efficiency is approximated by Pareto efficiency, defined as a situation in which it is not possible to any one person better off without making someone else worse off. In addition, any Pareto efficient situation can be shown to correspond to a competitive equilibrium, given an appropriate distribution of endowments (see Dasgupta (1986) for a critical assessment of these ideas).

These are powerful results, the derivation of which makes economists justifiably proud. It is also clear, however, that they have been achieved at the cost, not only of narrowing the scope of economics to what we have called an economising orientation concerned exclusively with the efficient allocation of scarce resources as per Robbins, but also to a largely uncritical view of the virtues of highly idealised “optimal” types of market structures. There has accordingly been a slew of criticisms of this approach, of which we will briefly consider two. In the first place it has been pointed out that its emphasis on self-interest maximisation has rendered economics free of any considerations or virtuous behaviour (Sen 1987). Second, the alleged optimality of “optimal” industry structures such as perfect competition and perfect contestability has been questioned. Both of these structures are characterized by the presence of free entry and costless exit by other firms, essential to establishing their “zero waste” property. As Baumol (1991) puts it:

“It is the costlessness of entry and exit under perfect competition or contestability that prohibits all inefficiency, because any firm that indulges in wasteful expenditure cannot long survive the incursion of efficient entrants.” (p.12)

² Robbins does not focus on optimal industry structures. Credit for exploring the link between market structure and technological change (or intertemporal efficiency) is due to to neoclassical IO scholars who have attempted to test the “so called” Schumpeterian Hypothesis (see Baumol 1991). With few exceptions, however, amongst whom Baumol is notable, they have subsequently failed to explore the relationship between optimal market structures, static efficiency and inter-temporal efficiency.
Baumol goes on to show that for this very reason firms in perfectly competitive or contestable markets will have an incentive to degrade and misrepresent product quality and to also abuse the environment. This will be so even in “repeated games” provided that some players are “transient”\(^3\).

There are related issues in respect of “intertemporal” efficiency. One of the stylized facts of the innovation literature is that it is neither the “midgets” nor the “giants”, but rather the medium-sized firms that innovate the most. Indeed there is considerable evidence that the relationship between the degree of competition within an industry on the one hand and its innovation performance on the other is of the inverse U-shape-type (see Aghion et al. 2005 for a recent re-statement). Large-sized firms are incompatible with perfect competition, albeit compatible with contestability. However, as Baumol (1991) notes, the conditions of free entry and costless exit deprives firms of the very incentive to innovate, namely Schumpeter’s (1942) “transient” monopoly profit. Assuming that innovations are good for intertemporal performance, ceteris paribus, “optimality” of market structures may be inimical to intertemporal efficiency.

Robbins’s conception of economics is more general than core microeconomics as characterised in the three points listed above. One reason for this is that he does not link his definition with ideal market structures that can deliver the efficient allocation of scarce resources. Another and perhaps more important reason is that Robbins is concerned not only with static, but also with dynamic/intertemporal efficiency (see NSES, pp. 68, 71, 79, 102-103, and below). Nevertheless, Robbins’s definition could lead to some confusion and could be criticized on some counts. We will mention two issues here, before moving on.

First, Robbins seems ambivalent as to the role of “time”. He refers to one’s time and resources, raising the question if time is (not) a resource. It could be argued that time is the ultimate resource as an individual could not do very much in its absence. In addition, while from the point of view of the individual time is the ultimate scarce resource – there is little one can do to extend it at any given point in time. Over time, it is possible to extend time, both at the individual level (for example through increases in life expectancy) and at the aggregate level (through increases in productivity and the size of the population). This challenges the notion of the scarcity of time and the distinction between resource allocation and resource creation, which we return to below.

A similar point can be made about knowledge. There is an extensive literature on knowledge that points to its “public good” characteristics, as well as its tacit, cumulative-increasing returns aspects (see Polanyi 1966; Buckley and Casson 1976; Stiglitz 1989, and the “endogenous growth” literature, for example Romer 1986)\(^4\). If knowledge is a resource (as argued for example by Marshall, 1920), and if it is not

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\(^3\) The performance of such market structures will instead be better in the case of another aspect of virtuous behaviour, that of racial, sex or other forms of discrimination. “Zero waste” suggests a tendency against discrimination, but here too the outcome is not always guaranteed, Baumol (1991).

\(^4\) For Stiglitz (1989) “Among the ‘commodities’ for which markets are most imperfect are those associated with knowledge and information. In many respects, knowledge is like a public good. Firms may have a difficult time appropriating their returns to knowledge, resulting in an undersupply; and to the extent that they are successful in appropriating, underutilization results (since they will have to charge for its use).” p.198.
scarce, at least not in all cases, Robbins’s definition may need revisiting and the relationship between knowledge, “technique”, market structures, institutions and organisations assume centre stage.

4. Robbins and Technique: Economizing or Innovating?

We have seen that according to Robbins, economics should be conceived as an approach that, beginning with the ultimate data of technology and institutions, and the assumption of rational behaviour, is concerned with the efficient allocation of scarce resources. In revisiting his essay in his Richard Ely lecture in 1981, Robbins (1984) restates these views but allows for what he called “political economy” (as opposed to economic science), to go further than economic science, by affording itself the luxury to get involved with issues that require value judgements.

The great advantage of the “modern” approach to theoretical economics, according to Robbins, is that it derives from a number of simple postulates:

“The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities” (NSES pp. 78-79).

Commenting on the apparently static conception of his approach, Robbins suggests that one can consider dynamics in two ways.

“In the first place, we may compare the equilibrium positions, assuming small variations in the data … [and we] may also endeavour to trace out the path actually followed by different parts of a system if a state of disequilibrium is given … And in so doing all this we make no assumption that final equilibrium is necessary” (NSES, p 102).

Finally, concerning “technical change and innovation” (NSES, p. 133) to include “changes in the legal framework” (NSES, p. 134), Robbins asks the question “how can we tell in advance what choice will be made?” (NSES, p. 134). Accordingly given such uncertainty “there are certain things which must be taken as ultimate data” (NSES, p. 135).

Our own concern, in the remainder of this paper is to delve a bit deeper into the following issues raised by Robbins. First, can the efficient allocation of scarce resources be separated from resource creation? Second to what extent do ‘technique’ and ‘institutions’ impact not only on relative scarcities but instead co-evolve, thus impacting on technology and resource-creation, and therefore scarcity, innovation, inter-temporal efficiency and macroeconomic performance.

A useful point of departure is Kaldor’s (1972) observations on increasing returns to scale:

“When every change in the use of resources - every reorganisation of productive activities - creates the opportunity for a further change which would not have existed otherwise, the notion of an ‘optimum’ allocation
of resources when every particular resource makes as great or greater contribution to output in its actual use as in any alternative use - becomes a meaningless and contradictory notion: the pattern of the use of resources at any one time can be no more than a link in the chain of an unending sequence and the very distinction, vital to equilibrium economics, between resource-creation and resource-allocation loses its validity. The whole view of the economic process as a medium for the ‘allocation of scarce means between alternative uses’ falls apart - except perhaps for the consideration of short-run problems, where the framework of social organisation and the distribution of the major part of available ‘resources’, such as durable equipment and trained or educated labour, can be treated as given as a heritage of the past, and the effects of current decisions on future development are ignored”. (pp. 1245-6, emphasis added).

For Kaldor (1972) economic theory went wrong when:

“the theory of value took over the centre of the stage – which meant focusing attention on the allocative functions of markets to the exclusion of their creative functions” (p. 1240).

Similar points are made, among others, by Nobel Laureate Douglass North (1981; 1990; 1994). Moreover in contrast to Robbins, Kaldor asserts that the most salient part of Adam Smith’s analysis pertains to the productivity benefits deriving from the division of labour through “dynamic economies of scale” (p. 1243) not the equilibrating role of markets. The point here is that resource allocation and resource creation may be hard to separate and that whether the creative or allocative functions in markets are most important may well depend on the issue at hand. For example, for issues involving change and economic development (North, 1994), or the growth of firms (Penrose, 1959), a focus on the creative-developmental aspects may be more appropriate than a focus on the allocative ones.

For North (1994), neoclassical theory is simply an inappropriate tool to analyze and prescribe policies that will induce development. It is concerned with the operations of markets, not with how markets develop. How can one prescribe theories when one doesn’t understand how economies develop?” (p.359).

For Penrose (1959), moreover, the neoclassical ‘theory of the firm’ “is but part of the wider theory of value, indeed one of its supporting pillars, and its vitality is derived almost exclusively from its connection with this highly developed, and still basically unchallenged, general system for the economic analysis of the problem of price determination and resource allocation” (p.11). While this theory serves a useful purpose, when ‘kept in its habitat’ (p.13), “Difficulties arise when an attempt is made to acclimatize the theory to an alien environment and, in particular, to adapt it to the analysis of the expansion of the innovating, multiproduct, ‘flesh-and-blood’ organisations that businessmen call firms” (p.13).

With respect to the relationship between technique, institutions and scarcity, a number of authors have suggested that the single most important determinant of the creation of knowledge and innovation in capitalist economies has been the capitalist firm (Penrose, 1959; Chandler, 1962, 1990; Baumol, 1991). Now knowledge is a resource, and one subject to increasing returns (Stiglitz, 1989). Institutions, intertemporal
efficiency through increasing returns and resource creation through knowledge and innovation (intertemporal efficiency), are linked in such complex ways, that to assert the exogeneity of technique and institutions may be questionable. Critically, the production of knowledge, engenders increasing returns and questions the optimality of “optimal industry structures” such as perfect competition and perfect contestability. This implies that apparently sub-optimal industry structures, such as big business competition, as well as non-collusive inter-firm cooperation (Richardson, 1972) may be more “optimal” from the point of view of resource-knowledge creation and (thus) intertemporal efficiency (Schumpeter, 1942; Penrose 1959; Chandler, 1962, 1990; Richardson, 1972; Nelson and Winter, 1982; Baumol, 2002).

The above suggest that both economizing and innovating, may well need to be part and parcel of economic analysis, and that reducing the one to the other or placing exclusive emphasis on the one at the expense of other may be unwarranted. Indeed, neoclassical industrial organisation (IO) scholars have spent significant resources in exploring the relationship between market structure and technological change (Baumol, 1991; Scherer and Ross, 1991) apparently disregarding Robbins’s advice. More recently whole schools of economic thought dwell on the nature, role and significance of innovation, see Fagerberg et al (2005) for a recent account. Some of this work has found its place in leading economics journals, as in the cases of Teece (1977), Dosi (1988), and Nelson and Winter (2002).

In addition and in partial recognition of their importance, some concerns of evolutionary economists and management scholars (such as increasing returns, knowledge spillovers, the importance of human capital and technological change) as well as the endogeneity of innovation, have more recently been recognised by endogenous growth theorists (e.g. Romer; 1986; Lucas; 1988) but also scholars of comparative institutions (such as Richardson, 1972; Williamson, 1985) and economic history such as Douglass North (1990, 1994). As already noted, the problem of some such scholars departs from Robbins in some important ways (see Stiglitz, 1989 and North, 1994).

To conclude, institutions and technique can affect relative scarcities, but also the very vehicles (such as market structures), with which we approach the study of economics. In this context considering them as data, outside the scope of economic analysis, may well unduly restrict the scope of the subject. The work of and indeed Nobel prizes to Ronald Coase (1937, 1960) and Douglass North (1981, 1990, 1994), who used transaction cost analysis to explain the firm and the law (Coase) and economic development (North), attest to that. In addition technical change impacts on (and is affected by) (optimal) market structures, making it difficult to explore the one by taking the other as datum. Importantly, one could well be justified to question even the direction of causality. For example Schumpeter (1942) and more recently the leading neoclassical scholar Harold Demsetz (1972), suggest that it is superior innovative capability (Schumpeter), or “differential efficiency” (Demsetz), that determine firm size and industry structure. Put simply, it may be innovation and efficiency that cause market structure, not the other way around. How interesting that Demsetz has used his view as a critique of the mainstream structure, conduct, performance model of IO!

In the past thirty years, or so, business scholars responded to increasing demands by students and business people alike, to understand, not just the economizing aspects of
modern capitalist firms, but also its strategising and innovating elements, by drawing
on neoclassical, transaction costs, resource-based, evolutionary and behavioural
views, such as those of Coase (1937), Schumpeter (1942), Penrose (1959), Cyert and
March (1963) and Nelson and Winter (1982). The result is some fascinating work on
the co-evolution of institutions, organisations, technological change, transaction costs,
resources and (dynamic) capabilities, and market structures and economic
performance, see for example Nelson and Winter (2002), Fagerberg et al (2005) for
some accounts. In a surprising turn, the neoclassical market structure analysis, has
been used by Porter (1980) to develop a “strategizing” (rent extraction through
monopoly power) approach to business strategy. Others, notably Oliver Williamson
(1991) lamented this, putting emphasis on economising, albeit in transaction (not
production) costs. The resource-based and dynamic capabilities views, have brought
production costs back in (see Penrose, 1959; Richardson, 1972; Wernerfelt, 1984;
Barney, 1991; Peteraf, 1993; Teece, 2007). The “systems of innovation” view drew
on the work of Schumpeter (1942), Nelson and Winter (1982) and others to focus on
the innovating aspects of organisations and institutions, and market structures. None
of these makes any value judgements and/or interpersonal comparisons of utility, such
as those that would lead Robbins to exclude them from positive economic science on
this basis. With notable exceptions (such as Coase, Williamson and North), no
genuine inroads into mainstream economics have been made from such approaches.
This is despite the ultimate recognition by the profession of many such scholars.
Perhaps the time has come for the discipline to embrace such contributions. As
originally noted by Robbins, it seems to us that it is not a waste of time to attempt
this, but “a waste of time not to do so” (NSES, p. 3).

5. Concluding Remarks

In the course of revisiting his original essay in his Richard T. Ely lecture of 1981,
Robbins reiterates his view that economics should be about “economizing” and that
“technique” and “institutions” should be viewed as ultimate data. This view has been
highly influential in the development of neoclassical economic theory. What has been
added since Robbins is the modern emphasis on the construct of optimal market
structures as vehicles for achieving static Pareto efficiency. It is this second feature, in
our view, that has led neoclassical economics astray in some important respects, not
least in encouraging a limited understanding of the relationship between market
structure and intertemporal efficiency.

Post-Robbins developments in neoclassical economic theory, notably in IO, and
endogenous growth theory, depart from the Robbins’s tradition of conducting
economic analysis on the assumption that “technique” can be treated as a given. In so
doing they acknowledge the importance of concerns of evolutionary, Schumpeterian
“systems of innovations” and management scholars, who have traditionally
emphasized the important role of innovation and technological change, not merely in
influencing relative scarcities, but also in affecting market structures and firm,
industry, resource creation and macroeconomic performance. The contributions of

5 Note, however that it is far too risky, to refer to particular references here, as the work amounts to
many hundreds of articles published in journals such as Academy of Management Review,
Organisation Science and Strategic Management Journal. Even a cursory look at any recent issue of
these journals would suffice to confirm our claim.
scholars such as Coase, Demsetz, Chandler, North and Stiglitz, have helped to add legitimacy to such concerns.

From the perspective of business, and business school scholarship, economizing, strategising and innovating are equally valid and interrelated concerns. Building on the work of economists such as those mentioned above, business school scholarship has developed some fascinating accounts of the co-evolution of markets, resources, knowledge, innovation, institutions, and firm and industry structures as well as macroeconomic performance. From a business school perspective treating “technique” and “institutions” as data is limiting, perhaps even boring. Perhaps the time is ripe to consider wealth creation and its key determinant, (technological change and innovation), as legitimate concerns of neoclassical economics too.
References


Is Robbins’s Definition Necessarily Imperialistic?
The Demarcation of Economics in Robbins’s Essay and the Concepts of Real and Formal Scarcity

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Abstract

This paper investigates whether the development of economic imperialism can be considered a direct consequence of Robbins’s definition of Economics and his characterization of real economic activities. Although we find that this approach does not follow directly from Robbins’s definition, we simultaneously find that some of his ideas have indirectly favoured its development. We show that a key element in understanding this influence is the association between real and formal scarcity that operates in his work and the synergy that the latter concept exhibits with some mathematical developments in the science.


1. Introduction

Robbins’s Essay can be considered one of the most influential books on economic science that has been written in the 20th century. This is attested to by the numerous reactions (both positive and negative) and vivid debates that this work has raised since its publication – see, for example, the exhaustive study by Backhouse and Medema (2007). Furthermore, many of the issues raised by the author in his book are still alive and exerting an influence on the development of the science, as indicated by the topics debated in this paper.

One of these issues and, at the same time, one of the most controversial points of the Essay concerns the limits of economic science. It is usually understood that Robbins’s definition of Economics, together with his argument that all human behaviour has an economic dimension, has directly favoured the unlimited expansion of the domain of economic science that took place after the publication of his Essay. According to this view, the “invasion” of non-conventional areas of knowledge by Economics has its roots in Robbins’s definition, which removed the old boundaries of the science, leaving the way clear for the aforementioned “expansionism”.

This paper investigates the extent to which this idea is correct, if at all. That is, we study whether the emergence of economic imperialism is a direct consequence of Robbins’s definition or not. Obviously, the answer to this question will determine if we can finish our research at this stage or if it must be continued one step farther. In particular, if this

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answer is negative, it will be necessary to clarify why Robbins’s definition has been taken to be imperialistic.

The paper is structured to answer the foregoing questions in the logical order in which they have been formulated.

2. Robbins’s definition and the subject-matter of the science: the concept of real scarcity.

Any study of Robbins’s definition cannot overlook the fact that it stemmed from his discontent with the existing definitions of Economics, which gave “material welfare” a central place in the science [O’Brien (1988:23)]. For this reason, it is necessary to devote some space to trace back the origins of this conception of the science.

2.1 The predecessors of Robbins’s definition.

As Kizner (1976:22-29) clearly explains, the existence of distinct subject-matter ripe for independent investigation was accepted by economists with little discussion from the very beginning of the science. Consequently, the definitions proposed by Adam Smith onwards followed a similar strategy: the science was defined as the study of (or knowledge about) subject-matter which was taken as something given – and in most cases synonymous with the concept of wealth. Thus, the resulting definition provides very little insight into the subject-matter of the science, and only tells the reader about the use that has been given to the term Economics: to denote studies whose main object of interest is “wealth”. To put it differently, this kind of definition is similar to what logicians call a nominal, “word-thing”, lexical definition: a definition that explains “the actual way in which some actual word has been used by some actual person” – see Robinson (1965:35).

According to the foregoing explanations, the definitions under consideration have the major shortcoming that the characterization of the science has to be postponed until the subject-matter is understood. Until this is correctly done it is not clear what the science deals with, and the definition remains incomplete. In this sense, this kind of definition requires investigating the nature of “wealth”. It needs to be emphasized that, in order to complete the definition of the science, such research should not aim at defining what the word “wealth” means – to provide a nominal word-thing definition of the term – but to ascertain what the true nature of “wealth” is, what constitutes “wealth” in reality – which, as Robinson explains, is an activity different from making definitions.


7 Notice that I am not claiming that they tried to produce a “word-thing” lexical definition. In fact, they might have tried to produce a thing-thing definition (see note 8) of Economics. The key issue is that they actually failed to produce it because their definitions were limited to reporting what had been understood by Economics up to the moment.

8 Using Robinson’s (1965: 149) terminology, they should not ask a question about the word “wealth” but ask a question about the thing “wealth”. As this author points out, though both types of questions have been
Although this is the kind of research that is undertaken by Adam Smith in his *Wealth of Nations*, the author does not provide a clear explanation of what constitutes “wealth” in reality. In fact, this term is given diverse definitions in the different parts of Smith’s book. As can be inferred from Cannan’s explanations (1903:14-31), these different definitions give rise to conflicting conceptions of wealth and this is where the distinction between productive and unproductive labour has its roots. This distinction is correctly designated by Kizner (1976:30) as the starting point of a lasting debate among economists about whether immaterial “utilities” should be included under the heading “wealth” or not. The problem is that, although all authors attempted to define “wealth”, the definitions they proposed were not of the same nature.

Thus, on the one hand, some authors have mainly tried to offer a real definition of “wealth” (if this kind of definition exists, see note 8), that is, authors who really inquired into the nature of the subject-matter of Economics. These authors invariably concluded from their research that actual wealth is also composed of nonmaterial goods or utilities – see, for example Say (1880/1964:119-127) and Lauderdale (1819:56-57). The problem is that the inclusion of nonmaterial goods within the scope of Economics seemed to be difficult to reconcile with its scientific character – which was firmly defended by (almost) all classical economists. This happened mainly for two reasons: (i) they introduced some vague elements into the language and concepts of Economics (i.e. what is meant by an increase, or decrease, in wealth); and consequently, (ii) they made studying the causes of the wealth of nations more difficult as well as measuring the changes in the wealth of a person or a country in empirical studies. Even worse, the limits of the science became blurred, because in this case the science of wealth would have to cover subject-matter from other sciences, and these kinds of definitions were accused of being imperialistic – on this point, see Malthus (1836/1986:23), and McCulloch’s criticisms of Lauderdale’s definition of wealth in Kizner (1976:30-31).

subsumed into the same name (definition) – see Robinson (1965: 12-16), they have different purposes. Moreover, as Robinson (1965: 171-178) goes on to explain, the search for answers to the second type of question has included several kinds of intellectual activities, which traditionally have been subsumed under the (false, according to Robinson) name of “thing-thing” definition.

9 It is clear that the work of Smith has the purpose of investigating the nature of real wealth – e.g. see the passages where he criticizes the way that wealth is commonly understood (Smith 1776/1994: 273, 371, 456ss). Even Robbins (1998: 128-129) describes the work of Smith as “(…) the work of a profound thinker anxious to discover the nature of things”.

10 It is clear that Smith has the purpose of investigating the nature of real wealth – e.g. see the passages where he criticizes the way that wealth is commonly understood (Smith 1776/1994: 273, 371, 456ss). Even Robbins (1998: 128-129) describes the work of Smith as “(…) the work of a profound thinker anxious to discover the nature of things”.

11 Thus, it was not possible to reach an agreement since, although all the authors believed that they were trying to do the same thing, this was not really the case. This kind of confusion can even be found in works by the same author. For example, take Cannan (1928:1): here, he argues that the question “what is wealth?” is exactly the same as “what is it most convenient to take as the subject-matter of Economics?” [emphasis added]. On the other hand, in Cannan (1903:1), he holds that “the first problem that confronts us is therefore the question of the nature of wealth that is the subject of production and distribution”. The first kind of question is a word-thing stipulative definition, whereas the second kind of question is a thing-thing or real definition (see note 8 and the text below).
On the other hand, other classical economists who were aware of these problems lost sight of the necessity to investigate the nature of “wealth” to close the definition of the science, and centred their attention on preserving the scientific character of their studies. Given the need to eliminate ambiguity from concepts in order to successfully pursue science, the definition of wealth and other elements should be as accurate as possible. As Robinson explains (1965:68-69), this is done by stipulating what it is going to be understood by “wealth”12. Thus, these definitions of wealth were not of the same kind as those of the previous group, for they were nominal, not real, definitions. In this sense, the authors who adopted this strategy usually agreed on reserving the term “wealth” to mean “material products” – see Malthus (1827/1986:234; 1836/1986:33-34) and Stuart-Mill (1848/1987:9 and 48). The adoption of this convention had at least two main advantages. First, it made the development of the science possible because, by removing the ambiguity introduced by real definitions, it became possible to establish precise language, as well as to accurately measure the changes in the wealth of a country – both necessary conditions for economic science, as we have seen. In addition, if wealth was understood as “material objects”, the limits of economic science seemed to be clear, since it was possible to trace an unambiguous division between the economic and non-economic areas of interest. Human behaviour could easily be divided into two separate domains (the economic and non-economic), according to whether the actions of the person were conducive to the production of the “material goods” that counted as “wealth” or not – see Kizner (1976: 21). This also helps to clarify why these earlier definitions of the science were apparently non-imperialistic.

2.2 The concept of real scarcity and the subject-matter of economics.

Obviously, each research strategy produced a different view of Economics. It is the second group of definitions, and those subsequent definitions that agree with reserving wealth to mean “material goods”, which are examined by Robbins in the initial pages of the Essay13. Here, Robbins detects a major shortcoming in the resulting definitions of the science, which is a consequence of the stipulation imposed on the meaning of “wealth”. As the meaning of this term is adopted by economists mainly to facilitate the scientific character of their studies, the restrictions that this convention imposes on the definition of Economics are found to be arbitrary as soon as they are subjected to scrutiny. First, the original definition of the science becomes a kind of tautology: Economics is the study of wealth, which, in turn, is what is defined by economists. Hence, as Robbins points out (1962:4), the definition of the science becomes incapable of describing its ultimate subject-matter. Second, the resulting division between the “economic” and “non-economic” spheres is found to be arbitrary because, as Robbins explains (1962:5-8), if

12 This does not mean that they define wealth in a completely arbitrary way – without taking into any consideration the real attributes of wealth – but that they artificially introduce limits by focusing their attention on some characteristics of real wealth, generally for the sake of clarity either in the language used or in the scientific concepts developed [see Stuart-Mill (1848/1987:46-53), Malthus (1827/1986: 4; 1836/1986:21-23 and ss.])

13 This is not to say that all the authors cited by Robbins were conscious of this fact: there were authors who recognized that they were “artificially” imposing limits to the subject-matter of the science [e.g. Pigou (1962:10-14)], while there were authors that remain silent in this respect [e.g. Marshall (1961:49,54-62)]. In any case, none of them can escape from the limitations detected by Robbins.
this conception of wealth is taken literally, the most relevant part of the generalizations of
the science fall outside the area of Economics.

Having detected these flaws, Robbins breaks with the tradition of taking it for granted
that wealth is the subject-matter of the science. But then, as a previous requisite for
proposing a new definition, he needed to characterize a new subject-matter for
Economics. And this is the issue that occupies the initial chapter of Robbins’s Essay, as
its title clearly suggests.

This inquiry leads Robbins (1962:14) to conclude that “every act which involves time and
scarce means for the achievement of one end involves the relinquishment of their use for
the achievement of another. It has an economic aspect” (emphasis added). Note that, in
this passage, Robbins is making reference to a real characteristic of human action, not to
a theoretical one: scarcity really conditions our behaviour, since it forces us to choose
among the different uses of means, sacrificing one use for another. This real trait of
human behaviour (i.e. scarcity as the conjunction of limitation of means with the
existence of alternative uses for them), which constitutes the subject-matter of the
science, is what I call real scarcity.

It should be noted that, though there are passages that clearly support my view [Robbins
(1962:78,83)], other authors defend that Robbins assimilates the subject-matter of
Economics with choice [e.g. O’Brien (1988:33)]. This view may have been favoured by
the lack of clarity of Robbins, which is a consequence of (i) the absence of a more
detailed analysis of the way in which real scarcity conditions human behaviour, (ii) the
confusion of concepts that takes place in his work. As this last point is explained in great
detail below, here I will limit to briefly elaborate point (i). Regarding this issue, Robbins
rapidly concludes (1934b:90; 1962:14) that when human behaviour is affected by real
scarcity, it inevitably assumes the form of choice. However, as Buchanan (1964:214)
complains, he remains silent regarding the entity of the chooser. This omission is the
result of his failure to notice that the level at which this problem of choice becomes
relevant can be different in each particular case: sometimes, it would be relevant at
individual level, while in other circumstances it would be only relevant at the social level.
This mistake leads Robbins to defend that when the individual has ample means to reach
a given end, his behaviour is not affected by real scarcity, and there is no economic
problem [Robbins (1962:13, 17)]. But this is a wrong conclusion because, even if the
economic problem is not relevant to the individual chooser, it still can be relevant to the
rest of society – see section IV.1 below. The example is also incorrect because it
implicitly associates the emergence of real scarcity with the size of the cost that it

\[14\] This is stated even more clearly in Robbins (1938:344).

\[15\] Obviously, this is an “opportunity cost”: the cost of foregone alternatives. It should be noted that the
argument of the text does not necessarily conflict with Buchanan’s (1969; 1973) approach to the subject.
Here, I am investigating the conditions under which an “opportunity cost” appears. These conditions can be
objectively determined, for they depend on the existence of alternative uses of a limited resource – as
Robbins claims (1962:17). To avoid confusion, this issue should be kept separate from the topics discussed
by Buchanan – e.g. is it always possible to calculate the size of this opportunity cost?, is the opportunity
cost subjective or objective in nature? Notice that the answer to the former question determines the
relevance of the latter, but not the contrary: if a given use of a means has no alternatives, nothing is given
imposes on human behaviour, whereas the relevant point to this question is whether means are limited and have alternative uses. When these conditions are fulfilled, our behaviour is affected by real scarcity, and there exists a cost of using means. On the other hand, the quantity of limited means that we have at our disposal influences the size of the cost that real scarcity imposes on our behaviour: the greater this quantity is, the lower this cost is — because the number of alternative uses that must be sacrificed is also reduced. And obviously, as this cost diminishes, our choice becomes less constrained. That is to say, whenever limited means have alternative uses our behaviour is constrained by real scarcity, independently of the fact that this constraint is currently binding or not. For this reason, means are scarce even when they are relatively abundant and we do not feel that our choices are constrained by real scarcity.

To these arguments it must be added that this characterization of the subject-matter of Economics incorporates its characterization in terms of choice, but not the contrary: when a limited means has alternative uses, a problem of choice appears; however there are problems of choice that are not caused by real scarcity — and, therefore are not studied by Economics. Taking the above into account, and despite the fact that Robbins is not clear in this respect, we reach the conclusion that real scarcity is a more accurate description of the subject-matter of the science than choice.

2.3. A preliminary assessment of Robbins’s definition.

Our research has led us to conclude that Robbins substitutes real scarcity for wealth as the subject-matter of Economics. With this change, Robbins’s definition improves upon its predecessors because: (i) contrary to the latter, it does not leave unresolved the problem of understanding the subject-matter of the science, (ii) it offers a better grasp of real economic problems and shows that the economic dimension of any action does not hinge on the materiality of the means. From this second point, it follows that Robbins’s definition resolves the futile discussions on the material and non-material dimensions of wealth, i.e. on the limits of the subject-matter of Economics. It is precisely this contrast with older definitions which make it seem imperialistic. Recall that the previous definitions imposed a limit on the scope of the science by dividing human activities into two independent spheres. Although this division was mainly based on stipulation and not on real grounds - and, in this sense, it was to some extent arbitrary [Robbins (1962:6)] - breaking with the established (though false) limits of economic science may give the impression that the definition necessarily advocates the invasion of other research areas.

But note that this is a wrong impression, because the main consequence of this definition is that, in so far as scarcity affects all human behaviour, this has an economic aspect or up when the person acts or chooses, so no opportunity cost appears. In this case, the second group of questions becomes irrelevant.

Notice that this does not imply that the opportunity cost disappears: each time we use a scarce means, it cannot be used for other purpose. Take the simple example of (monetary) wealth: the opportunity cost of spending money is very low for a very rich person. As he can dispose of a great amount of this scarce resource, the quantity of alternatives that he has to renounce when he spends one Euro is nearly insignificant. In this case his monetary constraint is not binding. However, this does not change the fact that each time he spends a Euro on a thing or activity, he cannot spend the same Euro on something different.
dimension – Robbins (1962:17). However, stating that any human action has an economic dimension is not imperialistic per se, for this does not prevent the recognition that it has social, ethical, or political dimensions – and that each dimension must be studied by its corresponding science. Hence, it does not follow from the notion of real scarcity that Economics provides the researcher with the correct tools to study all dimensions of human behaviour. Moreover, Robbins considers that the main advantage of this definition is that it improved our understanding of the subject-matter of the existing scientific generalizations [see Robbins (1962:1-3, 6-9 and 22)], not that it facilitated their extension to other areas of research – in fact, with the exception of war [see Robbins (1962:7)] there is no other possible excursion outside the “traditional” fields of Economics proposed by Robbins.

An additional piece of theoretical evidence that reinforces our arguments is supplied by the collective work edited by Radnitzky and Bernhold (1987): it is surprising that in a book entirely devoted to “Economic Imperialism” no reference to Robbins is made – if this approach necessarily followed from Robbins’s conception of Economics, one would have expected to find at least one reference to this author. Finally, and more relevantly, the term “economic imperialism” is already employed by Souter (1933) a fierce critic of Robbins’s conception of Economics – when referring to his own work. And, as Parsons (1934:512,522,535,545) explains, Souter’s “economic imperialism” is characterized by putting the neighbouring sciences into a straightjacket through the extension of the economic categories to cover the whole of concrete life. But this feature is also shared by the modern approach to Economics that has inherited this name, as we will see in the following.


It is not the fact that all human actions have an economic dimension that makes Economics an imperialist discipline, but the relentless application of the “economic method” to analyzing all human behaviour. In other words, the central element of economic imperialism holds that what distinguishes Economics as a discipline from others is not its subject-matter, but its approach, as Becker (1976:5) states. This “economic method” consists in the deduction of meaningful predictions about different kinds of human behaviour from the repeated application of a set of specific and restrictive assumptions about human beings. The core assumptions are the three stated by Becker (1976:5-6), namely: (i) individuals are rational maximizers, in the sense that their behaviour is forward-looking and consistent over time, and they correctly anticipate the uncertain consequences of their actions; (ii) individuals have stable preferences, which do

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17 It is worth emphasizing that this only implies that no human action can be a priori discarded as the possible object of study of Economics, since (almost) every human action has an economic dimension – which is a point accepted by both defenders [Hirshleifer (1985:53)] and opponents [Daly (1945:169-170)] of Robbins’s definition.
18 In fact, there is direct evidence that Robbins claims the independence of Economics from Psychology (1934b) and Ethics (1927). As Sanchez-Robles (1994:11-12) explains, from a correct reading of Robbins’s texts it follows that this author defends that there must exist a certain degree of cooperation among sciences in order to obtain valid findings, while at the same time each science must retain its autonomy. For a similar position regarding the relationship between Economics and its contiguous disciplines see Bye (1939:628-632), an author who agrees with Robbins’s definition.
not refer to market goods or services, but to underlying objects of choice that are produced by each household using these market goods and services, their own time and other inputs like human capital; (iii) there are markets that coordinate the actions and constrain the desires of the participants with different degrees of efficiency by providing prices and other instruments that allocate scarce resources within society.

Some authors add other “secondary principles” to these core assumptions. The most relevant ones are as follows: (i) incentives determine behaviour, so people do not act randomly but react systematically and predictably when they consider a possibility for action to be more advantageous or more disadvantageous; (ii) incentives are produced by preferences and constraints which are strictly distinguished, thus, behavioural changes are attributed to observable and measurable changes of the opportunity set determined by the constraints and not to non-observable and non-measurable facts [see Frey (1999:5-6)]. This latter principle is crucial, for it captures the way in which human behaviour must be explained by the “economic method”, if we wish it to be scientific: changes in human behaviour have to be attributed to observable and measurable changes of the opportunity set determined by the constraints. No resort to changes in preferences can be made, because these kinds of explanations are ad hoc, cannot be empirically verified, and hence are meaningless and unilluminating [see Frey (1999:6), Becker (1976:11-13)]. For this reason, the “imperialist” economist never uses preferences to justify an observed change in behaviour, but instead continues to search for the subtle forms taken by prices or incomes to explain it [Becker and Stigler (1977:76)].

As a consequence of this research strategy, the resulting explanations of human behaviour are stated in a language familiar to economists, that is, in terms of the traditional economic variables: prices, income, goods, demand functions, elasticities, etc. This is the distinctive trait that characterizes economic imperialism, for what the application of the “economic method” really does is reduce the explanations of very different kinds of human behaviour to economic categories or concepts – that is, in terms that were developed earlier in the history of economic theory [see Hirshleifer (1985:53) or Posner (1987:1-2)]. As it is clear that this procedure may generate distorted descriptions of human actions [see the examples Becker and Stigler (1977)], advocates of the “economic method” hold that it must be judged in terms of its predictive power, not in terms of the descriptive realism of its assumptions or explanations – see Becker (1993:402-403) and Mckenzie (1983:15-16 and 31). This not only protects the theories and their explanations against criticism, but also invests some fictions of economic theory with an aura of authority that may tempt the researcher into introducing them into other sciences – see section V.1 below.

Summing up, it can be said that economic imperialism is characterized by a dual condition: on the one hand, it is “reductionist” in terms of the method of analysis; on the other hand, it is “expansionist” regarding the explanatory power attributed to this method. This latter fact is reflected by the way in which “interdisciplinarity” is understood by economic imperialists: the application of this research method to many different non-

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19 Preferences are defined over fundamental aspects of life (health, prestige, benevolence, envy, etc.) that do not always bear a stable relation to market goods and services, and are assumed not to change substantially over time or even between persons – see Becker (1976:5).
traditional topics rather than the mixture of the methodological approaches emanating from various sciences [see Frey (1999:16)]. But, I have previously explained, this is not the view of Robbins (see note 18)\textsuperscript{20}, which reaffirms our conclusion that his definition is not necessarily imperialistic.


The two preceding sections have shown that Robbins’s definition does not necessarily lead to economic imperialism. However, this conclusion is challenged by the fact that the most relevant proponents of this approach have explicitly agreed with his definition of the science – see Becker (1976:8), Hirschleifer (1985:53) and Stigler (1984:301-302). In addition, Robbins is usually made responsible for the reductionism in terms of method of analysis and the invasion of other areas of research that characterizes the economic imperialist approach [see Marciano (2007:2-3)]. Thus, one may legitimately want to know the extent to which these intellectual positions are right, especially after reaching the opposite conclusion. In the following we centre our attention on the investigation of this point.

4.1. The alternative definitions of scarcity.

Only in the paragraphs that precede the explanation of what I have called real scarcity, Robbins characterizes the subject-matter of the science in a slightly different way: in these passages scarcity is understood as a relationship between limited means and the different ends to which they can be applied [Robbins (1962:12-13)]. It is clear that, to Robbins, both definitions of scarcity are equivalent, for he indistinctly employs one or another in his work. This is a consequence of the identification of “plurality of ends” with “alternative uses” that takes place in Robbins’s thoughts, who considers that these are only different ways of naming one of the conditions for the emergence of real scarcity – the other one being limitation of means. This subtle association was detected by Souter (1933:380), who additionally considered that as illicit. Two reasons can be given to support Souter’s criticism: first, “ends” of behaviour are not the same as “uses” of means\textsuperscript{21}; second, real scarcity cannot be determined by the number of objectives that the actor has in mind when performing an action or taking a decision.

\textsuperscript{20} Furthermore, Robbins admits that logically consistent theories can produce false explanations of facts in Economics [Robbins (1939:120-121)], which is not the view of economic imperialists.

\textsuperscript{21} Though the very idea of “use” refers to the existence of a purposive element in human behaviour (ends), it should be obvious that they are not the same reality. This is not the place to fully elaborate this point, but I will at least offer some suggestions concerning the distinction between the “uses” of a means and the “ends” of action. On the one hand, the “uses” of a means can be understood as the different ways in which it can be employed by the different members of society: these ways can be objectively determined (by technique, like goods) or subjectively determined (dependent on the chooser’s inventiveness, like time) – the existence of objectively determined uses of a means does not impede the existence of subjectively determined uses. On the other hand, the “ends” are equivalent to the “purpose” of action, that is, the outcome that it is intended to obtain – Mises (1996:92). Nevertheless, Robbins (1962,15n) understands ends in a more restrictive way: as the terminus of particular lines of conduct in acts of final consumption.
Let us consider this point. If the number of objectives considered by the individual was a factor that determined whether his behaviour is influenced by real scarcity or not, the subject-matter of the science would be subjectively determined, because ends of action are chosen by the individual – see Mises (1996:12). That is to say, the agent could easily get rid of the economic dimension of action (and of the problems raised by real scarcity) by simply disregarding some of the ends of his action and aiming at a unique objective. Consequently, it would be impossible to objectively determine if a given action is affected by real scarcity, for this would require knowing the number of objectives that are taken into account by the actor at the moment of choice. Clearly, this is not the position of Robbins, who considers that the influence exerted by real scarcity on human behaviour is an objective fact that does not depend on factors that can be subjectively modified – as it is shown by his defence that means can be scarce even if ends are not rational or consistent [Robbins (1934b:90; 1962:92)]. Hence, as long as real scarcity is an objective attribute of human behaviour, it cannot depend on factors that can be subjectively modified by the chooser, as it is the case of “multiplicity of ends”.

On the other hand, notice that the existence of alternative uses for a means is a condition that fulfils the last requirement. This can be easily seen as soon as we consider the case of man living in society: the uses of a given means are not only determined by the different ends pursued by the user, but also by the existence of different users that can employ the same means to reach their (possibly identical) ends. Clearly, this is the quality of real scarcity that Robbins wants to emphasize when he explicitly writes that scarcity denotes “(...) limitation with respect to demand. Good eggs are scarce because, having regard to the demand for them, there are not enough to go round” [Robbins (1962:46)]. However, Robbins fails to notice that this example reveals that multiplicity of ends is not a requisite for real scarcity, since in this case it affects human behaviour even if all consumers want eggs for a similar (and unique) purpose – which, on the other hand, is what happens under normal circumstances. To put it more technically, good eggs are scarce because they are rivals in consumption (see note 22). Even more, if we consider this property of rivalry, it is even clearer that multiplicity of ends cannot be a condition for the emergence of real scarcity, because, even if ends are multiple, no problem of scarcity appears when means are not rival. It is worth signalling that this is exactly the simpler idea of scarcity that has been proposed by other authors [e.g. Senior (1836/1965:7-8, 11-13), Bronfenbrenner (1962)]. Henceforth, if our analysis is accepted, this view of scarcity

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22 In other words, when man lives in society alternative uses of a given means appear simply because (i) there are many persons that can employ the same means, even though all of them want it for the same purpose, (ii) the use of a (quantity of a) means by a person impedes the use of this (quantity of a) means by others. But note that (ii) corresponds to the notion of rivalry in consumption [see Peston (1972:12-14)]. It should be taken into account that, as Foldvary (1994:12-13) explains, there exist more types of rivalry than “quantity rivalry”, so means can become scarce for different reasons.

23 Take the example of a public park: people can use it for different purposes, but no problem of scarcity appears, at least as long as it is not congested, – but this is a problem that requires further examination. Here, I will limit to comment that Robbins’s definition does not cover all types of economic problems, since pure public goods are not affected by scarcity as defined by Robbins – these goods are non-rival – but Economics does study the provision of this kind of goods. In fact, this problem can be viewed as a particular case of a more general type of scarcity, scarcity of incentives to undertake a given action – in the case of pure public goods, scarcity of incentives to provide them. This type of scarcity gives rise to a whole family of economic problems: how to foster motivation to undertake an action with the minimum cost of doing it.
comes to be a particular case of the characterization of scarcity that follows from Robbins’s writings.

This false association between “multiplicity of ends” and “alternative uses” may have been favoured by the fact that these alternative ways of characterizing the subject-matter of the science are not contradictory but, to the contrary, are complementary. Explaining the scarcity dimension of human behaviour as a relationship of means to ends brings to the forefront the purposive element of human behaviour which is implicit in the definition that put the emphasis on the alternative uses of means. Actually, “uses of means” exist because there are ends of actions: if people did not have ends to reach, they would not act, and means would not be used – see Mises (1996:92). As Robbins (1962:93) is aware of the crucial role played by the purposes of action, he might believe that substituting ends for uses would only serve to emphasize this fact.

4.2. The notion of formal scarcity.

Hence, if multiplicity of ends is not a condition for the emergence of real scarcity, one must wonder why this is the characterization of scarcity that has prevailed in Economics. To find the answer, it is necessary to take into account that Economics belongs to the category of knowledge that treats human behaviour as problem-solving, as Knight (1940a:464; 1940b:26-27) correctly explains. This is due to the fact that this science studies real scarcity with regard to the human problem that it gives rise to. As we saw above, real scarcity imposes a cost on every human action that cannot be avoided and should be taken into account in order to make a more intelligent use of the resources at our disposal. The point is that this problem of assigning scarce resources to different uses can take many different forms in reality, so it actually is a family of problems. In this sense, the task of Economics is to illuminate this family of problems, firstly by correctly understanding their foundations and secondly, by finding the most adequate solution to each particular problem. This, in turn, requires that the common elements of the different specific problems as well as their common structure are brought to the light in such a way that the influence of scarcity on human behaviour is correctly captured by the theory. It is just for this reason that economic theory is “analytical”, as Knight (1940a:462) defends: human behaviour is analyzed in terms of the fundamental categories that are present in each of these particular problems, so that cause-effect explanations can be deduced and the consequences of certain courses of actions (or choices) can be clearly established. Obviously, this is a kind of scientific knowledge that demands a formal representation of real scarcity.

This formal representation of real scarcity is developed in a two-stage process. In a first stage, human behaviour is framed in terms of two radical categories: ends and means. Accordingly, the elements that are going to play the role of ends and those that are going to play the role of means are clearly identified and distinguished in theory. But note that this clear-cut categorization is a formal and not a real requisite, since the different elements of human action do not admit of this radical classification. Recall that, as we saw above, ends are subjectively chosen by the individual, hence a given element can play the role of end or means depending on the desires of the chooser and the particular action considered. The very case of money, presented by Robbins (1962: 30-31) to deny
that money-making is the only motive of action that is relevant in Economics, is a good example of this indeterminacy: depending on the context considered, money is the end (the case of work) or the means (the case of consumption) of action. In fact, as Parsons (1934, 523-524) explains, human behaviour is articulated into a complicate chain of means-ends relationships, in which most of the elements are located between the polar categories of the “ultimate end” and the “ultimate means” [see also Crespo (2007: 376)]. In this intermediate sector, the end or purpose of a given human activity is the means to a further end or ends [see Kaufmann (1933:383)], hence the different links play the role of ends or means depending on the problem at hand. That is to say, ends (except the ultimate one) are as such only relative to the particular and immediate context of action, as Kizner (1976:126) points out.

But this complexity of human behaviour is not an obstacle to use ends and means as polar categories to analyze it. Notice that, at each stage of the sequence the influence of real scarcity on human behaviour gives rise to problems that have a similar structure. On the one hand, the immediate following links in the “string” are the elements that the actor wants to reach with his action – the ends. On the other hand, the last elements in the chain constitute the conditions that are going to be used by to reach the following link – the means. Finally, the tension that these elements exhibit in the theory formally captures the relative status of real scarcity and its influence on current choice24. And it is this common and simplified structure that is relevant to comprehend the foundations of economic problems and to solve them – the two aims of Economics. This helps us to understand why these elements of human behaviour are not relevant to Economics in themselves, but it is their relationship which interests economic science [see Robbins (1962:38)]; and also why Economics is not concerned with the process of selection of ends, though being a real problem, as Knight (1940a:464-465) defends.

In a second stage, it is assumed that those elements that play the role of ends in the theory must not only be ordered25 but also weighted in such a way that gradual comparisons among them are possible. This is a necessary assumption in order to illuminate real human economic problems with the economic science for, otherwise, they would not be relevant in formal terms. To see the point, notice that it is possible to have hierarchically ordered ends for which no rates of interchange or sacrifice among them (or among goods) can be established – take the example of the lexicographic preferences. In this case, a formal problem would still exist, though being a trivial one: the means should be allocated to reach the most preferred end that remains in the list. And the problem is trivial from a theoretical point of view because means cannot be redistributed to produce a preferred (or indifferent) combination of ends; hence economic science cannot be of

24 To formally capture the influence of real scarcity it is required that a multiplicity of ends (or “links”) is recognized, in other case the economic dimension of the problem will be overlooked. On the other hand, the influence of the ultimate end on the immediate ends of action is indirectly brought into scene by the introduction of and unspecified factor that serves as an homogenising magnitude that classifies these multiple intermediate ends – this is the role played by utility in modern Economics, which is an empty concept [see Etzioni (1988:29-31)].

25 Notice that, if ends are not hierarchically ordered, it is not possible to work out in theory the opportunity cost of displaced alternatives – which is the way in which economic theory understands costs, as Robbins (1934a:2-3) explains. However, this does not imply that this cost does not exist – as explained in note 15.
much guide. So, for a problem to be relevant to economic theory, not only is required the existence of a hierarchy of ends, but also the possibility of quantifying how much of one end a person is willing to give up to obtain more of another – the Hicksian marginal rate of substitution [see Hicks (1981:8-10)]. It is for this reason that, as Knight (1934:232n) explains, the indifference surface is indispensable if comparison and choice are to be used in the sense of cause and effect to explain behaviour. The key point is that, again, this is a formal requisite for producing a theory that is useful for practical purposes.

This view of scarcity as a relationship of limited means to multiple ends, that are hierarchically ordered and can be gradually compared, is what I call formal scarcity. It is formal because, as should now be clear, the characteristics explained above are requisites for capturing in theory the influence of real scarcity on human action, and are not conditions for its emergence. In conclusion, the conception of scarcity as a relationship between multiple ends and limited means is a scientific description of real scarcity, but it is not a good characterization of the conditions that determine the emergence of the economic aspect of human behaviour.


Contrary to our foregoing explanations, Robbins considers that the previous requirements are real conditions for the emergence of real scarcity, and not only formal methods of capturing it in theory. Robbins’s formulation of the conditions under which the division of Robinson’s time between income and leisure has an economic aspect exemplifies this confusion [see Robbins (1962:12-14)]. His analysis is misleading because, in this example, Robinson’s behaviour is affected by real scarcity even if he is not able to make a decision. Notice that, in this last instance, he is also incurring in an opportunity cost: the outcome he could have obtained if he would have made this decision – in this sense, by not choosing at all, the chooser spends time in doing nothing and wastes a scarce resource. Hence, the existence of a hierarchy of ends is irrelevant to the emergence of real scarcity, though it is relevant to the science.

Robbins thinks that the clear-cut means-ends division and the ordering of preferences are basic postulates of the science, in the sense that they are applicable whenever the conditions that give rise to economic phenomena are present and do not require controlled experiments to establish their validity – they have only to be stated to be recognised as obvious [Robbins (1962:72-80)]. Hence, to Robbins, the categorical distinction between ends and means takes place in reality: there are some elements in

26 As marginal utility is not quantitatively definable in the ordinal utility approach, the indifference surfaces are necessary to know what quantity of a good would compensate the chooser for the loss of a marginal unit of another good [see Hicks (1981:9)]. In the case of lexicographic preferences it is not possible to calculate this quantity because the indifference curves are single points. This problem is relevant only from a theoretical point of view: it affects the capacity of the theory to illuminate behaviour, but does not alter the fact that this behaviour is conditioned by real scarcity. This point should be kept in mind to avoid the kind of mistake made by Polanyi (1994:100), who erroneously considers that the example of the text is a case of insufficient, but not scarce, means.

27 In this respect, Samuelson and Zeckhauser (1988:8) explain that doing nothing or maintaining the status quo is an alternative that is present in (almost) all real-world decision problems.
human behaviour that, considered by themselves, will always play the role of ends, while there are other elements that will always play the role of means. In addition, Robbins—who was clearly influenced by Hicks’s (1981) development of the concept of utility, and who also held a formal view of human beings that was almost certainly adopted from Wicksteed, as O’Brien (1988:25) explains—firmly believes that everyday experience shows that individuals do actually arrange their preferences in an order. This way of thinking implicitly associates real scarcity with the way it is represented in theory, so the formal characteristics that serve to represent scarcity in the theoretical constructions are considered to be attributes of real scarcity. As a result, real scarcity (the subject-matter of Economics) and formal scarcity (the way real scarcity is studied by Economics) are confounded in the thoughts and works of Robbins— and this confusion is also reflected in his definition of the science.

Simultaneously, as formal scarcity is a stylized way of capturing the problem of choice that appears when real scarcity conditions our behaviour, this confusion of concepts that is inherent in Robbins’s arguments facilitates the expansion of the belief that choice, instead of real scarcity, is the subject-matter of Economics.

5.1. The consequences on the trend of economic thinking.

To understand why the foregoing association is relevant to the development of the economic imperialist approach it is necessary to make reference to the subsequent mathematical developments in Economics—concretely, the extension of the use of constrained maximization problems. The first step in this direction was pioneered by Hicks (1939/2001:89ss) who showed that constrained maximization problems can be used to analyze the main problems of choice in Economics: the theory of consumer and firm behaviour. Notice that in this kind of mathematical formulation the economic problem is represented as Robbins’s formal view of scarcity demands. First, the “ends” (maximization of the objective function) and the “means” (constraints) are clearly differentiated. Second, ends are not only hierarchically ordered, but in most of the cases they are weighted. Consequently, it is possible to make gradual comparisons among ends.

The second step was taken by Samuelson (1983:21-23,350), who promoted the application of this mathematical tool by demonstrating that constrained maximization (minimization) problems can be systematically used to derive meaningful theorems in

28 For example, the confusion between real and formal scarcity is at the heart of the disagreement between Cannan (1932, 426-427) and Robbins (1962:11), and it is the reason why both authors offer (partially) correct arguments and cannot reach an agreement. On the one hand, Robbins argues that the decisions regarding the use of time have an economic aspect and belong to the subject-matter of the science. According to our explanations, this is a correct appreciation, since these decisions are affected by real scarcity—note that they imply giving up alternatives. On the other hand, Cannan argues that most of these problems are “problems of life”, in the sense that they cannot be fruitfully illuminated by Economics. This is also a correct appreciation, since most of these problems cannot be productively analyzed in terms of formal scarcity, that is, this analysis provides no additional insight into their nature nor sheds light on their solution. This is due to the fact that, as Rivett (1955:218-219) correctly points out, “(…) all problems of Economics are problems of economy, but not all problems of economy are illuminated by Economics (…)”. This distinction raises the question of which kind of (economic) problem can be fruitfully illuminated by Economics, which is an issue that deserves further investigation.
economic statics. For this reason, it can be said that Robbins’s and Samuleson’s works complement each other: Samuelson put the formal model to work and obtained refutable results, whereas Robbins provides the link between these formal constructions and real scarcity, which he had previously demonstrated to be the economic dimension of real problems. In fact, many other points where the two works reinforce each other appear so that it can be said that there exists a synergy between them: the technical methods used by the economists to reach their conclusions – which were updated by Samuelson – are dressed with a real, and in many cases anthropological, content – which was the legacy of Robbins. To put it differently, the mathematical formalization of the models preserves the scientific character of Economics while the anthropological explanations indirectly invest economic models with the character of formal theories of human action.

It is possible to make explicit the implicit process that has taken place in Economics as a consequence of the triumph of Samuelson’s views and the synergy between his work and Robbins’s work. First, the resemblance that the structure of constrained optimization problems bears to the way in which formal scarcity analyzes choice facilitates the identification of this mathematical tool with the problems of choice that are studied with this concept. Second, this association – together with the expansion of the view that choice is the subject-matter of the science – leads to the conviction that whenever a real economic problem exists, it can be analyzed with a constrained optimizing problem – and vice versa, whenever a given problem can be formulated in terms of a constrained optimization problem, it has an economic dimension. Consequently, the economist reads an empirical content in his mathematical tools, which now are confused with real economic problems and are no longer view as a method of representing and analyzing them. In addition, the act of economizing – the rational disposal of goods – is also assimilated to this type of problems, and “rational behaviour” is conceived just like solving a constrained maximization problem. This chain of associations transforms the ultimate object of study of the science from real scarcity into a choosing, maximizing entity, as Buchanan (1975) denounces.

Other complementarities between the two works can be signalled. The most obvious is the defence of the ordinal utility approach to obtain the relevant results of the theory of demand [see Samuelson (1983:4, 97-98) and Robbins (1962, 84-86)]. Also, Samuelson (1983:103) explains that constrained maximization problems reach the same optimal solution either by maximizing a given end subject to an expenditure constraint, or by minimizing the cost (or expenditure) of reaching a certain goal, which parallels the two different definitions of the act of “economizing” that appear in Robbins writings: (i) the rational disposal of goods [Robbins (1962: 91-92)]; (ii) the securing of given ends with the least means [Robbins (1962; 145)], which in other works is identified with the “economic motive”(the desire to increase his power to satisfy ends in general) [Robbins (1939:115-119)]. Finally, Samuelson (1983:4, 20) rigorous explanation of “meaningful theorems” and “qualitative predictions” (i.e. predictions about the sign or direction of the change in the variables in response to changes in parameters) reinforces the idea defended by Robbins [(1962:99-101,108-112), (1981, 3-4)] that the explanations provided by the economic science are mainly qualitative in character.

The experimental studies of animal behaviour are examples of this association [see, for example, Kagel, Battalio, Rachlin and Green (1981a,b)]: as the scientists can explain the results of their experiments in terms of a mathematical model of constraint optimization, it is concluded that pigeons are solving an economic problem of choice among commodities. This problem is also detected and criticized by Polanyi (1994:91-103), who tries to solve it by distinguishing between the “formal definition” of Economics is intermingled and the “substantive definition” (the different ways in which human beings obtain their subsistence).
Also as a consequence of the foregoing chain of associations, economists do not have the impression that they are doing something mechanical or technical when solving their mathematical models. Instead these mathematical models are understood as anthropological (or real) explanations of human beings, so Economics is raised to the level of (scientific) anthropology – see Hirshleifer (1985:53). That is to say, in the first instance Economics is understood as a theory about human behaviour – more precisely as a science of choice [Mundell (1968)] – and, in the last instance, as a theory about human beings. This is shown by the fact that, in many cases the researcher makes the mistake of raising some economic fictions to the level of anthropological theories, i.e. to the level of descriptions of how human beings actually are [see Mckenzie (1983:5-7)]. The most prominent case is the homo oeconomicus construction, which is considered by Robbins to be no more than a mere expositional device [Robbins (1962:94-99)], but that some authors consider correctly depicts human beings – see Stigler (1982:24-26, 35).31

This also determines the way in which scientific progress is conceived: our understanding of real economic problems is improved either by advancing in the mathematical foundations of the theoretical problems, or by applying the existing mathematical tools to new kinds of problems. This explains the trend followed by economic research that has been denounced by Coase (1978:207): “(…) there are, at present, two tendencies in Economics which seem to be inconsistent but which, in fact, are not. The first consists of an enlargement of the scope of economists’ interests so far as subject-matter is concerned. The second is a narrowing of professional interest to more formal, technical, commonly mathematical, analysis”. Economic Imperialism represents the extreme intellectual position generated by this trend of thinking: it pretends that the relentless application of the mathematical tools and concepts of Economics will surely increase our understanding of all kinds of human behaviour – and this is what should be questioned, as Mckenzie (1983:96) points out.

It is now possible to reconcile the position of those authors who see Robbins as a predecessor of economic imperialism with my defence that Robbins’s definition is not imperialistic. Though it is clear that Robbins’s definition, if properly understood, does not result in an imperialistic conception of Economics, the confusions that are implicit in his work, together with the synergy that the concept of formal scarcity exhibits with some mathematical developments of the science, have indirectly determined the subsequent trend of economic thinking – and have favoured the emergence of economic imperialism. For this reason, it is possible to state that Robbins has indirectly facilitated the development of the economic imperialist approach; though other elements not considered here may have been more influential.

31 There is no consensus among the defenders of economic imperialism regarding this point. Some authors, like Frey (1999:7-8), and especially Stigler, cite self-interested behaviour as being one of the basic hypotheses of the economic approach. At the other extreme, Becker (1993:386) maintains that preferences are not necessarily egoistic, but include a wide range of motivations.
6. Conclusion

At the beginning of the paper, I planned to investigate whether Robbins’s definition necessarily leads to economic imperialism. The subsequent analysis has shown that not only is Robbins’s definition not a necessary condition for economic imperialism, as some authors have suggested [e.g. Marciano (2007)], but that it is not a sufficient condition for it either: agreeing with Robbins’s definition does not necessarily lead to economic imperialism. As this finding sharply contrasts with the extended idea that Robbins’s conception of Economics is imperialistic, I have pursued my research one step further and I have also investigated whether this view is correct. This second part of my inquiry has revealed that there are elements in Robbins’s work that support this belief, though his influence on the development of economic imperialism is more subtle than usually defended. Hence, it is more adequate to say that Robbins’s work has indirectly favoured the development of the economic imperialist approach.

This perplexing conclusion cannot be properly understood without making reference to the concepts of real and formal scarcity that are latent in Robbins’s Essay. These concepts are related to what Kizner (1976:119-124) calls the “breath” and the “formalism” of Robbins’s definition, respectively. On the one hand, the notion of real scarcity refers to the real conditions in which human behaviour presents an economic dimension (the subject-matter of the science). In this sense, the “breath” of Robbins’s definition is a consequence of the change in the subject-matter of the science that it introduces: by substituting real scarcity for wealth, this definition shows that the economic aspect is present in (almost) all human behaviour and that it is not limited to a certain type of action, as the previous definitions assumed.

On the other hand, the concept of formal scarcity refers to the method of analyzing the influence of real scarcity on human behaviour. Economics studies real scarcity with regard to the (human) problems of choice that it gives rise to. Then, the “formalism” of Robbins’s definition refers to the way in which the notion of formal scarcity characterizes these problems in order to illuminate them.

If both concepts are clearly distinguished and are kept separated, it becomes clear that Robbins’s definition is not imperialistic, for it only implies that every human action has an economic dimension, and not that all human actions influenced by real scarcity can be fruitfully illuminated by formal scarcity. The problem is that these concepts are mixed in the work of Robbins, and this makes the demarcation of the science blurred – which leads to the false conclusion that Robbins’s definition is imperialistic. This confusion has exerted a great influence in the progress of Economics due to the synergy that the notion of formal scarcity exhibits with some mathematical advancements in the science. The evolution of economic thinking that this association has favoured ultimately leads to the emergence of economic imperialism.
References


Peston, Maurice, (1972), Public Goods and the Public Sector, McMillan: London, etc.


Economics as a Moral Science

A B Atkinson

Abstract

“Economics deals with ascertainable facts; ethics with valuations and obligations. The two fields of enquiry are not on the same plane of discourse” (Lionel Robbins, The Nature and Significance of Economic Science, 1932, page 132).

“As against Robbins, Economics is essentially a moral science. That is to say, it employs introspection and judgement of value” (Lord Keynes, writing to Sir Roy Harrod).

1. Introduction

These two quotations illustrate well the subject of this lecture. The first is from Chapter VI of Robbins’ The Nature and Significance of Economic Science. The chapter opens with a totally justified criticism of the view that “developments in modern Economic Theory furnish by themselves a set of norms capable of providing a basis for political practice” (page 120), but goes on to argue for the complete separation of ethics and economics: as he says, “the two fields of enquiry are not on the same plane of discourse” (Robbins, 1932, page 132). With this, Keynes took issue. He argued that economics is essentially a moral science.32

If I have to choose between these two positions, then I would vote with Keynes. But the difference between them is in fact less stark than the two quotations suggest, and there is a lot of common ground. Indeed, in a later article in the Economic Journal, Robbins said that he was distressed if his Essay has suggested to the outside world “a disunity among economists which I am persuaded does not exist” (1938, page 640). Robbins is clearly right, in my view, in asserting that there are two different reasons why economists may disagree. We may disagree about the way in which we believe that the economy works; or we may disagree about the criteria to be applied in judging economic performance. A good example is provided by the 2 per cent tax recently introduced in France on the sale of fish, with the proceeds used to compensate fishermen for the rise in the price of diesel. In the debate about this policy, people may object on the grounds that the tax will not have the intended effect: that the tax will be borne by the fishermen. Such a statement depends on how we view the determination of prices in the market and on the relative elasticities (incidentally, Robbins took the price elasticity of demand for herring as an

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32 I owe this quotation to Wright (1989, page 473).
example in another chapter of *The Nature and Significance of Economic Science*). This is a disagreement of the first kind. Or, we may object to the policy on the grounds that there is no reason for giving priority to the compensation of fishermen. In the latter case, we are questioning the welfare criteria applied. It is a disagreement of the second kind.

Where I part company from Robbins, is that I believe that questioning the welfare criteria is a legitimate part of economics. My position is in fact that adopted by Samuelson in his *Foundations of Economic Analysis*:

> “Robbins is undoubtedly correct. … ethical conclusions cannot be derived in the same way that scientific hypotheses are inferred or verified. But it is not valid to conclude from this that there is no room [for] “welfare economics”. It is a legitimate exercise of economic analysis to examine the consequences of various value judgments” (1947, page 220).

The main thrust of this lecture is that welfare economics is not only a legitimate exercise, but that it is an exercise to which economists should devote more time and attention. Economics is a moral science. Welfare economics should be a central part of the discipline. But it is not. While welfare economics was a subject of importance half a century ago, today it has largely disappeared from the mainstream, and this is my starting point in Section 2. Yet economists go on making welfare judgments, and Section 3 examines the – undiscussed – assumptions that underlie these welfare judgments. I then in Section 4 take two concrete examples of current policy issues where I believe that we can learn from an examination of the underlying welfare economics. Economics is not only a moral but also a very relevant science.

2. **The Strange Disappearance of Welfare Economics**

Even if Robbins felt that what he was saying was not controversial, his “celebrated attack” on welfare economics, to use the phrase of Sen (1970), generated a strong response. The leading economic theorists of the day – Pigou, Harrod, Hicks, Kaldor, Lange, Samuelson and Scitovsky– all actively engaged in exploration of the foundations of welfare economics. The survey of “Welfare economics, 1939-59” in the *Economic Journal* by Mishan (1960) referenced more than 60 articles on the theory of welfare criteria, with titles such as:

> “Welfare propositions in economics” (Kaldor)
> “The foundations of welfare economics” (Hicks, Lange, and Little)
> “Some aspects of welfare economics” (Pigou)
> “Evaluation of real national income” (Samuelson).

There were important books on welfare economics by Little (1950), Baumol (1952), and Graaff (1957). These were all doctoral theses; welfare economics was the subject on which the best young scholars were working.

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33 This is the title of an earlier article, Atkinson (2001), on which I have drawn in this section.
As it was put by Arrow and Scitovsky in their Introduction to *Readings in Welfare Economics*, 1969, “recently, welfare economics has greatly increased in importance. ... Economists want to know exactly what they are after, what is the meaning, the limitations, and the importance of economic efficiency and economic progress.” The Editorial Forward to Winch’s *Analytical Welfare Economics*, 1971, claimed boldly that “welfare economics is back in fashion”.

But the 1960s were the high water mark. From 1970, in fact all went quiet. Welfare economics was side-lined. Today there are relatively few journal articles on welfare criteria. For example, the 2006 volume of the *Economic Journal* contained, in the regular issues, some 46 articles, totalling more than 1100 pages, but not one dealt with welfare criteria or the foundations of welfare judgments. There are few textbooks written on “welfare economics”. Nor do many departments offer courses in welfare economics. In many places, Oxford included, welfare economics has been incorporated into micro-economics courses or into courses called “general equilibrium and welfare economics”. Certainly that seems to be the case with textbooks. Indeed, in most micro-economics textbooks welfare economics has been marginalised. According to Kreps, “we will touch on the efficiency of various institutions, although this will be relatively deemphasised” (1990, page 7). The widely used textbook by Varian in one edition (1993) described welfare economics as an “optional extra”.

**Practice**

Welfare economics has largely disappeared from sight, a disappearance that is strange in the sense that economists have not ceased to make welfare statements. I should indeed stress that I am not asserting that economists have stopped offering policy advice, nor that they have stopped writing papers containing welfare propositions. Just taking the first 15 of the 46 articles in the 2006 *Economic Journal*, I found references to “optimal community grants”, “efficient and inefficient equilibria”, “the social welfare consequences of indexation”, “the policy maker’s loss function”, and “the welfare effects of regulatory adverse selection”, each drawn from a different article. These articles are reaching clear normative conclusions: for example, in the last case “legislation prohibiting the use of genetic tests for [life assurance] ratemaking may increase welfare” (Polborn, Hoy and Sadanand, 2006, page 327).

There is moreover a public demand for such normative statements. The central issues of global policy, such as climate change and world poverty, involve the setting of targets and the use of criteria to judge progress. To take the latter example, the World Bank regularly estimates the number of people living on less than $1, or $2, a day, as a means of monitoring progress towards the 2000 Millennium Development Goal of halving by 2015 the proportion of people living in extreme poverty. At a national level, in the United Kingdom the Government is committed, following a historic announcement by Tony Blair in 1999, to the reduction of child poverty, the aim being to halve it by 2010 and its total eradication by 2020.
Perhaps the most striking arena within which policy goals have been most debated is that of the European Union (EU). One of the little noted features of this new political organisation is that forming common policy has required different Member States to make explicit their objectives to a degree that was not previously the case. The formation of the EU has forced governments to agree on yardsticks to assess performance. What may have been taken for granted in national political debate has to be spelled out. A good example is provided by the 2000 Lisbon Agenda, which identified the EU primary goals as growth and employment, coupled with greater social inclusion. To these three pillars, an environmental dimension was subsequently added in June 2001 when a strategy for sustainable development was adopted by the EU. These four pillars form the central elements in the Shortlist of Structural Indicators now used to assess the performance of the EU.

The EU Structural Indicators, shown in Table 1, cover six domains: general economic background, employment, innovation and research, economic reform, social cohesion and the environment. Contemplation of these indicators raises a number of questions, and they could do with critical examination. I return to those concerned with growth and employment in section 4, but for the present I want simply to make the point that there is a yawning gulf between, on the one hand, the policy world where objectives and targets are central to political discourse and, on the other hand, economic theory where there is very little discussion of what may underlie such performance criteria. In what follows, I want first (Section 3) to tease out what is implicit in the welfare statements that economists do make today and then (Section 4) to seek to demonstrate what we can learn from a deeper analysis of welfare criteria.

3. What Underlies Welfare Statements Today?

Academic journals are replete with welfare statements. Economists do not confine themselves to positive statements – determining elasticities and explaining mechanisms. They are making welfare judgments, such as the following:

“The optimal policy is …”
“In this article, we examine the welfare consequences of …”,
“This change would increase social welfare.”

However, what is the underlying justification? From reading these and other propositions in the recent journal literature, I have formed the view that there are three main ways of interpreting what economists do.

Representative agents

The first approach is to assume away differences in all relevant economic interests. Many macro-economic models are populated by identical households, often described as “representative agents”. It is then assumed that changes in social welfare can be judged according to whether the representative household is better or worse off. I emphasise that it is a further assumption, since even if everyone were to be identical, there might be
reasons why social judgments go beyond what enters individual utility. As is remarked by Samuelson, “one does not have to be a John Donne … to find fault with the above assumption” (1947, page 224). For example, we may believe, as a society, that there are merits in a higher level of overall education. An educated society may be able to operate more effectively as a democracy. Such cases where we may over-ride individual preferences have been described by Musgrave as “merit goods”. I return to this concept later.

But, even if we stay within the framework of strictly individualistic welfare, we must certainly feel constrained by the assumption that everyone has the same interests. In most real-world policy decisions, there are conflicting interests. The assumption of a single representative agent may suffice for modelling macro-economic behaviour (although even this is arguable – see Solow, 2008), but it rules out most interesting welfare economic problems. If we just take the classic example, much used in the earlier welfare economic literature, of the reform of the Corn Laws, then at the heart of the political debate was the conflict between landlords and manufacturers, with the interests of the working class also coming into play. Today, the dividing lines may be rather different, but they exist and lie at the centre of many differences of opinion. For example, in the debate about labour market reform in Continental Europe, there are differing interests for workers in established jobs and those who are outsiders. It would not be possible to discuss the desirability of such reforms without recognising these different interests. In the debate about pensions, there are conflicting interests of different generations.

This last example brings us indeed to an awkward question. What about the unavoidable fact that we were born at different dates? All members of a birth cohort may be identical, but their consumption will inevitably occur, at least in part, at different dates from that of their parents. The typical answer to this question is that intertemporal differences are subsumed in a dynastic utility function, which takes account of all future consumption. Better or worse off is judged according to the sum of future discounted utility for infinitely-lived dynasties. It is assumed that those present today take into account the interests of succeeding generations. Just to give one example, this approach is used by Lucas in his (1987) calculations of the cost of business cycles and in his (2003) Presidential Address to the American Economic Association.

This may be a logically satisfactory answer, but it is not one that is easy to explain to non-economists. Since there is often more than one adult generation of a dynasty, they may reasonably ask – whose dynastic welfare function? Are we saying to 50 year-olds that their welfare is judged by their 75 year-old parents? Or the reverse? If the reverse, when does the baton pass? The uneasiness surrounding this construction is apparent when we consider the issue of the rate at which future utility is discounted (note that I am talking here about the discount rate applied to utility, not to the rate at which future consumption is discounted, which takes account of differences in how well-off future generations will be). The current dynastic head may apply quite a high rate of discount. Indeed, Lucas in his 1987 calculations uses a discount factor of 5 per cent, which means that the utility from consumption in 2025 is valued at under half today’s utility. The appropriate rate of discount to apply is a subject of controversy, as has been evident from
the debate following the Stern Report on Climate Change. Stern (2007) argues that, in terms of utility, only a very low rate of discount is justified. He works with 0.5 per cent, which implies that the utility from consumption in 2025 is valued almost as much (92 per cent) as today’s utility. And Ramsey, in his original article on optimal savings, argued that *any* discounting is “ethically indefensible” (1928, page 543).

**Assumed agreement on a utilitarian welfare criterion**

The second approach does not assume away differences between people. There are different interests – of capitalists, workers and landlords, or of insiders and outsiders, or of rich and poor. But it is assumed instead that there is agreement on the welfare criterion to be applied. Most economists today would, it is assumed, follow Robbins, who in 1938 said that “my own attitude to problems of political action has always been one of what I may call provisional utilitarianism”:

> “as a first approximation in handling questions relating to the lives and actions of large masses of people, the approach which counts each man as one, and, on that assumption, asks which way lies the greatest happiness, is less likely to lead one astray” (1938, page 635).

The Presidential Address of Lucas to the American Economic Association, given 65 years later, stated that

> “To evaluate the effects of policy change on many different consumers, we can calculate welfare gains (perhaps losses, for some) for all of them, one at a time, and add the needed compensations to obtain the welfare gain for the group” (2003, pages 1-2).

Lucas appears to regard this statement as self-evident: he describes it as “the general logic of quantitative welfare analysis”. However, this statement disregards the many objections that have been raised to this utilitarian approach. To begin with, we may not be content to add the welfare gains: the sum takes no account of how the utilities are distributed. As it was put by Sen, “maximizing the sum of individual utilities is supremely unconcerned with the interpersonal distribution of that sum” (1973, page 16). We may therefore want to consider more generally the Bergson-Samuelson individualist social welfare function, $W[U_1, U_2, U_3, ...]$, and this does indeed appear widely in all areas of economics in which welfare judgments are made.

The more fundamental difficulty with this approach is that it does not take account of the fact that moral philosophy has moved on beyond utilitarianism, and fails to recognise that there are plurality and diversity in the welfare criteria that could be applied. *Plurality* refers here to the fact that a single person may bring to bear more than one set of welfare

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34 I am not entering here into the problems that arise with the aggregation of money measures of changes in individual welfare (see for example the review by Donaldson, 1992).

35 Following Graaff (1957, page 9n), I insert the word “individualist”, because Bergson (1938) contemplated the case where the arguments of the social welfare function are not necessarily individual utilities; in most subsequent usage, this qualifier has been dropped.
criteria. A person may for example be concerned with the greatest happiness, but also with personal liberty. To cite Robbins again, in 1977 he gave a lecture entitled *Liberty and Equality*, both of which he recognised to be legitimate concerns. These two criteria may point in the same direction, but they may also conflict. *Diversity* refers to the fact that different people hold different sets of values. One may be concerned with personal liberty and another person with social justice. Where there are multiple welfare criteria, then it makes no sense to talk about *the* welfare consequences; instead we have to apply multiple criteria and consider how conflicts may be resolved.

In particular, welfare economics needs to take account of the alternatives to utilitarianism that have been advanced in the past half century, such as the theory of justice of Rawls (1971) and the concept of capabilities introduced by Sen (see, for example, 1985). The theories they have advanced are complex and, in their application to economic policy problems, have been grossly simplified by economists, myself included. When the work of Rawls was first discussed by public finance economists in the early 1970s, we tended to pay more attention to his difference principle than to his first, and lexically prior, principle of basic liberties. The difference principle required that inequalities in a society should work to the greatest benefit of the least advantaged. This appealed to economists, since they could see the Rawlsian principle as a limiting case of giving more weight to those less well-off in a Bergson-Samuelson social welfare function. With, say, the function $W$ having a constant elasticity of substitution between individual welfare, the Rawlsian case was reached as the elasticity tended to zero. But this ignored the fact that Rawls was concerned with the distribution of what he called primary goods, rather than with the distribution of individual welfare. In the same way, Sen was concerned to change the evaluative space – in his case to the consideration of individual capabilities, which we may define broadly as the freedom that people have to function in key dimensions.

The question we need to put to Lucas, and indeed all those who refer to *the* welfare consequences, is how their conclusions would change if Bentham were replaced by Rawls or by Sen. Whatever one thinks of the merits of the views put forward in these two alternatives to utilitarianism, one has to ask – when making statements about public policy – how adoption of a different view from utilitarianism would affect the conclusions reached. Where people disagree about the desirability of a particular policy reform, is it possible that they do so because they are motivated by a different view of the objectives of society?

**Dominance**

This leads directly to the third approach, which is to seek conclusions that do not in fact change with changes in the welfare criteria: i.e. to seek to identify situations of dominance. The best known such an approach is that based on Pareto dominance, where welfare statements are limited to changes that make everyone better off or at least no worse off. Such a change is described as a Pareto improvement. So that $(100,201,500)$ beats $(100,200,400)$. A situation where there are no further possibilities for Pareto improvements is described as Pareto-efficient.
Paretian welfare economics has been popular. It is however only an incomplete ordering. Even a very modest loss for one person prevents us reaching any conclusion. As put by Sen, if preventing the burning of Rome would have made Emperor Nero feel worse off, then we cannot conclude that its burning was a mistake. He goes on to say that “a society or an economy can be Pareto-efficient and still be perfectly disgusting” (1970, page 22, where I have replaced Pareto-optimal by Pareto efficient). Moreover, the Pareto approach is firmly based on individual welfare. As noted earlier, there may be reasons why social judgments may over-ride individual welfare, as with the case of merit goods. These are non-welfarist considerations.

Even, however, if we consider only individual welfare, we have to ask whether social welfare is always a non-decreasing function of individual welfare. Are there situations where we regard an improvement for one person as a worsening for society? The standard response is that this is simply envy, or “spiteful egalitarianism” as is called by Feldstein (2005) in his Presidential Address to the American Economic Association. But it is not evident that we can simply reject egalitarianism so easily. It has long had appeal. Plato argued that “if a state is to avoid … civil disintegration … extreme poverty and wealth must not be allowed to rise in any section of the citizen-body, because both lead to disasters. This is why the legislator must announce now the acceptable limits of wealth and poverty” (quoted by Cowell, 1977, page 26). Plato’s recommendation was that the limits be set at 4 to 1. More recently, Tawney argued that “a common culture … is incompatible with the existence of sharp contrasts between the economic standards and educational opportunities of different classes. … It involves, in short, a large measure of economic equality” (1964, page 22).

There are therefore arguments for concern about distance, concerns that may be particularly relevant today in the UK as we observe a fanning out of the wage distribution at the top. If the median earnings in 2006 were about £18,000 a year, then the earnings of the top 1 percent start at some 5 times this amount. This exceeds the Platonic 4:1. But it has not always been so: until 1991 the top 1 percent began at less than 4 times the median wage. The rise in top earnings began in the later 1970s, and was marked in the 1980s, but has continued strongly under New Labour. The top 1 per cent earned 4¼ times median earnings when Labour came to power, and now earn approaching 5¼ times. If this fanning out of the upper tail of the earnings distribution continues, then it will ultimately lead to the question being asked whether the distance has become unacceptably large. How big can the gap in earnings become before the contrasts come to be regarded as excessive?

Conclusions

In this section of the lecture I have critically examined the approaches adopted by modern economists to welfare judgments. The key conclusions may be summarised in terms of a

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36 These numbers are based on the Annual Survey of Hours and Earnings – see Atkinson and Votchovska (2008). The earnings refer to all full-time workers whose earnings were not affected by absence at the time of the survey.
two-person diagram – see Figure 1. The first point is that, if we assume that both are identical, located along the 45° line, then welfare economics misses out most of the interesting policy issues – from the repeal of the Corn Laws to Global Warming. The policy problems with which we are typically faced are more like those shown by the possibility frontier. If we start inside the frontier, then improvements for both may be achievable, where we move outwards in a north-easterly direction. There may be scope for Pareto-improvements. But once we reach the frontier, we have to trade gains for one person against losses for the other, and most actual policy changes involve some losers as well as some gainers. This means that a welfare judgment involves a stronger welfare criterion.

Most economists are at heart utilitarians, at least as revealed by their academic writings, leading to the choice of the point marked Bentham. The social welfare contours are the heavy dashed straight lines with slope minus 1. As noted above, there is nothing remotely egalitarian about the utilitarian position. The difference principle of Rawls, with its focus on the least advantaged (in the case shown, person 1), represents a contrast, and leads to different policy implications. In this case, the social welfare contours are right angles centred on the line of equality. Although we should note that the Rawlsian solution, too, is not necessarily egalitarian. The welfare of the less advantaged (person 1) is maximised, but – and I have drawn the frontier expressly for this reason – the maximum may fall short of the line of equality. This is a gross over-simplification of Rawls’ theory of justice. Rawls was not concerned with individual utilities, but with primary goods. Sen too would strike out utilities, and, as shown in Figure 1, replace them by capabilities. I shall return to some of the implications later in Section 4.

Finally, we may reject welfarism in another sense, which is that social welfare may be concerned with distance and hence view negatively additions to the well-being of the rich if it takes them too far away from the rest of society. The application of a maximum ratio may, as shown by the heavy dotted line, restrict us to a cone around the line of equality. In the case shown, it would preclude the utilitarian solution. Indeed, it could, as indicated by the lighter dotted line, take us to the left of the Rawlsian position and indicate the choice of an allocation where both were worse off. As the diagram illustrates, there are important issues at stake.
4. Why We Need Welfare Economics

So far, I have been critical of the absence of foundations for the welfare statements to be found in modern economics. In the last part of the Lecture, I want to be constructive, suggesting how welfare economics can be positively helpful. I have for this purpose taken two issues very relevant to current policy. They are drawn from the EU structural indicators cited earlier, but they have wider resonance.

Employment as an objective

I start with the choice of employment as an objective of policy. Why exactly is raising employment, for the whole working age population, or for older workers, an objective of EU policy? It should be stressed that we are concerned here with increasing employment, not with reducing unemployment. The argument for reducing unemployment is much more immediately compelling. Of course, raising employment may well lead to lower unemployment but it need not do so. The employment rate can be raised by inducing people to stay in the labour force, or to re-enter the labour force. This indeed has been much of the thrust of government policy. Governments in the UK have been trying to end early retirement and to encourage greater participation in the labour force by the disabled and by lone parents. The Joseph Rowntree Foundation has sponsored a great deal of important research in this area, examining the mechanisms by which employment rates may be raised and the implications for child poverty.

But there is a prior question that has been little asked. Why do we want to increase employment rates? Why should we want a larger labour force?

Figure 1
distinguish several different arguments. The first – often advanced in an EU context - is that Europe’s labour markets are heavily distorted and discourage work. People’s decisions are being tilted against work. They are influenced by taxes and transfers, rather than by the real costs and benefits of working. A classic case is where one member of a couple is receiving an income-tested benefit, so that the partner has little financial incentive to work, since each £1 earned will reduce the transfer received. In this case, the aim is to better align the incentives faced by individuals: to level the playing field. The end is welfare improvement, and increased employment is a by-product.

However, this is only part of the story. Governments appear to be concerned with more than the distortion of decisions, as is evidenced by the fact that they seem more interested in the total elasticity of labour supply than in the compensated elasticity relevant to welfare measures. Policy is directed not just at the fact that people choices are tilted but at the actual choices they make. This is particularly apparent when we look at the older end of the age spectrum. When studies of early retirement refer to "unused productive capacity", they are attaching a positive value to work, quite independent of how it is viewed by the worker.

We have therefore to recognise that social decision criteria may be influenced by considerations other than individual welfare levels. Market employment may be an objective in its own right. One way of representing this is to say that employment is a “merit good”, like the more usual merits goods such as education or health care. It is of course important to note that it is market employment. If a person aged 63 gives up his or her job so as to look after the grandchildren, then this activity is not counted. Or, as is increasingly likely with 4 generation families, the person of 63 may be looking after their 90 year old parent. Adoption of the employment rate target tilts the decision away from caring towards staying in the labour force, possibly of course as a paid carer for someone else’s parent. But this raises the question as to why unpaid work should not be counted.

There are of course possible answers, but we need to set them out. One such answer may be developed in terms of social exclusion. Here we may see a parallel between the literature on welfare economics and that on the measurement of poverty. The measurement of poverty in the UK has evolved, under the influence of the research of Townsend (1979) and of developments in Sweden and France, from a primary focus on financial resources to a broader concern with the capacity of individuals to participate in society. And we can trace the EU concern with employment back to just such a concern: the 1994 EU White Paper on Growth, Competitiveness, Employment argued that the creation of jobs was necessary to ensure that our children “be able to find hope and motivation in the prospect of participating in economic and social activity” (European Commission, 1994).

As it was put by Burchardt, Le Grand and Piachaud,

“an individual is socially excluded if he or she does not participate in key activities in the society in which he or she lives” (2002, page 30).
Employment may quite reasonably be regarded as one of these “key activities”.

The employment target may therefore be rationalised in terms of social integration; moreover, we can see why it is market work that is being prioritised. Making explicit such a rationale serves in my view two functions. First, in a democratic society, governments have to persuade members of the society of the legitimacy of the objectives, and the argument has to be made and tested. Second, it allows us to refine the resulting policy conclusions. For example, the socially inclusive nature of employment was justified in terms of young people, and one can see immediately the relevance to the banlieux of Paris, but the application to those aged 55-64 is less immediately apparent. And for young people, we can see that the degree to which employment promotes social integration depends on the quality of the jobs and the extent to which they do indeed offer future prospects.

The move from financial poverty to a broader concept of social exclusion has involved a move from a single-valued indicator to a multi-dimensional approach, and this, I would argue, is a key feature of moving outside the standard utilitarian welfare economics. In moving from Bentham to Rawls or Sen, we are not just changing the maximand but also changing dimensionality. Rawls had a list of primary goods. Capabilities have a number of different domains: Nussbaum (2000) for example lists ten. Set out schematically, we have a matrix of people and domains – see Table 2. The standard welfare economic approach is to assume that the domains are reduced to a single number representing individual welfare or utility, and the aggregation issue involves combining these into a single overall level of social welfare, as with the Bergson-Samuelson social welfare function.

This process may be contrasted with that implicitly adopted when formulating the employment objective. Here what we are doing is to aggregate for one domain across individuals: i.e. aggregating first vertically. This however misses the correlation across domains. We may reject the utility route, but be concerned about multiple deprivations. We may worry whether it is the same people who lose their jobs at 55 and who have low life expectancy and low income. The same applies to my second example, to which I now turn.

**Capability and the measurement of economic performance**

My second example also relates to the EU Structural Indicators, but it is of wider relevance. Indeed, it may be illustrated by reference to Australia. The 2006 OECD Survey of the Australian Economy concluded that “living standards have steadily improved since the beginning of the 1990s” (Policy Brief, page 3). The evidence cited however relates to the growth of national income. Growth in real gross domestic income had averaged over 4 per cent in Australia. This figure I am not questioning, but what is debatable is the equation of the growth in national income (GDP for short) with improvement in living standards.
Here I am not making a purely semantic point, but rather one that goes to the heart of much economic debate. In a number of countries, there is increasing concern among economic policy-makers that we cannot take for granted that there is a direct connection between GDP and the living standards of households or individuals. Improvement in the macro-economic numbers cannot be assumed to imply commensurate improvements in living standards across the population. As a result, politicians are rightly worried that success in securing economic growth, and raising the employment rate, has not been recognised by the population as a whole (or more crucially by the electorate as a whole). There is a sense among the citizens that their living standards have not risen. This is most obvious in the US, where has been much questioning as to where the fruits of growth have gone, as ordinary people seem to be no better off than 10 or 20 years ago. In France, there is much concern about “le pouvoir d’achat”. Yet in France GDP per capita has been rising, according to the IMF figures, real GDP per head at national prices in 2006 was nearly a fifth higher than in 1996. Even if the growth rate is less than in the past, and less than in the US, it is still definitely positive, but this macro-economic performance has not fed through into a sense of improved living standards.

This is causing a reconsideration of the basis for our economic assessment in terms of national accounts. National accounting is, I believe, one of the great social science success stories. The introduction of a systematic framework, broadly comparable across time and across countries, has transformed macro-economic policy-making. At the same time, the foundations are rooted in a number of compromises. If in fact one goes back to the origins of modern national accounting in the 1930s and 1940s, then one can see it as emanating from two different streams of economic thought. The first, and the most urgent in policy terms at the time, was the development of macro-economic management. It was no accident that Keynes was a strong advocate. For this purpose, what was needed were consistent national aggregates – it was very much accounting. The second is the expression of the level of national welfare, stemming from the earlier welfare economic tradition developed by Pigou. The title of one of the articles cited earlier by Samuelson was the “Evaluation of real national income”, and this article was essentially concerned to provide a welfare economic underpinning to the numbers appearing in the national accounts.

At the time, it was clear that the marriage of these two sets of concerns was to some degree a marriage of convenience. The time has perhaps come for divorce. A number of people have come to the view that we need to construct new indicators of economic and social performance. And new indicators have already been constructed, of which I cite just one – the Human Development Index – chosen because it has been very much influenced by the capability approach. The HDI was introduced in 1990 under the aegis of Amartya Sen and Mahbub ul Haq of Pakistan, and continues in a more refined form to be published by the UN Development Programme in its annual Human Development Report. The HDI is a very reduced form of the capability approach; indeed Sen has described it as a “vulgar” measure. At the same time, he has noted that it is of the “same level of crudeness as GNP” (1999, page 318, n41). Moreover, it is a concrete implementation of an alternative approach to the underlying concept of well-being.
The HDI has three main domains, slightly different from those used before. Countries are ranked on each of these three domains. The UK ranks 18th equal on life expectancy, 16th on education and 10th on GDP per capita. And then the domains are aggregated into the HDI. The UK is 16th overall, between Austria and Belgium. We are above Germany and below France. But what I am interested in is the procedure. What do capabilities bring to the party? Obviously they extend the dimensionality. This is perhaps the most important part, but the HDI also changes the way in which income is introduced. The index is based not on GDP per capita but on its logarithm. Why is this? According to the UNDP website, “the HDI uses the logarithm of income, to reflect the diminishing importance of income with increasing GDP” (UNDP website, 2008). Or, as put when the index was first published, there are “diminishing returns to transforming income into human capabilities” (UNDP, 1990, page 12).

But, if there are diminishing returns, this applies at the individual level, not to the aggregate national income. This means that the appropriate procedure is not that used in the HDI but to take the logarithm of income at the individual level and then aggregate. In other words, we want to take not the logarithm of mean national income but the logarithm of the geometric mean. Unless all incomes are equal, the geometric mean is less than the arithmetic mean, reflecting the reduced rate at which income is transformed into capabilities as income rises. Taking the alternative approach seriously suggests then that we should assess economic performance by the geometric mean of incomes, not by the arithmetic mean as in the national accounts. This seems a very modest change, but it leads us to take a rather different view of recent growth performance, as is illustrated in Figure 2 for the case of the United States. As we know, overall household income has grown in the US, particularly since 1990: from 1990 to 2006, the mean household income grew by a fifth. The geometric mean, on the other hand, grew more slowly. Over the period as a whole, it grew around 0.5% per annum more slowly than arithmetic mean income. This is a large amount: about equal to the difference in growth rate between the US and the UK in the past decade. And in the most recent period, the geometric mean rose in the Clinton years and fell in the Bush years, leaving the 2006 figure scarcely higher than in 1990.

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37 As with a logarithmic social welfare function, converted back to an income equivalent.
5. Conclusions

The findings just presented regarding the US economy may seem to justify the labelling of economics as “the dismal science”, but what I have tried to argue in this lecture is that economics should be thought of as a moral science. Many of the ambiguities and disagreements stem not from differences of view about how the economy works but about the criteria to be applied when making judgments. The first conclusion to be stressed is that we cannot talk about the welfare consequences: there are several welfare criteria that could be applied in evaluating a change or a policy proposal. People can legitimately reach different conclusions because they apply different theories of justice. This may seem self-evident to non-economists, but the economics profession in recent years has tended either to assume away welfare judgments or to assume that there is general agreement.

The second conclusion is that examination of the foundations for welfare statements can help us think constructively, and extract new insights, about key policy issues today. Among the examples given are the growing distance between top earners and the rest, the EU objective of raising employment rates, the construction of the Human Development Index, and the measurement of national living standards. These are all matters that concern individual citizens and they should be centre stage in economics.
### Table 1 EU Structural Indicators

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<tr>
<th>No.</th>
<th>Indicator</th>
<th>Domain</th>
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<tr>
<td>1</td>
<td>GDP per capita</td>
<td>Growth</td>
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<tr>
<td>2</td>
<td>Labour productivity</td>
<td>Growth</td>
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<tr>
<td>3</td>
<td>Employment rate 15-64</td>
<td>Employment</td>
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<td>4</td>
<td>Employment rate of older workers 55-64</td>
<td>Employment</td>
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<tr>
<td>5</td>
<td>Youth educational attainment</td>
<td>Social Inclusion</td>
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<tr>
<td>6</td>
<td>R+D spending % GDP</td>
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<td>7</td>
<td>Comparative price levels</td>
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<tr>
<td>8</td>
<td>Investment spending % GDP</td>
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<tr>
<td>9</td>
<td>At risk of poverty rate after social transfers</td>
<td>Social Inclusion</td>
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<td>10</td>
<td>Long-term unemployment rate</td>
<td>Social Inclusion</td>
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### Table 2 Different forms of aggregation

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Ethics and the Science of Economics: Robbins’s Enduring Fallacy

Amos Witztum∗

Abstract

The basic principles of Robbins’s Essay on the Nature and Significance of Economics Science are still present in most textbooks in Economics. We thus face a combined problem of historical and contemporary nature. On the historical front, the Austrian association attributed to Robbins, sometimes, hides the originality of Robbins’s work in combining Austrian and some Lausanne-ian principles even though a similar line of reasoning was attributed to his main Austrian inspiration - von Wieser. In this context, Robbins’s assertion concerning ethics-economics relationship has three main difficulties. Firstly, the presumption of means-ends analysis which is oblivious of the ends people seek to promote is not as neutral as it appears. Robbins (as does Wicksteed) chooses to ignore the ends by focusing on cost minimisation. This, implicitly (though not inherently), suggests another end - wealth, or means, maximisation - which by no means can be considered as ethically neutral. Secondly, there is an implicit assumption that whatever the ends people seek to promote, there will always be a co-ordinated outcome to their actions. As competitive prices are the means to achieve waste minimisation (through proper pricing of opportunity costs), the assumed co-ordination must be that of general equilibrium and thus, co-operative based co-ordination must be excluded. Thirdly, Robbins demands that the postulates of economics be based on empirically recognised introspection. His Means-End agent is clearly opportunistic regardless of his objectives. This suggests opportunism which may not be so universally compatible with all possible ends. In the face of mounting evidence (current) can we still uphold these Robbinsian principles of economics?

Keywords: Positive and normative Economics, Robbins, Ethics, Opportunistic behaviour

1. Introduction

The purpose of Robbins’s Essay on the Nature and Significance of Economic Science was to carve a niche for economics as an independent discipline and to maintain its scientific nature by divorcing it from anything remotely ethical. By offering a clear, almost mechanical, definition of what constitutes the subject matter of economics, he was fending off historicism, which opposed the breaking up of social phenomena into well defined and separate areas of investigation38. By relying on a notion of the Verstehen Doctrine, he was hoping to ward off the institutionalists’ demand for direct empiricism as a way to formulate the postulates of economic analysis39.

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38 See a discussion of this point in Hodgson (2001, pp.207-8).
39 By Verstehen Doctrine, in this context, I refer to Robbins’s suggestion that we are able to understand human action on the basis of introspection. “We do not need controlled experiments to establish [the
At the heart of this conception, therefore, we find the followings main tenets:

1. The subject matter of economics is the tension between scarcity and wants (the means-ends problem)\(^{40}\);

2. Economics is based on axioms (abstractions) which are derived from experience and which lead to statements about reality\(^{41}\) (hence, the ‘scientific’ nature of the subject);

3. Economics is not concerned with ends but only with the means available to achieve those ends. It is thus, value free\(^{42}\).

Irrespective of how well received was Robbins’s Essay at the time, these tenets are familiar to the modern reader and would be easily found in most contemporary textbooks. In this respect, the problem posed by Robbins is as much a current debate as it is a historical one\(^{43}\). In turn, this makes the analysis of Robbins’s methodology a bit more complex. While one has to bear in mind the context and influences which gave rise to Robbins’s Essay, one may want to ponder the significance of these propositions in terms of future developments. This is particularly so as Robbins’s intellectual heritage is complex and not necessarily particular to any of the existing schools of his time\(^{44}\).

Perhaps the most prominent and enduring element in Robbins’s story is the call for the clear separation of ethics from economics. This conclusion is logically derived almost directly from his conception of the subject. Therefore, to evaluate the validity of this separation, we must explore its logical consistency. The purpose of this paper is to do just that.

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\(^{40}\) “From the point of view of the economist, the conditions of human existence exhibit four fundamental characteristics: The ends are various. The time and the means for achieving these ends are limited and capable of alternative application. At the same time, the ends have different importance” (Robbins, 1935, p.12).

\(^{41}\) Section 2 of chapter 4 is devoted to show that economic generalisations are not historical or based on experiments. It is here that Robbins appeals to the Verstehen doctrine according to which we should based our axioms on some notion of introspection. “In the light of all that has been said the nature of economic analysis should now be plain. It consists of deductions from a series of postulates, the chief of which are almost universal facts of experience present whenever human activity has an economics aspect…” (Robbins, 1935, pp.99-100).

\(^{42}\) To which section 4 of chapter 6 in Robbins (1935) is devoted.

\(^{43}\) Denise O’Brien provides an account of Robbins’s microeconomics which suggests that it is almost identical to the modern account of it (O’Brien, 1990, pp.157-9).

\(^{44}\) “Robbins was an eclectic, seeking to assemble the common strands in a wide range of different sources in economics so as to weld together…a common body of developed, received and professionally attested economic theory” (O’Brien, 1990, p.155). Howson (2004) also provides an account of the diverse influences on Robbins.
On the face of it, Robbins makes a simple claim: People may have different ends but they will always need to allocate resources. If economics were to deal with such allocations, it would have been value free. While this may appear appealing there are two main problems with this approach. Firstly, economics would not have a well define criterion of economic performance as these would depend on the objective people seek to achieve. Secondly, the idea of efficiency in the sense of waste minimisation is not necessarily part of economic analysis. Taken together, these two difficulties make economics less, rather than more, well defined or analytical. Clearly, this could not have been Robbins’s intention. Nor is it, in my view, a correct reading of Robbins and the sources which inspired him, and in particular, Wicksteed.

There are three fundamentals which appear to help Robbins achieve his objective of separating ethics from economics. Firstly, he insists on divorcing the definition of the subject from any particular objectives and in particular, the one he perceives to dominate the English tradition. By insisting that economics is not about wealth creation he offers a view, which still resonates today, that economists have nothing to say about the objectives of human action. Instead, economics is merely concerned with finding solutions to the problem of choosing appropriate means to achieve certain ends when there is scarcity.

This step is essential in the Robbins scheme as it facilitates both the universalisation of economics and its value free agenda. The former is based on the absence of any substantive boundaries to the application of economic analysis except the presence of scarcity. By implication, this is a licence for what some people call today ‘economic imperialism’. As scarcity is prominent in almost all aspects of the social sciences, there are no reasons why economic analysis should not be applied across all disciplines.

The value free agenda is also assisted as by removing particular objectives (like wealth creation) from the economic analysis of human behaviour, economics can avoid the debate about the moral significance of these objectives. Hence, according to Robbins, current debates about whether growth maximisation is a legitimate social objective instead of, say, happiness, lie entirely outside economic analysis. Equally, a claim according to which pursuing the efficiency of competitive structures means a social accommodation of greed becomes a meaningless statement. In other words, economics does not distinguish between solutions in terms of what they are trying to achieve, or, in Robbins terms: “Equilibrium is just equilibrium” (p.143).

45 While I will take notice of the historical origin of some of Robbins’s idea, most of this paper is devoted to the logic of Robbins’s own claims. Robbins was a clear thinker and to a great extent, his writings speak for themselves.

46 O’Brien (1990, p.155) claims that in microeconomics Wicksteed (1933) was the primary source of influence on Robbins. However when we examine Wicksteed’s conception of the subject, the difficulties with separating ethics from economics become apparent. It is easy to see in his writing (in particular, in pages 182-185) why such a position is not as obvious as Robbins, and Wicksteed, try to portray. I will deal with this further, below.
However, we must bear in mind that Robbins is a committed consequentialist as far as ethics is concerned. Therefore, we need to know something about the objectives which have been pursued in order to judge the outcome. But if we consider ethics more broadly, we cannot accept this as a reason to exclude ethics. Not all forms of ethics are purely consequentialist and if the process of achieving an end - even if we do not know what it is - can become the subject of moral examination, the system is by no means value free.

But there is a far more immediate problem with Robbins’s approach. If indeed “there are no economic ends. Only economical and uneconomical means of achieving given ends” (p.144), then economics is about the minimisation of cost (both at the individual and social levels). However, is the minimisation of costs not the logical dual of maximising wealth? Wicksteed (1933) claims that by avoiding wastage, economics is about creating as many means as possible to achieve whatever ends individual/society wish to achieve. But unless we create a distinction between productive and unproductive economic goods - which, of course, neither Robbins nor Wicksteed subscribed to - this is virtually the same as output maximisation. Why then, is the accumulation of means not an objective which is open to ethical scrutiny?

The second fundamental in Robbins’s argument is the implicit presumption that there is always a potential co-ordinated outcome when individuals solve their end-means problem in the face of scarcity. In economic terms, this means that there will always be equilibrium irrespective of which ends people seek to promote.

Now equilibrium is a highly charged concept. This is particularly so when many attribute to Robbins Austrian tendencies. However, while one would find Austrian ideas in Robbins, the fact that he followed Wicksteed (1933) (who followed Wieser (1967)) suggests a broader agenda than the one adopted by the leading Austrian. In his Notes and Recollections von Mises claims that Wieser, a follower of Menger, has moved so far away from anything Austrian that one can say that he “was a member of the Lausanne School” (Mises, 1978, p.36). It is therefore not surprising that both in Wicksteed and Robbins (as well as Wieser) one can find a far greater commitment to general equilibrium - in a more Walrasian sense - than the mere nebulous idea of spontaneous order. Robbins clearly adopts the idea of general equilibrium as can be seen in his Essay (1935, 67-69), in his discussion of stationary equilibrium (1930), and in his analysis of costs (opportunity costs) (1934). O’Brien (1990) quite explicitly admits that “The focus of analysis [being] upon equilibrium-general and not partial equilibrium” (158). If

47 “Economics, then, is in no way to be conceived as we may conceive Ethics or Aesthetics, as being concerned with ends as such” (Robbins, 1935, p.32). In Wicksteed (1933) this view is expressed even more brutally by claiming that: “Any relation into which I enter for the fulfilment of my purpose may, in a sense, be called immoral, inasmuch as it is a means and not an end” (p.182).

48 “The tendencies of modern thought and the conditions of modern life have combined to sever the consideration of the administration of resources from the discussion of the ultimate ends...it has therefore become usual to treat Political Economy as concerned with increasing the communal means rather than securing the communal ends;” (Wicksteed, 1933, p.15).

49 See Salerno (2002).

50 Blaug (1990) criticises O’Brien for attributing general equilibrium to Robbins. The reason for this is that he objected to the potential connection between Robbins and Walras. For Blaug, Walrasian equilibrium is a
indeed, as O’Brien implies, Robbins’s microeconomics is very much the same as the modern one, it is not at all surprising to find a Walrasian concept of equilibrium at its heart.

But the presence of equilibrium in Robbins does not necessarily mean that it is the same as that of Walras. It does not necessarily contradict the view according to which the main interest lies in the process by which prices converge to equilibrium even if they never do. Equilibrium, in this broad sense, can be interpreted as a logical limit, or the benchmark, and it is in this way, which Robbins (and Wicksteed) conceived it.

The reason why the universality of equilibrium under any sort of motivation is important in Robbins’s ethics-economics scheme is that once the objective of wealth creation has been dismissed, there is not much left for economists to say. We saw that they would like to say something about cost minimisation. The other thing which they may say is to explain the relationship between prices. The two are, of course, related as one cannot properly account for opportunity costs without competitive equilibrium prices. Indeed, as Robbins says quite explicitly, the only thing, which economics can say, is to describe this equilibrium and to describe what would happen if there were a departure from complete freedom. In other words, economics only describes the co-ordinated outcome that would emerge under all possible sets of motives and characters but cannot comment on the significance of any institutions in resolving any particular economic problem. In other words, economics can either prescribe, for which it would need a well defined problem, or describe, for which it is needs a reference point.

Nevertheless, irrespective of how plausible is the claim that all possible sets of ends have a co-ordinated outcome, ethics again, raises its ‘ugly’ head. When Robbins describes the concept of equilibrium, he focuses on co-ordinated outcomes when certain principles of ‘freedom’ and ‘rights’ are established. It seems that for Robbins these are the conditions of what 18th century scholars would call ‘natural liberty’. However, to establish the conditions of natural liberty, 18th -19th century scholars went out of their way to try to understand why individuals have become dependent of each other in the first place. This, inevitably, is bound to be connected to what motivates their actions and subsequently, the natural order that would emerge if things were left for themselves. Thus, to assume that freedom and respect for rights is the natural order upon which all sets of ends can reach equilibrium requires a proper understanding of why would people adopt such ethical views and how could this affect their actions.

historical aberration. However, Robbins’s connection with the Lausanne school cannot be denied and in particular, as considered it as important influence on Wicksteed. Moreover, I will later try to demonstrate that Robbins could not have meant any other form of equilibrium but this does not mean that the Walrasian idea is incompatible with more process-based analysis. The problem, in my view, is more with Blaug than with Walras (see a discussion in Witztum 2007).

51 Though when Robbins says that “Instead of dividing our central body of analysis into a theory of production and a theory of distribution, we have a theory of equilibrium, a theory of comparative statics and a theory of dynamic change”(Robbins, 1935, p.68), it is difficult to see how could he have objected to Walras. The truth of the matter is that he has not at all objected. In the introduction to Wicksteed’s Common Sense, Robbins says that Wicksteed “was deeply influenced by the work of those who carried the application of mathematical methods furthest-by the work of Walras and Pareto”(Robbins, 1933, xviii).
The third fundamental, which is closely connected to the previous one, is the presumption of the opportunistic nature of human behaviour. Given the lack of economic interest in the objectives of human behaviour, there is not much to be said about the nature of these objectives. Hence, no matter what it is that individuals seek, when they face scarcity they always seem to be interested in the best means to an end, which is a clear case of opportunistic behaviour. Clearly, the ethical neutrality of opportunistic behaviour is also fed by the consequentialist nature which Robbins attributes to ethics. As in Wicksteed (1933, p.182) if there are no ethical restrictions on means, the choice of best means to an end cannot become the object of morals.

Apart from the question of whether this is a plausible conception of ethics, there is also a problem in terms of Robbins’s own methodology. Given that Robbins requires that the postulates of human behaviour would be naturally understood to us, the fact that there is mounting evidence that individuals have a social dimension somewhat discredits this position. Moreover, there is also the logical question of it would be possible for someone to be socially minded and opportunistic at the same time.

All three fundamentals are essential for the creation of a value free and universal science of economics. It is all about the irrelevance of objectives, the certainty of co-ordination and the absence of an underlying social dimension, or context, which governs the opportunistic nature of human behaviour. But in fact, none of these fundamentals are true. Economics is not neutral about objectives and it is promoting wealth maximisation which is indeed different from wealth creation-the Classical agenda-but nonetheless a subject of moral scrutiny. Nor would all types of purpose lead to a competitive general equilibrium and thus, impede economists from properly assessing how 'economical' are certain means. Nor is it a valid introspection to suppose the people are opportunistic. In what follows I will address each of the above fundamentals and examine their validity both from historical and contemporary perspective.

2. ‘Ends-Means’ or Economic Problem

2.1 Means-ends or cost minimisation?

Robbins defines economics as the investigation into the solving of means-ends problems in the face of scarcity. Economics is not interested in the ends but only with the ‘efficient’ use of means: “[T]here are no economic ends. Only economical and uneconomical means of achieving given ends” (p.144). But one may wonder whether the 'economical or uneconomical' use of means is indeed independent of the ends. Naturally, one can always say that one is interested in the efficient use of resources.

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52 Even if the objective is altruistic, we still have the potential paradox that in seeking to help the others, one may choose means that could potentially undermine society.

53 Also: “Economics…is concerned with that aspect of behaviour which arises from the scarcity of means to achieve given ends. It follows that Economics is neutral between ends;…..[it] is not concerned with ends as such” (Robbins, 1935 p.24)
irrespective of what the person using the resources aims to achieve\textsuperscript{54}. This may, at first impression, appear value free but why would one be interested in the efficient use of resources in the first place? Would anyone's interest in this question be independent of the aim which the people who employ resources try to achieve?

While Robbins seems to be quite clear about his intention, in this case it might be useful to examine the same theme in Wicksteed who, as we have acknowledged earlier, was one of the main sources of influence on him\textsuperscript{55}. Wicksteed (1933) defines economics as the study of “the administration of any kind of resources...in such a way as to secure their maximum efficiency for the purpose contemplated. It is administration with a minimum of waste” (1933, p.14). This sounds very much like Robbins’s conception but phrased in this way it also exposes the difficulty of this definition. What does it mean ‘to secure their maximum efficiency for the purpose contemplated’? Does this necessarily lead to the next sentence: ‘it is administration with a minimum waste’?

The first part of Wicksteed's definition suggests that the 'maximum efficiency' depends on the ‘purpose’. In other words, that the criterion of economic performance depends on that which one is trying to achieve. ‘Maximum efficiency’, therefore, can only mean the best way to serve a particular purpose. But why should this be 'minimum of waste’?

“All successful administration” writes Wicksteed, “consists in the purposeful selection between alternative applications of resources; and the ultimate value or significance of such success depends on the nature of the objectives at which the administrator aims” (ibid). Moreover “since the idea of ‘worth’ enters, as the regulating and dominating principle, into every act of administration, and since it is our ends or objects that determine the relative worth, or worthiness, of this or that achieved result, it follows that the ultimate ideals of any individual, household, or community - the nature of the ends it seeks and desires - must give the tone and character to its ‘economy’, and must be the soul and inspiration of its administrative system.” (p.14)

Unlike Robbins, Wicksteed seems to acknowledge the relevance of the ends people seek to the evaluation, and perhaps even the working, of the system. However, many pages later (p. 184), Wicksteed all of a sudden changes tack. He suggests that the reason why economics can nevertheless be neutral is because the free reign of intelligent people seeking to promote their own ends always promotes the other’s ends and produces more resources to help achieve whichever end we wish.

So in the end, it is not that ‘minimum waste’ has anything to do with a specific objective but rather because - and I will phrase this in terms of modern economics as it means

\textsuperscript{54} : “The criterion of economy which follows from our original definitions is the securing of given ends with least means. It is, therefore, perfectly intelligible to say of a certain policy that it is uneconomical, if, in order to achieve certain ends, it uses more scarce means than are necessary” (p.145).

\textsuperscript{55} In the introduction to Wicksteed’s Common Sense, Robbins clearly identifies the separation of ethics from economics as coming from him.
exactly the same\textsuperscript{56} - in competitive equilibrium everyone gains from trade and output is maximised so as to facilitate the achievement of any objective\textsuperscript{57}.

There can be little doubt that Robbins was in full agreement with this. He too talks about the conditions of perfect freedom as the benchmark of how things should be (without elaborating why this should be so)\textsuperscript{58}. Given the endorsement of Wicksteed's position in his introduction to the \textit{Common Sense}, it is quite plausible that the same logic applies to Robbins\textsuperscript{59}.

However, that which Wicksteed is actually saying is not as if economics is totally uninterested in the objectives of individual/social behaviour but rather that the economic objective of maximising output is useful for all other possible objectives. Better still, that the economic objective of output maximisation is consistent with all other possible objectives as it provides more resources for them.

One could be tempted to interpret Wicksteed as saying that the economic objective of output maximisation is just the means to achieve other objectives but this cannot be acceptable as the other reason for its neutrality is the fact that people \textit{always} benefit each other.

Robbins does not discuss these complex relationships in full but simply accepts the assertion that economics is about minimum waste (‘economical’), that it is consistent with all other objectives and therefore, that it is ethically neutral\textsuperscript{60}. Given the elaborate treatment by Wicksteed, it is easy to see that the claim, according to which economics does not care about objectives and is ethically neutral, is quite problematic. For one, it is difficult to see how the clear admission about output maximisation can be accepted as objective which is beyond the ethical\textsuperscript{61}. In particular, as it was coupled with the

\textsuperscript{56} “Division of labour and exchange, on which the economic organisation of society is based, enlarge our means of accomplishing our ends.”(Wicksteed, 1933, p.184).

\textsuperscript{57} We “no longer enquire concerning the causes determining variations of production and distribution. We enquire rather concerning the conditions of equilibrium of various economic ‘quantities’, given certain initial data, and we enquire concerning the effects of variations of these data” (Robbins, 1935, p.67)

\textsuperscript{58} See Robbins, 1935, pp.143-4.

\textsuperscript{59} “[Common Sense] is that most complete statement of the implicit philosophy of economic analysis which has been published in our day”(Robbins, 1933, xiv). He then argues that Wicksteed’s contribution to the methodology of subjective theory of value in the chapters to which I refer transcends any of his other contributions (xxi).

\textsuperscript{60} At one point Robbins slips slightly: “[W]hen time and the means for achieving ends are limited and capable of alternative application, and the ends are capable of being distinguished in order of importance, then behaviour necessarily assumes the form of choice. Every act which involves times and scarce means for the achievement of one end involves the relinquishment of their use for the achievement of another. It has an economic aspect” (Robbins, 1935, p.14). The last bit may mean - in Wicksteed’s context - that we must choose between the economic end and other ends. If indeed this is the case, economics is entirely within the domain of ethics. Given the rest of his writings, I do not believe that he meant it.

\textsuperscript{61} Robbins himself spends the entire first chapter to argue that modern economics is different from classical economics because the latter was interested in the maximisation of material wealth. However, in the last section of the chapter he concedes that if classical economics were to include non material economic goods, the definition would be acceptable.
assumption that people benefit each other in the process. More difficult would be to argue that this objective may be consistent with all other possible objectives.

Perhaps the best demonstration of this difficulty behind the Robbins-Wicksteed conception of economics emerges from Robbins’s own metaphor of the power of economic analysis. I will quote it at some length:

“Suppose, for instance, a community of sybarites, their pleasures gross and sensual, their intellectual activities pre-occupied with the ‘purely material’. It is clear enough that economic analysis can provide categories for describing the relationship between these ends and the means which are available for achieving them….Let us suppose the reprehensible community to be visited by a Savonarola. Their former ends become revolting to them. The pleasures of the senses are banished. The sybarites become ascetics. Surely economic analysis is still applicable. There is no need to change categories of explanation. All that has happened is that the demand schedules have changed. Some things have become relatively less scarce, others more so. The rent of vineyards falls. The rent of quarries for ecclesiastical masonry rises. That is all.” (Robbins, 1935, pp. 25-6, Emphasis added).

Here we have two different communities with different ends to promote, the one ‘purely material’ the other, ‘purely spiritual’. As both type of communities face scarcity, they are the subject matter of economic investigation. By referring to the differences in relative prices, Robbins assumes that in both cases there will be an equilibrium where the difference in prices will reflect the difference in taste (or what he calls, the initial data) and thus, opportunity costs.

As a result, the ‘economical’ allocation of resources would be different in the two societies.

If we accept it at face value then this vindicates both Robbins and Wicksteed as the ‘waste minimisation’ is perceived to be perfectly consistent with two extremely different sets of ends.

However, for the ‘waste minimising’ (productive efficient) allocation to emerge in both economies prices would need to be at a competitive equilibrium. Naturally, this is not a claim that the economy must at all time be at equilibrium but there must be the presumption that the co-ordinated outcome is possible in principle and that overtime, actual prices will tend towards these prices.

In the material society, when individuals seek to enhance their own material well-being without any clear regard to the others, the main problem, for society, would be the co-ordinated outcome of such behaviour. In a sense, the ends which individuals and society wish to promote in this society are identical to the declared end of economics: namely, to

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62 Otherwise, people will have a wrong conception of the opportunity costs and end up specialising in things in which they may not have a comparative advantage.
ensure plentiful of means (i.e. output maximisation through waste minimisation) while everyone’s pursuit is successful and benefits the other. The economic problem would be to find the institutions which facilitate this happy ending and the answer, as we know, is straightforward: free competition.

But what about the other society. If in the purely spiritual society individuals seek to live an ethical life based on minimal material comfort and respect for ethical principles (whatever they are), would they face a similar problem of co-ordination as the previous society? Is the benchmark of competitive equilibrium where prices reflect the real opportunity costs a meaningful point of reference?

Without delving into the question whether their co-operative streak invites any form of equilibrium as a relevant analytical concept, that which might govern their exchange could easily be ethical principles (for instance, the ‘just price’). Hence, while it is clear that the description of equilibrium relationship will be different-as suggested by Robbins-this type of statement would not necessarily be value free any longer. Whether or not the actual vector of ‘just prices’ is ‘economical’ is not really a question about which this society cares much. Nor would they care if they were told that their ‘administration’-to use Wicksteed’s concept-failed to produce the plentiful of means to support all other ends. As far as this society is concerned, to allow people to compete in the market place is a violation of their ethical principles and stands in contrast to the ends which they seek to promote.

2.2 Can resource allocation be independent of the economic problem?

Robbins is trying to persuade us-as do many modern textbooks-that economics is about resources allocation irrespective of what it is that we seek to obtain. However, if we examine the history of economic thought we will find that there were many attempts to deal with the problem of resources allocation yet there has not been a unified thinking about the best way of conducting such an allocation.

The reason for this is that while everyone was concerned about resources allocation, different scholars solved different economic problems. Thus, for instance, both Plato and Aristotle were concerned with the formation of the just society. In as much as there is economics in either of these writers, it is with reference to the type of economic institutions that would support the just society.

However, even though they shared the same economic problem, their conclusions with regard to the best institutional setup were very different. In Plato (1974, pp. 246-7 (462 b-c)) we can find the argument in favour of communal ownership of property as private property promotes divisiveness while Aristotle (1986, p.114 (1263a)) claims that private property is a source of virtue. The difference between them, in part, is due to the differences in their epistemology which affects the way they construct ethical notions like virtue or justice. What is, however, clear, is that both were concerned with resources allocation but the measure of how well different institutions performed depended on the
economic problem they formed as well as the epistemology by which they conceived this problem.

Equally, classical economists like Smith and Mill on the one hand and Marx on the other were trying to solve yet another economic problem (wealth creation) but they too ended up with very different institutional setups and criteria of economic performance. Their approaches differed from those of the Greeks because they were solving a different economic problem. Their solutions differed from each other as they followed different epistemologies.

In other words, while describing equilibrium relationship may indeed be value free—if we accept that equilibrium is a useful universal term to capture human interaction—the criteria of economic performance is bound to be dependent on the problem which society tries to resolve. In the above metaphor, it was clear that efficiency (or being economical in Robbins’s terms) is consistent with a society which wishes to co-ordinate the material wealth maximisation of its agents. Society’s objective cannot be described as other than wealth maximisation. In the case of the purely spiritual society, the criterion of efficiency became meaningless as it would have been in the case of an Aristotelian Platonic societies.

Robbins’s insistence on the distinction between wealth creation and his own definition of economics is based, primarily, on the claim that economics is not only about material wealth. However, he says, “by saying that services are material vibrations or the like [we] can stretch the definition to cover the whole field”(Robbins, 1935, p.21).

This brings Robbins very close to the position of Walras. The latter, in his Elements of Pure Economics asserts, like Robbins, claims that the Smithian definition of economics is not well conceived as to provide plentiful of revenue may be an admirable objective but it lies outside the science of economics (Walras, 1984, p.52). However, Walras has no problems with the subject matter as “social wealth means all things, material or immaterial that are scarce, that is to say, on the one hand useful to us and, on the other hand, only available to us in limited quantity” (p.65).

Nevertheless, there seems to be a remarkable similarity between Walras’s initial claim—that the science of economics cannot be construed as the study of wealth creation—and that of Robbins. Even though Walras is not at all disturbed by whether the concept of wealth includes only material wealth, his science of economics, as captured by his Elements of Pure Economics, is almost identical to what Robbins considers as the subject matter of economics.

Yet, there are a few fundaments, which distinguish Walras from Robbins. Firstly, while Walras draws a distinction between what he calls science, art and ethics, he does not consider economics to be comprised of only one aspect of it. He clearly insists that the

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63 While the word efficiency appears in Aristotle, it means that each person in a household should do that which they are good at. The purpose is to get as much as one can from household production but not for the purpose of general output maximisation as the ideal of virtue is ‘lying in the mean’, namely, moderation.
study of value in exchange is the subject of ‘science’, the study of production (industry) is the subject of art and the study of property/distribution (institutions) the subject of ‘ethics’. However, he also accepts that they are all “three generic phenomena or the three orders or groups of specific facts which result from the limitation in quantity of utilities or the scarcity of things” (p.68). Indeed, while the study of exchange is called ‘pure economics’ the study of ‘art’ and ‘ethics’ associated with social wealth contain economics in their title: applied economics and social economic respectively.64

The second element is somewhat obscured in Walras’s own analysis. While appropriation and ownership are matters for social economics, they seem to be, in his own words, the pre-conditions for the ‘science’ of economics. “We have seen a priori” writes Walras, “how scarce things, once appropriated, acquire value in exchange” (p.68). In other words, it seems that the natural phenomenon of value in exchange according to Walras, depends on a social phenomenon-the appropriation of scarce things.

Robbins clearly wanted to limit economics only to what Walras called the ‘science’ of economics. However, Walras would not have agreed that the subject of economics should be confined to only one of the categorical manifestations of social wealth.

In fact, in the light of Walras’s analysis, it is easy to see that what Robbins proposes is not really to divorce economics from a particular set of objectives (or economic problems). Instead, he simply suggests a different economic problem from the one adopted by classical economists. We are no longer interested in wealth creation but rather with the reconciling of un-satiated wants with scarcity. In other words, economics is about co-ordinating the activities of wealth maximising individuals.

Thus, it seems that Robbins’s attempt to separate the study of economics from a particular problem has not really been persuasive. Equally, to describe equilibrium relationships, even if they always exist, is not necessarily a value free exercise. Nor is the criterion of economic performance which is associated with efficiency. Whatever we wish to say about individuals’ objectives, if we evaluate the outcome of their chosen organisation by the word efficiency, we are implying a wealth maximisation exercise at least at the level of individuals. If society merely wishes to co-ordinate such an activity, society is effectively endorsing the objectives adopted by the individuals. Hence, as economics cannot be separated from the problem it aims to solve and as ends are natural objects of ethics, economic cannot really separate itself from ethics by claiming to be mute on the objectives which individuals seek to obtain.

3. Equilibrium and Institutions

3.1 The universal competitive benchmark

So far we focused on Robbins’s claim that economics is merely about choosing the best means to an end but not from the perspective of the end. Rather, the measure which

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64 In another place Walras says explicitly that the science of economics does not constitute the whole of the subject (p.71)
economics employs to evaluate the choice of means is from the point of view of efficiency, which we claimed was nothing short of subjugating all ends to a supreme end of maximising wealth (or means).

We now move to the second element of Robbins’s call for the neutrality of economics: co-ordinated outcome. From an Austrian perspectives this simply means that whatever people want to achieve, their actions and interaction constitute a process which will lead to an equilibrium, or some kind of order. The only thing we can say, in such a case, is that we may use utility to explain why prices relate to each other in a particular way but we will not be able to assert that these prices facilitate the minimisation of costs.

In Robbins (1934), the connection between the evaluation of cost and equilibrium is clearly stated:

“If we reflect upon the way in which equilibrium is established. It is surely obvious that it is only through regard for cost in the value sense that any harmony between technical displacements and prices can be conceived to come about. It is only in equilibrium that such a harmony exists….It is not merely true of the Austrian approach…. [It] is as essential a condition of equilibrium in the Walrasian system” (Robbins, 1934, p.4)

In other words, we have here both the Austrian story of process where Robbins’s claims require that people consider costs in terms of prices, and the Walrasian story according to which only in equilibrium will prices reflect the true opportunity costs. The ease in which Robbins swings between the two is more than a proof, in my view, that he was less convinced of their incompatibility than most Austrian writers. But more to the point, he could not have been more explicit about the fact that proper cost minimisation cannot be achieved outside of the Walrasian equilibrium.

Moreover, Kirzner (1999), in claiming Wicksteed - and by implication Robbins - to the Austrian says that: “Here we see Wicksteed, in an Austrian fashion, seeing the decisions of market participants not as the implications of equilibrium conditions somehow assumed already to exist, but as the initiating cause for (and stages) the process of equilibrium itself”. (Kirzner, 1999, p.111). Namely, that which distinguishes the Austrian story from the Walrasian one is that in the Austrian approach, equilibrium is not determined by a-priori exogenous conditions but rather by the process in the market place. However, there is nothing clearer than Robbins’s own statement that “[w]e enquire rather concerning the conditions of equilibrium…given certain data”(Robbins, 1935, p.67 my italic). And: “Now, of course, it is of the essence of the conception of equilibrium that, given his initial resources, each individual secures a range of free choice, bounded only by the limitations of the material environment and the exercise of similar freedom on the part of the other economic subjects”(p.143, my italic)65. By no stretch of imagination can this be interpreted as an Austrian narrative.

65 Robbins makes it even clearer when he states that from the fact that people rank their preferences (taste), we can derive equilibrium: “From this elementary fact of experience we can derive the idea of the substitutability of different goods, of the demand for one good in terms of another, of an equilibrium...
It seems to me quite obvious that Robbins is describing a Walrasian equilibrium where equilibrium prices depend on exogenous variables like taste and initial distribution of resources. He then goes on to say that society may choose not to allow such freedom to individuals (a different end). But the role of the economist is not to judge society’s choice but merely to say that-by referring to the potential competitive free equilibrium as the benchmark-the actual allocation does not satisfy the cost minimisation principle (that a policy is or is not ‘economical’). In other words, Robbins thinks that economists must always refer to the benchmark of efficiency (which solves the implicit problem of plentiful of means) to evaluate any existing equilibrium. However, by saying this, Robbins is contradicting the most important edifice of his structure: that the means-ends examination is value free.

There is an appearance of ethical neutrality in this kind of claim. Irrespective of what people want, economists would only comment on whether they do minimises cost. But this could only be meaningful if given what people want, there could have been a competitive free equilibrium. If we refer to the metaphor from section 2 it is clear that for the first society, if left to their own devices, competitive equilibrium would emerge. However, the purely spiritual society, if left in complete freedom would not necessarily produce a competitive equilibrium. Unless, of course, if we suppose that their ethical disposition is just a façade and that underneath every one of them lurks a member of the materially motivated society.

Therefore, for economics to be value free in a meaningful manner, there must be a co-ordinated outcome, or equilibrium, to all possible sets of ends. By adding Wicksteed’s qualification that each individual would benefit the other while achieving his own ends, we may say that such an equilibrium should be not only productively efficient but also efficient in the Pareto sense of it. Indeed, according to Robbins, the description of this co-ordinated outcome is what economics is all about (p.67). Without it, there is little economics could say which could potentially be value free. Thus, when we discuss taxation we are not interested in the influence it would have on production or distribution as we are interested in the comparative statics of an equilibrium without taxes and an equilibrium with taxes (p.69).

3.2 Equilibrium and the natural state

Robbins is clearly focused on equilibrium: instead of causes of wealth we “enquire rather concerning the conditions of equilibrium of various economic ‘quantities’” (p.67). But “we regard [the economic system] as a series of interdependent but conceptually discrete relationships between men and economic goods; and we ask under what conditions these relationships are constant” (p.68). This seems to suggest that equilibrium is a possibility but only achievable under some conditions. This already implies a departure from the ethical neutrality.

distribution of goods between different uses, of equilibrium of exchange and of the formation of prices” (emphasis added p.75).
We said in the previous sub-section that for economists to be able to say anything meaningful they must be able to compare a free competitive equilibrium to an actual equilibrium (or even just a set of prices - there is no obvious commitment to a continuous equilibrium although there is some ambiguity with this respect in Robbins). But why should the circumstances of ‘freedom’ in the sense required by the competitive model and which Robbins enumerates (p.143) constitute a benchmark? Is it not in itself an ethical choice? The fact the we allow society to choose other ends is just a pretence because in the end, economists would judge it on the basis of criteria about which society may not care. Does it not make economics irrelevant rather than ethically neutral?

However, the benchmark of free competition could become a meaningful-value free-benchmark if one argued that this is what would happened not in complete (state-protected) freedom but rather in the natural state of things.

On this question, it seems that Robbins is fluctuating between the empiricism of classical economists like Smith and the rationalism of Walras:

“Although Adam Smith’s great work professed to deal with the causes of the wealth of nations…the central achievement of his book was his demonstration of the mode in which the division of labour tended to be kept in equilibrium by the mechanism of relative prices-a demonstration which, as Allyn Young has shown, is in harmony with the most refined apparatus of the modern School of Lausanne” (pp. 68-9).

Obviously, Robbins treats Smith’s idea of ‘natural liberty’ as the conditions under which competitive general equilibrium emerges. Namely, if things were left to themselves, there will always be an equilibrium regardless of what ends people seek to achieve. In other words, equilibrium is a natural, empirical, phenomenon. The Walrasian system, in that vain, is merely an exposition of the nature of equilibrium which would emerge in natural liberty.

Robbins does not seem to be conscious of the shift in paradigm between Smith and Walras nor their epistemological differences. It is not very difficult to establish that Smith was an empiricist while Walras was a rationalist. The significance of this is that the Walrasian general equilibrium is reflecting the essence of things rather than what they are (their appearance). Therefore, in Walras, the idea of the order of natural liberty is universal and always holds, logically. But this does not mean that it also holds empirically.

To a great extent, this is what Robbins (and Wicksteed) is claiming. I have shown a few examples why Robbins’s equilibrium is more Walrasian than Austrian. We also saw that the way to accommodate the multitudes of ends with a neutral economics would come from having a universal benchmark of competitive efficient equilibrium. The Walrasian system offers exactly that. Moreover, even though Robbins finds it easy to move from “equilibrium of exchange and the formation of prices”, Walras faced a monumental task in answering the question of how does the world of ideas relate to the world of
appearances. This means that while reality may produce various types of equilibria, the value free one is the free competitive Walrasian equilibrium. So according to this story, every set of aims has a potentially efficient equilibrium which economists can meaningfully compare to the real relationship between economic quantities.

However, in the case of Smith, things are a bit more complicated. While it is true that Smith-in the true spirit of enlightenment-believed that if things were left to themselves, an order would emerge, the type of order was not independent of the ends which people seek to achieve. In his *Theory of Moral Sentiments* (Smith, 1976) he famously describes two types of society. The one, a society of benevolent individuals, will flourish and be happy as people help each other because they care about them. The other, a society of self-interested people, would also survive as mutual help is given in a mercenary way but would be considerably less happy. Naturally, the former, co-operative, society would not need a concept of equilibrium to explain the relationships between economic quantities. The latter would. So does this mean that the natural order is that of competition and equilibrium or that of co-operation?

Moreover, while Smith recognised that a natural order would emerge, his concept of equilibrium was very different from the Walrasian one. What mattered most were the dynamic implication of the static equilibrium rather than the mere existence of a co-ordinated outcome. This is why Smith makes a clear distinction between equilibrium at natural rates and equilibrium at market prices. The former is conducive to growth the latter not so. As the economic problem which Smith was trying to solve was that of growth maximisation, the mere co-ordination was, in itself, unimportant.

For Robbins, Smith’s contribution is not the distinction which he draws between the two types of equilibrium but rather the mere notion that there always exists a natural order. By removing the interest in the ends which society wishes to promote, he empties Smith’s distinction between types of equilibrium and invites the Walrasian system to describe them. In this way, the Walrasian logical structure becomes universal.

In short, it seems that by connecting Smith to Walras, Robbins turns the rationalistic tool of general equilibrium into what Smith treated as natural state. If so, it is logically clear why the benchmark of economic pronouncement would be the free competitive equilibrium. It is not an ethical choice but a description of the world before any institutional intervention. It is a good world. Smith, of course, would not have agreed to

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66 Smith argues that human beings are naturally social creatures who "stand in need of each others assistance" (Smith, 1976, p.85). It is the way in which this 'necessary assistance' is being provided which will determine the kind of society that will emerge. Smith proposes two possible frameworks of social organization which are both natural and viable. One, where the 'necessary assistance is provided from generous and disinterested motives', the other, where such assistance is provided for its utility, by means of 'mercenary exchange' (in other words, from interested motives). The former is an 'agreeable and happy state', the latter, much less so.

67 For many Smith’s scholars, the mere mentioning of general equilibrium in the context of Smith is a travesty. Nevertheless, I do believe that there is a concept of general equilibrium in Smith but it is very different from the Walrasian one. The question is developed in two of my papers Witztum (2007) and (2008b).
According to him, the natural state (and therefore, the ostensibly value free reference point) depends on what motivates individuals.

3.3 Natural state and society

Neither Walras nor Smith, would have agreed to the notion that economics can be separated from ethics. In the case of Smith this is quite evident as we saw through the example of the two types of societies that may exist. Surely the society of benevolent individuals would not find them compete against each other. Nor would the societies of monks, in the metaphor of section 2 above, end up in a competitive state which requires equilibrium to co-ordinate their behaviour. To compare the actual outcome that emerges to the competitive equilibrium would be a meaningless exercise.

In the case of Walras it would not hold either as while the science of economics describes the essence of the value in exchange, it is the social process of appropriation which pours content into it. In other words, we need to know first what it is that made people depend on each other before we can explore how they would co-ordinate this interdependence.

Robbins’s and the modern story begin with identifying the subject matter of economics as all those things which are both scarce and desirable. This, in turn, gives rise to the important distinction between allocations which are merely feasible and allocations which constrained our desirability (i.e. efficiency). Therefore, given the difference in individuals’ abilities, it stands to reason that the first action a rational person should take to solve his, or her, individual problem of reconciling want with scarcity, would be to specialise and trade. Thus, individuals become dependent on each other.

However, the reason why people become dependent on each other is really a rational construction. We assume that individuals, as a matter of fact, seek to maximise - among other things - their material wealth, and therefore, they become dependent on each others. In other words, there is no underlying social dimension to why individuals depend on each other and therefore, there is no need to add a social dimension to the co-ordinated outcome.

The absence of the social dimension in the description of what made people depend on each other is also the reason why Robbins can easily dismiss ethics. After all, by social dimension we refer to the way people view the others which, as a matter of fact, is the foundation of ethics. The question here is not what it is that they consider to be right or wrong but rather the fact that if they have any particular attitudes towards the others, it might affect the reason why they depend on them as well as the means which govern their interaction.

Smith too begins the Wealth of Nations with a description of specialisation and the subsequent dependence which exists among members of society. But Smith’s theory does not begin in the Wealth of Nations. It begins much earlier (in the Theory of Moral Sentiments and the Lectures on Jurisprudence) where he is trying to understand what it is that brings people together.
He first observes that people are social beings in the sense that they seek the approval of the others. He then describes how they go about it from early stages of society. Namely, Smith does not follow a rationalist agenda but rather approaches the subject as a true empiricist. Notwithstanding whether his anthropology is correct or not, Smith forms the view that people started to specialise and trade because they wished, at first, to confer presents on the other to acquire their approval. It is a long and complex story which I do not wish to repeat here but in the end, the driving force behind specialisation and trade in his theory is the pursuit of social approval (through the deceptive powers of wealth).

In the end, both the Robbinsian and the Smithian narratives lead to the same conclusion: individual specialise and trade and thus become dependent on each other. However, the fact that in Smith they do so in search of social approval makes a great deal of difference. The question we must answer is not whether we reached an outcome which is or is not economical but rather whether individuals succeeded in achieving their objectives through the co-ordinated outcome. The distinction he draws between equilibrium in natural rates or market prices partially answer this question.

Smith’s agenda has not been completed by him as there is a question which remains unanswered: how does the fact that individuals seek the others’ approval affect the development of social/ethical norms of behaviour and how do these affect the nature of human interaction. Would competition be the right mechanism of co-ordination if people become more socially minded?

Throughout history, the interest in the question of social and economic organisation has always been coupled with an understanding of what brings people together. Plato (1974, pp.115-122), for instance, claims-through Socrates’s voice-that there are two principle bringing people together: they need each other to supply their needs and they have different aptitudes (Book 2 part 2 section 1). Therefore, in a Hobbsian manner, individuals have no choice but to become members of society and therefore, the question of how to organise it becomes external to their attitudes towards one another.

Aristotle, on the other hand, claims that “observation tells us that every state is an association, and that every association is formed with a view to some good purpose” (Aristotle, 1957, p.54). This means that the reason which brings people together is fundamentally moral. Not surprisingly he ends up promoting self-sufficiency and condemning trade (beyond the satisfaction of needs) as immoral.

Nearer to today we have the example of J S Mill who believed in the evolving nature of human character. For him, individuals will become increasingly co-operative which, in turn, could make competition, and equilibrium, redundant concepts of social organisation.

All of this tells us that if indeed, equilibrium describes the natural state, we must begin to ask what it is that makes people dependent on each other. I believe that what makes people dependent on each other is bound to influence their attitudes towards the others.

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68 See Witztum 2007.
This, in turn, will inevitably feed into the way moral notions are formed (what I call positive ethics) which, in turn, is bound to affect whether social interaction is predominantly competitive or co-operative in nature.

4. Opportunism and Ethics

Thus far we have argued against the exclusion of ethics on the grounds that in Robbins’s system there is a clear, well defined - yet hidden - economic problem. Namely, he was right to say that in neo-classical economics the problem is not that of wealth creation (growth) but this is not because we no longer care about the aims. Instead, it is so because we have a different, more static, economic problem of wealth maximisation (output maximisation). As such, it cannot really be considered to be value free.

We also saw that as the benchmark of economic analysis is cost minimisation, and as costs are conceived in terms of opportunity costs, there is also a benchmark of competitive general equilibrium. Therefore, in order for economists to examine any outcome of interaction between individuals with diverse objectives, we must be able to compare this to a competitive outcome of that very system. But could all sets of objectives be conceived in this way without placing a greater value on competitive behaviour than any other form of interaction?

In the final stage we now move closer to examine the presumption behind the means-end domain of economic analysis: that individuals always seek to choose the best means to an end.

“The propositions of economic theory”, writes Robbins, “like all scientific theory, are obviously deductions from a series of postulates” (p.78). These postulates must be based on experience: “We do not need controlled experiments to establish their validity; they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious” (p.79).

The fact that he accepts the idea of the rational utility maximiser as one of economics postulates is well documented in chapter 4 section 5 (pp. 90-93)69. It means that the agent behind Robbins’s system is the opportunistic agent who would always choose the best means to an end regardless of what is the end.

But how does this subscribe to the ‘experience’ element of the postulate? While there might not have been a lot of information to the contrary during Robbins times, the long term validity of Robbins’s claims must be based on whether it survives the experience of time. The ideas of cost minimisation and general equilibrium still flourish in the economics literature but what about the opportunistic individual?

69 O’Brien (1990) reiterates this by saying: “Thus economics was essentially to be about choice models. Rational behaviour, which amounts to acting consistently in accordance with the ranking of different possibilities, lay behind demand” (p.158).
Recently, Rubinstein (2007) discovered an interesting aspect of rational behaviour. He showed that most respondents to a game who chose the less likely strategy predicted by a Nash equilibrium spent less time in making their move. Those who chose the ‘right’ rational strategy spent more time before responding. This may suggest that rational behaviour à la opportunistic nature is perhaps less intuitive than we think. By implication, it would somewhat discredit Robbins’s belief that the postulate of human behaviour is based on our experience and understanding of a Weberian nature (i.e. introspection).

Of course, one may argue that the intuitive and the rational are not the same thing and we should not expect them to correspond well as the latter implies some learning. But what it is that we learn cannot be divorced from the institutions which society creates. In other words, people must first figure out what are the rules of the game (literally speaking). However, they are not offered a choice between different types of games and the competitive nature of the interaction is not up for discussion. In this respect, the fact that it takes people longer to do the ‘right thing’ in terms of the competitive game means that doing this thing is not what is most natural or intuitive for them.

In another fascinating study of ‘Human Sociality’ which looks at responses to games across cultures and continents⁷⁰, the authors conclude: “there is no society in which experimental behaviour is consistent with the canonical model from economic textbooks….”(Heinrich et al, 2004, p.10). This too, flies in the face of Robbins’s demand for universality: “[Economic analysis] consists of deductions from a series of postulates, the chief of which are almost universal facts of experience present whenever human activity has an economic aspect” (Robbins, 1935, pp.99-100).

But the key element in Robbins’s conception of rationality which frees him from the chains of ethics is not so much the notion of consistency as it is the notion of an ‘isolationist’ value free individual. Again we must ask whether this too, is a postulate which is so obvious and based on experience that we must accept it at face value.

There is growing evidence that the ‘others’ do matter in the consideration of individuals. Fehr and Schmidt (1999), for instance, observe that the results of a number of experimental games do not seem to be consistent with our traditional views of the self-interested rational utility maximiser. Instead, they found that by allowing a certain distribution of 'other-regarding' characteristics (notably, 'selfishness' and 'fair-mindedness') they could better explain these results.

More recently, Algan and Cahuc (2006) showed that the ability of the Danish model which combines high unemployment benefits with low job-security to generate high participation rates could be explained by civic attitudes. Namely, the reason why people would not just opt to claim the high unemployment benefits instead of engaging in the precarious world of work is embedded in the public perception that it is immoral to claim

⁷⁰ There are, of course, many methodological issues associated with such a study from the significance of experiments in general to the ability of people across cultures to understand the issues in particular. Nevertheless, in as much as these anecdotal information relates to our individual experience, there is a lot in here to reject the claim that the rational utility maximiser is based on our introspection of the world.
something which has not been earned. This means that not only do ‘others’ matter as individuals but also society, as an institution, matters.\textsuperscript{71}

If we accept that individuals are social beings who have views about the others then these views are bound to determine how rules of behaviour develop. Hence, for instance, in an Aristotelian society that which brings people together is the search for the good. Aristotle observes (so he claims) that people want to do good things. He then also observes that what people consider as a virtue is ‘lying in the mean’, namely, moderation is the core of virtue. In a manner of speaking he combines these two to create the economic institutions which would support the just society. As virtuous people behave moderately, they will not seek material wealth beyond what is needed for a moderate life-style. Property should therefore be privately owned as people specialise within the household to extract as much as they can from their land. Whatever they need beyond that which they can supply themselves with, they can exchange in the market under rules which are consistent with their virtue-based ethics. As the objective of society is the just society, the criterion of economic performance should be justice and in this case, the just price. As everything is based on moderation, the just exchange is based on proportional reciprocation.\textsuperscript{72} It does not really matter much that a cross household division of labour would have provided much more output; it would be meaningless to say that against a benchmark of a competitive economy, the just economy is inefficient.

Can we describe the behaviour of a household which divides labour internally rather than across households, and is still unable to directly satisfy all its needs as choosing the best means to an end? We know that had they specialised and traded across households, their needs would definitely be satisfied. If their end is to be fitting participants in a just society, they may have chosen the best means to an end but this would not be consistent with the cost-minimising end of Robbins’s economic agent. In fact, it would have nothing to do with cost minimisation as Aristotle denounces trade with the purpose of wealth augmentation as immoral.

But while our Aristotelian agent seems to be choosing the best means to an end, it is not entirely opportunistic for two main reasons. Firstly, the rule of ‘lying in the mean’ implies that the means by which you achieve your objectives must be moderate. You cannot, for instance, take over someone else’s more fertile land because it would allow you to satisfy your needs at greater ease. In other words, the means are determined by the collective rather than the individual end. The individual end of being part of society means only to conform to the rules laid down by society. Secondly, the rule is not opportunistic as whatever other end individuals seek to achieve, it has to be followed according to the rule of ethical behaviour. In other words, the social rules prevent the individual from using means he, or she, might have chosen had they not wished to be part of this ethical society.

\textsuperscript{71} Meier (2006) provides a summary of public good games which tend to highlight the importance of others in the considerations of rational individuals.

On the other hand, Adam Smith thought that people are social beings who are in need of social approval. This means that they specialise and trade and become dependent on each other but the measure of whether natural liberty has solved their economic problem depends on whether people have managed to get what they wanted out of the system. In particular, it is possible to show that the ethics of those who inhabit the Smithian world would be offended if the distribution of things was left in the hand of ‘nature’. In other words, because individuals seek social approval, they created a system of ethics which is based on sympathy and according to which, unintended consequences and the distribution of income which will emerge in natural liberty are morally unacceptable\textsuperscript{73}. This means, that society must interfere for the co-ordinated outcome to be consistent with the motivation which gave rise to it in the first place.

What the Walrasian system managed to show is that if people seek only to maximise their own material well-being by specialising and becoming dependent on each other, individual objectives could be achieved by the co-coordinated outcome. Namely, if we begin the story without any reference to what brought people together, then it is possible to ignore those elements in individuals character. However, as we have been demonstrating, this is not consistent with searching for postulates which are based on recognisable experience or, even more contentious, on our introspection.

But there is a far more serious problem with the Robbinsian presumption that economics, based on rational utility maximisers, is neutral to the end which people seek to obtain. It is the question whether it is at all consistent to have a social dimension and at the same time, behave opportunistically. For Robbins’s conception to work this must be true as otherwise, how could we say that economics is relevant whatever it is that people seek to promote?

Weber (1922) suggests that there are two forms of rational actions. One, which he calls \textit{Zweckrationalität} (instrumental rationality) and the other, which he calls \textit{Wertrationalität} (Expressive/value rationality). The difference between them is as follows: instrumental rationality corresponds perfectly to Robbins’s definition (which is the same as the rational utility maximiser) where we always choose the best means to an end regardless of what is the end. Expressive (or value) rationality, on the other hand, is when an agent acts out of commitment to a value (\textit{wert}). He, or she, is overwhelmed by his objective so that he does not necessarily choose the best means to an end.

Naturally, acts of expressive rationality would correspond more to what we said about individuals’ initial interest in the others as well as the ethical values which had been formed. In other words, expressive rationality seems the relevant form of behaviour in that which forms society. Instrumental rationality, on the other hand, requires no such references whatsoever. To some extent one can say that expressive rationality represents actions which are dominated by ethical/social values while instrumental rationality actions are dominated by one’s own interest.

\textsuperscript{73} See a discussion in Witztum (2008c).
In line with Robbins’s claim that ends do not matter, many modern economists extend the model of the rational utility maximiser to include a great deal more social dimensions. Nevertheless, the opportunistic nature of individuals is never examined in the light of these new dimensions. In fact, the opportunistic nature of individual behaviour seems to emerge as one of the strongest elements in economic analysis. Even at the time when faith in the other aspects of the neo-classical paradigm, notably, general equilibrium and welfare economics, is fading, the rational utility maximiser remains the fundamental feature of any ad-hoc work in economic analysis. Moreover, with the rise of the new political economy, the same opportunistic behaviour has now become the foundation of far reaching grand theories attempting to explain democracy (Acemoglu and Robinson (2005)) on the one hand, and theories about beliefs in a just world and redistributive policies (Benabou and Tirole (2006a), on the other.

Thaler (2000), predicted that in the new millennium, “Homo Economics will evolve into Homo Sapiens” (140). He meant to say that the agent in economic analysis would be depicted in a more realistic manner allowing for other-psychological and social-dimensions to become part of it. But what we see instead, is just an expansion of the domain of opportunistic behaviour rather than a move away from it.

Indeed, cognitive dissonance, identity, social norms, beliefs etc., have all become part of the application of the rational utility maximiser into as many areas of social analysis as possible. However, the notion of the individual as a machine, which is constantly engaged in choosing the best means to an end, has not changed much. The agents behind all of these models are utility maximisers who either face complex constraints or have preferences defined over some social aggregates. In the end, however, all decisions are based solely on the proposed consequences of the actions for the actor and all actors are characterised by the same rule of behaviour.

In other words, economists are willing to engage in extending both the psychological and sociological aspects of human action but without forgoing the opportunistic nature of the rule of action. The complex social arena presents itself through the fact that other people’s action may affect the consequences of one’s own actions.

But how can we reconcile the social dimension we observe in individuals and the opportunistic behaviour which is embedded in the rational utility maximiser? Consider the following diagram:

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The text continues with a discussion of the integration of social dimensions into economic analysis, mentioning various researchers and their contributions, such as Conlisk (1980), Banerjee and Besley (1990), and Bernheim (1994), among others. The text also references Akerlof and Dickens (1982) on cognitive dissonance and willpower and motivation, Benabou and Tirole (2006b) on social attitudes, and Witztum (2008a) on the distribution of social attitudes and their influence on economic outcomes.

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74One line of research has been focused on how the actions of the others may influence the behaviour of a traditionally self-interested individual (see, for instance, Conlisk (1980), Banerjee and Besley (1990), and Bernheim (1994)). Another, focused on how other social constructs, like custom, equity values, stigma and status may influence the behaviour of the same self-interested individual (see, for instance, Akerlof and Yellen (1990), Agell and Lundborg (1995), Bingley and Walker (1997) and Fershtman and Weiss (1993)). There are also a host of papers on psychological effects (Akerlof and Dickens (1982)), cognitive dissonance, willpower and motivation (Benabou and Tirole (2006b) and many others. In Witztum (2008a) I am trying to add to this literature by considering social attitudes and their distribution as another means of departure from the monolithic rational utility maximiser.
The horizontal axis depicts the domain of an agent’s consideration in terms of how far does a person consider the consequences of his actions to others. The vertical axis depicts the agent’s objectives in terms of what he intends to the others.

If our conception of rationality is based on Robbins’s expectation of consistency then surely we would expect people who are self-interested not to care about the effects of their actions on others. Equally, we would expect people who are socially minded and who wish, in their actions, to work for the others, to take into consideration as far as possible the effects of their actions. Thus we have three domains. In the middle domain we find a consistent view of Weber’s instrumental rationality. Points within this domain suggest that there is a proportional correspondence between our intentions and the degree to which we consider the effects of our actions. A selfish individual would be at the bottom left end where he intends nothing for the others and takes no notice of how his actions may affect them.

The top domain contains what Weber referred to as expressive rationality. Here, people aim at benefiting a great number of people but do not consider the effects of their actions on them predominantly because they are unable to do so. In other words, they are overwhelmed by the objective and do not necessarily choose the best means to an end. The bottom right domain is that of irrational behaviour. We intend nothing for the other but we spend a lot of energy in assessing how our actions might affect them.

There is no problem at all in saying that Robbins’s rationality is the instrumental one. However, it would have to mean that his system is inhabited by self-interested
individuals. To be socially minded and instrumentally rational (as Robbins would have us believe with the universality of ends) sounds an impossible task. Our ability to calculate the effects of our actions on ourselves is quite limited let alone on the multitudes of others. Thus, if we accept that our computational skills are limited then the only type of instrumentally rational agent would be the self-interested one; any type of socially minded individual is more likely to be expressively rational.

In other words, once we take into account that experience suggests that people have a social dimension to their behaviour, we can no longer attribute to them the opportunistic behaviour embedded in the rational utility maximiser. Therefore, the type of ends which people seek to achieve will affect the way in which they act. Therefore, the co-ordinated outcomes of such interactions are bound to be different to the one produced by a Walrasian system. Hence, the economics of Robbins is the economics of self-interest and consequently, open to ethical criticism.

5. Conclusions

There were two explicit foundations and one implicit foundation to Robbins’s call for the ethical neutrality of economics. The two explicit elements were where the focus was on the means in the means-ends analysis and the presumption of opportunistic behaviour (i.e. that people always choose the best means to an end). The implicit foundation was the presumption of the universality of competitive equilibrium.

Through reference to Robbins’s sources of influence (notably, Wicksteed and Wieser) we tried to show that what Robbins meant by claiming that economics is only dealing with the means is cost minimisation. In other words, while economics is totally disinterested in what people aim to achieve, it should always judge whether the choice of means had been ‘economical’ in Robbins’s language or ‘waste minimising’ in Wicksteed’s terminology.

However, this creates two main problems. Firstly, Robbins followed Wicksteed in defining costs as opportunity costs which are evaluated through prices, and only in equilibrium (competitive one) would prices reveal the real costs. Secondly, cost minimisation is really the dual of some sort of output maximisation which, while different from the Classical problem of wealth creation (growth), is nevertheless an end which should and could be the subject matter of ethical scrutiny.

The second problem suggests that economics is not really about the ‘means’ element of the means-ends problem but an end which seems to be superior to all others. While it is true that Robbins agrees that societies may choose to tolerate lesser degrees of efficiency, the mere fact that economists are there to tell them that this is the cost of their choice implies that the end of output maximisation (or the maximisation of means, as Wicksteed puts it) should always be the measure against which all other aims should be compared.

The first problem suggests that economists can always compare the efficient outcome to any actual equilibrium. Namely, that whatever it is that motivates people, there can always be, in principle, a competitive equilibrium to their interaction against which any
actual choice of policy can be compared. But this assumes not only that people are opportunistic in the sense that they will always choose the best means to an end but also that they are always competitive. Would all sort of motivations necessarily lend themselves to competitive behaviour and subsequently, to a competitive equilibrium?

In other words, Robbins seems to suggest that competitive equilibrium corresponds to the notion of ‘natural liberty’ in the classical era. However, while it is true that major classical economists wondered about what would happen in complete liberty, some of them realised that this depends on what motivates individuals and what characters they develop. In other words, the degree to which individuals care about the other is bound to affect the kind of social norms and characters that would evolve in natural liberty. In some conditions this may indeed be the competitive paradigm but in others, more co-operative notions of co-ordination may need to be developed.

Opportunistic behaviour is an important element in Robbins’s claim to separate ethics from economics. The validity of the cost minimising exercise can only be meaningful if people, at all times, are interested in finding the best means to an end. However, Robbins expects the postulates of economics to be so obvious that they need no further examination. While this would not have been known to him, there is mounting evidence to shatter the belief in the introspection which claims people to be opportunistic. Yet as his definition of economics still lives in contemporary textbooks, this point has to be made.

Moreover, through the use of the Weberian distinction between two forms of rationality (instrumental and expressive) to show that if internal consistency is something which matters in the economic conception of rationality, then computational limitation would imply that instrumental rationality could only be guiding the behaviour of self-interested individuals. Therefore, the pretence as if modern economics is not limited to the study of self-interested behaviour must be rejected. As such, it is a perfect target for ethical discourse.
References


Robbins and Welfare Economics: A Reappraisal*

Roger E. Backhouse†

1. Introduction

According to standard accounts (see, for example, Blaug 1997),75 the significance of Lionel Robbins’s *An Essay on the Nature and Significance of Economic Science* (1932, 1935) for welfare economics was that it undermined the utilitarian foundations of Pigovian or Cambridge welfare economics, paving the way for the emergence of the new welfare economics based on Paretian foundations. Inter-personal comparisons of utility could not be part of economic science, implying that it was not possible to make any scientific judgements about the distribution of income.

This paper argues that, irrespective of whether or not this was the effect of Robbins’s essay,76 it seriously misrepresents Robbins’s aims in the essay. His argument that there was no scientific basis for inter-personal utility comparisons was part of a much broader argument about the inter-relations of ethics and economic science. His target was not Pigou (though he did make some specific, technical criticisms of Pigou’s work) but more radical welfare economists, such as J. A. Hobson and Ralph Hawtrey. There are four steps in the argument. (1) The *Essay* is written in a way that makes it seem very unlikely that Robbins’s main target was Pigou and extremely likely that it lay elsewhere. (2) Pigou’s commitment to utilitarianism was limited: given that he tried to confine his attention to what could be analysed scientifically, Robbins (who was in any case a utilitarian when it came to policy prescriptions) would have had little reason to disagree with him. (3) There was, in the 1920s, a significant group of writers who based conclusions about welfare on specific ethical judgements that went far beyond those made by Pigou, and (4) these writers were taken seriously by their contemporaries as welfare economists. The conclusion is that it was these economists, not Pigou, whom Robbins considered dangerous and wanted to controvert: to present Pigovian welfare economics as his target is to misrepresent the context against which his essay was written.

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*I wish to thank the many colleagues who have commented on various incarnations of these ideas. A partial list of those to whom I am indebted includes Susan Howson, Steven Medema, Tamotsu Nishizawa and Donald Winch.*

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75 Honesty requires that I also cite Backhouse (1985) as having accepted this account.

76 Evaluating this proposition would require a different type of argument and is not attempted here.
2. Robbins’s Critique of Welfare Economics

Robbins develops his arguments about welfare economics in three places. In chapter II he distinguishes between “Ends and means”. His central argument, that the determination of ends lies outside economics, follows inexorably from his famous definition of economics as “the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” (Robbins 1932: 15). In contrast to earlier writers, such as John Neville Keynes (1997 [1890]) who saw economics as comprising both science and art, Robbins equates it simply with the science, and hence with the relation between means and ends, not the determination of ends themselves.77

The discussion get closer to Pigovian welfare economics in chapter III, section 6, where Robbins criticises the approach of considering separately the production and distribution of wealth. Such an approach might follow from the materialist definitions of economics that he had considered and rejected in chapter I, but it was flawed in that “the idea of changes in the total volume of production has no precise content” (1932:66). Outputs are heterogeneous and all that we have are indices of total output, for which there is no justification at the level of theory. Instead, there should be a theory of equilibrium and variations. Production and distribution were part of the theory of equilibrium.

Finally there is chapter VI in which the first significant argument is a seven-page critique of the law of diminishing marginal utility (1932: 120-6). Here we find the well known argument, on which Hicks-Allen consumer theory was based, that preferences are orderings and that as a result it is illegitimate to draw conclusions that rest upon inter-personal utility comparisons. There is no scientific basis on which to compare one person’s satisfaction with that of another. Normative conclusions based on “social utility” have to be abandoned. After pointing out (in section 3) that the notion of equilibrium does not imply approbation he moves on (in section 4) to discuss two attempts to bring ethical criteria into economics. Here, his focus is on Hobson and Hawtrey. He presents them as having “urged that the boundaries of economics should be extended to include normative studies”: that economics “should pronounce on the ultimate validity of ultimate judgements of value” (Hobson) and that it “cannot be dissociated from ethics” (Hawtrey) (Robbins 1932:132, citing Hobson 1929 and Hawtrey 1926).78

Thus although Robbins does argue with great clarity and precision against there being any scientific basis for inter-personal utility comparisons, it is part of a broader argument in which clarity about ends and means, and hence about what can be seen as scientific, is central. This explains the otherwise puzzling feature of the essay that, if it were primarily an attack on Pigovian welfare economics, he does not focus at all on Pigou. Pigou is cited but not in any of the passages discussed above. He cites Pigou’s definition of economics as “the study of economic welfare” but, though he clearly rejects it along with other

77 It is interesting that Robbins appears not to even mention Keynes.
78 In Robbins (1935:147) the wording is changed, “valuations and ethical standards” replacing “judgements of value”.

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attempts to define economics in terms of its subject matter, he does no more than point out that extant definitions have very different implications (Robbins 1932:2). He follows this up (1932: 20, footnote 2) by including Pigou in a list of economists whose definitions define economics in terms of the problems discussed by Ricardo: they confine economics to “valuations of the market” instead of “press[ing] through to the valuations of the individual”. The text focuses on German writers alone.

There are, furthermore, other places where Robbins could easily have included Pigou in the list of those whom he was criticising but did not do so. When he lists economists who define economics as “the study of the causes of material welfare”, he cites Edwin Cannan, Alfred Marshall, J. B. Clark and, perhaps surprisingly, given that Robbins considered his approach different in other respects, Vilfredo Pareto (Robbins 1932:4). In arguing against making the distinction between production and distribution central to economics, his target was clearly Cannan. Marshall’s *Principles* is criticised, but only by comparing “the spineless platitudes” of Book IV with the “masterly sweep of Book V”, the latter dealing with “problems that are strictly economic in our sense” (Robbins 1932:65). Even more surprising is the failure to cite Pigou in the discussion of diminishing marginal utility and inter-personal utility comparisons. He cites Francis Edgeworth and, yet again, Cannan (Robbins 1932: 120). Perhaps he had Pigou in mind when referring to “numberless works on Applied Economics” or “the great majority of English economists who regard these propositions [concerning diminishing marginal utility] as axiomatic” but he did not mention him (1932:121).

Of even more interest is the discussion of the relation between economics and ethics in the final chapter of the *Essay*.

In recent years, certain economists, realising this inability of Economics, thus conceived, to provide within itself a series of principles binding upon practice, have urged that the boundaries of the subject should be extended to include normative studies. Mr. Hawtrey and Mr. J. A. Hobson, for instance, have argued that Economics should not only take account of the valuations and ethical standards as given data in the manner explained above, but that also it should pronounce upon the ultimate validity of these valuations and standards. “Economics”, says Mr. Hawtrey, “cannot be dissociated from Ethics”. (Robbins 1932:132)

He criticises Hawtrey and Hobson for, essentially, not accepting his definition of economics: for not accepting that there was a “logical gulf” between positive and normative studies that no amount of ingenuity could bridge (ibid.) Whilst conceding that economists needed to concern themselves with normative issues, these were, from the viewpoint of economics, “outside interests” (1932: 134). Precision in economic arguments required that discussion of ends be kept strictly separate from discussion of

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79 This discussion of Marshall was removed in the second edition (1935:65) where Robbins stops short at endorsing Schumpeter’s “shame at the incredible banalities of much of the so-called theory of production”.
means. Ends were “ultimates” on which there was either agreement or disagreement, whereas rational analysis should produce agreement on the relation between means and ends.

Shut Mr. Hawtrey up in a room as Secretary of a Committee composed of Bentham, Buddha, Lenin and the Head of the United States Steel Corporation, set up to decide upon the ethics of usury, and it is improbable that he could produce an “agreed document”. Set the same committee to determine the objective results of State regulation of the rate of discount, and it ought not to be beyond human ingenuity to produce unanimity – or at any rate a majority report, with Lenin perhaps dissenting. (Robbins 1932: 134-5).

It is in this section that Robbins is addressing contemporary welfare economics and what is significant about it is that that he does this not by arguing that Pigovian welfare economics smuggles in unjustifiable value judgements but by criticising two economists who sought deliberately to extend the scope of welfare economics to encompass ethics.

3. Pigovian Welfare Economics

Pigou was clearly the most prominent welfare economist of the period on account of his position as Marshall’s successor and through the magisterial quality of his two books, *Wealth and Welfare* (1912) and *The Economics of Welfare*, which went through four editions between 1920 and 1932. The contents of these two volumes owed much to his Cambridge predecessors, Henry Sidgwick and Alfred Marshall. He drew on and developed Marshallian theoretical tools to analyse the role of the state in a manner reminiscent of Sidgwick. As O’Donnell (1979: 588) put it, he offered “Sidgwickian philosophy couched in Marshallian methodology”. This tradition had its roots in utilitarianism – it was aggregative and hence formally required inter-personal utility comparisons in order to be able to add together different individuals’ utilities. To this extent it was intrinsically utilitarian. However, by Pigou’s time, the utilitarian element had become severely attenuated (see Backhouse 2007). In his *Methods of Ethics* (1874), Sidgwick had tried to defend utilitarianism, but he was not successful, for he could not prove that utilitarianism was superior to egoism. Marshall, whose youthful analysis of behaviour was in terms of learned routines, was more evolutionary than utilitarian (see Raffaelli 2003). For Pigou’s generation, there was also the influence of G.E. Moore who moved even more decisively away from utilitarianism and any notion of theism. His thoroughly secular *Principia Ethica* (1903) became the bible of the Cambridge “Apostles”: for him the good was apprehended directly, and could not be justified by appeal to utility or any other criterion.

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80 In a footnote, Robbins (1932: 132, n. 1) claims that his discussion differed from that in Robbins (1927) because he had come to understand the importance of precision in economic generalizations.
In his two books on welfare economics, Pigou took much from his Cambridge predecessors. Welfare comprised states of consciousness and ‘economic welfare’ comprised that part of welfare that arose ‘in connection with the earning and spending of the national dividend’. Welfare increased with both the size and distribution of national income and decreasing in its variability. Using this as his framework, Pigou then proceeded to analyse the case for state intervention, finding many cases in which private enterprise would not result in a welfare-maximizing allocation of resources. Yet though his aggregative approach can be seen as utilitarian, dependent on inter-personal comparisons of utilities, it was a highly qualified utilitarianism. He talked in terms of “satisfactions” and “desiredness” rather than utilities, implying that he was thinking more in terms of preferences, thereby distancing himself from hedonism.

Moreover, in making it clear that “economic welfare” was only a part of overall welfare – that part of welfare that could be brought into relation with the measuring rod of money – he was clearly limiting his attention to what he believed could be analysed scientifically.

Economic welfare, however, does not contain all welfare arising in this connection [the earning and spending of the national dividend]. Various good and bad qualities indirectly associated with income-getting and income-spending are excluded from it. It does not include the whole psychic return, which emerges when the objective services constituting the national dividend have passed through the factory of the body; it includes only the psychic return of satisfaction. Thus economic welfare is, as it were, a part of welfare. (Pigou 1912: 3-4; emphasis in original)

However, after recognising that welfare depended on cognitions, emotions and desires as well as satisfactions, and after having and discussed the importance of feeling, character and conditions of work on welfare, he neglected them. (c.f. Levin 1956: 128). National dividend was treated as a good proxy for welfare. Pigou also found many reasons to shy away from the radical implication that utilitarianism, given his assumptions about utility functions, implied equalizing income. Incentives mattered. In other words, having simplified his utilitarianism, by basing welfare economics on a minimal set of ethical judgements, Pigou found reasons why not all of its conclusions were to be followed. One might conclude from this that Pigou was using utilitarianism as little more than a framework within which to pursue the ‘scientific’ and ostensibly ethically neutral economics of Marshall. Robbins will have been sympathetic with much of this.

4. Welfare Economics as a Critique of Economic Science

A more radical approach to welfare economics came out of Oxford, where, T. H. Green put forward an ethical creed based instead on idealism, using the language of broad-church Christianity (though some of his critics questioned whether he had in fact abandoned this) and where John Ruskin was developing a clear ethical critique of...
economic values.\(^{81}\) Oxford was the Christian Socialism of men like Bishop Charles Gore with its roots in the Oxford Movement. Between them, these thinkers inspired a group of economists who brought much more specific ethical judgements to bear on questions of welfare than did members of the Cambridge tradition. These included J. A. Hobson, who left Oxford to become involved in various ethical societies in London,\(^{82}\) and R. H. Tawney, a lifelong Anglican and Christian Socialist.

The most developed welfare economics came from Hobson, whose *Work and Wealth* (1914) was presented as being to complete the task identified by John Ruskin: ‘to determine what are in reality useful or life-giving things, and by what degrees and kind of labour they are attainable and distributable’ (*Munera Pulveris*, quoted in Hobson 1914: 10). He wrote of the need to develop a human standard of value and to analyze ‘organic welfare’, taking account of the organic structure of society, the value of whose parts depended on the whole. Hobson agreed with Pigou’s remarks (quoted on page 90 above) about the difference between welfare and economic welfare, but saw this difference as highly significant. The main reason would appear to be that Hobson was willing to use a broader range of value judgements than Pigou, many of those taken directly from Ruskin, even though these could not be quantified. He attacked the idea of economic science, with its links to quantification and the search for exactness.\(^{83}\) The values involved came from shared human experience: “the nature and circumstances of mankind have so much in common, and the processes of civilisation are so powerfully assimilating them, as to furnish a continually increasing community of experience and feeling. It is, of course, this fund of ‘common sense’ that constitutes the true criterion [of welfare]” (Hobson 1914: 321).

Perhaps the most widely known representative of an “Oxford” inspired welfare economics was R. H. Tawney’s analysis of the acquisitive society, defined as a society in which priority was given to protecting economic rights, in particular property rights, whilst leaving economic functions to fulfil themselves, except under exceptional circumstances (Tawney 1920: 17). Tawney argued that goods and activities should be judged according to the contribution made to the public purpose. People were not isolated individuals but parts of societies that had common goals and purposes, or moral principles. After saying that increased production was important, he claimed that ‘plenty depends upon co-operative effort, and co-operation upon moral principles’ (Tawney 1920: 5). However, individualism had destroyed these moral principles and the purpose, without which society could not exist.

An echo of Green’s idealism is found in Tawney’s rejection of the utilitarian criterion, as too individualistic, denying the existence of any common end for society (Tawney 1920: 17). Society was not an economic mechanism but a community of wills (Tawney 1921:

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\(^{81}\) A more detailed discussion is provided in Backhouse and Nisizawa 2007.

\(^{82}\) These were essentially secular groups, though some of them had Unitarian roots, that sought to provide institutions in which ethical values could be explored and promoted, but without traditional Christian doctrine.

\(^{83}\) It is possible to argue that Hobson was wrong to adopt such a narrow view of science and that his own work should be regarded as a contribution to science not as an attack on it. See Backhouse (2007).
Given this view that societies had, or should have, common purposes, he was able to argue that some goods and activities were better than others, and that part of wealth was waste, that should never have been produced when there was still useful work to be done (Tawney 1920: 21).

A similar line was taken by Henry Clay, a colleague of Tawney’s in the Workers Education Association in his very widely used *Economics: An Introduction for the General Reader* (1916; 1918). Clay did not go so far as Tawney in his view of society as involving a shared purpose, or in arguing for a functional view of society, in which rewards are clearly linked to the functions performed, where these are valued in relation to society’s goals. However, he shared Tawney’s view that social welfare has to be judged against shared moral values and that the dominance of commercial activities can undermine those moral values. For both of them, there is simply no basis on which positive and normative economics could be separated. Normative judgements had to be made on the basis of moral values that are themselves a function of the economic organisation of society.

Clay distinguished between “economic wealth” and welfare. Economic wealth is the output of the economic system and comprises both good and bad wants and is, in principle, measurable (1918: 415–7). In contrast, welfare depends on ethical views, counting wants differently according to whether they were good or bad. It was subjective and might not be measurable. He found many reasons why there might be little relationship between welfare and economic wealth, notably that preferring less would raise welfare.

Materialism is the subordination of the internal sources of satisfaction to the external; most religions exalt the internal over the external, and teach that welfare lies in the former, to which the latter must be sacrificed: ‘The Kingdom of Heaven is within you.’ (Clay 1918: 447–8)

Because welfare would rise if people wanted less, wants could not be taken as given.

It was not just Oxford that produced approaches to welfare that were more radical than those of the Cambridge School. Hawtrey came from Cambridge, a member of the Apostles who shared Moore’s belief that good and bad were elementary properties that were perceived directly. In an argument criticised by Robbins, he proclaimed that ‘economics cannot be dissociated from ethics’ (Hawtrey 1926: 184), on the grounds that to say that anything, whether wealth or utility, was the end of economic activity was to commit oneself to an ethical proposition. To presume that pleasure or happiness was the right end to pursue was to participate in the ‘cult of individualism’ (Hawtrey 1926: 182). Instead, welfare judgements, therefore, had to be based on ‘the common ethical judgements of mankind’ – on those judgements that are common to all ethical systems (Hawtrey 1926: 188).
What these economists have in common is a willingness to base welfare conclusions on a wider set of ethical judgements, for which the main justification was the claim that they were widely accepted. This approach was more radical and, especially when presented as an attack on economic science, potentially more dangerous than that of Pigou.

5. Perceptions of Welfare Economics Before the Essay

There is no doubt that what, for want of a better shorthand, can be called the “Oxford” view of welfare economics was not taken seriously in Cambridge. In correspondence, Marshall dismissed Hobson as being in too much of a hurry – one might say a journalist rather than an academic economist. Neither did the younger generation, such as those at Cambridge and LSE who later contributed to the *Review of Economic Studies*, take notice of his work. Tawney’s reputation outside socialist circles was based on his economic history. Their writing might offer important interventions in political discussions but they did not need to be taken seriously as economic theory. As for Clay and Hawtrey, they were buried in an elementary textbook and a collection of essays: both were influential as applied economists whose work on practical problems bore no obvious relation to their writings on welfare. This seems to confirm the conventional, retrospective view, according to which welfare economics was, prior to the revival of the Paretian approach in the 1930s, essentially Pigovian.

However, whilst this may have been the view at Cambridge and amongst the young economic theorists, it ignores the fact that “Oxford” welfare economics was taken seriously elsewhere, in particular in the United States and among institutionalists (then a broad-based, influential grouping within the profession, not a heterodox minority). Wesley Mitchell (1969), in a lecture probably first delivered around 1918, picked out as the representative of welfare economics, not Pigou but J. A. Hobson, exploring his work in a lengthy chapter. Walton Hamilton not only wrote a review article on Clay (1919a) but elsewhere (1919b: 318) referred to the English “welfare school” comprising Webb, Hobson, Tawney, Cannan and Clay. Pigou was not even mentioned. Paul Homan (1927:776, 790; cf 1928), surveying the state of economic theory, focused on Hobson as the author of one of ‘the most influential attempts to modify the content and purpose of economic theory ... the only economist who has developed any comprehensive body of dissident thought’. Pigou, in contrast, was described merely as perpetuating Marshall’s scheme of thought (Homan 1927:789). Even someone more critical (Wolfe 1931) considered that *Wealth and Life* merited a 15-page review article (as did the editor of the *American Economic Review*). Alternatives to Cambridge welfare economics were also taken seriously in China (Liu 1934) and Japan (Nishizawa 2007). As for the others, Clay’s elementary textbook was no match for the work of Marshall or Pigou but was taken very seriously by contemporaries. The British edition (1916) and the American (1918) were widely reviewed, including a ten page review article in the *Journal of Political Economy* (Hamilton 1919a). Though his reputation lay elsewhere, Hawtrey was

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84 See Marshall to R. T. Ely, July 11, 1901, in Whitaker 1996 II: 335
85 See Rutherford 2007 for discussion of the “British connections” of institutionalism.
86 He does not specify whether he refers to Sidney or Beatrice.
clearly anything but a marginal figure, which may explain why *The Economic Problem* was the subject of a review article by Robbins, much of which was later incorporated in his *Essay*.

6. Conclusions

In the two decades before Robbins wrote his essay, a more radical welfare economics, exemplified by Hobson, Tawney and Hawtrey, existed alongside its more conservative Cambridge counterpart. Its radicalism arose from its willingness to base welfare on specific ethical beliefs that enabled them to pass judgement on the merits of various human activities, justifying these beliefs as stemming from shared human values. That the main supporters of this approach were American Institutionalists will not have endeared it to Robbins; to the contrary, he was strongly opposed to Institutionalism, which he sometimes bracketed with Historicism and which he had attacked some years before in the lectures from which the essay was derived (1935: 114)87. Hobson’s organicism and Tawney’s invocation of a public purpose might also have been uncomfortably close to collectivist or “corporatist” ideas, then highly influential in continental Europe, to which he was strongly opposed. Their disparagement of economic science and their willingness to impose ethical judgements derived from values that were allegedly universally shared were highly dangerous.88

These reasons suggest that, quite apart from the textual evidence summarized in Section 2, Robbins had far stronger reasons to attack the welfare economics of Hobson and Hawtrey than that of Pigou. Criticisms of inter-personal utility comparisons might involve criticism of Cambridge welfare economics, but they applied *a fortiori* to Hobson and Hawtrey. In comparison with them, Robbins and Pigou were on the same side, as Hobson well understood when he described Robbins as a supporter of Pigou.

Supporters of Pigou contend that, if we introduce distinctively ethical criteria, we and ourselves in a region not merely outside measurable facts, but outside agreed facts. This is clearly put by Mr. Lionel Robbins. “It is not because we believe that our science is exact that we wish to exclude ethics from our analysis, but because we wish to confine our investigations to a subject about which positive statement of any kind is conceivable.” (Hobson 1929: 128, quoting Robbins 1927: 176).

87 I owe this point to Susan Howson.
88 It is worth noting that Robbins made similar criticisms of Josiah Stamp’s (1929) attempt to bring aesthetics into economics.
They might differ on how one might set about this, but Robbins and Pigou were united in trying to make economics scientific, severely restricting the role of ethical judgements in economics.

It follows, therefore, that the context in which the remarks on welfare that Robbins made in his *Essay* is not what one might deduce from viewing it retrospectively, in the light of the “new welfare economics”. Instead, it is the widespread attempt, in the first three decades of the twentieth century, to find a place for ethics in economics.90 The work of Hawtrey, Hobson and their colleagues formed a part of these ethically-driven inquiries. To Robbins, this literature was not just technically deficient: it was potentially dangerous.91

89 For example, Robbins made the “technical” point that Pigou’s appeal to the measuring rod of money was flawed (1932: 20).

90 The broader story of the role of ethics in economics during this period has yet to be written. It is worth noting, however, that Frank Knight paid great attention to ethics (see Knight 1997; see also Emmett 2008). His ethics is religious in exactly the same way as Hobson’s involvement in the Ethical Societies.

91 Insofar as concern to bring ethical criteria to bear on economics was a response to the mid-Victorian loss of faith in traditional Christian doctrine (see Backhouse 2008), it could be argued that Robbins’s *Essay* should be seen against the background of ongoing engagements between economics and religion.
References


Can Economics be Founded on ‘Indisputable Facts of Experience’?
Lionel Robbins and the Pioneers of Neoclassical Economics

Robert Sugden∗†

Abstract

Robbins argues that the fundamental propositions of microeconomic theory are deductions from the assumption that individuals act on consistent preferences; this ‘indisputable fact of experience’ does not need to be validated in controlled experiments. While recognising that some neoclassical pioneers based the theory on psychological hedonism, Robbins claims that his own approach of ‘pure theory’ belongs to a parallel and sounder tradition exemplified by Menger and Wicksteed. This paper argues that Robbins’s methodological defence of pure theory is incoherent, and that his claim to find an intellectual lineage in the works of Menger and Wicksteed overlooks important discontinuities.

1. Introduction

Lionel Robbins’s *Nature and Significance of Economic Science* is probably best known for its definition of economics as ‘the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses’ (p. 16). It is also famous for its uncompromising defence of the claim that economic science is, or should be, value-free. But, re-reading this essay after having worked for many years on experimental investigations of the principles of rational choice theory, I am more struck by its discussions of the nature of economic generalisations and their relation with reality. Robbins asserts that those principles are so obviously true that economics has no need of experiments to verify them, and has no need to refer to the findings of psychological research into how in reality human beings make decisions. This conception of the role of rational choice theory in economics is implicit in the work of many leading economic theorists of the present day, and echoes of Robbins’s claims can still be heard in some of their pronouncements. Given that *Nature and Significance* has been one of the twentieth century’s most influential statements of economic methodology, it is of more than historic interest to look at how Robbins defends these positions.

Since the early 1980s, experimental economists have tried to answer questions such as: Does the decision-making behaviour of a typical individual reveal a stable pattern of

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92 Otherwise unattributed quotations from *Nature and Significance of Economic Science* are taken from the second edition, that is, Robbins (1935); page references are to that edition.
preferences? Are the preferences revealed in choices independent of the individual’s endowments? Are they invariant with respect to changes in the framing of problems? Do they satisfy transitivity? Do they satisfy Savage’s sure-thing principle? Do they respect stochastic dominance? In none of these cases does the evidence support an unconditional ‘Yes’. Here is what Robbins has to say about this kind of activity:

The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience relating to the way in which the scarcity of goods which is the subject-matter of our science actually shows itself in the world of reality. The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. ... These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious. (pp. 78-79)

According to Robbins, then, there is no need for experiments to test the validity of the assumptions that economic theory makes about preference orderings. The theorist has merely to state these assumptions to recognise their truth.

Economists who have investigated decision-making behaviour experimentally have often found that apparently surprising findings can be explained by using ideas from psychology. Here is what Robbins has to say about the role of psychology in relation to the theory of preferences:

In pure Economics, we examine the implications of the existence of scarce means with alternative uses. As we have seen, the assumption of relative valuations [i.e. preference orderings] is the foundation of all subsequent complications.

It is sometimes thought, even at the present day, that this notion of relative valuation depends on the validity of particular psychological doctrines. The borderlands of Economics are the happy-hunting ground of minds averse to the effort of exact thought, and, in these ambiguous regions, in recent years, endless time has been devoted to attacks on the alleged psychological assumptions of Economic Science. ...

Unfortunately, in the past, incautious utterances on the part of economists themselves have sometimes afforded a pretext for these strictures. It is well known that certain of the founders of the modern subjective theory of value did in fact claim the authority of the doctrines of psychological hedonism as sanctions for their propositions. This was not true of the Austrians. From the beginning the Mengerian tables [i.e. Menger’s presentation of value theory] were constructed in terms which begged no psychological questions. ... But
the names of Gossen and Jevons and Edgeworth, to say nothing of their English followers, are a sufficient reminder of a line of really competent economists who did make pretensions of this sort. ...

[But] no one who was acquainted with recent value theory could honestly continue to argue that it has any essential connection with psychological hedonism, or for that matter with any other brand of Fach-Psychologie. If the psychological critics of Economics had troubled to do these things they would speedily have perceived that the hedonistic trimmings of the works of Jevons and his followers were incidental to the main structure of a theory which – as the parallel development in Vienna showed – is capable of being set out and defended in absolutely non-hedonistic terms. (pp. 83-85)

Robbins seems to be claiming that economics is a science entirely separate from psychology. More precisely, it is entirely separate from Fach-Psychologie – from professional psychology. Robbins advocates a subjective theory of value, and sees this as requiring propositions about mental experiences. The propositions of analytical economics, he insists, ‘most unquestionably involve elements which are of a psychological – or perhaps better said a psychical – nature’. He rejects as a ‘queer cult’ the kind of behaviourism favoured by Vilfredo Pareto, and later to be formalised by Paul Samuelson (pp. 86-88). The implication is that economic theorists need to make assumptions about human psychology, but do not need any help from psychologists in doing so. The psychological propositions that economists use are so obviously true that there is no need to verify them by the methods of empirical science.

Robbins is presenting a picture of a form of economic theory – or of what, significantly, he often calls ‘pure economics’ or ‘pure theory’ – whose fundamental assumptions are deemed not to be in need of scientific explanation or empirical test. This conception of pure economics, about which I will say more later, derives from Pareto and from the earlier work of Léon Walras (1874/1954). The distinction between pure (or theoretical) and applied economics is understood analogously with that between pure and applied mechanics (Pareto, 1906/1971: 103-105). The idea is that pure economics analyses the implications of certain fundamental principles, abstracting from other factors which might need to be considered in specific applications. As the analogy with mechanics suggests, there is a presupposition that those fundamental principles are secure. Thus, pure theory is understood as a predominantly mathematical enterprise: it is not a programme of empirical research whose aim is to discover fundamental principles that are as yet unknown.

Leaving aside for the moment the specificities of Robbins’s argument about why the assumptions of economics have this privileged status, it is a plain fact about the practice of economics that, for at least fifty years following the publication of his essay, much of the most highly-regarded work in economic theory was ‘pure’ in Robbins’s sense. De facto, the basic rationality assumptions of economics were treated as uncontested. Heated methodological debate resulted when, from the 1980s, experimental and behavioural economists began to treat those assumptions as empirical hypotheses calling for explanation and test.
In the context of this so far unsettled debate between rationality-based and behavioural economics, it is useful to look closely at Robbins’s account of pure theory and to trace its intellectual descent. It is particularly significant that Robbins has to reject a tradition of psychologically-based theorising to which three of the most important pioneers of neoclassical theory – Hermann Heinrich Gossen, William Stanley Jevons and Francis Ysidro Edgeworth – all belong. His way of doing this (as can be seen in the second quotation above) is to treat these writers as honorary practitioners of pure theory: their psychological assumptions are unnecessary and dated trimmings which the modern reader should overlook. Yet these authors certainly did not see their psychological assumptions as incidental to their main arguments. As Luigino Bruni and I have argued in another paper, Jevons and Edgeworth were clear-sightedly following a methodological strategy which drew on (what was then) state-of-the-art experimental research in psychophysics. Neoclassical economics, as practised by these pioneers, was based on empirical hypotheses about human psychology. Economics separated itself from empirical psychology only in the early twentieth century, as a deliberate change of direction in which Pareto (1906/1971) was a prime mover. In the light of recent developments in experimental and behavioural economics, it is not at all clear that this was a progressive move (Bruni and Sugden, 2007).

In defence of his radical re-reading of Jevons and Edgeworth, Robbins appeals to what he sees as a parallel ‘Austrian’ tradition of neoclassical economics which is independent of Fach-Psychologie. The founding work in this tradition is Carl Menger’s Principles of Economics (1871/1950), exactly contemporary with Jevons’s Theory of Political Economy (1871/1970). Robbins (pp. 55, 75, 96, 99) presents Philip Wicksteed’s Common Sense of Political Economy (1910/1933) as another key text of this alternative tradition. When Common Sense was republished in 1933, Robbins wrote the introduction, praising the book as ‘the most exhaustive non-mathematical exposition of the technical and philosophical complications of the so-called marginal theory of pure Economics, which has appeared in any language’ (Wicksteed, 1910/1933, p. xii). And Robbins ends the Preface to the first edition of Nature and Significance by ‘once more’ acknowledging his ‘especial indebtedness’ to Wicksteed’s Common Sense (Robbins, 1935, pp. xv-xvi). So, in Robbins’s account, Menger and Wicksteed stand for the theory of value as it ought to be, shorn of the hedonistic trimmings that Jevons and Edgeworth unfortunately added.

In this paper, I offer readings of Menger’s Principles and Wicksteed’s Common Sense, considered in relation to Robbins’s conception of pure theory. Then I assess that conception itself. Of course, it is rather late to be presenting a critique of Robbins’s essay. My interest is in Robbins’s role as a spokesman for pure theory, as that was beginning to be practised in the 1930s. I argue that Robbins’s methodological defence of pure theory is incoherent, and that his claim to find an intellectual lineage for this form of theory in the works of Menger and Wicksteed overlooks important discontinuities.
2. Menger and Jevons

Menger and Jevons are generally and rightly credited with the independent discovery (or rediscovery) of the fundamental ideas of the ‘marginal revolution’ – particularly the principle of diminishing marginal utility and the idea that, in equilibrium, for every pair of goods and for every individual who trades them, the ratio of the marginal utilities of the two goods is equal to the ratio of their prices. However, a glance at Menger’s *Principles* and Jevons’s *Theory* is enough to show that these are very different books.

Jevons’s book *looks* like modern economics. It uses the mathematics of calculus to derive general theorems; the analysis is presented in equations and graphs. The central theorem is stated with economy and precision: ‘The ratio of exchange of any two commodities will be the reciprocal of the ratio of the final degrees of utility of the quantities of the commodity available for consumption after the exchange is completed’ (p. 139). Jevons is self-consciously writing a manifesto for mathematical economics. The first sub-section of the Introduction has the heading ‘Mathematical Character of the Science’ and begins with the sentence: ‘It is clear that economics, if it is to be a science at all, must be a mathematical science’ (p. 78). In contrast, Menger presents his analysis in words and through the numerical examples of his ‘tables’ (described below). It is clear to the modern reader that Menger has a working understanding of the theorem about price and marginal utility, but his exposition is opaque; his translators feel the need to add a whole page of footnote to explain the notation of the tables, and even they are not entirely sure what he means (Menger, 1871/ 1950, pp. 126-127).

A second difference is that Jevons explicitly grounds his analysis of consumption in empirical psychology. The first two substantive chapters of the *Theory* are ‘Theory of Pleasure and Pain’ and ‘Theory of Utility’. The latter chapter includes the programmatic statements ‘Pleasure and pain are undoubtedly the ultimate objects of the calculus of economics’ (p. 101) and (in opposition to a pronouncement by John Stuart Mill) ‘But it is surely obvious that economics does rest on the laws of human enjoyment; and that, if those laws are developed by no other science, they must be developed by economics’ (p. 102). The principle of diminishing marginal utility is presented as a psychophysical relationship between consumption and sensation (pp. 112-114). Jevons’s treatment of the theory of production is similarly grounded in physiological (although not psychological) research. Indeed, Jevons has a strong claim to be the first experimental economist in the modern sense. His experiments, reported in *Nature*, investigate the relationship between fatigue and the effectiveness of human muscular effort (Jevons, 1870).93 This, of course, was at a time when manual labour was a major input to most processes of production. In the *Theory*, Jevons says that the purpose of these experiments was to illustrate ‘the mode in which some of the laws forming the physical basis of economics might be ascertained’ (pp. 215). Clearly, for Jevons, the fundamental principles of economic theory are matters for empirical investigation. He is not a pure theorist manqué.

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93 The historical and methodological significance of these experiments is discussed by Maas (2005).
Menger’s analysis is certainly not grounded on empirical psychology or physiology in the sense that Jevons’s is. There is nothing in the *Principles* to suggest that Menger is particularly interested in those sciences. But that is not to say that Menger has found a way to eliminate hedonistic concepts from economics. The fundamental concept in Menger’s theory of value is *need*. He postulates a hierarchy of human needs, ordered by the ‘importance’ of their being satisfied. At the top is the maintenance of life; then there are less important forms of ‘well-being’ (such as the preservation of health); and finally there are progressively weaker levels of ‘pleasure’, apparently understood in terms of a rough-and-ready hedonism: ‘With the same intensity, [economizing men] will prefer pleasures of longer duration to pleasures of shorter duration, and with the same duration, pleasures of greater intensity to pleasures of less intensity’ (pp. 122-123).

In his tables, he assigns a numerical scale to this hierarchy of satisfactions. He introduces his notation with the table reproduced in its entirety in Figure 1. Explaining the table, he says:

> I shall designate the importance of satisfactions on which life depends with 10, and the smaller importance of the other satisfactions successively with 9, 8, 7, 6, etc. In this way we obtain a scale of the importance of different satisfactions that begins with 10 and ends with 1. (p. 125)

To the consternation of his translators, he never says explicitly what the rows and columns mean. He says only:

Suppose that the scale in column I expresses the importance to some one individual of satisfaction of his need for food, this importance diminishing according to the degree of satisfaction already attained, and that the scale in column V expresses similarly the importance of his need for tobacco. (p. 127)

He then argues that this individual will not consume any tobacco until his need for food has been satisfied to such a degree that a further satisfaction of this need has the importance denoted by 6; at this point, ‘the first acts of satisfying his need for tobacco’ become more important than ‘further acts of satisfaction of his need for food’ (p. 127). Presumably (but this is never made clear) Menger is assuming that the ‘acts’ which give rise to the numbers in the cells of the table have equal exchange value, so that the individual optimises by allocating his budget to the highest-valued acts.
A further clue to what Menger might mean can be found when he discusses ‘error’:

The importance of a satisfaction to us is not the result of an arbitrary decision, but rather is measured by the importance, which is not arbitrary, that the satisfaction has for our lives and for our well-being. The relative degrees of importance of different satisfactions and of successive acts of satisfaction are nevertheless matters of judgment on the part of economizing men, and for this reason, their knowledge of these degrees of importance is, in some instances, subject to error. ...

But what has been said by no means excludes the possibility that stupid men may, as a result of their defective knowledge, sometimes estimate the importance of various satisfactions in a manner contrary to their real importance. (pp. 147-148)

The implication seems to be that the ‘importance of a satisfaction’ – the concept to which he assigns his numerical representation – is a subjective judgement made by the individual concerned; but this judgement is about something objective, namely the real importance of human needs, and so can be correct or incorrect.

I see nothing here to support Robbins’s reading of Menger’s theory of consumption as an exercise in pure theory that has freed itself from issues of empirical psychology. Rather, Menger seems to be struggling to formulate a psychological theory of needs and satisfactions that coheres with his intuitions about economic value. Where Jevons draws on the findings of psychophysical research, Menger relies on armchair hypotheses about

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94 Robbins (1935, p. 56) argues that the crucial ‘slash of Occam’s razor, [which] extrudes for ever from economic analysis the last vestiges of psychological hedonism’, is the recognition that the valuations implied by prices are ordinal. He asserts that this conception ‘is implicit in Menger’s use of the term Bedeutung [‘importance’ in the 1950 translation] in his statement of the Theory of Value’. This seems a rather strained attempt to find a twentieth-century theory of value in Menger’s Principles.
human psychology. To suggest that Menger’s theory of needs is an advance on Jevons’s theory of pleasure and pain is as far-fetched as it would be to claim that Menger’s tables are mathematically more precise than Jevons’s equations.

3. Wicksteed and Pareto

Wicksteed’s *Common Sense* was published in 1910, almost forty years after Menger’s *Principles* and Jevons’s *Theory*. Menger and Jevons see themselves as launching a revolution in economic theory; Wicksteed sees himself as summarising and expounding the fundamental principles of economics that have emerged from the success of that revolution. However, it must be remembered that Wicksteed (1844-1927) belongs to the same generation as Jevons (1835-1882), Menger (1840-1921), Edgeworth (1845-1926) and Pareto (1848-1923) – a generation that was middle-aged before Robbins (1898-1984) was born. Wicksteed came to economics relatively late in life, and learned the principles of the discipline from Jevons’s *Theory*; but he was making significant scholarly contributions from the 1880s.

Wicksteed’s *Common Sense* is written in the light of, and partly in response to, Pareto’s proposal to separate economics from psychology. Writing for the *Economic Journal*, Wicksteed reviewed Pareto’s *Manual of Political Economy* when it was first published (in Italian) in 1906. Wicksteed’s reactions to this revolutionary work help to explain the main themes in his *Common Sense*. I therefore preface my discussion of Wicksteed with a brief account of Pareto’s book.

Pareto presents the *Manual* as a manifesto for a form of economics in which the fundamental building-block is the concept of indifference, understood as a property of individuals’ preferences that is directly observable:

[T]his entire theory ... rests on no more than a fact of experience, that is, on the determination of the quantities of goods which constitute combinations between which the individual is indifferent. The theory of economic science thus acquires the rigour of rational mechanics; it deduces its results from experience, without bringing in any metaphysical entity. (1909/1971, Ch. 3, § 36b).

Pareto’s claim is that the theory of value, exchange and price can be built up from propositions about individuals’ ordinal preferences, as represented by indifference curves. Economics does not need a theory of the psychological mechanisms that underlie preferences; it is sufficient to be able to observe the terms on which individuals are willing to substitute one good for another.

The reference to mechanics in the previous quotation is significant. Pareto describes his subject-matter as *pure* economics. He understands pure economics by analogy with pure mechanics, that is, as an analysis of particular (and particularly important) causal mechanisms in isolation from other complicating factors. This analogy suggests that a conception of pure economics requires a clear specification of which causal mechanisms are being investigated and which are being abstracted from; and it requires criteria by
which we can recognise real-world cases in which the idealising assumptions are approximately true. In mechanics, for example, we might analyse the path of a projectile under the assumption of no air resistance. But because we have some understanding of air resistance, we can judge whether, in a particular application of the theory, that assumption is likely to lead us astray. And we can test whether, as should be the case if the pure theory is correct, divergences between prediction and observation tend to zero as we approach the limiting case represented in the pure theory.

Pareto makes a serious attempt to specify the domain of pure economics in a way that meets these methodological requirements. He defines this domain as a particular category of what he calls ‘logical action’. An action is logical if and only if it is the result of valid instrumental reasoning from objectively true premises. He begins his exposition of ‘economic equilibrium’ by saying:

We will study the many logical, repeated actions which men perform to procure the things which satisfy their tastes. ... [W]e are concerned only with certain relations between objective facts and subjective facts, principally the tastes of men. Moreover, we will simplify the problem still more by assuming that the subjective fact conforms perfectly to the objective fact. This can be done because we will consider only repeated actions to be a basis for claiming that there is a logical connection uniting such actions. (Ch 3, §1)

Pareto’s reasons for restricting his attention to repeated actions are made more clear in the following passage:

A man who buys a certain food for the first time may buy more of it than is necessary to satisfy his tastes, price taken into account. But in a second purchase he will correct his error, in part at least, and thus, little by little, will end up by procuring exactly what he needs. We will examine this action at the time when he has reached this state. Similarly, if at first he makes a mistake in his reasoning about what he desires, he will rectify it in repeating the reasoning and will end up by making it completely logical. (Ch. 3, §1)

The idea is that pure economics deals with choices that are free of error. According to Pareto, we can expect that if an individual repeats the same class of actions sufficiently often, he will learn to avoid error. Thus, observed behaviour will converge to the predictions of pure theory as choices are repeated.

In his review, Wicksteed (1906) takes issue with this proposal to restrict the domain of economic theory. Referring to Pareto’s definition of logical action, Wicksteed says:

[H]e hardly seems to realise how very much this definition must be stretched if it is to include more than a very small part of the actual phenomena of the business [of economics]; nor does he show any consciousness of what an immensely greater area is covered by his diagrams, his ‘curves of
indifference’, his ‘hills of pleasure’, and his ‘paths of ascent’, than is covered by his definition of economics. May it not, indeed, be doubted whether there is ever room for frequent repetitions of choice on a large scale while the objective relations (including in this connection those relations between subject and object which are experienced, as distinct from those which are anticipated) remain constant?

Wicksteed is making two distinct points here. First, and surely correctly, he is pointing out that Pareto’s proposal removes much of what economists have traditionally regarded as their subject matter from the domain of pure economics. Second, he is claiming that this domain restriction is unnecessary: Pareto’s microeconomic theory applies in a much wider domain than that of repeated choice.

Pareto’s analysis is ‘pure’, not only in the sense of isolating the effects of ‘logical action’, but also in the highly abstract way in which these effects are treated. To a reader who has been trained in modern economic theory, this kind of abstraction is so familiar that it is hardly noticeable; but in the first decade of the twentieth century, it was surprising, even shocking. Wicksteed’s review conveys a sense of disorientation. He comments on Pareto’s ‘extraordinarily condensed and abstract exposition of economic science’, which, he conjectures, few readers ‘even on the Continent’ will be able to understand. He notices that Pareto rarely refers to such real-world economic phenomena as consumption, distribution, bargaining and exchange, but instead conducts his analysis in terms of formal concepts that are defined only within his own theory – concepts such as ‘curves of indifference’, ‘hills of pleasure’ and ‘paths of ascent’, all defined on abstract spaces. It is as if Pareto is more at ease in the mathematical world he has created than in the real world in which people buy and sell, work and consume. Wicksteed admits that he himself has not yet fully understood the Manual, and ‘looks forward to a long period of continued and intensified study’ of it. Pareto read these comments as insults; he seems to have been entirely sincere. Wicksteed’s Common Sense is (among other things) the culmination of the continued study that he had looked forward to in 1906.

The economic theory expounded in Common Sense is in most essential respects that of the Manual, with perhaps some fudging of Pareto’s conceptual rigour. However, Wicksteed sets out to make that theory more accessible to ordinary readers by replacing Pareto’s abstractions with discussions of concrete and familiar features of the real world. This, I take it, is what Wicksteed means when he describes his book as ‘common sense’. He also makes a very deliberate attempt to show that the fundamental principles of neoclassical economics – the ‘general laws of the administration of resources’ – are not, as Pareto claims, specific to any narrowly-defined domain. They are not even specific to the traditional domain of economics: they apply ‘from end to end of life’ (1910/1933, p. 159). This allows a further reading of the ‘common sense’ of Wicksteed’s title: any person who acts with common sense in ordinary life will already be applying the fundamental principles of economics, whether consciously or not.

95 For Pareto’s reactions, see Bruni (2002, pp. 116-117).
Curiously, in his introduction to Wicksteed’s book, Robbins treats its title as a gross misnomer: ‘The title conveys less than nothing; indeed, never was a work of this kind more unfortunately named’ (pp. xi-xii). One might have thought that, if economics really were based on simple and indisputable facts of experience, those facts would be matters of common sense. Perhaps Robbins is embarrassed to find concrete facts of experience, such as how a housewife makes puddings and feeds the cat, intruding into the abstract spaces of pure economics.

Wicksteed’s first chapter develops the idea of a ‘relative scale’ – that is, a preference ordering. He develops this idea through an extended example of a housewife or ‘materfamilias’. This, he says, is an economic role that will be familiar to all his readers (p. 18). The woman he describes is something of an ideal type, both in her job specification (she is the manager of all the affairs of her household) and her capabilities (she is sensible, far-sighted and fair-minded); but she is recognisably a real person from everyday life, not a theoretical abstraction of the kind used by Pareto. Wicksteed argues that the implicit guiding principle of the housewife’s actions is to move the household to the highest attainable level of a scale of preference; that scale represents her subjective judgements about the relative importance of different activities and forms of consumption:

But in all cases, whether she is spending the money, helping the potatoes, pouring out the cream, or exercising a general vigilance over the bread and milk, she is engaged in the same problem of the administration of resources and she is guided by the same principle. She is trying to make everything go as far as it will, or, in other words, serve the most important purpose it can. She will consider that she has been successful if, in the end, no want she has left unsatisfied appears, in her deliberate judgment, to have been really more important than some other want to which she attended in place of it. (p. 20)

For the housewife, this kind of ‘economising’ results from attention and thought, even though she may not actually be conscious of having a preference scale. But, Wicksteed claims, even apparently impulsive actions can show a similar underlying logic. For example, he asks us to imagine a man who, without hesitation, would dive five feet into water to help a drowning stranger. But what if the drop was greater? If he had to dive eight feet, we are told, he would feel a conflict of motivational forces, but would still dive; at 12 feet, he would not dive, but would still feel this sense of conflict; and so on (p. 29). In other words, all human choice involves trade-offs and reveals preferences:

We have thus arrived at the conclusion that all the heterogeneous impulses and objects of desire or aversion which appeal to any individual ... may all be regarded as comparable with each other; for we are, as a matter of fact, constantly comparing them, weighing them against each other, and deciding which is heaviest. ... We may conceive of a general ‘scale of preference’ or ‘relative scale of estimates’ on which all objects of desire or pursuit (positive or negative) find their place, and which registers the terms on which they would be accepted as equivalents or preferred one to the other. (pp. 32-33)
As passages like this make clear, Wicksteed has separated the concept of preference from the measurement of pleasure and pain. A person’s preference for one thing relative to another is understood simply as a disposition to choose the first rather than the second. This disposition may reflect a deliberate judgement (as in the case of the housewife) or a pre-reflective psychological response to a decision problem (as in the case of the man deciding whether to dive into the water). In marked contrast to Jevons and Edgeworth, Wicksteed shows no interest in the findings of experimental psychology.

Crucially, however, Wicksteed stops short of claiming that all choices reveal consistent preferences. Immediately after the paragraph I have just quoted, he concedes that ‘no man’s scale, however, is completely consistent’. He then presents some convincing examples of inconsistent preferences. One of these examples is particularly interesting, because it shows a cycle of pairwise preferences for which there is a credible psychological explanation:

A man might be willing to give a shilling for a knife because he thought it cheap, and might refuse to give a shilling for a certain pamphlet because he thought it dear, and yet if he had been offered the direct choice between the pamphlet and the knife as a present he might have chosen the pamphlet. That is to say, he would prefer the knife to a shilling and would prefer a shilling to the pamphlet, and yet he would prefer the pamphlet to the knife. (p. 33)

Wicksteed’s hypothesis here is that people are averse to paying more than the customary price to buy a good, even when (abstracting from this attitude to the price) they prefer the outcome of buying to that of not buying. The man in the example prefers having the pamphlet to having the knife, but because a shilling is a low price for a knife and a high price for a pamphlet, he can cheerfully pay for the knife while not being able to bring himself to pay for the pamphlet.96 Other anomalies discussed by Wicksteed include decisions that are sunk costs (p. 93), failures of self-control (p. 118), and part-whole inconsistencies (p. 122).97

Wicksteed goes a little way towards Pareto’s position when, in passing, he suggests that people are more likely to act on consistent preferences, the wider the range of choice problems they have experienced (p. 34). But Wicksteed’s treatment of inconsistencies has a strongly normative flavour. Rather than investigating the implications of inconsistencies for economics (as a modern behavioural economist would do), and rather than identifying the conditions under which inconsistencies are least likely to occur (as Pareto does), he instructs his readers on how to become more consistent. He talks about inconsistencies yielding ‘to the light of reason’, and about how ‘the man of alert intelligence and sound judgement’ reduces them to a minimum’ (p. 34). His third

96 In the dialect of Wicksteed’s native Yorkshire, there is a special word for this attitude: the man can’t thoil to pay a shilling for the pamphlet.
97 These last three effects are now familiar topics of investigation in behavioural economics. The phenomenon of ‘bad-deal aversion’, as illustrated by the case of the pamphlet and the knife, has been discussed by Thaler (1985) and analysed more formally by Isoni (2007).
chapter, entitled ‘Economical administration and its difficulties’, contains practical advice on how to avoid common mistakes in decision-making. Presumably, it is because economical administration is difficult – because inconsistencies do not yield easily to the light of reason – that there is a point to giving this advice. It seems that the economising capability displayed by Wicksteed’s housewife is not automatic. It is a skill that has to be learned: ‘the whole art of wise expenditure consists in bringing about a coincidence between “price” and marginal significance, which by no means looks after itself’ (p. 93).

Wicksteed seems to be painting himself into a corner. His theory of market exchange is intended as an explanation of what in fact happens in markets, not of what would happen if everyone overcame the difficulties of economical administration. But the central idea of this theory is that markets tend to equilibrium, defined as a state in which each commodity ‘occupies the same place at the margin on the scales of all who possess it, and is higher at the margin on all their scales than on the scales of any one who does not possess it’ (p. 212). In modern language (and ignoring corner solutions): for every pair of commodities, the marginal rate of substitution between those commodities is the same for all individuals who possess both of them in positive quantities. This definition presupposes that marginal rates of substitution are well-defined; and that requires individuals’ preferences to satisfy certain consistency conditions. But Wicksteed has shown us that these conditions reflect standards of reason, intelligence and judgement that cannot be counted on. Take the case of the man with the anomalous preferences between the shilling, the pamphlet and the knife. His marginal rates of substitution between pamphlets, knives and money (for given holdings of the three goods) cannot be defined independently of the contexts in which they are revealed. In the neoclassical theory of markets, as propounded by Wicksteed, it is essential that preferences are defined independently of prices, so that prices can be determined by preferences; but that independence condition fails if preferences vary according to whether goods are perceived as ‘cheap’ or ‘dear’.

Sometimes, Wicksteed seems to suggest that this problem can be evaded by using a sufficiently flexible interpretation of preference, as in the following passage:

At any given moment, under the circumstances that exist, the marginal values of all manner of things are arranged de facto upon a scale which registers how much of this would actually be accepted as equivalent to so much of that by the individual in question, and at the moment; or if this and that group of alternatives should be presented to him, which of them he will choose. It does not follow that this scale is either wise or consistent. ... But bewilderingly complicated and perpetually fluctuating as this scale of preferences may be, it is always there. Any alternatives, however constituted, which could conceivably be offered to the man would find him either decisively preferring one to the other or unable to decide between them; that is to say, every conceivable alternative stands either above or below any other that you may select, or on a level with it. (pp. 122-123).
But this line of argument cannot help. Wicksteed seems to be saying that if a man has to choose between A and B, he either decides to choose A, or decides to choose B, or fails to decide which of the two to choose. But that is a tautology. If that is the justification for the claim that the scale of preferences ‘is always there’, then that claim too must be a tautology, and cannot provide the foundation for empirical hypotheses about markets. The truth is that the neoclassical theory of markets assumes the existence of preferences with certain properties of consistency, and those properties are not satisfied by all of the bewilderingly complicated and perpetually fluctuating preferences we can imagine.

A more charitable reading of Wicksteed is that his theory of markets depends on the assumption that consistent preferences exist, and that this is an idealisation or model, not a tautology. In the light of his detailed and psychologically acute discussion of the difficulties of economical administration, his account of the economising housewife is most naturally interpreted as a model of rational decision-making in everyday life. By appealing to his readers’ experience of capable wives and mothers, he is trying to establish the credibility of this model as a stylised representation of behaviour across a wide domain of economic decision-making (much wider, in particular, than that claimed by Pareto for pure economics).98 In endorsing the neoclassical theory of markets, he is proposing the hypothesis that, on the whole, real-world markets work approximately as if the agents who trade in them had consistent preferences. He recognises that there are anomalous cases in which the assumption of consistency does not hold, but (I take it) he thinks that a theory based on that assumption works well enough for most of the purposes of economics.

From a methodological point of view, that is a coherent and reasonable position. Notice, however, that it does not ground economics on indisputable facts of experience. On Wicksteed’s account, our experience tells us that the fundamental assumptions of microeconomics are often approximately true, while also revealing that sometimes they are not. That these assumptions are generally accurate enough for the purposes of economics is an empirical claim, for which Wicksteed does not provide any systematic supporting evidence. Such casual empiricism cannot entitle economics to treat as irrelevant the potential findings of controlled experiments. Of course, Wicksteed never lays claim to such an entitlement. But Robbins does.

4. Robbins

Like Pareto and Wicksteed, Robbins treats the subjective theory of value as the bedrock of economics. He asserts that the most fundamental propositions of economic analysis are the propositions of ‘the general theory of value’, and that in that theory, the theory of exchange has a ‘pivotal’ position. Writing six decades after Menger and Jevons and two decades after Wicksteed, he feels able to declare that ‘it is clear that enough has been done to warrant our taking the central propositions [of the theory of exchange] as established. We may proceed, therefore, to inquire on what their validity depends’ (1935, p. 73). Here is his answer to that question:

98 For more on the idea of theoretical models as ‘credible worlds’, see Sugden (2000).
[T]he foundation of the theory of value is the assumption that the different things that the individual wants to do have a different importance to him, and can be arranged therefore in a certain order. This notion can be expressed in various ways and with varying degrees of precision, from the simple want systems of Menger and the early Austrians to the more refined scales of relative valuation of Wicksteed and Schönfeld and the indifference systems of Pareto and Messrs Hicks and Allen. But in the last analysis it reduces to this, that we can judge whether different possible experiences are of equivalent or greater or less importance to us. From this elementary fact of experience we can derive the idea of the substitutability of different goods, of the demand for one good in terms of another, of an equilibrium distribution of goods between different uses, of equilibrium of exchange and the formation of prices. (p. 75)

Robbins’s indebtedness to Pareto and Wicksteed is evident. Like them, he grounds microeconomics on assumptions about preference orderings, and he defines preference independently of measurements of pleasure and pain. But notice what he has edited out of his predecessors’ theories. For Pareto, preference orderings can be assumed only in the context of repeated choice, in which there is reason to expect ‘errors’ to have been eliminated. For Wicksteed, the consistency conditions that are required for the existence of preference orderings reflect a normative ideal of rationality; Wicksteed postulates that, in most areas of economic life, these conditions hold approximately, but he recognises and even analyses cases in which they do not. Robbins sees no need for such qualifications. For him, preference orderings are an ‘elementary fact of experience’. Unlike Wicksteed, Robbins does not actually describe, even in informal terms, any of the experience that has led him to this conclusion. He contents himself with such ex cathedra pronouncements as: ‘No one will really question the universal applicability of such [an] assumption as the existence of scales of relative valuation’ (p. 81).

At one point, Robbins comes close to claiming that the existence of a preference ordering can be deduced from his proposed definition of economics. Referring to the ‘main underlying assumption’ of economic analysis, that is, ‘the schemes of valuation of the different economic subjects’, he says:

But this, we have seen already, is really an assumption of one of the conditions which must be present if there is to be economic activity at all. It is an essential constituent of our conception of conduct with an economic aspect. (p. 76)

I take this to mean that, since the subject-matter of economics is human behaviour understood as a relationship between ends and scarce means which have alternative uses, conduct can have ‘an economic aspect’ only if it involves the purposive pursuit of ends; and a person’s preference ordering is nothing more than a statement of his ends. In a similar exercise in deducing apparently substantive conclusions from definitions, Robbins claims that the law of diminishing returns can be deduced from the assumption that ‘there is more than one class of scarce factors of production’ (p. 77). These passages suggest
the influence of the deductive ‘praxeology’ of Ludwig von Mises,\(^99\) to whom (together with Wicksteed) Robbins records his ‘especial indebtedness’ (pp. xv-xvi).

It seems clear that Robbins feels some attraction to an aprioristic approach in which the axioms of economics are necessary truths. *Nature and Significance* would perhaps have been more coherent if it had taken such an approach wholeheartedly, but I conjecture that it would have struck less of a chord with the economics profession. Robbins hints at some awareness of this when, in the preface to the second edition, he reports that he has been accused of ‘barren scholasticism’ (p. x). ‘Barren scholasticism’ seems an entirely apt description of an argument which derives substantive properties of preferences from the definition of ‘economics’. That argument amounts to saying that the existence of preference orderings is not a fact of experience at all, but merely a defining condition of an ‘economic’ problem. There is no independently-specified domain of economic problems in which we experience the existence of preference orderings. Rather, if we come across a situation in which no preference orderings exist, we can infer that it is not an economic problem. This approach would make economics an investigation of the logic of rational choice – or, more accurately, of a formal system that is asserted to be the logic of rational choice. If economics were understood in this way, the facts of experience would have no role. Experimental investigation of the validity of the postulates of economics would not be unnecessary: it would be conceptually misconceived. However, as the quotations with which I began this paper make clear, Robbins is committed to the proposition that the existence of preference orderings is a fact of experience. My concern in this paper is with his defence of that proposition.

Robbins’s assertion that the existence of preference orderings is an undisputable fact of experience seems to rely on conceptual slippage in the interpretation of ‘preference’. If we use a sufficiently flexible interpretation, it becomes plausible to say (as Wicksteed does in one of the passages quoted in Section 2) that the scale of preferences is ‘always there’. Consider any given choice problem (at a given point in time, in a given context) in which there is a well-defined pair of mutually exclusive and exhaustive options A and B, one of which must be chosen. Thinking about facing such a problem, one can imagine the internal feelings associated with the act of choice. One can describe the feeling that A is more choosable than B as a ‘preference’, and the feeling that the two options are equally choosable as ‘indifference’. That every act of binary choice is associated with some such feeling of preference or indifference is not a conceptual necessity, but the proposition that this is true of most such acts might arguably qualify as a fact of experience in Robbins’s sense. But Robbins requires much more than this as a foundation for microeconomics: he requires that all the feelings of preference and indifference that a person would have, conditional on every different conceivable choice problem (defined in terms of both its opportunity set and its contextual framing), can be integrated into a single preference ordering. And that does not sound at all like an undisputed fact of experience.

Robbins recognises this objection to his position when he discusses a criticism of the first edition of *Nature and Significance*, made by Joan Robinson.\(^{100}\) He reports that Robinson

\(^99\) For the fullest development of this approach to economics, see von Mises (1949).
has argued that the assumption of the existence of preference orderings ‘depend[s] upon a more general psychological assumption – upon the assumption of completely rational conduct’ (p. 90). Robinson’s objection seems to me to be entirely right, and the incoherence and bluster of Robbins’s response suggest that he has been unable to find an escape from an intellectual checkmate.

After creating a tactical diversion by rehearsing the argument that economics is value-free in the ethical sense, he makes a grudging concession:

But in so far as the term rational is taken to mean merely ‘consistent’, then it is true that an assumption of this sort does enter into certain analytical constructions. The celebrated generalisation that in a state of equilibrium the relative significance of divisible commodities is equal to their price does involve the assumption that each final choice is consistent with every other, in the sense that if I prefer A to B and B to C, I also prefer A to C. (p. 91)

Since this ‘celebrated generalisation’ is the core theorem of the neoclassical theory of exchange, one might suppose that Robbins is either conceding defeat, or is preparing to argue that the transitivity of preferences is an indisputable fact of experience. Instead, there is another tactical diversion – a discussion of the idea that, because time and attention are themselves scarce goods, a fully rational individual might not always calculate marginal utilities exactly. This idea, he says, can be modelled by allowing some form of stochastic variation in individuals’ valuations. But then he has to admit that this is not answering Robinson’s objection.

He tries another line of argument:

It is perfectly true that the assumption of perfect rationality figures in constructions of this sort. But it is not true that the generalisations of economics are limited to the explanation of situations in which action is perfectly consistent. Means may be scarce relative to ends, even though the ends be inconsistent. Exchange, production, fluctuation – all take place in a world in which people do not know the full implications of what they are doing. (p. 92)

But this move is hopeless. Robbins seems to be saying that we all know that people’s preferences are in fact inconsistent, but economics still manages to explain many regularities in real economic life. In other words, economics is successful even though its assumptions about preferences are known to be false. How can this be compatible with the claim that that those assumptions are indisputable facts of experience?

100 Robbins cites Robinson’s ‘interesting pamphlet entitled Economics is a Serious Subject’, but gives no further bibliographical details.

101 Of course, this argument is one of the principal themes of Nature and Significance, developed in detail in other parts of Robbins’s essay. But it seems out of place as a response to Robinson’s critique.
Finally, Robbins seems to acknowledge that the consistency of preferences is only a modelling assumption:

The fact is, of course, that the assumption of perfect rationality in the sense of complete consistency is simply one of a number of assumptions of a psychological nature which are introduced into economic analysis at various stages of approximation to reality. ... The purpose of these assumptions is not to foster the belief that the world of reality corresponds to the constructions in which they figure, but rather to enable us to study, in isolation, tendencies which, in the world of reality, operate only in conjunction with many others, and then, by contrast as much as by comparison, to turn back to apply the knowledge thus gained to the explanations of more complicated situations. In this respect, at least, the procedure of pure economics has its counterpart in the procedure of all physical sciences which have gone beyond the stage of collection and classification. (p. 94)

Now we are being told that consistency of preferences is not, after all, the foundation of the theory of value. It is not the crucial assumption from which we can deduce the core theorems of microeconomics. It is just one component of a toolbox of alternative psychological assumptions, any one of which economics might use, depending on what is to be explained and the degree of accuracy that is required.

Robbins is surely right to say that the methodology he is now describing is characteristic of the physical sciences. It is broadly consistent with the methodological position that I have attributed to Wicksteed. The arguments I have presented in this paper provide no objections to it. But Robbins cannot endorse it consistently while also claiming that the psychological assumptions from which economic theories are derived are indisputable facts of experience for which experimental evidence is not needed.

It is one thing to present the rational economic agent as an ideal type which might capture certain tendencies in human behaviour, and to propose that economics uses it as a working model in those types of application for which it proves to have predictive and explanatory power. It is quite another to claim that economists can be so confident in this particular model that they do not need to subject it to controlled tests. If economics has to choose between alternative psychological assumptions with the aim of isolating tendencies in real human behaviour, why should it not use the empirical methods of the relevant science, psychology?

5. Conclusion

Robbins’s attempt to deduce the central principles of economic theory from a few self-evident axioms and definitions may seem eccentric to the modern reader, but many of the arguments he deploys will be familiar to economists who have presented experimental or behavioural research to sceptical audiences. This should not be surprising. Robbins

102 Since most economic theorists do not write methodological essays, these arguments belong to an essentially oral tradition. However, some of them appear in Rubinstein’s (2001) paper, ‘A theorist’s view
was trying to find a convincing defence of a methodological strategy that was being developed by some of the most highly-regarded microeconomic theorists of the 1930s. This strategy has been widely used ever since, but is now being challenged by behavioural economics. The strategy is that of ‘pure’ economics, understood as a mode of analysis that is formal, abstract and general. The theories of pure economics are constructed by deduction from a core set of fundamental postulates, supplemented where necessary by case-specific auxiliary assumptions. Among those fundamental postulates, the idea that individuals choose in accordance with consistent preferences has an especially privileged status; it is not regarded as an empirical hypothesis that might be disconfirmed by experimental evidence.

Bruni and I have argued that this twentieth-century form of neoclassical economics is not in continuity with the main line of development of economics in the last third of the nineteenth century. In that earlier period, leading economists such as Jevons and Edgeworth were developing psychologically- and experimentally-based theories, more in the spirit of modern behavioural economics. Those lines of enquiry remained viable, but were abandoned after the methodological revolution launched by Pareto (Bruni and Sugden, 2007). Robbins’s essay gives a different reading of the same history, in which Jevons and Edgeworth are clinging to redundant hedonistic assumptions; a parallel Austrian tradition is wielding Ockham’s razor, cutting away those ‘trimmings’ to reveal the pure theory underneath. In this paper, I have argued that Robbins’s account is misleading. Menger cannot credibly be presented as having a deeper understanding than Jevons of the potentialities of the marginal revolution. The kind of pure theory favoured by Robbins builds on the work of Pareto and Wicksteed, but it ignores crucial qualifications recognised by those earlier authors. As a result, Robbins finds himself defending an untenable methodological position.

If economics is to be a successful empirical science, I submit, it cannot insulate itself from the need to make psychological assumptions; and whatever assumptions it makes must be subject to validation by the normal methods of empirical science.\(^{103}\) The idea that substantive conclusions about economic reality can be deduced from indisputable facts of experience is a mirage.

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\(^{103}\) I am not claiming that assumptions must be \textit{realistic}. I take ‘the normal methods of empirical science’ to encompass the form of instrumentalism advocated by Friedman (1953), in which assumptions are acceptable to the extent that they lead to predictions that are confirmed.
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Robbins, Positivism And The Demarcation Of Economics From Psychology

Don Ross

Abstract

This paper argues that the most common reading of Robbins’s Essay on the Nature and Significance of Economic Science in the methodology literature, according to which it was an historical foil for subsequent positivist-empiricist ideas, underestimates its contemporary relevance. In light of recent scholarship on 1930s positivism in philosophy, Robbins’s Essay is better interpreted as representing an attitude I call ‘broad positivism’, which remains a live option in contemporary philosophy of science. In consequence, the basis of Robbins’s preference for clear demarcation between economics and psychology should be regarded as not merely historical in interest, but as raising valid considerations against the widespread current trend towards ‘correcting’ aspects of economic theory by reference to psychological experiments.

JEL Classification: B31, B40, B53

Keywords: Robbins, methodology, positivism, demarcation of economics and psychology

1. Introduction

This paper argues that Lionel Robbins’s Essay on the Nature and Significance of Economic Science remains the classical statement of the mainstream economic attitude. In particular, the attitude to which the Essay gives classic expression still merits status as the basis for demarcating economics from neighbouring disciplines, particularly psychology. This view swims against currently prevailing tides among both economic methodologists and everyday economists, for different reasons.

The standing of Robbins’s Essay among everyday economists (by which I mean mainstream economists who are not methodologists) may be outlined as follows. Most are familiar with the Essay’s famous definition of the subject matter of economics, and many teach it to students (especially because it continues to appear in the opening pages of some textbooks). The minority who occasionally engage with the themes that dominate the methodology literature are not generally sympathetic to Robbins’s approach. Robbins, following the Austrians, thinks that the foundations of economics are

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104 Throughout this paper, all citations will be to the better-known 2nd, 1935, edition of the Essay, which introduced non-trivial revisions to the 1st edition.

105 Economics is “the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” (Robbins 1935, p. 16).
based on something other than scientifically controlled observations – either they are a priori or derived from casual introspection plus a minimal degree of armchair reflection. Contemporary economists, by contrast, are almost universally committed to the empirical character of their science as a fundamental principle. There is disagreement among them about what kinds of empirical data are relevant. At the moment, a major conflict rages between those who think that the relevant empirical phenomena are behavioural choices, and (confusingly) so-called behavioural economists who think that the ultimate underlying data are turning out to be processes of valuation and other computations in the brain (Camerer and Loewenstein 2004; Camerer, Loewenstein and Prelec 2005). The latter group must reject Robbins’s strong separation of economics from psychology. The former group may agree with Robbins on that point, but still deny without caveat, following Samuelson (1972, p. 761) himself, Robbins’s claim that the basic principles of economics are logically prior to scientific observation.

Professional methodologists tend to be even less sympathetic to the Essay. They are more likely than everyday economists to emphasize the connection between Robbins’s methodological views and his definition of the subject matter of economics. Thus they are apt to doubt that one can easily retain the latter while rejecting the former. In the most influential mainstream methodological polemics written in the past few decades, those of the Popperian falsificationist Blaug (1980) and the self-described “pluralist” (with Austrian sympathies) Caldwell (1982), Robbins is of historical interest only. They offer similar narratives according to which, in holding that basic economic postulates are known intuitively and are not open to empirical disconfirmation, Robbins provides the dialectical foil for the ‘logical positivism’ of Terence Hutchison (1938), who wrote his own classic monograph on methodology just a few years after Robbins.

That Robbins was Hutchison’s historical foil is clear. The logic of the relationship, as I will explain, is more complicated. Much of the philosophy of science known to a typically trained economist comes directly or indirectly from Blaug’s and Caldwell’s accounts. Notwithstanding the divergence between Caldwell’s sympathy for Austrian ideas and Blaug’s unconcealed disdain for them, they present complementary surveys of the philosophy of science narrated as the story of a positivist orthodoxy rightly overthrown shortly after mid-century by Popper, Kuhn and Lakatos. They emphasize that Hutchison absorbed the views of the logical positivists from the Vienna Circle, and note that Hutchison learned his logical positivism while lecturing in Germany. However, what is presented as the doctrine of logical positivism is not the positivism of either the Vienna Circle or of their close associates in Berlin led by Reichenbach, but, roughly, the version introduced to English-speaking intellectuals by A.J. Ayer in his Language, Truth and Logic (1936). Traces of the Blaug-Caldwell narrative are found in subsequent methodologists. Thus Barotta (1996), writing about the relationship between Kant’s philosophy and von Mises’s methodology, contrasts a priorism with what he calls “neopositivism”. This appears to again be the form of mid-century ‘received view’ empiricism characterized by Caldwell.

As Michael Friedman (1999), supported by Coffa (1991) and Richardson (1998), has shown, though most philosophers know that Ayer distorted the views of the logical
positivists in popularizing them, the distortion is seldom fully factored out in non-specialist accounts. In particular, postwar commentators generally project back into logical positivism aspects of British empiricism which were at best a minority view among the members of the Vienna Circle. The great distal influence on the Vienna Circle, Friedman demonstrates, was Kant, not the classic British empiricists. The Austrian economists, likewise, developed their views in an intellectual environment dominated by Kantians and neo-Kantians (Parsons 1990). Given the influence of the Austrians on Robbins, presenting him as a foil for logical positivism is thus problematic. Kant does not occur in Caldwell’s index, and is mentioned by Blaug only incidentally and tangentially, as indeed are the logical positivists themselves.

The importance of philosophy to the history of economic thought should not be exaggerated. In general, philosophers’ doctrines tend to be invoked by economists when they serve to buttress principles already adopted for economics-driven reasons. In the work that has had the largest influence on economists’ conception of their discipline, methodology is mainly implicit and entangled with first-order economic theorizing. Robbins’s description of the economist’s role reflects this, and thus mirrors no specific doctrinal philosophy of science. He mentions no philosophers in the Essay.

Though I will argue for a Kantian-cum-positivist reading of Robbins’s Essay, my motivation is not to try to showcase an instance in which philosophy was important for economics. Such Kantianism as might have filtered through Robbins to influence economists’ practice was minimal at best. Rather, my main motivating interest lies in the increasingly widespread conviction among economists that they should attend more closely to psychology. In this context, it is worth investigating the grounds for Robbins’s insistence on a clear demarcation between psychology and economics. My contention is that misunderstanding of the philosophical background to the Essay has obscured the basis for Robbins’s view on this question. This basis lies in what I will call ‘broad positivism’, a stance taken seriously in contemporary philosophy of science.

The paper is organized as follows. Section 2 describes the foil for the view I will urge, the standard interpretation of logical positivism and its relationship to economics in the most widely read methodological histories. Section 3 offers the corrected interpretation of positivism, giving particular emphasis to its so-called structuralism. ‘Broad positivism’ is my name for the view of science held in common by logical positivists and contemporary structuralists. Section 4 argues that Robbins’s Essay can consistently be given a broadly positivist reading, and that such a reading allows us to make sense of the Essay’s apparent equivocation on the relevance of psychology to economics. Section 5 shows that if the Essay is interpreted in this way, then we can rationalize the combination of theses that has caused the most trouble for attempts to read Robbins charitably over the years. These are his claims that, on the one hand, people know on the basis of immediate experience that they rank their preferences, while, on the other hand, they cannot gauge or report on the relative intensities of these preferences and so cannot interpersonally compare them. Section 6 concludes with some reflections on the relevance of Robbins, read as a broad positivist, to the contemporary relationship between economics and psychology.
2. Logical Positivism and the Standard Account of Twentieth-Century Methodology

Economic methodologists are hardly alone in having an historically confused understanding of logical positivism. The version of that doctrine found in Blaug’s and Caldwell’s books also occurs in some work by leading philosophers of science (e.g., Giere 1988). I am thus not suggesting that methodologists should be ashamed of their scholarship. However, the fact that most economists who have any view at all about the logical positivists entertain a caricature of them is worth correcting.

Caldwell (1982, Chapter 2) says relatively little about the beliefs of the 1930s logical positivists, despite devoting a chapter to them. Most of it is given over to introducing the so-called verifiability criterion of meaningfulness. According to that idea, cognitively meaningful bodies of discourse are distinguished from analytic and cognitively meaningless ones by having recognized testable empirical implications that can in principle (not necessarily in fact) be checked through observation or experiment. The second positivist theme Caldwell emphasizes was, as Friedman (1999) demonstrates, more important to the positivists and more generally agreed upon among them. This is idea that science is unified – that is, that if an accepted statement of any one science has logical implications that contradict those implied by an accepted statement in another science, a mistaken inference must have been made somewhere, or a putative observation misreported or misinterpreted. Furthermore, completeness was held to be a regulative ideal of the sciences together.

The rest of Caldwell’s brief discussion of logical positivism mainly sets the stage for his subsequent chapter on logical empiricism. This has two themes over and above elaboration on verificationism and unity. The first is that theoretical terms in scientific theories – that is, terms making reference to in-principle unobservable entities – are legitimate objects of predication and generalization just in case statements that employ them have as deductive consequences sets of statements that make reference only to observable states of affairs. In the standard philosophical usage, theoretical terms must be reducible. According to Caldwell, what statements containing theoretical terms were held by the positivists to be reducible to were statements in languages containing only ‘physical’ predicates, meaning (roughly) terms that assign individual objects and events to classes on the basis of operationally measurable properties. Caldwell says that this ambition, which he attributes to Carnap, “won out” over an alternative view defended by another leading logical positivist (Neurath) that the reductive basis should instead be statements reporting ‘raw’ sense data (e.g., ‘red here now’). Caldwell’s second theme, the logical empiricist understanding of the relationship between laws and explanations, did not feature at all in early logical positivism and will not concern us here.

Caldwell’s account is a partial representation of the common image of the logical positivists, which Friedman (1999, pp. 2-3) characterizes as follows:

The positivists – so this story goes – were concerned above all else to provide a philosophical justification of scientific knowledge from some privileged,
Archimedean vantage point situated somehow outside of, above or beyond the actual (historical) sciences themselves. More specifically, they followed the lead of the logicist reduction of mathematics to logic, where the latter is also understood as fundamentally foundationalist in motivation and import. Just as the logicists attempted to justify mathematical knowledge and place it on a more secure foundation by means of a derivation from (supposedly more certain) logical knowledge, so the positivists attempted to justify empirical science and place it on a secure foundation by logically constructing the concepts of empirical science on the basis of (supposedly more certain) data of sense. Thus formal logic furnished the foundational enterprise with the required Archimedean standpoint located outside of the actual (historical) sciences themselves, and phenomenalist reductionism, carried out rigorously using the methods of formal logic (as epitomized in Carnap’s [1928] Der logische Aufbau der Welt), then provided the desired epistemological justification of the sciences.

This standard interpretation reads the logical positivists as radical empiricists from the beginning. According to it, they implemented the project that had been promoted by Bertrand Russell in his Our Knowledge of the External World as a Field for Scientific Method in Philosophy (1914), which he in turn inherited from Berkeley and Hume. What are knowable according to this programme are primitive atomic facts of one sort or another. Justification of the abstract reaches of scientific theory must then rest on strict derivation from these atoms, which modern mathematical logic was thought to make possible for the first time. However, the story goes on, Carnap’s attempt to construct atomic physical states of affairs out of phenomenal atoms in the Aufbau failed – and this was indeed that project’s most notable, though negative, lesson. Thus the ambition of empiricism was tempered, with the later Carnap and his colleagues attempting only to ground science in atomic physical observations rather than the supposedly more certain reports of sense data. The history of the evolution of logical empiricism from logical positivism is thus told as a movement from phenomenalism to operationalism. This standard account of the main currents in mid-twentieth-century epistemology can be presented as isomorphic to the development of economic methodology, also as standardly read, with some temporal lag. On this account, Robbins is interpreted as a kind of phenomenalist, whose views carry the seeds of Samuelson’s later operationalism.

Blaug and Caldwell read Robbins as providing the final statement of a traditional neoclassical view according to which the basic postulates of economic theory are truisms furnished by direct introspection. In Robbins’s specific case, what is taken to be known in this way by each person is that they subjectively rank possible and actual states of the world with respect to their ordinal preferability. The individual infers from what others report that people do this generally. An anticipatory element of the standard reading of positivism can then be identified in Robbins’s rejection of the possibility of interpersonal comparisons of utility, since this seems to rest on the claim that one person can neither perceive nor construct from perceptible data another person’s relative intensities of preference. The resulting overall picture is unstable: why should we suppose that a person
can know that both she and others introspectively order preferences, but can in principle infer nothing about relative interpersonal magnitudes of these preferences?

A progressive telling of the history of economic thought which can interpret this tension was being dissolved in Samuelson’s *Foundations* (1947). Appeal to an introspective basis of economic knowledge is abandoned with the turn to revealed preference. The development of methodology, it seems, thus mirrors the evolution of positivist / empiricist philosophy of science in beginning from phenomenalism and ending with operationalism – that is, with conceptual reduction of the elementary theoretical aggregate of consumer theory, demand, to physically measurable sequences of choices by subjects.

In my own rationalization of twentieth-century methodology in Ross (2005), I endorsed a version of the above story. I say a ‘version’ because, on the basis of the considerations raised by Friedman (1999) and reviewed below, I pointed out that Robbins’s position was not antithetical to early logical positivism, as the standard account presupposes, and was indeed in accord with key aspects of it. Linking Robbins to a version of logical positivism in turn helps to support a view of economic theory as involving no epistemological discontinuity, but as instead following a continuous line of development from Edgeworth, Pareto and Hicks though Robbins to Samuelson and beyond, which at every step involved reducing dependence on psychological hypotheses. I continue to endorse this view. However, I now think that I accepted too uncritically the assumption made by Blaug and Caldwell that Robbins must be read as asserting that people know they order their preferences by means of introspection. Since it is untenable to imagine that contemporary economics – or any other science – has its foundations in introspection, accepting this assumption entailed my concluding that Samuelson’s operationalism improved on Robbins’s epistemology of economics. I amplified this mistake by not taking Friedman’s message fully enough to heart either, which led me to represent Samuelson as also a ‘better’ positivist than Robbins. As we will see, a more charitable epistemological rationalization of Robbins is possible, one on which Robbins’s and Samuelson’s foundational remarks are entirely complementary, and on which Robbins has as at least as much claim as Samuelson to unqualified membership in the positivist club.

3. Broad Positivism

Friedman describes the standard account of early logical positivism, summarized in the earlier quotation from him, as “an almost total perversion of [their] actual attitude” (1999, p. 3). Their starting point, he reminds us, was “rejection of all … philosophical pretensions”. By ‘pretensions’ Friedman refers to the idea that philosophy can stand outside of or ‘above’ science – science in general, or any particular science – and ‘justify’ it. This modesty allows space for two possible attitudes to the traditional aim of metaphysics, that of identifying what is ‘ultimately real’. One possibility is to hold that science tells us directly what is ultimately real; this is the position known as ‘scientific realism’. The other possibility is to deny that questions about ‘ultimate reality’ ask anything of possible significance once we grant that the epistemological role of science answers to no external (or Archimedean) court of appeal; science is our body of
institutions and practices for collective objective inquiry and metaphysics is not part of that body. The traditional aim of metaphysics is therefore not to be taken over by science, but should be rejected as a deluded enterprise. ‘Broad positivism’ is the name I use here for the second of these attitudes.

The ‘logical’ adjective celebrated by the early positivists reflected their commitment to using formal logic to construct concepts out of others. More specifically, they endorsed Russell’s “supreme maxim in scientific philosophizing”: “Whenever possible, logical constructions are to be preferred to inferred entities” (Friedman 1999, p. 117). This attitude promotes conceptual economy and elegance, but more importantly for the logical positivists it promotes unity: logical constructions make explicit the structural relationships among objects of discourse.

Friedman understands logical positivism as a whole mainly by reference to the development of Carnap’s thought, though he acknowledges that Carnap’s opinions never constituted a ‘party line’ for the Vienna Circle. When Carnap moved from physics to philosophy at the time he began his doctorate, he inherited his problem space from Kant. That is, he took the aim of epistemology to be to show how scientific knowledge can be objective, despite the fact that, as Hume had argued, individual judgments about empirical, contingent matters of fact are subjective. Kant had tried to ground objectivity in what he called the synthetic a priori. This is a domain of intuitively grasped propositions in terms of which all experiential judgments are categorically framed. Grasping the content of these judgments is thus a precondition for any objective knowledge according to Kant. His most famous example of a synthetic a priori truth is that physical space has the structure of Euclidean geometry. In order to grasp Newton’s laws, Kant argues, one must first conceive Euclidean space and recognize that this space describes the physical world. The centrality of this example in Kant’s thought caused a crisis in European philosophy when alternative non-Euclidean geometries were developed and then applied by Einstein to overturn classical mechanics. First special relativity entailed rejection of Kant’s claim that specifically Euclidean geometry is an intuitive prerequisite to physical understanding. Then general relativity more radically called into question the idea that formal geometry can be considered separately from physical (or ‘applied’) geometry at all.

Friedman emphasizes two other aspects of Kant’s philosophy as crucial background to positivism. First, Kant regarded his epistemology as anti-metaphysical. That is, he did not think that philosophical speculation preceded or constrained the boundaries of empirical science. His perspective was ‘transcendental’ only in the sense of being concerned with grounds for possibility. He assumed, rather than aimed to show that, classical physics had discovered objective truths. Then he wondered what made this achievement possible. Thus for Kant there is an important sense in which science is epistemologically prior to philosophy.

Second, the idea of objectivity depended on a strong distinction between psychology and logic. Psychology, according to Kant, studies the mechanisms of subjective, partial and idiosyncratic perception and response. Logic, by contrast, concerns itself with reasoning
that is disciplined by norms. It encapsulates the kinds of responses that careful observers would offer when reasoning critically together. A main source of the over-blown skepticism to which empiricism led Hume, in Kant’s view, was the former’s assimilation of logic to psychology. Now, Kant knew nothing of what we call ‘logic’, that is, the science of the foundations of algorithmic computation, which the logical positivists initially followed Russell and Frege in mistakenly believing to also be the science of the foundations of mathematics. It was precisely the new conception of logic that inspired the positivists to believe there was a straightforward way of rescuing the core ambition of Kantian epistemology from the demise of the synthetic a priori indicated by the revolution in physics.

Carnap responds to the crisis in post-Kantian thought by accepting (along with other important contemporaries, especially Poincaré) a conventional element in the choice of mathematical frameworks used for what we might anachronistically call ‘modeling’. With respect to concern for objectivity, conventions have the useful property of being intersubjective – that is, being stabilized as conventions within a scientific community. But since it is objectivity of empirical knowledge in which we are interested, intersubjectivity of conventions is relevant only to the extent that conventions help to fix – through what Reichenbach called ‘axioms of coordination’ – the physical domain of reference of formal elements. Here is where Carnap finds the crucial philosophical value of formal logic. If logic can be shown to constrain, through constructions in which it is used, the types of objects that feature in different conventions and the types of objects that feature in empirical reports and generalizations (what Reichenbach called ‘axioms of connection’), then (i) conventions can be objectively distinguished, and (ii) the conventional part of science can be distinguished from the empirical part. Logic and the recognition of the role of convention in science replace, according to Carnap, the synthetic a priori in explaining the possibility of scientific objectivity.

This approach is unlike the project of foundationalist empiricism, in at least three general respects. First, it does not attempt to found all of science on the basis of empirical observations. The positivists, for example, rejected the idea of responding to Einstein’s achievement by attempting to build an empiricist account of geometry (Friedman 1999, p. 60). Second, logical positivism is holistic rather than atomistic. The objects of philosophical study are whole mathematical frameworks and their relationships to bodies of theory and observation reports, not elementary constituents of reality. Third, logical positivism is not about justifying the truth of scientific theories by reference to a supposedly indubitable basis. This is the sense in which logical positivism is anti-metaphysical. The philosopher’s task according to Carnap is not to show that the types featuring in scientific theories are ‘real’ because they can be built out of atoms the philosopher has independently argued are real. Instead, the philosopher’s task becomes part of the task of science: elucidating, through rigorous logical constructions, the structural relationships amongst conventional mathematical frameworks and bodies of statements reporting and generalizing empirical findings. It is because there must be such relationships in order for science to be empirical in the first place that Carnap was committed, like all of his Vienna Circle colleagues, to the idea that statements that seem to be to about objects of experience but have no logically derivable testable consequences
are to be rejected as meaningless. This is not claimed for the sake of providing a basis for rejecting traditional metaphysical statements as meaningless; rather, it turns out that they do have his character and so they are meaningless for that reason.

Carnap exemplified this new role for the philosopher in his *Aufbau* (1928 / 1967). It is on the basis of superficial readings of this work, Friedman argues, that Carnap specifically, and thus logical positivists more generally, are read as classic empiricists. For in the *Aufbau* Carnap indeed sets out to construct first physical objects, and then higher types, out of the elementary phenomena of sense experience. As Friedman shows, the point of this was to demonstrate the method and power of logical construction at work, in an arena familiar to philosophers. Carnap does not view his philosophical programme as hostage to the success of this specific demonstration. Thus, having logically constructed enduring coloured patches on the perceptual manifold out of fleeting colour flashes, he is relatively casual and inexact in his construction of coloured external objects from this basis. Furthermore, he is nonchalant about the question of whether the most useful language of empirical reports is a phenomenal language or a ‘thing’ language – this is taken to be a matter for a conventional judgment, to be informed pragmatically by the progress, or lack of it, of projects in logical construction. Years later he said

> When I developed the system of the *Aufbau*, it actually did not matter to me which of the various forms of philosophical language I used, because to me they were merely modes of speech and not formulations of positions … The system of concepts was constructed on a phenomenalistic basis … However, I indicated also the possibility of constructing a total system of concepts on a physicalistic basis … The ontological theses of the traditional doctrines of either phenomenalism or materialism remained for me entirely out of consideration (Carnap 1963, p. 18).

Friedman emphasizes that the project of the *Aufbau* is closer to Kant’s than to the classical empiricist project because it takes *structure* to be the basis of objectivity: “Scientific knowledge is objective solely in virtue of its formal or structural properties, and these properties are expressed through the ‘places’ of items of knowledge within a single unified system of knowledge” (Friedman 1999, p. 98).\(^\text{106}\)

As Friedman stresses, Carnap’s structuralism brings him into affinity with his leading contemporaries among *avowed* neo-Kantians, philosophers whom no one has ever regarded as classic empiricists. The most important of these figures is Ernst Cassirer (1874-1945). Cassirer’s version of structuralism is especially interesting because its later versions were directly motivated by attention to quantum mechanics, which ought to play at least as great a role as relativity theory in motivating a philosophy of science. As French (1999, 2000) and French and Ladyman (2003) argue, Cassirer understood individual objects in modern physics to be ‘constituted’ group-theoretically as sets of invariants under symmetry transformations. Consideration of field theory leads Cassirer

\(^\text{106}\) Philosophers will want to see this idea expressed a bit more precisely. The idea is that all scientific concepts can be expressed through purely structural definite descriptions (Friedman 1999, p. 103).
to an expression of structuralism that very closely resembles Friedman’s reconstruction of Carnap’s structuralism:

The field is not a ‘thing’, it is a system of effects (*Wirkungen*), and from this system no individual element can be isolated and retained as permanent, as being ‘identical with itself’ through the course of time. The individual electron no longer has any substantiality in the sense that it *per se est et per se concipitur*; it ‘exists’ only in its relation to the field, as a ‘singular location’ in it (Cassirer 1936, p. 178).

One cannot get further from the atomism of classic empiricism than that.

Friedman thus maintains that at least the best-developed expression of logical positivism, Carnap’s, has more in common with neo-Kantianism than with radical empiricism.

Suppose we then agree to usher Cassirer’s structuralist neo-Kantianism, along with logical positivism, into a ‘broad positivist’ tent, from which we exclude foundationalists and metaphysical atomists. Who else can we bring into this tent? Carnap himself suggests extreme liberalism:

The so-called epistemological schools of realism, idealism and phenomenalism agree within the field of epistemology. Construction theory represents the neutral foundation which they have in common. They diverge only in the field of metaphysics, that is to say … only because of a transgression of their proper boundaries (Carnap 1928 / 1967).

Note that Carnap includes ‘realism’ here. He is referring to realism in the sense rejected as ‘metaphysical’ by Kantians, that is, a realism that “attempts to base objectivity on the relation of sensory data to a ‘transcendent’ object existing somehow ‘behind’ the data” and which “creates an unbridgeable gulf between thought and reality in virtue of which objective judgments are just as impossible for us as they are on a strictly empiricist or ‘positivist’ conception” (Friedman 1999, p. 126). Carnap sees the empty dispute between this sort of realist and the phenomenalist as dissolved by his new conception of philosophy. But what about the kind of realism that became more common in philosophy during the final quarter of the twentieth century, ‘scientific realism’ – the view that well-confirmed scientific conclusions should be read as direct, true claims about reality? To what extent can scientific realism be reconciled with positivism as Friedman has reconstructed it?

A main motivation for scientific realism since its renaissance in the mid-1970s was reaction against the perceived implications of logical empiricism’s holism about meaning, when combined with presumed atomism about the objects of scientific belief. The worry was as follows. Suppose that the meaning of a term for a type of entity, such as ‘electron’, is determined by its role in physical theory. It then follows that when physical theory is revised in light of new discoveries, the meaning of ‘electron’ must change. This in turn seems to deprive us of any basis for saying that our understanding of
electrons has *improved*, since we lack grounds for saying that the discarded theory and the new theory are rival accounts of the same objects. But this implies an absurd history of science. The distinctive core claim of the scientific realist is that a term such as ‘electron’ refers to a theory-independent constituent element of reality with whatever properties electrons *in fact* have – as opposed to whatever properties passing theoretical *descriptions* of electrons *take them* to have. This allows us to say that later physicists offered more accurate models of electrons than their predecessors.

The rise of scientific realism has inspired a revival of unabashed metaphysics in much of philosophy. Ladyman and Ross (2007), following and supplementing van Fraassen (1980, 2002), point out that the motivation to account for scientific progress, though well justified in itself, does not overturn the case against strong metaphysics made by Hume, Kant, Russell and the logical positivists. Such metaphysics is not part of the positive content of science, and presupposes the possibility of objective knowledge being achieved through exercises of pure reason and / or intuition that are independent of controlled observation and experiment. This is extravagant hubris and incompatible with the scientific attitude.

Problems over the basis for ascribing progress to science arise from taking the basic subjects of scientific generalizations to be the kinds of *objects*, such as electrons, about which successive scientific theories have disagreed through history. However, Ladyman and Ross argue, building on earlier work by Ladyman (1998, 2002) and French and Ladyman (2003), that the basis for reading the history of science as one of radical discontinuities (along lines made popular by Kuhn and others) falls away if the content of science is understood on *structuralist* lines. New theoretical proposals taken seriously by scientists generally enrich and extend the mathematical structures developed by earlier scientists studying what they take to be the same phenomena. Apparent discontinuities at the scale of putatively self-subsistent objects tracked from limited, subjective perspectives threaten scientific objectivity only to the extent that scientific theories – as opposed to everyday folk ontological principles – endorse such objects. However, the progress of fundamental physics (that is, quantum physics, especially field-theoretic quantum physics) has strongly confirmed Cassirer’s basic insight on this question. The confirmed fact of quantum entanglement constitutes scientific refutation of the objectivity of the folk model of the world as ‘made of’ ‘little things’ that ‘occupy space’, ‘endure through time’ and change one another’s ‘accidental properties’ by banging into one another. These metaphors derived from the manipulation of everyday objects by people find no counterparts in modern mathematical physics.

The Ladyman-Ross version of structuralism fits comfortably within Carnap’s large tent. The aspect of Carnap’s mature philosophy which, according to Friedman, makes it ultimately untenable is the one place where Ladyman and Ross introduce a novelty that has no precedent in logical positivism. To avoid the kind of objection that makes empiricism incapable of accounting for objectivity, Carnap depended on the distinction between ‘analytic’ and ‘synthetic’ statements, which he was required to interpret in a way that was fatally undermined by Gödel’s incompleteness results. (For details, see Friedman 1999, chapters 7-9.) Where Carnap’s structuralism relies on analyticity to
Ladyman and Ross appeal instead to a distinguished (‘fundamental’) body of physical generalizations which, if quantum theory is true, constrain every measurement taken at every scale everywhere in the universe. Should quantum theory fail to be true and universally applicable then this philosophy loses a premise on which it absolutely depends. But that is just the sort of state of affairs that should prevail in philosophy that is antecedent to, rather than prior to, science.

Ladyman and Ross speak of ‘naturalization’, rather than ‘rejection’, of metaphysics. In the present context, however, this difference between logical positivism and contemporary structuralism is mainly semantic. Structuralist metaphysics is heavily deflated by comparison with traditional metaphysics. By ‘metaphysics’ is meant only an enterprise that aims to unify the various special sciences – another goal structuralists emphasize in common with Kantians and logical positivists. Structuralism grants to the scientific realist that fundamental physics directly describes what exists. But most of science (including most of physics) is not fundamental physics. Such deflated metaphysics as structuralism allows is thus of no direct consequence for economics. Economics may not contradict the accepted generalizations of physics, but this has no bearing on any serious questions that preoccupy economists. Where special sciences like economics are concerned, the structuralist view is broadly positivistic. Thus broad positivism remains a live option in philosophy of science. An alternative contemporary version is offered by Friedman (2001).

Broad positivism commits philosophers to trying to explain why the scientific enterprise as a whole divides into sub-provinces – the different special sciences – by reference to distinctive mathematical structures, notwithstanding the fact that all sciences describe a common universe. Within the broadly positivist framework, we should expect a scientist writing about the nature of her discipline to aim at elucidating its general structural parameters – the parameters that make it the discipline it is, rather than some other discipline. If her science is mature, we should find her doing this using mathematics, the only representational technology both rich and general enough to allow for contrastive comparison of her disciplinary structure with others. We should not expect her to try to improve her own or anyone else’s understanding of her discipline by applying to it a tendentious epistemological or metaphysical doctrine she has gleaned from a philosopher, based on principles that originate from outside the experience gained in using her discipline’s structure to achieve new empirical discoveries.

With this perspective in mind, we turn to Robbins’s Essay.

4. Robbins as Positivist

I will argue that Robbins’s philosophical attitude, as expressed rather than declared in the Essay, is closer to logical positivism than was Hutchison’s, who was a British empiricist. The relationship between Kantians and logical positivists on the one hand, and between Kantians and the Austrian economists, on the other hand, explains this. The Austrians were directly influenced by reading Kant (Parsons 1990), and came to intellectual maturity in a strongly neo-Kantian environment. The point is not to suggest that Robbins
self-consciously embraced (second hand) positivism and applied it to economics. The point is merely that the entire style of thinking – especially, in this instance, about mathematics, the nature of ideas, and the concepts of objectivity and logical priority – that Kant bequeathed to everyone educated in German-speaking universities were part of Robbins’s intellectual background to an extent that was unique among major Anglo-American economists.

As we have seen, the positivist attitude to the elucidation of science enjoins that one avoid trying to ‘found’ the structure of inquiry in a discipline on general philosophical principles. One important aspect of Robbins’s positivism lies in the fact that all of his key premises in the Essay are developed from the immediate history of problem-solving in economics that he inherited, not from philosophy. In this respect Robbins differs strikingly, and to his credit, from Mises, a literal and self-conscious – and, as Barrotta (1996) argues, frequently confused – Kantian, who tried to promote praxeological over mathematical economics on the basis of philosophical dogma that he derived from a clumsy reading of Kant.

If all it took to be a broad positivist were abstemiousness about introducing metaphysical assumptions into science, then broad positivism would be too watery a notion to be worth making much of. An interestingly positivist stance should at least also conform to the characteristic positivist privileging of objectivity over subjective empirical descriptive detail. We will see that Robbins is an exemplary positivist in this respect. In addition, as we will see in the next section, identifying Robbins’s approach as positivistic explains the otherwise puzzling pair of theses to which he subscribes concerning knowledge about preferences.

To show how Robbins derives the premises of his arguments from the history of problem solving in economics, some historical context must be introduced. It is useful to begin with a quotation from the marginalist founding figure who was closest to the main current of classic British empiricist philosophy, Jevons. “Far be it from me,” he cautions in the Theory of Political Economy (1871), “to say that we shall ever have the means of measuring directly the feelings of the human heart. A unit of pleasure or of pain is difficult even to conceive; but it is the amount of these feelings which is continually prompting us to buying and selling, borrowing and lending, laboring and resting, producing and consuming” (Jevons 1871, p. 13). Here is the economist who is typically credited or blamed for setting the new economics firmly on Benthamite hedonic utilitarianism – the doctrine against which Robbins was supposedly rebelling in rejecting interpersonal utility comparisons – already expressing doubt about not only the practical possibility of scaling preference intensities but about the very conceivability of such a scale.

Jevons also recognized that although ‘pleasure’ and ‘pain’ might literally be thought to denote hedonic sensations of the sort that Jevons expected psychologists to eventually physically measure, the ‘utility’ of which they indicate opposed valences can also be interpreted in what Bentham explicitly called a “wide and expansive” sense that encompasses all possible motivators. Thus, says Jevons, we can “call any motive which
attracts us to a certain action pleasure and that which deters pain” (1871, p. 31). He then raises the problem that remains a favourite concern of sceptics about the psychological adequacy of neoclassically derived microeconomics: the tautology objection. If one adopts Bentham’s ‘wide and expansive sense’ of ‘utility’ then “it becomes impossible to deny that all actions are prompted by pleasure or by pain,” in which case invoking a person’s will to maximize their utility as an explanation for their actions looks empty. This prompts Jevons to draw a distinction that Robbins devotes much work in the early part of his Essay to undoing: dividing pleasures into “higher” and “lower” categories, in which the former include those that involve moralized or altruistic motivations, while the latter are restricted to the satisfaction of self-focused “material” sensations. The aspect of behaviour concerned with such material and individually self-interested well-being is then taken by Jevons to be the proper domain of the economist. Marshall followed him in this view. Given how hard Robbins works in the Essay to restore the ‘wide and expansive’ interpretation of ‘utility’, this respect in which Robbins moved to correct a key foundation stone of methodological individualism in economics, his Austrian affinities notwithstanding, is worth noting for later consideration. We should also note that Robbins was not the first marginalist to break with Jevons and Marshall on the hedonic and materialist interpretation of utility. Wicksteed (1910) urged that the scope of motivations encompassed by the principle of marginal-utility maximization includes “all the heterogeneous impulses of desire or aversion which appeal to any individual, whether material or spiritual, personal or communal, present or future, actual or ideal” (ibid, p. 32).

This widening and distancing from psychology of the general concept of purposeful motivation is already a major step away from British empiricism in a Kantian direction. Kant and philosophers he influenced strongly separate motivation in the logical sense from motivation in the psychological sense. It is the former, normative, form of motivation that they take to be relevant to the objectivity of science. Thus for such philosophers the domain of ‘action in general’ is an appropriate object of analysis distinct from a psychologist’s study of the pursuit of any particular end, such as material wealth. The former domain is a suitable candidate for the application of a body of systematic logical relations, whereas activity aimed at satisfaction of material wants is a rationally arbitrary concatenation yoked together by reference to practical human purposes. Economists since Robbins’s generation have generally – at least until the recent vogue for behavioural economics – adopted the perspective of the Kantians more than that of the empiricists in this regard. It is the basis of the point, now often made derisively by anti-economists and heterodox economists, that mainstream economics models ‘rational agents’ rather than ‘real people’.

In Robbins the distinction that a contemporary philosopher would draw by reference to ‘psychology’ versus ‘logic’ is made instead using unsystematic reference to two different senses of ‘psychology’. At one point, less confusingly, he resorts to German for a semantic resource and refers to psychology in the empiricist’s sense as “Fach-Psychologie”, of which he says that psychological hedonism is one “brand” (Robbins 1935, p. 85). He regrets that certain “English” economists “did in fact claim the authority of the doctrines of psychological hedonism as sanctions for their propositions” but
happily “[t]his was not true of the Austrians. The Mengerian tables were constructed in terms which begged no psychological questions” (p. 84). The ‘English’ flirtation with Fach-Psychologie is then said to be merely that: “trimmings” and “ex post facto apologia” around a core of “logic” about which the Austrians were clear. We need hardly wonder where Robbins would stand on the current drumbeat of announcements of a ‘paradigm shift’ in economics driven by experiments showing that people are not rational, when he says:

The borderlands of Economics are the happy hunting-ground of minds averse to the effort of exact thought, and, in these ambiguous regions, in recent years, endless time has been devoted to attacks on the alleged psychological assumptions of Economic Science. Psychology, it is said, advances very rapidly. If, therefore, Economics rests upon particular psychological doctrines, there is no task more ready to hand than every five years or so to write sharp polemics showing that, since psychology has changed its fashion, Economics needs “re-writing from the foundations upwards”. As might be expected, the opportunity has not been neglected (pp. 83-84).

Thus when Robbins says, a few pages later, that “if we are to do our job as economists, if we are to provide a sufficient explanation of matters which every definition of our subject matter necessarily covers, we must include psychological elements” (p. 89), we must understand ‘psychological elements’ in terms of something other than Fach-Psychologie. Unfortunately, at this point Robbins is far from precise. His favourable reference to Max Weber here has directed several commentators to attribute to him the idea that economics depends upon Verstehen. Textual grounds for this interpretation are weak, however. Robbins never uses the word, though as an admirer of Weber he was obviously familiar with it. Verstehen is an epistemological method, and in this part of the Essay Robbins is not discussing epistemology. Chapter IV, where the attack on incorporation of Fach-Psychologie and the citation of Weber occur, concerns the scope and subject matter of economic generalizations. One might almost think Robbins’s focus was ontological, since in the paragraph in question he refers to “links in the causal chain which are psychical, not physical” (p. 90). Yet if there is scant evidence for attributing commitment to the method of Verstehen to Robbins, there is none whatsoever for imagining that he hankers after metaphysical dualism. So how are we to understand his insistence on the need for a psychological element in economics, in the same book which a few chapters earlier advises economists to ignore psychology?

Bearing in mind the Kantian intellectual environment of the Austrians renders these remarks of Robbins easier to interpret. What he is talking about here is logic in the sense in which a Kantian philosopher prior to Frege, Russell and the logical positivists would have used the term. We might speculate that Robbins does not say ‘logic’ here because he knows that logic now means something more exact and technical in nature, but isn’t sure what to say instead. And well he might not know, because his contemporaries among the broadly positivistic philosophers were divided and unsure of themselves in this terrain also. Cassirer, for example, still used ‘Psychologie’ in contexts where, as an avowed neo-Kantian, he certainly did not mean Fach-Psychologie. Carnap and other logical
positivists use a variety of German words marking subtle distinctions, which their English translators tend perforce to leave in German. ‘Gedanke’ is probably a good rendering of what Robbins is getting at; but its normal translation as ‘thought’ would hardly have aided clarity in English. He might have been well advised to use ‘reason’. That this semantic association was near the front of his mind as he wrote these passages is suggested by the fact that the topic of his next paragraph is the extent to which the generalizations of economics depend upon “the assumption of completely rational conduct”. Although by this he clearly intends reference to the contemporary economist’s sense of ‘rational’, he refers to this as “a more general psychological assumption”. This use of ‘psychological’ does not accord with contemporary usage. I suggest it is exactly the alternative sense of ‘Psychologie’ – alternative, that is, to Fach-Psychologie – intended in the Kantian tradition by Cassirer.

Rationalization of this part of the Essay in terms of Kantian philosophy is further encouraged by Robbins’s gloss of the ‘psychological elements’ with which the economist cannot dispense as “the conception of purposive conduct” (p. 90). He later says “it is arguable that if behaviour is not conceived of as purposive, then the conception of the means-end relationships which economics studies has no meaning. So if there were no purposive action, it could be argued that there were no economic phenomena” (p. 93). In a footnote, he attributes this view to Mises, who in turn understands it in explicitly Kantian terms. 107 What Robbins is mainly saying here, in plain modern terms, is that economics is essentially about ‘agency’. ‘Agency’ is necessarily rational, in precisely the sense of ‘purposive’. It does not necessarily involve sophisticated computational processing or self-consciousness – a negative point which the greatest of the Austrians, Hayek, regarded as especially important (see Hayek 1952). This is why, I have argued at length elsewhere (Ross 2005), economic theory applies readily – indeed, surpassingly well – to non-human animals, 108 but not to rocks or planets. The former have conditions of better and worse flourishing that motivate (consciously or not) actions, whereas no state of affairs is better than another for a rock. Thus we can construct utility functions for, and assign opportunity costs to, the behaviours of, animals (and plants, and, in the new domain of neuroeconomics, brain cells), but not to rocks. It would thus have been more consistent with Robbins’s conception of the domain of economics had he dropped the restriction in his definition of the scope of economics to ‘human’ behaviour. But the science of ethology did not yet exist by name when Robbins wrote, and the view of animals as deterministic robots was then still widespread.

Robbins is again at one with neo-Kantian philosophers in thinking that rational agency is constitutive of purposiveness, but does not require or entail “perfect constancy” – the sense of ‘rational’ that is at issue when contemporary critics of mainstream economics attack ‘rational economic man’. In this connection, Robbins gropes for a distinction we might now draw as that between an agent with perfectly rational expectations and an agent whose preferences merely do not cycle. Cycling, he notes, is excluded in equilibrium lest arbitrage be possible (p. 92). Rational expectations, he suggests, is an

107 Barrotta (1996, 1998) and Parsons (1997) agree about this, but quarrel over the extent to which Mises’s understanding of Kant on the question is sound.

108 See, for evidence, the papers in Noë, van Hoof and Hammerstein (2001).
idealization with which the economist interested in limiting conditions begins, and which is relaxed in applications (p. 94).

Robbins’s treatment of these matters, which still cause difficulty in contemporary economic theory, cannot be said to be handled by him in a completely satisfactory way. He might have noted that both the requirement of acyclicity in equilibrium and the idealization of rational expectations are both properties of aggregate states of an economy (notwithstanding their later reductive interpretation in the microfoundations literature). I suggest that we should not regard them as psychological elements even in Cassirer’s sense of ‘psychological’; they are rather system-scale properties that need not decompose. But then we confront the general problem that relaxing these system-scale idealizations does not generally work in economics in the way in which, as Robbins mentions, it works in physics: in economics, systems not at equilibrium don’t reliably tend to be near equilibrium. But this hadn’t yet been understood in Robbins’s time.

Robbins might be accused – as I accuse him in my 2005 book (p. 94) – of waffling on the importance of acyclicity of preference in the individual agent. He clearly aims to minimize the extent to which economic logic is committed to this property. “In so far,” he says, “as the term rational is taken to mean merely ‘consistent’, then it is true that an assumption of this sort does enter into certain analytical constructions” (p. 91). This is a considerable, if it should not be called whopping, understatement of the truth. Earlier in the Essay Robbins has enshrined the fact that preferences are normally ordered as one of the two foundational assumptions (along with the ubiquity of scarcity) for the empirical significance of economics. But acyclicity is part of what it means to say that preferences are ordered. Robbins makes clear that he thinks that inconsistent preferences are common but that consistent ones characterize enough of a typical person’s behaviour for economic analysis to gain purchase. This must be regarded as an incomplete account if the conception of economic analysis as consisting in deduction from basic postulates, which Robbins also asserts (pp. 75-76), is not to be threatened. ‘People have many consistently ordered preferences’ is not a postulate from which any precise claims about aggregate demand characteristics can be derived.

I argued in my 2005 book that there is a straightforward way out of this problem we can urge on Robbins’s behalf, which is to acknowledge that an entire person’s biography does not map onto one enduring economic agent. A person is instead a succession of economic agents, changing ‘agent identity’ when her tastes change. It may be objected that this implicates economics in ad hoc ontological principles, allowing agents to be multiplied without limit merely to make a certain style of formal analysis tractable. This objection is misplaced against a broadly positivist conception of economics, however. For the positivist, a scientific discipline is distinguished from others partly by identifying its conventional analytic categories. Psychological generalizations apply to a person named Lionel Robbins. Biological generalizations apply to another entity tracked by many of the same perceptible markers as Robbins, but as an instance of the species H. sapiens rather than as a person. Economic generalizations apply to an agent maximizing its present utility given a consistent preference ordering. We might casually say that the entity in question at time $t$ is Lionel Robbins executing the purchase of some shares. But we...
should not assume that this entity will still be found hanging around the LSE at \( t \) plus five years’ time merely because the person Lionel Robbins will still be found there. Agent identification is governed by pragmatic considerations, not metaphysics; thus we can be practical concerning how sensitive models must be to small variations in preference profiles. As long as the entity we are modelling keeps preferring a secure but lower-return asset to a riskier but higher-returning one (for fixed prices) during the time he is wondering how to dispose of a few months’ income, that is generally good enough for the economist’s purposes. If he grows permanently fed up with brandy, which he formerly enjoyed, during the same period, we might simply decide to ignore this fact because we deem it unimportant to our analytical projects, even if, strictly speaking, it constitutes a change in the ‘complete’ utility function.

The conventional nature of the concept of agency does not undermine objective judgments about an agent’s expected consumption behaviour in response to a given price change once that agent has been identified (by specification of a utility function). As we saw in section II, this is the basic point of the broadly positivist attitude to science. It might be thought that this attitude conflicts with the following passage from the Essay: “[I]t does not follow in the least that [economic] generalizations have a ‘merely formal’ status – that they are ‘scholastic’ deductions from arbitrarily established definitions … It is true that we deduce much from definitions. But it is not true that the definitions are arbitrary” (p. 105). That ‘economic agent’ has the conceptual structure it does as a function of its role in economic argument patterns and generalizations, and that we have degrees of freedom over where to apply it in advance of a specific problem context does not imply that facts do not determine where the agents are after a problem has been identified. Some models capture the economic influences actually at work in a circumstance and others do not, or do so less completely. As Robbins says “the validity of a particular theory is a matter of its logical derivation from the general assumptions which it makes. But its applicability to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation” (pp. 116-117).

In saying that the bounds of agency are set by convention, the positivist does not say that agents ‘exist only in theory’ or ‘are merely theoretical entities’. Claims about the ontological status of a type, except where these are relative to a specific model, are metaphysical claims. These are just the sorts of claims, I am arguing, that Robbins should not be interpreted as making. There is no need for any economist to try to come to a view on such matters. Indeed we might go even further and suggest that she may make herself a less effective economist if she does so, because if the resulting philosophical opinion makes any difference to her practice at all, it must make her less flexible as a model builder.

In section 3 I identified the contemporary expression of broad positivism with structuralism. This applies well to Robbins, especially as an aid to understanding his polemic against historicism and institutionalism in chapters IV and V of the Essay. His problem with institutionalism is not that institutionalists mention institutions. Most contemporary development economists believe in a sort of Gresham’s law of institutions to the effect that institutions which encourage rent-seeking tend to drive out efficiency-
promoting ones. I can find nothing in Robbins’s Essay that would bid us to reject such a
generalization about institutions and their economic effects. The basis for Robbins’s
critique of the historicism of his time is that these historicists deny themselves a stable
basis for sorting causally relevant factors in a situation into sets of endogenous and
exogenous factors. In consequence they produce historical descriptions instead of
generalizations, and deprive themselves of bases for projection of their knowledge to new
instances. The economist is directed by Robbins to search for the aspects of a situation
which represent structural parameters featuring in economic “laws”, and which thereby
allow deduction of further structural parameters. It is this perspective that licenses the
following claim:

Economic laws describe inevitable implications. If the data they postulate are
given, then the consequences they predict necessarily follow. In this sense they are on the same footing as other scientific laws, and as little capable of “suspension”. If, in a given situation, the facts are of a certain order, we are warranted in deducing with complete certainty that other facts which it enables us to describe are also present … If the “given situation” conforms to a certain pattern, certain other features must also be present, for their presence is “deducible” from the pattern originally postulated (pp. 121-122).

This level of authority in analysis would not be plausible, as Robbins explains, if the economist took herself to be responsible for predicting all or even most details of social states of affairs (including stereotypically ‘economic’ details such as exact prices or rates of output). The domain of the economist is a carefully circumscribed network of general structural relationships that apply whenever an interacting group of agents confront scarcity in the means to the ends over which they each have ordered preferences.

On the basis of the evidence mustered to this point, I claim that Robbins exemplifies the philosophical minimalism of the broad positivist. The basic implication of this so far as methodology is concerned is that metaphysical opinions are irrelevant to the conduct of economics. The economist travels about with a toolkit full of structural relations which, when applied to situations in which people (or, I add, other entities to which the concept of agency can be applied) confront scarcity with respect to the means to their ends, allow her to deduce further structural relations. General agreement among economists about the representation and nature of these structural relationships, and about conditions for their applicability, provide the basis for objectivity in economics. Economists of course routinely disagree amongst themselves. Such disagreement, if it is genuinely economic disagreement (rather than political or ethical disagreement in disguise) should always turn out to be disagreement over whether some instances of relevant structural parameters have been left out of the model of the situation at issue. Are there agents with influence who are not being accounted for? Do some agents have incentives that have not been noticed? Will different consumption patterns with respect to the scarce resources at issue influence patterns of scarcity in other resources that have not been foreseen? The contemporary economist would raise other potential questions, unavailable to Robbins, about distributions of asymmetrical information among the agents, and about constraints on the agents’ own modeling of the expectations of one another. Current economists
might in addition have disagreements about methods – over, for example, whether one econometric test or another is more likely to reveal robust dependencies among variables. None of these disagreements are philosophical.

One major task remains. I said that understanding Robbins’s broad positivist stance can help us make sense of his curious conjunction of views on what can and cannot be known about preferences. According to him, we can know that people order them but cannot know anything about their relative intensities. My reading of Robbins motivates searching for an economic justification of these views that does not appeal to one or another tendentious doctrine of general epistemology. To this search we now turn.

5. Robbins on Properties of Preference Relations

Let us begin by setting out the problem that Robbins raises for us in interpreting his views on the properties of preference sets.

In such a history of economic thought as is influenced by standard methodology, which stresses empirical psychological hypotheses underlying economic theory, the rejection of hedonistic sensationalism and the campaign for ordinalism in the interpretation of utility functions tend to be run together as two aspects of the same view. We have seen how Robbins’s broad positivism rationalizes anti-sensationalism: the latter is a thesis about specific psychological causation, a topic for Fach-psychologie, which the economist is advised not to incorporate into her models. However, Robbins seems then to claim that the possibility of economic analysis rests on one general psychological fact, namely, people’s introspective awareness of processes of deliberative choice phenomena (pp. 75-76). Allowing that this one datum from psychology, however general, is foundational for economics while all other psychological phenomena are to be rigorously excluded as irrelevant seems prima facie surprising, and to call for philosophical argument which Robbins does not provide. Furthermore, the brief basis Robbins does offer for exclusion of relative preference intensities appears to be of a behaviourist and operationalist character:

[S]uppose that we differed about the satisfaction derived by A from an income of £1,000, and the satisfaction derived by B from an income of twice that magnitude. Asking them would provide no solution. Supposing they differed. A might urge that he had more satisfaction than B at the margin. While B might urge that, on the contrary, he had more satisfaction than A. We do not need to be slavish behaviourists to realize that here is no scientific evidence. There is no means of testing the magnitude of A’s satisfaction as compared to B’s. If we tested the state of their blood-streams, that would be a test of blood, not satisfaction. Introspection does not enable A to know what is going on in B’s mind, nor B to measure what is going on in A’s. There is no way of comparing the satisfactions of different people (pp. 139-140).

We might agree that in the imagined circumstances asking A and B to compare their preference intensities would be an ill-advised procedure. Their evidential circumstances
are obviously asymmetrical between themselves and the other if introspection is admitted as a viable source of evidence in the first place, as Robbins seems to allow. Furthermore, A’s and B’s claims will be undermined by moral hazard if redistributive policies or considerations of social equality are operative (something Robbins hints is relevant here). But Robbins also rules out the possible relevance of evidence gathered independently of A’s and B’s avowals. If some property of the blood-stream were a reliable indicator of satisfaction, then why would a ‘test of blood’ not be an indirect ‘test of satisfaction’? (We may indeed soon have the capacity to use neuroimaging probes to measure intrapersonal emotional states. Whether we will ever be able to justify comparisons of these measurements on an inter-personal scale is much less clear.)

Earlier in the Essay, when defending the economist’s appeal to everyone’s experience of ordering their preferences, Robbins waves away operationalist scruples:

In recent years … partly as a result of the influence of Behaviourism, partly as a result of a desire to secure the maximum possible austerity in analytical exposition, there have arisen voices urging that this framework of subjectivity should be discarded. Scientific method, it is urged, demands that we should leave out of account anything which is incapable of direct observation. We may take account of demand as it shows itself in observable behaviour in the market. But beyond this we may not go. Valuation is a subjective process. We cannot observe valuation. It is therefore out of place in a scientific explanation. Our theoretical constructions must assume observable data … It is an attitude which is very frequent among those economists who have come under the influence of Behaviourist psychology or who are terrified of attack from exponents of this queer cult.

At first sight this seems very plausible. The argument that we should do nothing that is not done in the physical sciences is very seductive. But it is doubtful whether it is really justified (p. 87).

No argument is given for the final assertion in this passage; Robbins merely asserts that “we do in fact understand terms such as choice, indifference, preference and the like in terms of inner experience” (pp. 87-88).

Once it is imagined that people phenomenally experience their preferences as ordered, it is then peculiar to suppose that they experience them as merely ordered. Hume, by contrasting example, clearly thought that we infer our preference orderings from our phenomenal awareness of differing levels of ‘vivacity’ in our passions for outcomes. In maintaining this doctrine, Hume surely speaks for folk psychology. In the second passage above Robbins seems to baldly appeal, over the heads of scientific psychologists, to folk psychology – while also appearing to defend a view of economic psychology, one we might dub ‘psychological’ ordinalism, which would perplex the folk and scientists alike.

I do not think there is any way to avoid finding Robbins guilty of at least careless writing in fostering this tension. If some degree of behaviourism is ‘slavish’ this implies that
some lesser degree – the degree to which we can have recourse in justifying rejection of interpersonal comparisons – is not. Yet in the earlier passage the most basic of behaviourist commitments, to the conviction that introspection is never a valid source of scientific evidence, is dismissed by implication as the dogma of a “queer cult”. No one can write two such things in the same book, without further explanation, and reasonably expect readers to clearly understand what he believes.

In showing how to best rationalize these apparently conflicting ideas about knowledge of preference structures I will need to convict Robbins of one straightforward semantic slip. My interpretation will require us to assume that the inclusion of the word ‘inner’ in the statement “we do in fact understand terms such as choice, indifference, preference and the like in terms of inner experience” is a mistake. I will suggest that it is an explicable mistake because recognizing it as such depends on subtle philosophical distinctions with which it would not be reasonable to expect Robbins to have been acquainted.

Before we return to this, we must locate the economic motivation for Robbins’s ordinalism. The key comes in his earlier argument that, appearances notwithstanding, economists are not concerned with measuring quantities:

[T]he valuations which the price system expresses are not quantities at all. They are arrangements in a certain order. To assume that the scale of relative prices measures any quantity at all save quantities of money is quite unnecessary. Value is a relation, not a measurement (p. 56).

To this Robbins appends the following footnote:

Recognition of the ordinal nature of the valuations implied in price is fundamental. It is difficult to overstress its importance. With one slash of Occam’s Razor, it extrudes for ever from economic analysis the last vestiges of psychological hedonism. The conception is implicit in Menger’s use of the term Bedeutung in his statement of the Theory of Value, but the main credit for its explicit statement and subsequent elaboration is due to subsequent writers. See especially Cuhel, Zur Lehre von den Bedürfnisse, pp. 186-216; Pareto, Manuel d’Economie Politique, pp. 540-2; and Hicks and Allen, Reconsideration of the Theory of Value (Economica, 1934, pp. 51-76). In this important article it is shown how the most refined conceptions of the theory of value, complementarity, substitutability, etc., may be developed without recourse to the notion of a determinate utility function (p. 56).

This is among the most revealing passages in the book. It is a footnote in part because it is a citation of specific sources, but in part because Robbins is interrupting his discourse to the general reader and addressing fellow economists. Crediting Menger with the original insight, Robbins lists the recent highlights in what he sees as progress in separating economics from any interpretation in terms of folk psychology. As we will see shortly, the final sentence indicates Robbins’s stand on the contested question of how to relate the traditional economic concept of utility to the newly developed analytic
framework of indifference curves. Like Samuelson just a few years after him, Robbins would prefer to eliminate ‘utility’.

I suggest that much of the difficulty in interpreting Robbins’s treatment of preference relations in the Essay stems from the fact that he did not come up with (perhaps did not try to come up with) a way of explaining the recent technical developments in economics to his non-specialist readership. He therefore fell back on prosaic formulations from the Austrians, which, in his opinion, captured at least the implications, if not the primary motivations, of these technical developments. The main text in the passages we have been following resumes thus:

[I]t follows that the addition of prices or individual incomes to form social aggregates is an operation with a very limited meaning. As quantities of money expended, particular prices and particular incomes are capable of addition, and the total arrived at has a definite monetary significance. But as expressions of an order of preference, a relative scale, they are incapable of addition. Their aggregate has no meaning. They are only significant in relation to each other. Estimates of the social income may have a quite definite meaning for monetary theory. But beyond this they have only conventional significance (pp. 56-57).

This is sufficient to explain why Robbins would reject the argument for income redistribution from diminishing marginal utility that is used as the motivation for his attack on interpersonal utility comparisons in Chapter VI. That argument was no doubt seen by him as a more dangerous application of a fallacy to a policy debate, and so a more important focus when addressing a popular audience. At the policy level, A’s and B’s incentives to exaggerate the downward slopes of their utility curves seem like serious considerations; and then a pinch of the kind of empiricism running so strongly in 1930s British intellectual currents might as well be thrown into the kindling. But the decisive basis for rejecting interpersonal comparisons had emerged from economics, not philosophy, and had been stated several chapters earlier for the cognoscenti to see. Aggregate demand buries information about opportunity costs. But opportunity cost, as Robbins explained back in Chapter I, is the very subject matter of economics. Now, aggregate demand, being expressed in monetary expenditure, is at least meaningful if monetary expenditure is what interests us. However, there is no analogous thing to be said about aggregate utility; that is no quantity at all. But the unsophisticated tend to interpret ‘utility’ as denoting a quantity, and the first generation of British marginalists – but not Menger – had unfortunately encouraged this. So, Robbins says in his footnote, better to have done with ‘utility’ altogether.

Stated precisely, ordinalism of the sort Robbins defends is the thesis that objective functions – which, notwithstanding the apparent preference of Robbins and the clear later preference of Samuelson, continue to this day to be called ‘utility functions’ – are defined only by reference to properties preserved under monotonically increasing transformations. Diminishing marginal utility is not such a property – which is just why the mainstream economists of the 1930s welcomed its apparent elimination by Hicks and
Allen in the 1934 paper to which Robbins refers in his footnote. Their replacement property, diminishing marginal substitutability, guarantees downward sloping demand curves by supposing that agents will exchange less of any commodity \( x \) for another commodity \( y \) as their stock of \( x \) increases (except where \( x \) and \( y \) are complements or \( y \) is inferior), but makes no reference to any sensationalistic or other basis in *Fach-Psychologie*.

Mandler (1999, pp. 85-96) distinguishes between the diminishing marginal utility principle as Jevons had understood it and the weaker property Mandler calls ‘psychological concavity’. The former is the thesis that agents are introspectively aware of the *rates* at which the marginal utilities of particular commodities diminish on the margins, while psychological concavity denotes the property of *mere awareness that* marginal utility diminishes. Mandler defines a model of psychological concavity as follows: “At any point \( x \), the set of psychologically accurate utility representations of preference on any line intersecting \( x \) is nonempty and consists of all of the concave utility representations of the agent’s preferences on that line. In other words, agents experience diminishing marginal utility in all directions but no further nonordinal psychological reactions; on any line, any concave function representing the agent’s preferences is psychologically accurate” (1999, p. 87). This specification allows assessment of the formal relationship between psychological concavity and diminishing marginal utility. Mandler shows that the set of utility-function transformations respecting Jevonian diminishing marginal utility is a proper subset of those respecting psychological concavity, so the latter is a weaker assumption. However, psychological concavity is still not strictly ordinal.

In my 2005 book I argued (pp. 98-99) that Robbins *should have* embraced psychological concavity, which allows the possibility of *qualitative awareness* by agents of diminishing marginal utility, but in Chapter IV he goes unnecessarily far (by his own lights) and effectively endorses the purely behavioural principle of diminishing marginal substitutability. He says in a footnote that despite wholly approving of Hicks’s and Allen’s accomplishment, he still “prefer[s] the established terminology” – i.e., diminishing marginal utility – but gives no reason for this preference. Might he have thought that the difference between ‘diminishing marginal substitutability’ and ‘diminishing marginal utility’ was merely ‘terminological’? That would be a very natural thing for a logical positivist to think. And I suggest that it is a quite natural thing for Robbins to think, because although the two principles have quite different implications for the relationship between economics and psychology – with psychological concavity having yet other implications – they make no difference for economic analysis if the data for that analysis are demand schedules rather than reported preferences.

That is exactly what the leading economic theorists of the 1930s and afterwards thought their data ought to be. They did not think this under the direct influence of philosophers, but their ambition for a separate and systematic economics is, as it happens, just what both Kantians and logical positivists would have regarded as equivalent to pursuit of ‘objective’ economics. Since broad positivists claim to derive their philosophical principles *from* science instead of deriving them independently and imposing them *on*
science, a broad positivist should welcome evidence that the evolution of objective
economics had been continuous, and endogenously driven. There is no shortage of such
evidence. The achievement of Hicks and Allen that Robbins welcomes marked a
milestone in a long development that began as early as 1881 with Edgeworth, was
substantively completed by Samuelson in 1947, and was technically finished by Debreu
in 1959.

My saying (following Mandler) that the process ‘began’ with Edgeworth alludes to his
introduction of indifference curves to represent marginal analysis. It was, however, Fisher
(1892) who made more than a mere representational device of the indifference curve. In
particular, Fisher eliminated all assumptions about cardinal utility beyond the
indifference judgment itself, which in his treatment becomes primitive. The indifference
curve assumes comparison of signs of marginal utility (i.e., measurement of relative
utility is unique up to monotone transformation), but presumes no measurements of any
quantitative sums or totals of utilities. Fisher showed that relative price-levels at
equilibria – points where agents could not improve their satisfaction by shifting their
consumption – can be determined strictly by the gradients of indifference curves.
Therefore, if we can derive families of indifference curves for all consumers and all
consumption bundles, then we can do our economic analysis without having to know
anything about cardinal magnitudes. Pareto (1909 / 1971) took this analysis one step
further, arguing that since indifference curves can be constructed on the basis of
sequences of observed choices by agents, we need not begin microeconomic analyses
from any independent measurements of utility, if utility is interpreted as some sort of
psychological aspect or coefficient.

As Mandler (1999) demonstrates, neither Fisher nor Pareto was consistently anti-realist in
their attitude toward utility as a psychological force. Fisher’s specific analysis
presupposed that the utility an agent derives from a particular commodity is often
meaningfully separable from the utility she derives from other commodities; and if
utilities can be separated then they are not yet formally redundant. Pareto makes the same
move, and, at at least one point, offers a cardinalist interpretation of the meaning of
indifference indices (Mandler 1999, p. 121). This is the point of entry for Hicks and Allen
(1934).

Hicks and Allen begin from Pareto’s insight that preference-maps sufficient for
prediction of consumption can be based on indifference curves that need not themselves
be derived from utility functions. Since, following Fisher, indifference curves incorporate
no presumption of cardinal comparability beyond primitive indifference judgments, any
analytic use of utility functions built only out of the elements necessary and sufficient for
the construction of indifference curves could be interpreted as harmless from the anti-
cardinalist perspective. Furthermore, Hicks and Allen showed that downward sloping
demand does not require that diminishing marginal utility be given a psychological
interpretation. It requires only the behavioural property of diminishing marginal rates of
substitution. Note that if this is treated as an assumption instead of an empirical
observation, it can be justified (to the extent that it is generally justified) by a purely
economic (as opposed to psychological) argument: its violation is consistent with the possibility that an agent could maximize her welfare by consuming only one commodity.

From here the motivation for Samuelson’s project of constructing revealed preference theory is immediate. I argue elsewhere (Ross forthcoming) that this motivation was buttressed by the Keynesian encouragement – encouragement accepted by Hicks and Samuelson, along with Robbins – of treating aggregate demand schedules as basic data. It has been under-appreciated (though see Davis 2003) that in Samuelson the agent disappears altogether – the word occurs nowhere in his *Foundations* – and is replaced for modelling purposes by ‘floating’ preference orderings that can in principle be mapped onto any part of nature where (looking back to Robbins) ‘purposiveness’ might reside. The excess demand literature of the 1970s showed that economists were not in fact ready to let the attachment of agency to individuals slip away – otherwise that literature would not have been regarded as exposing a problem – but I contend that this concern derived from the relationship between demand theory and welfare economics, not from ontological anxieties about ‘what agents are’, or epistemological issues about how to discover their properties.

We have now got ahead of Robbins. But it is necessary to follow the story a few steps past him to appreciate that he stands in the middle-to-late stages of a steady course of development internal to economics, which a broad positivist would gloss as aiming at increased disciplinary objectivity. I now believe I made a mistake in my 2005 treatment of this development. Because positivism among philosophers drifted from a Kantian to an empiricist emphasis between the 1930s and the 1950s, I took this direction to constitute progress by broadly positivist lights. I therefore read Robbins as a confused precursor to Samuelson. But this implicitly treats the historical drift of opinion among philosophers as though it carries persuasive weight of its own, which is not an attitude in conformity to broad positivism. I therefore now propose an amended rationalization of Robbins’s stance on preference relations that is clearer about giving priority to science (that is, that does not privilege Ayer-style empiricism over original positivism for reasons that come from outside of economics).

If Robbins is read as being committed to the idea that the epistemological basis of demand and consumer theory must be introspection, then my earlier verdict would be unavoidable: we would have to say that Robbins let the philosophical tail wag the economic dog. I do not think the text requires this reading, however. Given the economic context, Robbins assumes that relative differences in preference intensities must allude to hypothesized quantities. Thus, in light of the direction in which he sees that progress in economic analysis had been going from Edgeworth through Hicks and Allen – and in light of where he thinks the Austrians had been all along – he must reject cardinal preference rankings as relevant to economics. In my book I wondered why Robbins did not, in light of his belief in ordinal psychological rankings, embrace Mandler’s psychological concavity construct. I suggested that it simply did not occur to him. But this argument presupposed that he thought he had a good reason to hold on to introspective epistemological foundations if possible. Since I took it, and continue to take
it, that there was in fact no such good reason, this yielded a Robbins who was muddled by comparison with Samuelson.

We are only led to think that Robbins was committed to introspective foundations for economics, however, if we fail to draw the distinctions between Fach-Psychologie and ‘psychology’ in Cassirer’s sense that were explained earlier. Robbins encourages us to fail to draw this distinction by inserting the word ‘inner’ into his claim that “we do in fact understand terms such as choice, indifference, preference and the like in terms of inner experience.” Suppose, however, that this word were excised from the remark. Recall that several of Robbins’s other comments on the ‘psychology’ that is presupposed by economics suggest that this is merely the recognition by agents that they are agents – that is, ‘have purposes’ – or, better still in the context of Robbins, have ends. For Kantians and neo-Kantians this recognition, though a priori, is synthetic; although it is a condition for a certain sort of experience, it occurs in experience, not prior to it. A logical positivist would have treated the modelling of economic behaviour in terms of agency as conventional at the formal level, but non-optional once ‘economic behaviour’ was defined in terms of means to the achievement of ends and in terms of scarcity of those means. The Austrians who influenced Robbins were Kantians, not logical positivists. Notice, however, that it makes no difference to anything else Robbins says about economics in the Essay whether we attach a neo-Kantian or a logical positivist interpretation to the ‘psychological elements’ as long as we do not reference these to Fach-Psychologie. Any broadly positivist framework will do; inside that conceptual space, the considerations that drive argumentation in the Essay all come from economics.

I am not here claiming that if we could bring Robbins back from the grave and ask him: “Are you sure you were right to put ‘inner’ in that sentence?” he would recognize a casual error and be moved to retract it. Robbins was not a philosopher. The subtle philosophical distinction between ‘experience’ and ‘inner experience’ is not even clear to many philosophers if their starting point is classic British empiricism. Ayer, for example, obscures the distinction, and it is altogether absent in Hutchison. It is likewise missing in the standard narrative in the methodology literature, due to its under-appreciation of the affinities between the Kantian background of the Austrians and logical positivism. My claims are instead as follows. Robbins interpreted economic theory as presupposing agency and as abjuring reference to hypothetical perceptions of relative psychological preference intensities. That was an accurate interpretation of the theory as he found it. In the Essay he tried to articulate these assumptions in non-technical terms. Here he borrowed repeatedly from the Austrians, who were more philosophically sophisticated than he was. Thus he lost some careful distinctions they might have made. But this matters only to a reader with a philosophical agenda, that is, one who wants to know whether Robbins was an empiricist or an a priorist. He was not really either. Like most scientists who prefer to leave philosophy to philosophers, he took a broadly positivist attitude to his discipline. In the Essay he does not justify his discipline on the basis of philosophical assumptions; he instead describes what is distinctive about it, including what distinguishes it from psychology.
6. Conclusion

I opened by saying that Robbins’s Essay remains the gold standard for descriptions of the ‘mainstream economic attitude’. In light of what I have subsequently argued, the reader will infer that this reflects endorsement of broad positivism. The current wave of ‘behavioural economics’ challenges the form of separation between economics and psychology that I have said constitutes the basis for economic ‘objectivity’ in a broadly positivist framework. How then can my attitude toward the critical behaviourist economists be consistent? Must I not, in saying that Robbins is still the gold standard for identifying the basis of economic objectivity, be implicitly criticizing a recent body of economics on philosophical grounds? Yet isn’t that exactly what the broadly positivist attitude forbids?

I intend no philosophical criticism of the scientific value of the products of behavioural economics. Of course this does not involve my swearing off all economic criticism of these products, but scientific criticism must always proceed one specific product at a time. And in any case, some leading empirical findings of behavioural economists are well established: framing effects are ubiquitous in human choice, people are dreadful statistical reasoners, and if they don’t establish and rely on institutional safeguards they tend to meliorate instead of maximize, thus reversing their preferences and making themselves vulnerable to money pumps and other forms of manipulation by more sophisticated agents. What I join Robbins in rejecting is the often-heard claim that these psychological discoveries must prompt “re-writing of economics from the foundations up”. That sort of claim does not rest on any discovery in economics; it instead asserts some implicit (or occasionally explicit) philosophy. The philosophy it asserts is a very simple-minded realism to the effect that a science must take found objects of everyday experience – in this case, individual human organisms – as its direct subject matter.

Scientific disciplines study distinct structural networks of functional and causal relationships. Economics studies relationships between scarce resources and achievement of ends towards which agents put such resources. People implement agency, though no whole person is ever just one agent. Other kinds of entity, including firms, households and groups of neurons, also implement agency and face scarcity; so economic generalizations apply to them too. Human economic behaviour is mediated by human psychology, and if we want to predict the activities of people we must therefore consider psychological factors (plus physical and chemical and evolutionary factors) in addition to economic ones. A precondition for consistently applying these factors in careful conjunction is keeping them distinguished. This is especially important because the extent to which environments induce people to behave more like consistent economic agents is highly variable (Binmore 2007). Economists should read Robbins’s Essay to learn that there is a recurring need to be reminded of this point, which was often overlooked in the 1930s just as it is often overlooked today. I think it would be helpful if what is now called ‘behavioural economics’ were instead called ‘psychology of valuation’; but I know this hope is even more forlorn than Samuelson’s similarly motivated early preference to banish the word ‘utility’.
In saying that Robbins gives an accurate account of the distinctive economic attitude, one of course need not endorse the content of 1930s economics as against the economics of the early twenty-first century. I do not think that interpersonal comparisons of utility make no sense; I think we can infer from the equilibria of bargaining games a significant, but non-psychological, construct that is well described by that label (see Binmore 1998). I thus think that there is such a field as welfare economics. In light of all we have learned over the past seven decades, it would indeed be ludicrous to hold up Robbins’s Essay as the gold standard for (first order) economics. It is the gold standard for how to think about what economics is without going over into metaphysics to do so.
References


Effective Tension in Robbins’s Economic Methodology∗

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Abstract

Lionel Robbins 1932 Essay is one of the most influential methodological works in 20th century economics. This said, the Essay is not philosophically seamless; it exhibits certain tensions that are not easily reconciled within any specific philosophical characterization of scientific knowledge. The paper discusses these issues, but also emphasizes that these tensions did not inhibit the influence of the Essay within economics. In fact, it is argued that these philosophical tensions actually contributed to its influence. Marginalist economics was under attack from a number of different directions and Robbins’s Essay provided an effective response to these critics – a response that would have been much less effective if Robbins had consistently adopted (only) one of the prevailing philosophical conceptions of scientific knowledge. It was a methodology for economics, not for philosophers, and its influence needs to be understood within the historical context of marginalist economics in the 1930s.

An eminent industrial psychologist once genially assured me that “if people only understood industrial psychology there would be no need for Economics”. With considerable interest, I at once enquired his solution of a problem of foreign exchange which had been perplexing me, but to my great mortification no answer was forthcoming. [Robbins, 1932, p. 32, cut from the second edition]

1. Introduction

Lionel Robbins’s 1932 Essay109 is one of the most important methodological works in twentieth century economics. In fact it is one of a small handful of the most influential methodologically-oriented publications – article or book – in the entire history of economics. Robbins defended a number of claims about the nature, significance, and policy relevance of economic science in the Essay, but the most enduring were his basic definition of economics and his argument against the possibility of interpersonal utility comparisons.

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109 The first edition of An Essay on the Nature & Significance of Economic Science was published in 1932, followed by a second edition in 1935 (Robbins 1952). The central thesis and most aspects of the argument were the same for both editions, although there were also some significant changes. Some these changes will be discussed below. Essay will be used throughout to refer to the book in general (that is, to refer to arguments common to both editions).
Robbins’s *Essay* has been the subject of extensive methodological commentary, although less, perhaps, than some other major methodological works, particularly John Stuart Mill (1874) and Milton Friedman (1953). The majority of the critical commentary has focused on the degree of *a priorism* of Robbins’s position and his arguments against interpersonal utility comparisons (see the discussion in surveys of methodology such as Blaug 1992, Caldwell 1994, and Hands 2001).

This paper also offers methodological commentary on Robbins’s *Essay*, but with a new twist. First, I will argue there are certain philosophical tensions in Robbins’s *Essay*. By this, I mean it contains arguments and/or positions that are difficult to reconcile with other positions within the text, and/or with any well-established philosophical positions (either from Robbins’s day or ours). Of course these philosophical tensions – recognized or not – did not adversely affect the reception of Robbins’s methodological ideas within the economics profession. Second – and here is the twist – I will argue that not only did these tensions not prevent Robbins’s position from becoming influential, if these tensions had not existed within the text it would not have been as influential as it was; the tensions are precisely what allowed Robbins’s approach to accommodate, and steer a path through, the complex problem-situation that confronted marginalist economics during the first third of the twentieth century.

2. Robbins’s Methodology

This section will review three important aspects of Robbins’s characterization of economics in the *Essay*: his definition of economics, his critique of interpersonal utility comparisons, and his introspective approach to knowledge about individual economic agents. The first two of these are well-known and thus require little elaboration. The third has received less attention and requires more discussion.

Robbins definition of economics from chapter one of the *Essay* still graces the first chapter of almost every introductory economics textbook.

> Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses. (Robbins, 1932, p. 15)

According to this definition, contrary to the Marshallian definition popular at the time, economics does not study a “kind” of behaviour – such as that involving money or wealth – but rather a particular “aspect” of almost all human behaviour. The ends are taken as given (“as economists we cannot go behind changes in individual valuations” ibid., 115) and they need not be self-interested (they “may be noble or they may be base” ibid., p. 24). What matters is that the means for achieving the end are scarce and thus a *choice* must be made.

> Economics is not concerned at all with any ends *as such*. It is concerned with ends in so far as they affect the disposition of means. It takes the ends as
given scales of relative valuation, and enquires what consequences follow in regard to certain aspects of behaviour. (ibid., p. 29)

The Robbins’s definition basically equates economics with rational choice, and that has been the main point of contention about the definition over the years (i.e. should economics be more broadly defined). Robbins argued that his definition was not meant to change professional practice, but rather simply to capture what most economists had long been doing.

Equally well-established – although certainly more controversial and receiving a lot more attention over the years (in both opposition and support) – is Robbins’s argument against interpersonal utility comparisons in chapter six of the Essay. As he explains:

It is a comparison which necessarily falls outside the scope of any positive science. To state that A’s preference stands above B’s in order of importance is entirely different from stating that A prefers n to m and B prefers n and m in a different order. It involves an element of conventional valuation. Hence it is essentially normative. It has no place in pure science. (ibid., p. 123)

Economics is a positive science while interpersonal utility comparisons are normative judgments of value, and between “the generalizations of positive and normative studies there is a logical gulf fixed which no ingenuity can disguise and no juxtaposition in space or time bridge over (ibid., p. 132). This means, in particular, that the well-known utilitarian argument for increased income equality based on the diminishing marginal utility of income is “in fact entirely unwarranted by any doctrine of scientific economics” (ibid., p. 121) and is thus “entirely illegitimate” (ibid., p. 125).

The third aspect of Robbins’s argument to consider here has attracted less interest than either his definition or his arguments against interpersonal utility comparisons; it is the introspective character of the knowledge about individual agents that Robbins ascribes to the economic scientist. There was some discussion of this issue in the first edition – particularly in chapter four – but it received significantly more attention in the second edition. It is not clear whether Robbins changed his mind on this issue, or whether it was simply a matter of using the second edition to improve what he had said, or tried to say, in the earlier text. In both editions Robbins explains that the knowledge we have about the preferences of individual agents does not come from objective scientific observation based on the type of controlled experiments available in the natural sciences (ibid., p. 74). But if not on experimental evidence, then what is such knowledge based on?

In the first edition – here sounding more Misean than in the second edition – Robbins makes the case that preference orders (scales of relative valuations) are simply a necessary consequent of scarcity-constrained economic choice: “the elucidation of the implications of the necessity of choice in various assumed circumstances” (ibid., p. 83).

[A]ll that is assumed in the idea of scales of valuation is that different goods have different uses and that these different uses have different significances
for action, such that in a given situation one use will be preferred before another (ibid., p. 86)

Although this argument was retained in the second edition, Robbins added a number of additional pages in chapter four that put more emphasis on the fact that agents have preferences that consistently order various outcomes and also stressed the introspective nature of our knowledge of these preferences.

The ability to order various possible choices is presented as fundamental to economic choice in the second edition: the “foundation of the theory of value is the assumption that the different things that the individual wants to do have a definite importance to him, and can be arranged therefore in a certain order” (Robbins, 1952, p. 75).\(^{110}\)

The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates … The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. (ibid., pp. 78-79)

Not only do individuals arrange their preferences in an order, it is a consistent (what we would now call transitive) order.

The celebrated generalization that in a state of equilibrium the relative significance of divisible commodities is equal to their price, does involve the assumption that each final choice is consistent with every other, in the sense that if I prefer A to B and B to C, I also prefer A to C … (ibid., pp. 91-92, emphasis added)

Robbins is also more clear in the second edition about both where this knowledge does not come from – “We do not need controlled experiments to establish their validity” (ibid., p. 79) – and where it does come from; it comes from introspective “inner experience” (ibid., p. 88). Even though the key postulates of economics do not rest on any particular psychological theory, it is clear that

… they do most unquestionably involve elements which are of a psychological – or perhaps better said a psychical – nature … the subjective or psychological theory of value; and, as we have seen, it is clear that the foundation of this theory is a psychical fact, the valuations of the individual. (ibid., pp. 86-87)

The psychological fact is that individuals have ordered and consistent (transitive) preferences and make valuations on the basis of those preferences.

Of course Robbins is not suggesting that all knowledge relevant to economics comes from introspection. There are also many things that we know based on empirical

\(^{110}\) This, and the next nine quotes are from the material added to the second edition and are not contained in the first edition.
observations of the standard “objective” or “interpersonal” sort – what today we would simply call “observations” – including the fact of scarcity, trade, competition, monopoly, and such (in addition to the obvious rocks and trees). Introspection was certainly not the source of all knowledge for Robbins, but it was the source of our knowledge that individuals have preferences and can arrange the desirability of various outcomes in a consistent order.

Robbins makes it entirely clear in the pages added to the second edition that this is not – and in fact is completely at odds with – the kind of human science a behaviourist would authorize.\footnote{Behaviourism is mentioned in passing in the first edition but is given much more attention in the second edition, particularly in the material added in chapter four (much of what was added originally appeared in Robbins 1934). In the preface to the second edition Robbins explains that certain readers of the first edition “have accused me of ‘behaviourism’” and his desire to set the record straight is undoubtedly one of the reasons for his explicit critical discussion of behaviourism in the second edition. But, as I will argue below, there is also a connection with his views on the definition of economics and interpersonal utility comparisons.} Behaviourism restricts human science to only that which is objective and interpersonally observable, and the valuations that undergird the postulates of economics are subjective and non-observable. For the behaviourist:

Valuation is a subjective process. We cannot observe valuation. It is therefore out of place in a scientific explanation. (ibid., p. 87)

Those who would exclude such subjective valuations reflect “an attitude which is very frequent among those economists who have come under the influence of Behaviourist psychology or who are terrified of attack from exponents of this queer cult” (ibid., p. 87). Economic actions are forward-looking and based on expectations of future events, and “It is obvious that what people expect to happen in the future is not susceptible of observation by purely behaviourist methods” (ibid., p. 88). Behaviourists would reconstruct social science entirely in the image of natural science and such a social science is unable to capture the purposefulness, the valuations, or the choice that is essential to understanding economic behaviour. Economics is a science for Robbins, but it is not a science exactly like, or strictly following the method of, the natural sciences.

… the procedure of the social sciences which deal with conduct, which is in some sense purposive, can never be completely assimilated to the procedures of the physical sciences. It is really not possible to understand the concepts of choice, of the relationship of means and ends, the central concepts of our science, in terms of observation of external data. The conception of purposive conduct in this sense does not necessarily involve any ultimate indeterminism. But it does involve links in the chain of causal explanation which are psychical, not physical, and which are, for that reason, not necessarily susceptible of observation by behaviourist methods. (ibid., pp. 89-90)
Although Robbins’s explicit anti-behaviourism comes out more clearly in the second than the first edition of the *Essay*, it is a position that he consistently endorsed in later work. As Robbins explains in 1953:

> Pure behaviourism has not proved a particularly helpful method in psychology proper. Why, at this time of day, we should go out of our way to shackle ourselves with its self-frustrating inhibitions is not at all relevant … I do not think that it is sensible to restrict our generalizations to observables and I see no objection to explanation in terms of assumed calculations and estimates. (Robbins, 1953, p. 102)

And again a few years before his death in 1984:

> Influenced presumably by behaviourism in psychology, there are those who urge that in economics we must exclude any hypothesis which relies on conceptions which are not *directly observable* … I confess that I fail to see the necessity, or indeed the desirability of the self-denying ordinance. (Robbins, 1981, p. 2)

It is important to note that even though Robbins considered it necessary to “invoke elements of a subjective or psychological nature” (1952, p. 88), this in no way commits economic theory to the doctrine of “psychological hedonism” (1932, pp. 83-86). In both editions – and in essentially identical language¹¹² – Robbins argues that economics does not in any way depend on the calculus of pleasure and pain of psychological hedonism. It is true that certain early marginalists were sympathetic to hedonism and it figured prominently in their economic theories, but these “hedonistic trimmings” were “incidental to the main structure” of the theory which “is capable of being set out and defended in absolutely non-hedonistic terms (ibid., p. 86). For Robbins, economic agents have consistent preferences, but these preferences need not be related in any direct way to any subjective feelings of pleasure or pain the agent might experience from the possession or consumption of the relevant goods.

Robbins vision of economic theory clearly excludes both *psychological hedonism* and *behaviourism*. The first is a discredited mental state-based theory of human valuation liked to utilitarian ethics, while the second is an overly scientistic attempt to force economics into the purely observational straightjacket of the natural sciences. Both are inappropriate for an economic science concerned exclusively with “securing of given

¹¹² It is not relevant to the argument in this paper, but interesting nonetheless, that many of the changes that Robbins made to the second edition seemed to have nothing to do with either modifying his position or making it more clear, but were simply a matter of toning down the rhetoric from the first edition. One nice example of this occurs in this section of chapter four where he is criticizing those who attack economics because it rests on out-of-date psychological (i.e. hedonistic) foundations. In the first edition they “are the happy hunting ground of the charlatan and quack” (1932, p. 83) while in the second edition they have “minds averse to the effect of exact thought” (1952, p. 83). Earlier a sentence referring to Marshall’s “spineless platitudes” (1932, p. 65) was dropped entirely from the second edition, and later something that filled him with “unutterable fury” (1932, p. 126) in the first edition, only left him with “indignation” (1952, p. 142) in the second edition. There are many other such examples.
ends with least means” (1932, p. 129 and 1952, p. 145). As I will argue in more detail in
the next section, these twin exclusions put Robbins on a bit of a tightrope in the Essay. On
one side is psychological hedonism which was not only the psychological foundation
of early (at least British) marginalism, it was also linked to classical economics through
Mill and others as well as to introspection (which Robbins supported). On the other side,
behaviourism was in vogue in psychological circles when Robbins was writing and was
increasingly promoted as the only truly scientific approach to human behaviour. Robbins
wanted to improve the scientific credibility of the discipline during the heyday of
behaviourism, but to do so without sacrificing the core tenets of marginalism and
while endorsing introspectionism. A tightrope indeed.

I will close this section with a nice example of this delicate balance Robbins maintains
between hedonism and behaviourism from chapter four of the Essay where he is
criticizing psychological hedonism and separating it from marginalist economics. In the
first edition he says:

All that we need to assume is the obvious fact that different possibilities offer
different stimuli to behaviour, and that these stimuli can be arranged in order
of their intensity. (1932, p. 86).

Although this sentence does make the point against hedonism, the use of the term
“stimuli,” and particularly “stimuli to behaviour,” could easily be given a behaviourist
interpretation. In the second edition this sentence becomes:

All that we need to assume as economists is the obvious fact that different
possibilities offer different incentives, and that these incentives can be
arranged in order of their intensity. (1952, p. 86)

This is a statement less likely to bring comfort to either a hedonist or a behaviourist.

3. Tensions in Robbins’s Essay

In this section I would like to discuss two of the tensions – broadly philosophical tensions
– in the Essay. The theme of both of these tensions was suggested by the last few
paragraphs of the previous section. The first concerns introspection and the second
concerns interpersonal utility comparisons. It is perhaps useful to talk as if they were two
separate issues, but in fact they are simply two facets of the balancing act discussed in the
previous section.

Whether Robbins was merely reporting what most economists already believed, or trying
to change the character of the discipline, is really irrelevant to the tensions discussed in
this section. The fact is, whether he was reporting or redefining, Robbins’s Essay had
three goals – to define economic science as the study of scarcity-constrained rational
choice, to put economics on what was perceived to be a firmer epistemological
foundation (which entailed moving away from psychological hedonism), and to
persuasively make the argument against interpersonal utility comparisons – and he
delivered effectively on all three: the first and third directly, and the second with the help of his LSE colleagues Hicks and Allen (1934) and the various other key figures in the ordinalist revolution (Hicks 1982). Achieving these three goals would provide marginalist economics with exclusive rights to the title of scientific economics; the profession would still need historical data and other empirical evidence provided by the institutionalist or historical schools (Robbins mentions “a Schmoller, a Veblen, or a Hamilton,” 1932, p. 105), but it leaves the pure science of economics to marginalism alone. It would also provide a response to the vast array of critics who based their criticism of marginalism on its hedonistic foundations: some advocating a more up-to-date psychological theory (Robbins mentions Cassel and Pareto, pp. 87-88, 1952) and some advocating direct statistical-empirical estimation of market demand functions thus skipping individual choice and/or psychology altogether (Robbins discusses Mitchell in this regard, 112-114, 1952). And finally of course, the argument against interpersonal utility comparisons would undercut the scientific justification of a wide range of utilitarian-inspired income redistributions schemes (from moderate Marshallian to Fabian socialist). Clear goals that were effectively accomplished, so where is the tension?

Robbins introspectionism appears to be decidedly at odds with the goal of putting economic theory on a more solid scientific foundation. In Mill’s day, empiricists generally accepted introspection – inner observation – as a legitimate form of observational experience, but by the time Robbins was writing this had changed. By the first third of the twentieth century most philosophers and scientists restricted the experiential basis of empirical science exclusively to “objective” – that is, interpersonally observable – evidence. There were many forces contributing to this change but the decline of strict Descartian dualism, the rise of experimental psychology, the ascension of early positivist ideas in the philosophy of science, and the rise of behaviourism in psychology, were all clearly important (and interrelated) factors. By the time Robbins was writing the Essay the intellectual tide in both psychology and epistemology had clearly turned in favour of intersubjective observability and behaviourism, and away from introspection. As B. F. Skinner summed up the attitude many years later: “A completely independent science of subjective experience would have no more bearing on a science of behaviour than a science of what people feel about fire would have on the science of combustion” (Skinner, 1974, p. 243).

Given that Robbins was concerned about the epistemic status of marginalist economics – particularly the criticisms by institutionalists and others that economics had not kept up with the times in psychology and empiricist epistemology – and wanted to ground it on more acceptable foundations, it seems rather strange that Robbins would retain introspection as the basis for his core presuppositions about consumer preference and choice. In the 1930s, the heady days of positivism and behaviourism, why not turn to behaviourism for the philosophical support of marginalism (as the young Paul Samuelson attempted to do a few years later in 1938)? It was clear that psychological hedonism had to go, but why not go all the way and also reject the introspectionism of Mill and the early marginalists?
One way to try to answer this question would be to examine the various intellectual influences and personal contacts during Robbins’s education and in the years immediately preceding publication of the Essay. Some excellent research already exists in this regard (Howson 2004, O’Brien 1990 and others) and more archival-based studies are undoubtedly underway, but my approach is more exegetical and contextual than archival. In this section I will argue that while resisting positivist behaviourism may have been at odds with one of Robbins’s three goals – making the case for more epistemically acceptable foundations for marginalism – it was in fact quite consistent with the other two: the scarcity constrained choice definition of economics and the argument against interpersonal utility comparisons. Introspection served these two purposes, but behaviourism would have been a serious handicap. As the good marginalist he was, Robbins was willing to make tradeoffs among the Essay’s three goals on the basis of the various constraints he faced. In the next section I will examine how Robbins’s choices were effective solutions to the problem situation facing marginalist economics more generally during the 1920s and 1930s (and not that of psychology or positivist philosophy). The bottom line will be that Robbins needed introspection and the profession needed Robbins’s solution.

First consider Robbins’s definition of economics as scarcity constrained choice. Notice it is choice not conditioned response or mechanically determined action. As the various Robbins quotes in the previous section make clear, economics was exclusively about choice and such volitional action is precisely what behaviourism rules out. Behaviourism not only black-boxed what went on inside the mind at the moment an action was undertaken, it established law-like empirical regularities connecting the antecedent to behaviour (stimulus) and the behavioural consequent (response). The entire epistemec foundation of behaviourism is based on finding such constant conjunctions of interpersonally observable facts: stimulus (x) precedes, and has a law-like connection to, behaviour (y), making y=f(x) the only form of scientifically legitimate behavioural law. As John Watson explains in his classic statement of behaviourism:

"The behaviourist asks: Why don’t we make what we can observe the real field of psychology? Let us limit ourselves to things that can be observed, and formulate laws concerning only those things. Now what can we observe? Well, we can observe behaviour – what the organism does or says … The rule, or measuring rod, which the behaviourist puts in front of him always is: Can I describe this bit of behaviour I see in terms of “stimulus and response”? (Watson, 1924, p. 6)"

Now of course the contemporary psychologist or philosopher of mind may protest and say that this is a very early, strict, and simplistic characterization of behaviourism and that over time the program became much more sophisticated and encompassed a much wider methodological stance. True enough, but the point is irrelevant here. The behaviourism that Robbins faced in the 1930s – and the behaviourism that was continually being thrown up to marginalist economists as the cutting edge of scientific psychology by institutionalists and others – was precisely this strict early behaviourism, and that version of behaviourism was not a methodological position that could underwrite
economics as the science of rational choice. Remember for Robbins “no purposive action” meant “no economic phenomena” (Robbins, 1952, p. 93) and driving occult concepts such as purpose and teleology out of psychology was precisely what behaviourism was all about. Moving sharply in the behaviourist direction during the 1930s would have facilitated the effort to demonstrate that marginalism had effectively shaken off its nineteenth century hedonistic past and was now truly scientific, but it would also have meant that economics could not be defined as the science of scarcity-constrained rational choice.

Notice also that retaining a certain element of introspective knowledge and resisting the behaviourist-positivist strictures of natural science, Robbins could not only underwrite his scarcity-constrained choice-based definition of economics, it even allowed him to make the case that economics was on more solid foundations than natural science. Natural science had only “objective” observation; economics had both observation and introspection.

In Economics, as we have seen, the ultimate constituents of our fundamental generalisations are known to us by immediate acquaintance. In the natural sciences they are known only inferentially. There is much less reason to doubt the counterpart in reality of the assumption of individual preferences than that of the assumption of the electron. (ibid., p. 105)

Although from the perspective of philosophy of science (then or now) Robbins use of introspection leaves his characterization of scientific economics open to criticism, from Robbins’s perspective it provided an appropriate foundation for his choice-based conception of economics and gave the discipline a potentially more solid foundation than mere observation-bound natural science.

Similar remarks can be made about Robbins’s argument against interpersonal utility comparisons. The first thing to note is that Robbins used introspection to establish the argument against interpersonal utility comparisons. As argued above, Robbins recognized two sources of knowledge: knowledge based on empirical observation (of the now standard “objective” sort) and the introspective knowledge that we have subjective preferences that can arrange goods in a consistent order. Since we cannot see inside the minds of others, we do not have objective knowledge of their individual subjective preferences (and of course behaviourism doesn’t help since it “explains” behaviour without knowledge of, or even reference to, such preferences/goals/desires). But one also does not have introspective knowledge of the minds of others; inner observation will tell us that we order goods but not that others do so. Since interpersonal utility comparisons are neither empirically nor introspectively observable, they are not scientifically legitimate. Of course we continually make such interpersonal comparisons in everyday life – which Robbins fully admits – but like so many of our folk practices such inferences have no scientific standing.

*Introspection* does not enable A to discover what is going on in B’s mind, nor B to discover what is going on in A’s. There is no way of comparing the
satisfactions of different people. (Robbins, 1932, p. 124 and 1952, p. 140, emphasis added)

And as he explains in later work, the problem is that interpersonal comparisons involve neither (objective) observation nor introspection.

The assumptions of the propositions which did not involve interpersonal comparisons of utility were assumptions which had been verified by *observation or introspection*, or, at least, were capable of such verification. The assumptions involving interpersonal comparison were certainly not of this order. (Robbins, 1938, p. 637, emphasis added)

I still cannot believe that it is helpful to speak as if interpersonal comparisons of utility rest upon scientific foundations – that is, upon *observation or introspection*. (ibid., p. 640, emphasis added)

To see how important introspection is to this position, suppose for the sake of argument that one takes the position that the only knowledge we have of human action/behaviour is that which is based on (non-introspective) empirical observation. In this case what we would know about any human behaviour would have the same source, objective observation, whether it was the behaviour of ourselves or others. Thus if our objective observations could somehow tell us about the preferences of others (the promise some neuroeconomists see for MRI and other neural imaging) then we could learn about our own preferences in the same way, and vice versa; if we could infer our own preferences from various empirical observations of our behaviour then we could apply the same technique to others. In any case, the knowledge we have of others would be the same as our self-knowledge. Thus we would have either: 1) no knowledge of our own preferences or, 2) we would have the ability to make interpersonal utility comparisons. Introspection as the source of self-knowledge provides Robbins a way around this problem. Since introspection is the source of our self-knowledge it is qualitatively different than the objective knowledge we have of others.\(^{113}\)

Thus we find there are certain tensions in Robbins’s tripartite project of defining economics as choice under scarcity, improving the scientific foundations of choice theory, and banning interpersonal utility comparisons from scientific economics. The introspectionism that works so effectively in preserving volitional choice and against interpersonal utility comparisons, seems, in the context of the 1930s, to undermine his effort to provide economics with more philosophically and psychologically acceptable foundations. A “purer” position, philosophically and psychologically, such as behaviourism, would facilitate one aspect of Robbins’s project while undermining others. Robbins’s project enjoyed a substantial measure of success, but it did so without the relative seamlessness associated with certain earlier approaches such as Mill’s.

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\(^{113}\) As some have pointed out over the years (Little 1949, Walsh 1996) this still leaves Robbins with the question of how we know other minds exist at all, but that issue is beyond the scope of the current discussion.
4. The Methodological Problem-Situation of Marginalism of the 1930s

The previous section focused on Lionel Robbins the individual: what he was trying to accomplish in the *Essay* and some of the tensions that developed among the various parts of his project. This section will focus on the economics profession. Tensions or not, Robbins three core arguments in the *Essay* came to be generally accepted within the economics profession during the decades following its publication. There were criticisms of course – as mentioned above, most centering around his criticism of interpersonal utility comparisons and the question of the *a priorism* of his position (e.g. Hutchison 1938, Souter 1933) – but his definition certainly became standard and there was general acceptance that the ordinal revolution (with contributions from many others) had helped solve the foundational and epistemological problems associated with the earlier psychological hedonism.114 This section will argue that if one understands the problem situation facing marginalism during the 1930s – the goals and constraints the profession faced – the broad-based endorsement of Robbins position becomes quite understandable. Although tensions existed that might have concerned members of other scholarly communities (e.g. philosophy or psychology), the constellation of arguments that Robbins provided appeared to offer a reasonable solution to the various problems facing marginalism at the time. Since Robbins’s stance on interpersonal utility comparisons was contested at least until the second half of the twentieth century – and again recently (e.g. Kahneman and Krueger 2006) – this section will focus on the other two main aspects of Robbins’s position: the definition of economics and the effort to find more adequate scientific foundations for rational choice theory.

As discussed above, marginalism was under attack from a variety of different directions during the 1920s and 1930s: from Marxists and historicists of various stripes in Europe and from institutionalists in North America. And one of the consistent critical claims was that marginalist economics was based on psychological hedonism, and that such hedonism was an outmoded psychological theory that was either empirically inadequate (false), or simply untestable, but in either case, certainly not the kind of foundation that would should use to build the house of scientific economics. Of course critics offered a wide range of alternative approaches to economic phenomena – Marxism, various institutionalisms, purely empirical/statistical approaches, and a host of others – but none of these were consistent with the core impulses of marginalism. Marginalism thus faced a *scientific credibility problem*. The problem was to develop an economic theory that was

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114 One of course needs to be careful talking about the “economics profession” as if “the profession” were homogeneous. There were certainly Marxists, institutionalists, and heterodox economists of a variety of other stripes that never accepted any of Robbins’s three main propositions. One is tempted to use the term “mainstream economics” in this context, but in the 1930s and 1940s many of those supporting Robbins position were opposed to Keynesian macroeconomics (a version of which did eventually become quite mainstream). So too, but on the other side of the political fence, many Austrian economists – while broadly sympathetic to much of what Robbins had to say – were critical of specific aspects of his approach. Finally, if one is using the term “mainstream” in any meaningful way regarding economics during the first part of the twentieth century, then one would certainly need to count Marshallians as part of the mainstream, and yet Marshallians generally did not accept Robbins’s arguments against interpersonal utility comparisons. In light of all of this it is probably best to think of the “economics profession” as those economists generally endorsing some version of marginalist/neoclassical economics, but remember that even here there were critics of Robbins’s position (particularly with respect to interpersonal utility comparisons).
consistent with the core marginalist commitments, but to do so without accepting the no-longer-scientifically-acceptable hedonistic characterization of the individual economic agent. This has been called the “empiricist motive” for the “escape from psychology” (Giocoli, 2003, p. 43) – and in a sense that is exactly what it was – but one needs to be careful about both the terms “empiricist” and “psychology.” The scientific credibility problem centered around psychological hedonism – that was the brand of “psychology” that the profession needed to escape from – because hedonistic psychology was based on the subjective mental-state-based feelings of individual agents and thus was not “empirical” in the way that “empirical” had come to be used by philosophers and scientists by the 1930s. Finding a way to characterize marginalism that was more epistemically acceptable – thus rejecting psychological hedonism – was thus the core of the scientific credibility problem.

But the scientific credibility problem was not the only problem marginalism faced at the time. If it were, then some version of behaviourism would have been the easy solution (again recall that was Paul Samuelson’s goal in 1938). There were many other issues and constraints, but the one that seems most relevant to Robbins’s Essay was the need to retain the notion that economic action was about choice (constrained, but still voluntary, choice). For Robbins the lesson of Robinson Crusoe is that the economic aspect of human behaviour – economizing – is necessarily choosing behaviour:

... he has to choose. He has to economise. Whether he chooses with deliberation or not, his behaviour has the form of choice. The disposition of his time and his resources has a relationship to his system of wants. It has an economic aspect. (Robbins, 1932, p. 12, emphasis added)

As argued above, the laws of stimulus and response at the center of behaviourism left no room for a scientific theory of voluntary choice.115 The relationship between stimulus and response take the form of universal scientific laws; given x, y could not have been otherwise. On the other hand, if the behaviour is the result of a choice, it could always have been otherwise. Choice is at the heart of what distinguishes market economies from other economic institutions and it is also what distinguishes (rational) purposive human behaviour from the (biologically) purposive behaviour of other living creatures, and as such, free volitional choice (not conditioned response) was something that most economists found essential to economic science. This is the reason that economics was a moral science for John Stuart Mill, and although the term “moral” was not longer used in the 1930s, the idea of individual choice (thus excluding exclusively biological, neurochemical, or behaviourist theories of human behaviour) continued to be essential to the science of economizing. In contrast to the scientific credibility problem, this was the choice problem.

While there were clearly other issues facing marginalism during the 1930s – the Great Depression for example – it seems to be quite clear that both the scientific credibility

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115 Of course behaviourism is not alone in this respect. The mechanical forces of late nineteenth century psychophysiology have the same difficulty as do (more recent) positions within the philosophy of mind such as eliminative materialism (see Hands 2007).
problem and the choice problem were serious issues. If marginalism was to move forward and take its place as the cornerstone of scientific economics it would be necessary to provide an effective response to these two problems: place marginalism on firmer foundations by eliminating its ties to hedonistic psychology and do so without sacrificing the key role of the freely choosing economic agent. Despite the various tensions discussed above, providing what seemed to be an acceptable solution to these two problems is exactly what Robbins did. The Essay excluded both psychological hedonism and behaviourism from economics; excluding hedonism addressed the scientific credibility problem while excluding behaviourism addressed the choice problem. He was not alone of course – the ordinal revolution of Pareto, Slutsky, Hicks & Allen and others certainly played a role – but the Essay clearly made a fundamental contribution.

Notice that this contribution did not just come about in spite of the tensions discussed in the previous section, but rather because of them. A position with fewer such philosophical tensions – strict behaviourism on one hand, or a return to Mill’s methodological position on the other – would have run aground on either the scientific credibility problem or the choice problem. Behaviourism would have provided an effective solution to the former, and Mill’s methodology to the latter, but Robbins’s Essay seemed to offer an effective solution to both. Thus it not only becomes clear why Robbins needed all of the various pieces that he assembled in the Essay, but also why the finished product was so well received.

5. Conclusion

I have argued that there were fundamental tensions in Robbins’s Essay and that the tensions are essential rather than accidental. The tension is exactly what allowed Robbins to defend the Essay’s three main theses – his definition of economics, replacing psychological hedonism with a more acceptable foundation for choice theory, and making the case against interpersonal utility comparisons – but it was also precisely what marginalist economics needed in response to the broader scientific credibility problem and the choice problem of the 1930s. Effective solutions to problems often involve trade-offs and substitution at the margin – in methodology as in economic life.

I would like to close this discussion of Robbins’s Essay and the problem situation of economics in the 1930s with a few more speculative remarks about how the lesson we garnered from Robbins’s work might have something to say about the current situation in economic science. The profession is currently experiencing an explosion of research activity in a number of subfields that did not exist even a decade or so ago – behavioural economics, experimental economics, behavioural finance, neuroeconomics, and others – and in many ways the results of these new fields challenge the “solution” in Robbins’s Essay. Clearly many of the results of experimental and behavioural economics challenge the standard assumptions of rational choice theory – in particular the assumption of the stable, consistent, and given “ends” (i.e. preferences) – and we seem to again be at a time where the demand for greater consideration of recent developments in psychology is increasing. One approach to trying to understand why these changes are taking place and to understand the possible impact on the profession is to consider the arguments directly
on a case by case basis: Is this result valid? Is this experiment well-constructed? Is it relevant to agents in “the wild”? Is there a way to amend rational choice to integrate this particular anomaly? And so forth. This is of course extremely important; we do need to understand the details of the particular models, experiments, and anomalies that are challenging the rational choice received view. But the above discussion of Robbins suggests other approaches as well. In order to understand Robbins’s Essay I tried to demonstrate that it was useful to understand Robbins’s particular problem situation – the various goals and constraints he faced – and that in order to understand the impact of the Essay (despite, or because of, various tensions) it was useful to understand the profession’s problem situation during the 1930s. So to perhaps for the situation today. Historians and methodologists of economics have certainly started to investigate the former – the problem situations of various key theorists and research groups within these newly developing literatures (Bruni and Sugden 2007, Guala 2004, Lee and Mirowski 2008, Sent 2004 and many others) – but perhaps it is time to consider the problem situation of the profession more generally. What are the contemporary analogues of the scientific credibility problem and the choice problem faced by 1930s marginalism? How did they come about? And how does the research in behavioural, experimental, and neuro- economics provide a solution to the contemporary problem situation? As I said, this is speculative, but it is a way that the above discussion of Robbins’s Essay might give us a better understanding of recent developments within the discipline.
References


Hutchison, Terence (1938), The Significance and Basic Postulates of Economic Theory. London: Macmillan.


Disciplining Boundaries: Robbins’s Essay and the 
Borderlands of Economics and Psychology

Harro Maas

Abstract

This paper investigates Lionel Robbins’s use of Max Weber’s criticism of psychophysics in managing the boundaries between economics and psychology in his Essay. Max Weber’s criticism of psychophysics hinged on the notion of goal-oriented action. The logical structure of goal-oriented action (instrumental rationality) made experimental and statistical methods of investigation redundant, turning its study into an analytical, rather than empirical exercise. However, Weber emphasized that goal-oriented action, as exemplified in the economics of the Austrian School, historically rose to prominence with the emergence of capitalist, market-based economies. Robbins wholeheartedly accepted Weber’s criticism of psychophysics, rejecting Weber’s limitation of instrumental rationality to market societies.

There were two important consequences: the subject matter of economics turned into the a-historic study of constrained optimizing behaviour and the preferred methods of study were analytical, rather than experimental or statistical. The tensioned relation of what became the economist’s reliance on “rationality” as explanatory principle of economic situations with empirical methods of research haunts economics to date, and affects the distinction between the prescriptive and descriptive character of the discipline.

JEL Classification: B25, B31, B41

You cannot vivisect a moral agent
Francis Ysidro Edgeworth, New and Old Methods of Ethics, 1877, 20

1. Introduction

In a letter to J.M. Clark of 1 March 1951 Lionel Robbins wrote that “the Nature and Significance was always intended to be a sort of preliminary manifesto designed to forestall the criticism that I did not know where the borderline between the different disciplines really lay” (quoted from Howson 2004). The disciplines Robbins referred to were “history, psychology, and political philosophy” which he studied at LSE under the influence of the left-wing political theorist Harold Laski, and “economic theory” which he followed “as an outsider” in Edwin Cannan’s class.

This paper will focus on how Robbins made use of Max Weber in his Essay in managing the boundaries between economics and psychology. It was not so much psychology as such, I will argue, but one particular version of psychology Robbins was concerned with,
namely that kind of psychology that reduced psychological to physiological states, as exemplified in German psychophysics. In his dismissal of physical reductionism Robbins explicitly referred to Max Weber’s well-known essay on the relevance of psychophysics for economics of 1908, in which Weber had sufficiently “refuted” this view.

Robbins and Weber’s arguments hinged not so much on psychology, as on the structure of human action; that is its intentionality in terms of means and ends. While Robbins considered this structure still a psychological theory of sorts, Weber argued that this particular structure of action had become historically dominant in the modern western world. For Weber, goal-oriented rationality was at no point a psychological theory – it was an ideal-type characteristic of capitalist society.

This paper will first sketch why psychophysics became such a promising approach for two of the founders of marginalism in England, Stanley Jevons and Francis Ysidro Edgeworth. After a discussion of Weber’s criticism of psychophysics, particularly in relation to contemporary experimental studies of psychophysicists into the efficiency of factory work, I will close off with a comparison of Weber and Robbins’s criticism of psychophysics.

My concern is not so much with answering the historical question whether Robbins knew all details of the skirmishes over the merits of psychophysics for economics in Germany. Rather it is my aim to show that his acceptance of Max Weber’s criticism of psychophysics effectively closed borders between two disciplines, where these borders had been so much more fluid at the turn of the century. There was an important consequence: following Max Weber, Robbins’s successful demarcation of economics and psychology also successfully degraded the experimental method as of no importance to economics as a discipline.

2. Robbins’s Verdict over Jevons and Edgeworth

Robbins addressed the “borderlands” between economics and psychology explicitly in Chapter IV, section 4. The chapter as a whole addresses the “nature of economic generalisations”. Section 4 is about economics and psychology and invokes Max Weber on two central occasions. First, to dismiss the relevance of psychophysics to economics; Second, to affirm the distinction between what we now commonly call positive and normative statements in economics. Below I will concentrate on the first issue.

Robbins argued as follows. After delineating the subject of “pure Economics” as being about “relative valuations”, he challenged those who argued that relative valuations were dependent upon “the validity of particular psychological doctrines”. He straightforwardly dismissed such arguments as resting on inexact thought. Unfortunately, these “borderlands of Economics are the happy hunting-ground” of all those who advanced the one or the other psychological theory for providing the foundations for the economic “assumption of relative valuations”. Those susceptible to this were very willing to make the foundations of economics dependent upon current fancies in psychology that shift “every five years or so” and were not willing to confront the subject proper of economics,
that is “the implications of choice in a world of scarcity”. The result was to be “bamboozled into believing” that “fashionable” psychology matters, while economics really had nothing to do with it.

It was the economists’ own fault, unfortunately. Robbins gave instances of economists who illegitimately crossed the border, Jevons and Edgeworth, whose names were “a sufficient reminder of a line of really competent economists who did make pretensions of this sort.” The Austrians (Menger, Böhm-Bawerk) carefully avoided “this kind of misconception”.

What was this misconception precisely? Robbins referred to it in two different ways, as “psychological hedonism” and as “a theory of pleasure and pain”. These are by no means the same, since almost any psychological theory from Locke onwards (if not before) in one or the other way was about pleasures and pains, without necessarily being “hedonistic”. Robbins’s interchangeable use of both labels was understandable enough, however, because Edgeworth persistently referred to his own theory of pleasures and pains as “hedonic psychology”. One may thus guess that it is Edgeworth in particular Robbins had in view.

Robbins quotes from Edgeworth’s Mathematical Psychics (1881) in which Edgeworth compared the human agent to a “pleasure machine”. And he makes note of attempts to “exhibit the law of diminishing marginal utility as a special case of the Weber-Fechner Law”. These attempts were also made by Edgeworth, particularly in his intriguing New and Old Method of Ethics of 1877. Robbins added a footnote to Weber’s famous rejection of attempts to identify marginalism in economics with marginalism in psychophysics. Weber’s article on marginalism and psychophysics (Weber 1975b) was in its turn prompted by the German economist Lujo Brentano’s review article on developments in marginalist theory in which Brentano showed himself enthusiast for attempts to connect marginalism to psychophysics (Brentano 1908).

Robbins’s use of “hedonistic” spills over to the second issue treated, namely the distinction between “objective” and “normative” judgements. In this context, hedonism refers rather to Bentham’s theory of pleasure and pain, and its normative principle of the greatest good for the greatest number, which was considered to entail cardinality of preferences. Robbins used Pareto to argue that economics really was about ordinal preferences only, and this would become the foundation for his well-known distinction between “objective” and “normative” judgements, though his precise argumentation invoked, as he explicitly acknowledged, Max Weber’s argument about this distinction as well.

Very different issues, then, were convoluted in Robbins’s rejection of the relevance of psychology for economics. Without claiming to give an exhaustive summary of all issues touched upon so far, these were: (1) Economics needs stable foundations; these cannot be found in psychology, because “fashionable psychology” changes once in five years; (2) hedonic psychology is wrong anyway, because it confuses value judgements with

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116 Both works are reprinted in (Newman 2003).
scientific, “objective” judgements; (3) a theory of pleasure and pain rests on “hedonic postulates”; (4) it is wrong to consider man as a pleasure machine; (5) diminishing marginal utility has nothing to do with psychophysics; (6) the problem of economics is logically independent of fancy psychological theories.

Given these strong claims, it comes as a surprise that Robbins went on arguing that if we were to do our “job as economists” right, we “must include psychological elements” in our explanations. But the psychology Robbins now thought of was not one of those “fashionable” theories; it was psychology as far as it dealt with “purposive conduct”. In making this argument Robbins crucially referred to Max Weber.

According to Robbins, the concept of purposive conduct had an inherent psychological meaning that could not be fully captured by the methods of the natural sciences. There was a point then where economics did depend on psychology, but not in terms of a naturalistic kind of psychology; that is not in terms of psychological theories that try to explain mental states in terms of physiological states.\footnote{I think it is misleading to draw a distinction here between “specific” and “general” psychological theories, as for example in (Giocoli 2003, 86). There is nothing general about means-ends behaviour vis-à-vis psychophysical theories; the differences really is one of kinds, namely whether one uses bodily states to measure mental states, or whether one thinks bodily states are of no relevance to states of mind. It is this last position that Robbins defends.} Rather, this dependence was to be sought in the structure of the kind of behaviour that economists were interested in. In its most general form this structure could be characterised as “means-ends” behaviour. This structure was sufficient to enter as a causally explanatory term – to enter “in the chain of causal explanation”. This structure was “psychical, not physical”; to see an action as serving a purpose was different from measuring something physically, it was to understand what “choice” really meant.

Understanding the structure of choice was best characterised in terms of “consistency”. Robbins explained this with the almost trivial example of transitivity of choices. He wrote that his psychological theory only involved the “assumption that each final choice is consistent with every other”. Robbins’s rejection of the relevance of specific psychological theories was therefore to embrace another specific psychological theory that hinged on a quasi-logical explanation of means-ends rationality. Let me stop my discussion of Robbins’s text here and let us first see why economists like Stanley Jevons and Francis Ysidro Edgeworth favoured naturalistic versions of psychology in the first place.

3. The “Physical Groundwork” of Economics

My concern in this section is with the idea that man is, just like any other animal, some kind of pleasure machine. Elsewhere, I have dealt extensively with Jevons’s theory of pleasure and pain (Maas 2005a, 2005b). The upshot of my argument was that it was not so much Bentham’s hedonic calculus, but developments in what was referred to as psychophysiology in Britain that underscored Jevons’s theory of pleasure and pain. The
label suggests more precision than there actually was; psychophysiology captured all attempts to reduce states of mind to physiological states.

Let me briefly summarize this history here. In the second half of the nineteenth century, attempts to search for what Jevons called the “physical groundwork” of political economy became increasingly popular once again, after a period in which political economists, largely following John Stuart Mill, maintained strict boundaries between the natural sciences and the science of mind, including political economy, though this did not mean that they denied political economy the lawlike character they so much admired of the natural sciences, rational mechanics in particular. Well-known is Mill’s resolute negative answer in his *Logic* on the “vexed question” whether mental states are reducible to physiological states. This answer enabled him to ascribe causal power to mental motives and to maintain freedom of the will as well. Thus Mill could uphold the lawlike character of political economy - a science of motives - and freedom of the will because an individual could always resist to act upon a motive.

Mill's delicate squaring of the circle in moral philosophy explains the puzzled, if not straightforwardly dismissive, reactions to sparse attempts to ground the principles of political economy in “the afferent trunks of nerve-fibre” as pursued, for example, by an outsider to political economy like Richard Jennings ([1855] 1969). According to the Irish political economist and Mill adept John Elliot Cairnes, such attempts would turn “political economy into a wholly different subject than the world has hitherto known it” - reason enough to think this alien to the business of the political economist (see also White 1994).

In the second half of the century, two developments gave increasing credibility to attempts to ground economics on man's physiology. The first were developments within psychophysiology itself. (Psycho-)physiologists like Thomas Laycock, William Carpenter and Henry Maudsley with increasing confidence questioned the existing boundaries between the phenomena of mind and matter that lay at root of Mill's solution to the scientific character of political economy, though not all of them went as far as Laycock in claiming that all mental states were, after all, just emanations of brain states, turning consciousness into an epiphenomenon.118

The second development was the emergence of thermodynamics that, rightly or wrongly, gave credibility to the idea that mental phenomena were a particular manifestation of energy, just like electricity or mechanical force. These last ideas can be found widely in the nineteenth century, not only in very different intellectual groups in Britain, but all through Europe.119 The idea that the human body was no more than a heat engine was expressed in France, Germany, and Britain alike and physiologists ardently attempted to discover the precise mechanism of how the human body converted food into useful effect (Rabinbach 1992).

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118 On (reactions to) British psychophysiology, see (Daston, 1978, 1982); (Hall, V.M.D., 1979); (Danziger, 1982); (Jacyna, L.S., 1983).

119 It is no coincidence that this same period was characterized by an exuberant interest in mesmerism, again, not only in England. See (Winter 1998); (Gordin 2004).
Even John Stuart Mill considered the idea of a conversion of energy into different forms, including mental states, in an exchange of letters with his close friend, the psychologist Alexander Bain, though Bain (a former adherent of phrenology) was more sanguine about this than Mill. William Carpenter's “correlation of forces” was a particular instance of this idea that aimed to salvage a non-reducible status for mental states, and so would save freedom of the will as well. Thomas Huxley by contrast was much more sympathetic to the “shibboleth of materialism” that “thought is a secretion of the brain” and famously expressed this in his essay in the *Fortnightly Review* in 1874 in which he compared man to a machine.

The unifying concept was the concept of work: Just as heat could be measured by its mechanical equivalent in units of work, so we could measure the mechanical equivalent of the work of the mind. The best examples of political economists in Britain who were clearly inspired by developments in psychophysiology and thermodynamics are William Stanley Jevons and Francis Ysidro Edgeworth, just the two economists Robbins explicitly referred to as mistakenly relying on “fancy psychological theories”. Jevons’s dismissed Mill’s “convenient” approach to political economy as methodologically naïve in that it did not needed any “instruments” or “apparatus” to measure its subject of study (Maas 2005b). For Jevons, by contrast, precision measurement by means of experiments turned mere speculations into matters of fact that could be observed.

To show how mathematical, numerical precision could be given to economics Jevons published an experimental study into the “natural laws of fatigue” in *Nature* (1870) in which he made three different experiments, the one throwing weights, the second, lifting weights with pulley and block, the third, holding a weight on a stretched arm. These experiments threw light on the “physical groundwork of economics”. It is informative that Fechner at the time made similar experiments. There is no indication that Jevons knew of this.

In his *New and Old Methods of Ethics, or “Physical Ethics” and “Methods of Ethics”* (1877) a highly intriguing work that he published on his own account, Edgeworth made a term-for-term translation of the Weber-Fechner “law” into the hedonic calculus. Let me point to one page of this to show how much Edgeworth was influenced by both psychophysics and thermodynamics (see Figure 1).

We see that Edgeworth translated German psychophysics into the “hedonic” language of pleasure and pain. He refers to the experimental work of Wundt, Fechner, and others and translates this into his theory of pleasure and pain. We see that the energetic discourse

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120 Some crucial references are: (Wise, M.N and C. Smith 1989a, 1989b, 1990); (Rabinbach, A. 1992); (Helmholtz, H. 1995); (Brain, R.M. and M.N. Wise 1999); (Debra, C. 2001); (Sibum, H.O. 2005).

121 For a recent extensive account of the relation of Jevons to British thermodynamics, see (White, M.V. 2004); (Mirowski, P. 1989, 1994); (Chaigneau, N. 1997). Chaigneau's analysis of the relation of Edgeworth to Fechner is no doubt the best available to date.


123 Edgeworth also considered other formulae for measuring sensations, like that of Delboeuf.
Edgeworth links with psychophysics gives rise to a discussion of the functional form of the “sentient being’s” “pleasure curve”. He identifies Laplace's notions of “fortune physique” and “fortune morale” - that is the spheres of matter and mind - for measurement purposes. Also, note that Edgeworth, and quite consistently so, speaks of a “sentient being” rather than a “human being”, and how this “sentient being” is some sort of heat engine:

“He is not to be considered as throwing all his fuel at once on one furnace, but as lighting up the same furnace, or others, at different times” [HM - these furnaces are the “different organs of sensation”.

According to Edgeworth, economics searched for explanations on the “axiom” of self-interest, and such explanations pointed to a theory that was based in man's physiology. Because “the necessary cerebral investigations” were impossible (Edgeworth 1877, 20), we needed to take recourse to other, indirect methods of measurement that could provide evidence on the relation of man's physiology to his behaviour. Edgeworth considered the new experimental practices of psychophysics of Helmholtz, Fechner, Wundt, and Delboeuf as indirect methods of investigation that were to be preferred over the “introspective marks of brain activity” that were favoured by John Stuart Mill and others. Approvingly Edgeworth (1877: 6) quoted Alfred Barrett, the author of Physical Ethics (1869), who had argued that bodily states could be used to indicate mental states, just as the thermometer was used to measure temperature.

From these examples, it should be clear that Jevons and Edgeworth looked at the economic agent as an energetic system which optimises pleasures and pains. Anson Rabinbach (1992) aptly refers to this system as “the human motor”; man was some sort of engine converting food into useful effect. The lack of direct methods to investigate this “human motor” made Jevons and Edgeworth search for indirect, experimental methods of research. Edgeworth found such an indirect method precisely in the German discourse of (experimental) psychophysics. There was a clear aim to mathematize, to think about agency in functional form and in terms of optimisation. For both Jevons and Edgeworth to link the principles of political economy to man's biological frame entailed the introduction of new tools of research, that had been considered alien to the field before then: mathematics, diagrammatic expositions, and, eventually, experiments. To think about the relation of mind and matter in terms of “energy” and “correlation” was present in neurophysiological research at least up to the fifties, as is witnessed from the following quote of the 1950s from the neuroscientist Sir Charles Scott Sherrington: “… we find that the energy-system with which we correlate the mind has of course extension and parts.”

Edgeworth did not distinguish between the physiological reductionist experiments of the Helmholtz School, and Fechner who emphasized the functional parallelism of mental and physiological states (and so not their reducibility to physiological states). British marginalists like Jevons and Edgeworth considered pleasure and pain in terms of intensity and duration. Measuring an intensive magnitude would encounter the same problems as the measurement of temperature. The historical and philosophical intricacies of measuring temperature have been recently spelled out in (Chang, H. 2004).
In 1932 (the same year Robbins’s *Essay* appeared) Sherrington received the Nobel Prize for uncovering the exact mechanism of the motor system. It was from Marshall Hall’s early nineteenth century theory of the motor system that speculations about conversions and/or reductions of mental to physiological states all started. I know of no references of Robbins to Sherrington, but it should be clear that Sherrington was building on similar sets of ideas as Jevons and Edgeworth. These were not just “psychological fancies”, but ideas that could get you the Nobel Prize. These ideas were clearly far removed from the means-ends discourse that Robbins felt comfortable with. It is now time to investigate this discourse more closely. Robbins’s reference is to Max Weber, and so we will turn to Max Weber’s assessment of the irrelevance of psychophysics for economics to see where means and ends come in.

4. The German Context: Max Weber against the Psychophysicists

My much truncated examples of the previous section also served to undermine the idea that rationality always has been on the agenda of political economists, as contemporary economists may be inclined to think. The historical record tells a different story. Early Victorian marginalists searched to convey to their audiences the somewhat uncanny message that man, after all, was perhaps nothing more than a heat engine, conversing food into useful effect. John Stuart Mill’s struggles with the theories of his father and Bentham on the one hand, and the free will issue on the other, were also about how to reconcile political economy with the higher moral purposes in life: better to be Socrates dissatisfied, than a fool satisfied. Against this view, Edgeworth dryly repeated Bentham’s remark about pushpin and poetry. Against Mill’s recourse to introspection to salvage political economy as a science and free will as a moral fact, Jevons and Edgeworth took recourse to physiological research in Britain and Germany to gain insight in the functional form of the driving forces of human conduct: pleasure and pain.

Keeping this in mind, let me now turn to Max Weber. For the purpose of this essay I will focus on three interventions that all, as will be seen, gravitate around the importance of the notion of instrumental, or goal-oriented rationality for economics as a science. These interventions are (1) Weber’s critique of the German Historical School; (2) his critique of psycho-physical studies in work and fatigue; (3) his critique of the importance of psychophysics more generally for economics. Weber’s interventions run through the first decade of the twentieth century, and his criticism of psycho-physics can be seen as its end-result.

The immediate background for these interventions is the famous *Methodenstreit* between the Austrians and the German Historical School. The remote background is concerned with Weber's more general ideas on the differences and similarities between the natural and the social sciences. With respect to the last, Weber drew upon the work of his philosophy teachers Heinrich Rickert and Wilhelm Windelbandt, and on Wilhelm Dilthey's emphasis on the role of *understanding* as a hallmark distinction between natural and social explanations. Following Georg Simmel, Weber aimed to show that

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125 There exists, of course, an outstanding scholarly literature on Max Weber. For a very good account of the *Methodenstreit*, and for a more general account of Weber, see (Caldwell, B. 2003, chapters 3 and 4). A recent biography of Weber is (Ringer, F. 2004).
explanations in terms of understanding are consonant with causal explanations as pursued in the natural sciences – this is a theme that is picked up by Robbins in his Essay as we have already briefly seen.

Carl Menger's theory of human economizing behaviour was for Weber as a particular instance where the understanding of economic agency in terms of rationality produced such causal explanations. In this context Weber introduced the notion of instrumental or goal-oriented rationality as identical to Menger's conception of economizing behaviour. On this notion, Weber argued, there was no need to explain man's economic behaviour from his biological or psychological frame. The labour studies of psychophysicists of the school of Wilhelm Wundt (on which more anon) served to galvanize Weber's thoughts on this issue, and he expressed them most explicitly in his famous article on psychophysics marginal utility theory, first published 1908.¹²⁶

From my discussion of Weber it will transpire how he reframed topics that had been of concern to John Stuart Mill in a new setting, replacing Mill's recourse to a categorical split between the realms of matter and mind by a categorical split between understanding natural causal relations and understanding the causal structure of human actions as fundamentally intentional and goal-oriented. For Weber, intentionality and rationality were highly related concepts. On the macro-level rationality served to distance his own “interpretative” approach in the social sciences from the irrationalism of the Historical School; on the micro-level rationality served as the foil against which to understand individual human conduct. Thus, Weber could deny physiological research relevance to the concerns of economists, and he could substitute goal-oriented rationality as its simple and effective alternative. In economics, no recourse needed to be made to either the physiology of mankind, or to any psychological theory; instrumental rationality was the panacea to all.

5. Weber's Criticism of the Historical School

Histories of economics commonly reckon Weber to the German Historical School. But it is well known that Max Weber fits in uneasily. In the Methodenstreit Weber sided with the Austrians. Weber succeeded Knies in 1896 as professor of economics at the University of Heidelberg. His Roscher und Knies und die logischen Probleme der historischen Nationalökonomie¹²⁷ emerged from a planned Festschrift (in 1902) to honour the university. The end result was a collection of essays that contained a crushing criticism of the approach of the German Historical School to economics.

In its most general terms, Weber denied the German Historical School coherency. On the one hand, and following on ongoing debates in Germany at the time over the distinction between the natural sciences and history, Roscher and Knies claimed for political economy a separate route to truth, distinct from that of the so-called nomological or nomothetical sciences - like mechanics. Economics focused on the full complexity of

¹²⁶ Reference will be made to the English translation. See (Weber, M. 1975b).
historical events, and because of this focus was unable to present its results in the form of general causal laws. On the other hand, Roscher and Knies took general assumptions about human nature as their point of departure. These general assumptions were considered universally true, and only expressed themselves differently under different institutional arrangements.

Weber's most explicit concerns were directed against the irrationalist assumptions and overtones of the approach of the Historical School. Weber contrasted this approach with that of Dilthey and others, who searched for “meaning” in history; that is, who aimed to “understand” the actions of individual agents as “intelligible”. The approach of the Historical School was clearly in opposite direction. Roscher and Knies “hypostatized the concept of an essentially irrational and unique 'Volksgeist'”, and they even made this “Volksgeist” the “individual” that transformed through history (Weber 1975a).

Such a monolithic approach to history fitted within the conservative and nationalistic agenda that had come to dominate in Germany after the unification of the German Reich. This agenda was one of the major concerns of social scientists in a liberalist tradition, many of whom made part of the Verein für Sozialpolitik of which Max Weber was an important member. The Verein tried to distance itself from radical socialist ideas on the one hand, and political conservatism on the other, and attempted to redress the incivility, authoritarianism, and intolerance they increasingly found after 1871 with adherents of both sets of ideas, in favour of a more liberalist politics, without becoming involved in party politics (Sheehan 1966; Hagemann and Rösch 2004; Goldman 2005). The difficulty of such a balancing act was in fact contained in the very name of the Verein.

In line with this liberalist agenda Weber argued against the Historical School that it overlooked the “fundamental and substantive problem of economics” that was posed “before and after Roscher”: “How are the origins and persistence of the institutions of economic life to be explained, institutions which were not purposefully created by collective means, but which nevertheless - from our point of view - function purposefully?” (Weber 1975a, 80). This was the problem posed by Mandeville, in his notorious Fable of the Bees, and “many of his successors, consciously or unconsciously agreed with [his] view: economic self-interest is that power which ... 'always wills evil but does good'“ (Weber 1975a, 83).

Weber showed particular concern with the weak empirical and theoretical underpinnings of Roscher's strong psychological claims. Although Roscher generally disagreed 'with Mandeville and the Enlightenment”, he followed Mandeville in considering self-interest as an “instinct” on which man invariably acted. Weber argued that both historical development and the actions of the individual were thus conceived as fundamentally irrational, because instinctive. According to Weber, Roscher's simplistic psychology bypassed the very complexity of different and counteracting motives that determine human conduct.

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128 We hear Hayek's assessment of Mandeville in these words. See (Hayek, F.A. 1966), reprinted in (Hayek, F.A. 1978).
The emphasis on the irrationality of individual human agency was even heightened in the work of Knies. According to Knies, the fact of free will itself implied that individual actions could only be considered as irrational. Knies, so Weber argued, conceived of “irrationality” as synonymous with “unpredictability” or “incalculability”, and rationality apparently meant the opposite (Weber 1975a, 97, 120). In his criticism of Knies's overly simplistic argument, Weber introduced his main argument for thinking about human behaviour as categorically different from natural processes. The difference was in the intentional character of human agency. That is to say, in its inherent possibility to be interpreted rationally.

Apart from the fact that properties of unpredictability and incalculability were no different for many natural processes, and so human action was no more, no less “irrational” than natural, law-like processes, Knies (and Roscher) ignored the distinguishing feature of human actions, namely that we “can attempt to understand it”; human actions were susceptible to “a meaningful interpretation”. For that reason, Weber considered “individual human conduct ... intrinsically less irrational” than “the individual natural event” (Weber 1975a, 125). According to Weber this argument was best formulated by the German philosopher and sociologist Georg Simmel and in his critique of Knies, Weber included an extensive discussion of Simmel's ideas.

Hence, the possibility of understanding human actions rationally was the distinguishing feature between events in the natural and in the social realm. In the realm of nature we can search for causal explanations, but we cannot ask for a reason. To ask for a reason was to ask for the intentions of the individual agent. To understand individual human conduct was to ascribe “a rational interpretation in terms of intentions and beliefs” to it (Weber 1975a, 127). This did not exclude causal explanations. Rather, as Simmel had argued, to ascribe rationality to an action was to causally explain this action from a reason.¹²⁹ In his criticism of the Historical School Weber exemplified this with one important type of rational ascription that was of particular use in economics, namely the “rational interpretation which employs the categories of 'ends' and 'means'” (Weber 1975a, 186). From the sequel, it was clear that Weber had Carl Menger's theory of economizing behaviour in view (On the relation between Weber and Menger, see Caldwell 2003).

Thus, Weber used the notion of “understanding” as a methodological device in different regards. Firstly, it served to distinguish between explanation in the social and the natural realm. Secondly, it served to squeeze out “irrationality” as a useful concept for historical and social causal explanations. Thirdly, it served to focus on the intentions of individuals, rather than on their psychology, when searching for social explanations. Fourthly, focusing on rationality served as a heuristics to explain deviations in concrete cases. Understanding, intentionality, and rationality were micro and macro related concepts in causal social explanations.

¹²⁹ This argument is emphasized in (Ringer, F. 2002). Weber's argument sounds strikingly similar to recent arguments of analytical philosophers such as John Searle and the late Donald Davidson. See (Davidson, D. 1980); (Searle, J.R. 2001).
With the benefit of hindsight it is remarkable how many of the themes Weber discussed more extensively in his famous 1908 article on marginal utility and psychophysics were already there: the rejection of psychological and physiological explanations of human conduct as irrelevant to economics, the use of instrumental rationality as an “idealtype”, the possibility to making causal explanations by using instrumental rationality as an organizing term, the conformity rather than opposition of law-like explanations in terms of instrumental rationality with the notion of free will. Before investigating Weber’s more general dismissal of psychophysics, I will turn to Weber’s detailed criticism of psychophysical experiments on work and fatigue.

6. Weber and the Psychophysics of Work

The beginning of the twentieth century saw a flourishing of studies into the efficiency of industrial work. As described in detail in (Brain 2001) and (Rabinbach 1992), experimental psychologists considered that their investigations into the measurement of reaction time or the measurement of fatigue could be made to bear on more mundane and policy relevant subjects like the measurement of the efficiency of factory work. Experimental psychologists in Germany in particular extended the relevance of their experiments under the highly stylized conditions of the laboratory to the “extra-mural” conditions of the factory (Brain 2001, 651).

These experimental psychologists essentially asked similar questions as Jevons had hinted at in his Theory as being of particular relevance to the economic theory of labour supply, only in a much more detailed fashion. Jevons mentioned the “many interesting questions” in the theory of labour that might be solved if we had “a determination of the exact relations of time, space, and fatigue” (Jevons [1879] 1970, 216). Psychophysicists investigated these relations in a great many studies on the exact numerical relations between work load and fatigue, the relation of different lengths of breaks to recovery time and work efficiency, and so forth.

Weber had already noted the “extraordinarily importance” (1975a, 111) of the psychophysical experiments of Wundt, Münsterberg and others in his essays on Roscher and Knies, but now the writings of these experimental psychologists served Weber to critically exemplify his worries with this type of investigations when applied to the extramural world of the psychological laboratory. Weber chose the psychiatrist Emil Kraepelin's study on the “work curve” as representative of such experimental studies. As Jevons and Edgeworth before him, Kraepelin considered the pattern of work and fatigue of an individual workman indicative of his “energetic disposition” and so in the last instance reducible to his physiological frame. The “work curve” traced the expenditure of energy of body and mind over time.

130 (Rabinbach, A. 1992) is an indispensable source.
131 Weber engaged with industrial psychologists between 1908 and 1912, published in the collected works as (Weber 1995).
Kraepelin's experimental studies were designed to shed light on how to adapt the conditions of the factory as much as possible to the energetic disposition of the workforce, and vice versa. The practical importance of such studies can be gathered from heated debates about what was called the “labour” or “social question” in Europe, which was concerned with issues like the (moral) habits of the workforce, the length of the working day, and more generally was about how to integrate the newly emerging working class within the tissue of society. Kraepelin's (and his similars) “work curves” promised to be an instrument to decide on some of the more practical issues, like the length of the working day or the optimal length of a break. Indeed, as an outcome of some of such studies a German entrepreneur reduced working hours to eight hours a day (without reducing wages) to enhance productivity.132

Weber granted the psychophysicists the “theoretically of course indubitable point of view” that it should be possible “in principle” to gain insight in industrial conditions of work from “physiological, experimental-psychological and perhaps also anthropological insights” (1995, 62) and he ask the reader some patience with a “complete layman” in experimental psychology as he was. Nevertheless, his criticism did not leave much in doubt about his final verdict on the value of such studies.

According to Weber, psychophysical studies in work and fatigue tacitly assumed that the constrained situation of the experiment effectively mimicked working conditions in the factory. However, according to Weber, factory conditions of work contained an “array of built-in conditions which are alien to the laboratory”, such as housing conditions, sanitary conditions, financial needs, the wage system, to mention just several of them (Brain 2001, 668). Weber's criticism of the laboratory experiments' undercomplexity was enforced by a “showcase article” (Brain 2001, 664) Kraepelin published in a Festschrift for Wilhelm Wundt.

In this article “Die Arbeitscurve” Kraepelin (1902, 489) decomposed the observed simple “labour curve” into several components, thus showing its “truly complex composition” (eine recht verwickelte Zusammensetzung). Kraepelin’s meta-study discussed several experiments in which the experimental subjects were given a task, for example adding or memorizing numbers.133 The length of the task was then varied, or intermixed with shorter or longer breaks, to then measure the effect of such variations on task performance. Kraepelin's diagram in the Festschrift (see figure 2) decomposed the labour curve into the training, fatigue, excitement, habit, and will-enforcement curve. According to Weber, Kraepelin thus only showed that it was not at all clear what the simple initially observed “labour curve” measured in the first place. Also, it was not clear how the labour curve could be made to relate to the psychological and physiological conditions of an individual factory worker, and a fortiori it was not clear how such a curve could give insight into issues of economic valuation involved in factory work.

132 See (Rabinbach 1992, chapter 6).
133 Interestingly, Jevons made similar experiments on the “power of numerical discrimination”. See (Jevons, W.S. 1971).
Weber's criticism of the undercomplexity of the approach to work of the psychophysicists pointed in two very different directions. The one was that investigations into factory work should investigate in detail all (causal) factors involved in factory work to show their complex interrelations. This was the direction sociology was to take. As an example, the Verein für Sozialpolitik initiated social surveys into industrial work. These surveys made extensive use of questionnaires to gain insight into the complexity of conditions of factory work. Though nowadays generally seen as a failure (with the exception of Marie Bernay's study of a textile factory in Gladbach), these survey studies were generally in accordance with the methods of research propagated by the German Historical School, and designed to gain insight into laws governing the social domain from an as wide and varied collection of data as possible.

Given Weber's criticism of Kraepelin's psychophysics of work, one may be inclined to concur with Robert Brain (2001) that Weber moved into the direction of the “discursive analysis developed by the German historical economists.” But this was not quite so. We have already seen Weber's tense relations to the historical school. Even in Weber's discussion of Kraepelin, it transpires that Weber defended an explanatory strategy that was much closer to Austrian economics and actually orthogonal with German historicism. Given the tremendous difficulties encountered by the problem of the undercomplexity of psychophysical studies into industrial labour, Weber asked himself if in view of this “disappointing state of affairs”, there might perhaps be another point of view that might come to the rescue. And here Weber turned “naturally” to economics, because economics was equally concerned with issues of labour efficiency.

In sharp contrast with mass sociological investigations and in sharp contrast with detailed psychological experimental research, the economists' point of view was the most elementary. It was only concerned with problems of profitability, that is, with questions of choosing the best course of action on the basis of calculations of means to ends. A manager made systematic, that was “rational”, use of labour and means of production for a given end. That was to say, managers used means of production efficiently. Weber argued that “we can [rationally] decide upon the rules [Maxime - HM] that guide such “pragmatic inferences” by calculating their “utility effects.” (Weber 1995, 131-32).

Such “pragmatic inferences” were about choosing the best means to an end from a given number of alternatives. According to Weber, the simple calculations involved in such a choice made it “evident” that these were “far off the methods of measurement that we find in the laboratory of the experimental psychologists” (1995, 128). Because psychophysics was concerned with the “functioning” of the “psychophysical apparatus” (that is the workman), while this functioning was taken as given in economic calculations of choosing the best means to an end, the calculations of the economist were even to be contrasted [entgegengesetzt - HM] with the experimental methods of measurement of the psychophysicist (1995, 130). Let me now turn to Max Weber's more general refutation of the relevance of psychophysics to economics.
7. Weber on Psychophysics and Marginal Utility Theory

In his criticism of the Historical School, Weber gave the following explanation of the “logic” that could be used to evaluate goal-oriented behaviour. Given a certain intention or goal $x$, an actor needs to select a means $y$, rather than $y'$ or $y''$, as the best means (on the basis of existing empirical evidence) to obtain that goal. Though such judgments were evaluative, they were empirical in the sense that, for example, $y$ rather than $y'$ was more appropriate in terms of sacrifices. Weber emphasized there was nothing “subjective” in such estimations; everybody would come to the same conclusion and so nothing hinged on one’s psychological dispositions.

Hence, in such evaluations no assessment needed to be made of the psychology of the individual in making this judgement. The only assumption to be made was that an individual was able to rank the objectively given means in regard to the given end. But that was trivial in terms of logic, not in terms of psychology. Note that no recourse was made here to the notion of preferences or to any similar notion. In case of a manager deciding on labour input, this decision was made in view of its profitability - that is the situation of the previous section. Though evaluative, such an analysis was completely “objective” in the sense that the different possible courses of actions could be assessed by anyone. Also in this case, the choice of the best course of action was not dependent upon the psychology of the manager. In the sense of being the “best” choice, such an evaluation could be called “rational”.

Weber emphasized the “self-evident” character of such evaluations. As an outcome they produced a causal account of action, though not nomological in the sense as was used in the natural sciences. There might easily be deviations from the “best”, that is “rational” course of action, and such deviations might be explained as the “nonrational elements of actual economic action” (Weber 1975a). For that reason, Weber called such explanations “idealtypical”, to distinguish them from the causal nomological accounts of the natural sciences; we don't infer actual actions from them, but only the “objectively possible” courses of action (1975a).

This was how rational explanations were used in economics. In economics, the assumption of “pure rationality” served to “theoretically” deduce the consequences of “economic situations”. Similar to the use of rational explanations in history, these deduced consequences could be compared to their real world counterparts to distinguish the rational from the nonrational aspects of concrete actions. Though lawlike, rational explanations differed from natural law explanations in that they provided only interpretations of events, as opposed to a “law of nature” that “must be true in concrete cases” (Weber 1975a, 190). Means-ends interpretations thus implied the rationalization of empirical reality, but it did not imply that reality itself was rational. They served to circumvent the concrete complexity of empirical, real-world given situations by proceeding on the assumption that these situations were the result of intentional optimizing actions.
Thus, economic explanations in terms of rationality differed from sociological explanations in that no attempt was made to make any inductive inference about the full complexity of causal social factors, and their relations, involved in concrete social settings. Nor did economics claim that individuals, in their actions, really were behaving rationally. Rather, goal-oriented rationality functioned as a hypothesis; as a methodical principle the economist could use to make sense of individual actions in the first place.

Economists, so Weber argued, were not interested in the psychological intricacies of “needs” or any other psychological factors *per se*. They were only interested in the way individuals valued such needs in the light of possible means to fulfil them. In his earlier criticism of Knies Weber had pointed out that the image of man as being *instinctively* driven by self-interest, or selfishness, as Knies had assumed, and “Mandeville and Helvetius” before him (Weber 1975a, 201), was of no help in explaining how man proceeded to fulfil this basic instinct. If such a “simplistic” psychology was abandoned for a more complex search into mankind's psychology this proved of as little help. To examine the “truly complex composition” of the work curve, for example, did not teach us anything about questions of valuation of either the workman or the entrepreneur, and those were the questions of interest to the economist. For the purpose of economics, the only thing we needed to assume, so Weber argued, was that such valuations were performed *rationally*. That is, that individuals in their search to fulfil their needs could chose the best means to do so.

These were also the conclusions of Weber’s famous 1908 essay on the relation of marginalism to psychophysics. According to Weber “common experience” gave sufficient grounds to proceed on the assumption of rationality. “Common experience” taught us that “men ... are motivated by 'needs'”. Common experience taught us also that people made a ranking of needs according to their “urgency”. And common experience taught us that men were able to act “expeditiously”, that is on the basis of “prior calculation” (Weber 1975b, 29). Weber claimed that these assumptions did not ask for any detailed investigation into the psychological complexity of these needs. On the basis of these “entirely trivial, but undisputable facts of everyday experience”, the economist could “theoretically conceive of a relatively large number of people” each of whom used his available resources “for the sole and exclusive purpose of peaceably achieving an “optimum” of satisfaction of his *various* competing needs” (Weber 1975b, 29).

Weber was well aware that from the viewpoint of a psychologist, concepts like “purposive action”, “experiencing”, or “prior calculation”, were all highly complex. But this was simply irrelevant to the economist. The economist could understand economizing behaviour from these “trivial facts of everyday life”. To think of such trivialities as the “foundations” of economics was perhaps dazzling, but “yet this is the situation”. Weber emphasized that the economist did not need to recast these everyday facts “to make them susceptible to the psychologists' usual work with revolving drums or other laboratory apparatus!” (Weber 1975b, 30). He could even obtain “mathematical formulations for his theoretically conceived course of economically relevant action”
without taking recourse to any refined psychological theories, because his concern was not an investigation into man's psychology, but an evaluation of given means to an end.

Interestingly, Weber added an historical note to his discussion in which he claimed that the notion of goal-oriented behaviour had gained increasing empirical validity in the “capitalist epoch”. To think about the agent as optimizing the use of limited means for a given end was exactly the situation of “an agent who constantly carries on 'economic enterprise,' and it treats his life as the object of his 'enterprise' controlled according to calculation.” (Weber 1975b, 32). In so far as the economist assumed anything about the individual’s “psyche”, he assumed a “merchant's soul” (1975b, 32). The economist theorised on the “increasingly true assumption” that “everyone were to shape his conduct towards his environment exclusively according to the principles of commercial bookkeeping - and, in this sense, 'rationally.'” (1975b, 32-33). For Weber, rationality was “an approximation to reality that has implicated the destiny of ever-wider layers of humanity. And it will hold more and more broadly, as far as our horizons allow us to see” (1975b, 33). Economics rested on the truth of this historical fact, not on the truth of the psychophysics of Fechner and others.

8. Back to Robbins’s Essay

Let me return to Robbins’s Essay. If we compare the very different stances on the relevance of psychophysics to economics of Jevons and Edgeworth on the one hand, and Weber on the other, we see that they entail very different views of the economic agent. For Jevons and Edgeworth, economics was concerned with an analysis of how individuals, considered as pleasure machines, behave in the market. Their explanatory strategy was to turn to man's biological frame and to consider how this behaviour followed from an alleged optimisation process of man's energetic dispositions. Balancing pleasures and pains, Jevons considered economic man (a “trading body”, not a self-conscious individual) as an energetic system following the principle of least action. This was not a social, but a natural fact - pleasures and pains governed the individual by necessity and in accordance with rules of optimisation. For Edgeworth something similar can be said.

Things look very different for Max Weber. His focus was the business man, the “merchant” who used “double book-keeping” to decide on the best course of action. There was no assumption needed about his biological frame, whether such assumptions were about “instincts” or “self-interest” or whatever. Rather, our economic agent acted in a specific historical (and social) context that put one specific goal, profit maximisation, upfront. Given this goal, everyone could assess the best means to obtain this end. But that was not a natural law; it was a rational assessment that nevertheless provided a causal explanation for economic action. For Weber, it was no different for other economic agents, and for that reason no assumptions were needed about mankind's biological or psychological constitution. For Weber the emergence of a market economy was a social, not a biological fact.
It was Weber's view of rationality, rather than Jevons and Edgeworth's view of man as an energetic system, that carried the day in twentieth century economics, and this despite the economists' use of the doubtlessly psychologically loaded notion of “preferences” - absent in Weber. Lionel Robbins's *Essay* serves as exemplar, certainly because economists read Robbins, not Weber. In his famous essay on the nature and significance of economics, Robbins explicitly chose Weber's side. Robbins argued that “unfortunately ... certain of the founders of the modern subjective theory of value” had claimed “the authority of doctrines of psychological hedonism as sanctions for their propositions.” Robbins mentioned “Jevons and Edgeworth, to say nothing of their English followers” as a “reminder” of “really competent economists who did make pretensions of this sort” (Robbins 1984, 84). Edgeworth's description of man as a “pleasure machine” was an attempt to “exhibit the law of diminishing marginal utility as a special case of the Weber Fechner Law” (85), which went straight in the wrong direction.

Just like Weber, Robbins argued that the only thing an economist needed was to “realise that the foundation of the theory of value” resides in the “elementary fact of experience” that “different things that an individual want to do have a different importance to him, and can be arranged in a certain order.” Just like the merchant, we might add, is able to arrange means to an end. “Purposive”, “goal-oriented”, “intentional” action did not reduce the individual’s act of choice to his biological frame. According to Robbins economic valuations were not difficult: everyone could rank A over B over C, and hence A over C, and we did not need much more, as an economist, to build up our theories. That is not that different from Weber's ranking of y as compared to y' and y" as the best course of action.

That's *logical*, or “common sense” knowledge, rather than psychological or physiological. Robbins and Weber also agreed in using this goal-oriented type of behaviour as a crucial causal element in economic explanations. They differed in the – call it – ontological status of means-end rationality. For Weber it was an “ideal-type” that became increasingly true historically in contemporary capitalist society. Robbins, by contrast, argued that this basic tenet was about psychology. Why limit goal-oriented behaviour only to capitalist society? Every means-end decision has the same structure, whether here or there, now or then, and such a decision does assume that an individual is able to order these means. In contemporary terms, this is “folk psychology”.

Though for a full history more needs to be said, I think it is undeniable that twentieth century economists by and large subscribed to the basic tenet of Robbins' reasoning. One can read as much of Robbins as of Weber in the basic instinct of economists to take rationality as a logical, rather than psychological fact, and yet as an important causal factor in social explanations. Way up into the 1970s, means-end rationality was what rationality meant to an economist. In his Fels lectures Arrow wrote that an economist “by training thinks of himself as the guardian of rationality, the ascriber of rationality to others, and the prescriber of rationality to the social world”. At the end of the day, rationality was about means and ends: “Rationality, after all, has to do with means and ends and their relation” (Arrow 1974, 16-17).

\[^{134}\text{The original essay was published in 1932. The second edition of 1935 was considerably revised.}\]
After Robbins, most economists fell into a kind of Kantian dogmatic slumber before being awakened by systematic violations of the trivially easy ranking of A over B over C somewhere in the seventies (Bruni and Sugden 2007). It was only once it became increasingly indisputable from psychologists’ experiments that even very simple choice situations are really not that simple, that economists gradually became convinced that the almost tacit assumption that individuals can rank alternatives for a given end, should, perhaps, be exchanged for a reinvestigation of how individuals do or do not optimize in concrete situations. That does mean, for a growing community of economists, that one needs to reinvestigate the “physical groundwork of economics”.

I think Robbins would have agreed with John Elliot Cairnes that an investigation in the “afferent trunks of nerve-fibre” would turn economics into a very different science to that which the world has known so far. Against his dismissal of attempts to investigate the physiological basis of concepts like “preferences” and “expectations” as intractable or even unnecessary, one might use Jevons’s words of the Theory: “In matters of this kind, those who despair are almost always invariably those who never tried to succeed” ([1879] 1970, 81).
has been proposed to substitute for Fechner’s law, e.g. that of Helmholtz, \( \frac{a}{G - \gamma_0} \log \left[ \frac{\gamma_0 + \gamma}{G + \gamma} \right] + C \); and that of Delboeuf, \( k \log \left( \frac{c + \gamma}{c} - k \log \left( \frac{m}{m - \gamma} \right) \right) \). The properties of the functions express the circumstances that for every increase of stimulus there tends to be some increase of pleasure (sensation), but that, as the stimulus increases, the sensation increases less rapidly (its rate of increase decreases). These circumstances appear to have been assumed by all, including Laplace, who have touched upon the relation of “fortune physique” to “fortune morale”; and they are sufficiently evidenced by every-day experience. It is obvious indeed to remark, that the increase of stimulus beyond a certain point destroys pleasure (and ultimately sensation); that in the curve of pleasure there is a “Wendepunkt,” as exhibited by Wundt (and compare Delboeuf’s theory of fatigue). But no error will be produced in the following reasonings, by considering the stimulus not to exceed that corresponding to the “Wendepunkt.” For where it is concluded that a sentient should have a greater share of stimulus, he is to be considered not as applying the whole stimulus at once to the organ of sensation, but at different times, perhaps to different organs, in appropriate subdivisions. He is not to be considered as throwing all his fuel at once on one furnace, but as lighting up the same furnace, or others, at different times. The first condition then, under proper reservations, may be assumed. As to the second condition, and the second differential of our function, I know of only one consideration which need give us pause, the form of the pleasure-curve as delineated by Wundt; which, after all, may be only a diagrammatical accident. This curve from the “Schwelle” up to the “Wendepunkt,” is not continuously concave to the abscissa, as our condition demands; the lower part is convex, the upper con-

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1 “Physiolog. Psychol.,” p. 432.
Figure 2. Emil Kraepelin's Work curve, From *Philosophische Studien* (1902), table II (from a study of Lindley into the effects of work-time breaks on work performance). The horizontal axis measures time (each five minutes), the vertical axis measures the “found values” of performances of a task for one hour. The dotted line indicates a break of 30 minutes, after which the experimental subject (A) restarts the task. Since A does not have to perform for 30 minutes, the “will-curve” is interrupted for 30 minutes as well. The courses of the other curves are theoretical conjectures which Kraepelin argues for in his article. These are all subjected to intense scrutiny by Weber.
References


Edgeworth, Francis Y. (1877), *New and Old Methods of Ethics, or “Physical Ethics” and “Methods of Ethics*”. Oxford: James Parker & Co.


Some Legacies of Robbins’s
Nature and Significance of Economic Science

Address To Conference Celebrating
the 75th Anniversary Of Lionel Robbins’s

Richard G. Lipsey*

Abstract

This paper criticises three Robbinsian positions still often found in modern economics: (1) the methodology of intuitively obvious assumptions; (2) treating facts as illustrations rather than as tests of theoretical propositions; (3) assuming that theory provides universally applicable generalisations independent of the characteristics of individual economies and so are independent of specific historical processes. Two corollaries of point (3) are that theory cannot assist in explaining unique historical events such as the emergence of sustained growth in the West and that economists need not interest themselves in the details of the technologies that produce the nation’s wealth.

Keywords: methodology, economic generalisations, measurement, positive economics, historical specificity

JEL classification: B41, B31.

Some legacies of Robbins’s Nature and Significance of Economic Science

Lionel Robbins was a great human being. He accepted and lived by liberal values. He was appalled by events in Europe, particularly Nazi Germany and he did his best to help refugees from that terror. Among other things, he did a great deal both emotionally and financially for a colleague of ours who had spent much of the war in a German concentration camp and was the only one of his family to escape the gas chamber.

He espoused the importance of a rational approach to problems and of the power of economics to assist in it. Asking himself about the value of his subject, he wrote (1935, p.152):

“Surely it consists in just this, that, when we are faced with a choice between ultimates, it enables us to choose with full awareness of the implications of what we are choosing. Faced with the problem of deciding between this and that, we are not entitled to look to Economics for the ultimate decision….

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But, to be completely rational, we must know what it is we prefer. We must be aware of the implications of the alternatives. For rationality in choice is nothing more and nothing less than choice with compete awareness of the alternatives rejected. And it is just here that Economics acquires its practical significance. It can make clear to us the implications of the different ends we may choose. It makes it possible for us to will with knowledge of what it is we are willing. It makes it possible for us to select a system of ends which are mutually consistent with each other.”

1. Personal Background

I first encountered Robbins’s essay as an undergraduate in 1949. I was mightily impressed and learned much from it. But I balked when I came to his discussion of the place of facts in economics. I read: “The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience….(1935, 78 Italics added). If the premises relate to reality the deductions from them must have a similar point of reference” (1935, p.104).

I read and reread this material and said to myself: “This cannot be right; facts derived from empirical observation must be more important to the development of theory than to act as ex post illustrations of what we already know to be true.”

Some four years later I entered the LSE as a PhD student and attended Lionel Robbins’s great Wednesday afternoon seminar. In an age of increasing specialisation, this seminar was a breath of fresh air. Everything in economics was grist for Robbins’s mill. At the beginning of each year, Lionel would ask around to see what were thought to be the most important new ideas in the subject and then make them seminar topics. Sometimes we had a different topic each week and at other times, as with Patinkin’s *Money Interest and Prices*, we spent a whole term on one publication. The sense of being Renaissance people interested in any and every topic in our subject was exhilarating.

As the weeks passed, however, Lionel’s expressions of the then prevailing methodology described above revived my interest in his essay. As the theories we discussed in the Wednesday seminar became based on increasingly complex and less intuitively obvious assumptions, we found ourselves frustrated by the inconclusiveness of arguments concerning their intuitive plausibility. A group of us who were thinking along the same lines formed the LSE staff seminar on Methodology Measurement and Testing in Economics that became known as the M²T seminar, which Jim Thomas discusses in another paper in this volume. We talked to philosophers of science such as Joseph Agassi and, a bit later on, Imre Lakatos (who became a good friend of mine). Agassi introduced us to Popper and under his influence we came to reject the Robbinsian methodology and accept the position that economic theories were to be judged by the ability of their predictions to stand up to empirical testing. We disagreed with Friedman’s (1953) argument that only predictions were to be tested against evidence and held that if a
theory’s predictions pass test in spite of being derived from assumed relations that were empirically false (such as all demand curves have positive slopes), we learn by asking why? (To discuss this matter fully requires distinguishing among the various uses of assumptions in economic theory. I have discussed the issue of empirical relevance of assumptions in some detail in Lipsey (2001).)

From 1960 to 1963, I wrote An Introduction to Positive Economics which was designed to promote the methodology of testing as opposed to the Robbinsian methodology of intuitively obvious assumptions. The book had an immediate impact and went through five reprints in the four-year life of its first edition.135

While there is much that I could say in praise of Robbins’s essay, given the space constraints, I must concentrate on my criticisms. Here I want to take up three issues that I think pose serious problems, all of which have modern manifestations: the methodology of intuitively obvious assumptions, the relegation of facts to be illustrations of theoretical propositions rather than as tests of their validity, and the belief in the general applicability of economic theory without the need for specific context. It would take someone better versed in the history of economic thought than I to determine where Robbins stood in the chain that leads from the first statement of each of these ideas to their modern manifestations. There can be little doubt, however, that Robbins was an important link in their transmission to modern economists, both where he was initiator and where he was such a superb populariser that he helped to make many of them the conventional wisdom of economics for generations to come.

2. The Methodology of Intuitively Obvious Assumptions

At the outset of his essay, Robbins states his main thesis that the “generalisations” of economic theory are both certain and empirically relevant: “The efforts of economists over the last hundred and fifty years have resulted in the establishment of a body of generalizations whose substantial accuracy and importance are open to question only by the ignorant or the perverse” (1935, p.1). Lest we have any doubt about their relevance: “It is a characteristic of scientific generalisations that they refer to reality” (1935, p.104). Lest we have any doubt about their certainty: “[O]ur belief in these propositions is as complete as belief based upon any number of controlled experiments” (1935, p.75).

135 The successor to this book, now called Economics, by Lipsey and Chrystal (2007) is currently in its 11th U.K. edition. The American adaptation was first published in 1966 as Economics by Lipsey and Steiner and is now in its 13th U.S. edition authored by Lipsey, Ragan and Storer. How much credit the first few editions of Positive Economics should get for replacing the Robinsian methodology with a more empirically oriented one in the U.K. is for others to judge. But within a decade or so after its publication the old methodology had disappeared so completely that students found it hard to believe that serious economists assessed theories by arguing about the intuitive plausibility of their assumptions. As the proportion of the population attending university rose slowly but steadily over the years, the sophistication of first year text books was steadily reduced. As a result, the long anti-Robbinsian first chapter on methodology, and the chapters labeled “Criticisms and Tests” that ended each section were progressively simplified and finally eliminated.
What is hard for modern economists looking back from today’s vantage point to believe is that we really did spend the bulk of our time discussing the plausibility of the assumptions of the theories we were attempting to assess. Lest one thinks we were some local backwater, I can attest that as London secretary from 1954 to 1957 of the Oxford-Cambridge-London joint economics seminar where graduate students and junior staff in the three universities met to hear papers and exchange views, the Robbinsian methodology was dominant. So when we switched from the Robbinsian to the Popperian methodology, we made a sea change in how we approached our subject.

The view that economics can be about the world and yet be based on intuitively obvious assumptions pervaded much of economics long after Robbins wrote. For example, much economic theorising of both a positive and normative sort was, and still is, based on the twin assumptions that technology and tastes are given and that the latter can be expressed by a utility function in which the goods and services an individual consumes are the only arguments. The first is typically thought to be an assumption of mere convenience although it is not innocuous because when technology is assumed to be endogenous, as it undoubtedly is in the real world, many comparative static results are altered, some even reversed. (For elaboration see Lipsey et al (2005, Chapter 2) and Lipsey (2007a, p. 335).) The second assumption was long thought by many (most?) economists to be intuitively self-evident. Only in the last decade or so has it been called into question. Sen’s (2000) capabilities approach was an elegant redirection away from this assumption and further research as summarised by Layard (2005) showed that it could be seriously challenged on empirical grounds. Hence, the strong advice given, then and now, about policy measures required to increase economic welfare were, and still are, based on a very shaky foundation. (I have discussed this in much more detail in Lipsey (2007a).)

Another assumption that I think most of us took as self evident, that “bygones are forever bygones,” was discussed at length by Robbins (1935, p.52). This is an important normative proposition for those who wish to maximise something. But as Robbins would have it, as a self-evident assumption on which to base positive predictions about behaviour, current research has shown it to be flawed. People’s behaviour often reveals them to be acting as if bygones did matter. (For examples, see Tversky and Kahneman (1992) and Kahneman and Thaler (2006) and for another general critique of Robbins’s a priorism see Blaug (1992, pp. 76-79).)

3. Facts as Illustrations Rather Than Tests

Having taken his position on the truth of assumptions and predictions, Robbins had to come to terms with the place of factual observations in economics and he outlined what he saw as their three main uses. First, as “…a check on the applicability to given situations of different types of theoretical constructions” (1935, p.116). For example, did something offsetting occur that violated the ceteris paribus assumption? Second, behaviour, and hence the applicability of a particular theory, may depend on second order assumption such as institutional constraints on what can be done. Thus the facts may suggest “auxiliary postulates” that are needed. For example, regulatory constraints on banking behaviour, require amendments to the predictions from the theory of unregulated
bank behaviour. Third, factual observations may expose “…areas where pure theory needs to be reformulated and extended. They bring to light new problems” (1935, p.118). The meaning of “extended” seems clear enough: issues that current theory has not investigated are revealed. But “reformulated” sounds suspiciously like using empirical studies to reveal the need to amend the theory. Unfortunately, the example Robbins gives covers extension to new areas but not the reformulation of existing theories, so we cannot be sure what he intended here. (I do not find the discussion of this point on page 118-120 altogether clear so it is possible that I have not quite grasped what Robbins intended, but I think I am approximately on track here.) In summary: “Realistic studies may suggest the problem to be solved. They may test the range of applicability of the answer when it is forthcoming. They may suggest assumptions for further theoretical elaboration. But it is theory and theory alone which is capable of supplying the solution” (1935, p.120 italics added).

Not only are facts not to be used to test theories, measurements of aggregates are typically irrelevant to theory. For example: “Estimates of the social income may have a quite definite meaning for monetary theory. But beyond this they have only conventional significance” (1935, p.57). Later, Robbins asks: “Ought we not to wish to be in a position to give numerical values to the scales of valuation, to establish quantitative laws of demand and supply?” (1935, p.107). He then goes on to argue that with demand elasticity “…there is no reason to suppose that uniformities are to be discovered.” Thus such measurements taken at a particular time and place have no “…permanent significance - save as Economic History” (1935, p.109 italics in original). I do not read this passage as do some as a warning that specific measurements taken at one time and place cannot necessarily be generalised to other times and places. Instead, I take Robbins to mean that the truths of economics are all about the signs of changes, such that all demand curves have negative slopes, and not about magnitudes, such as measured values of the income and price elasticities of most foodstuffs fall as incomes rise going well below unity in high income countries. I criticised this position in the first edition of An Introduction to Positive Economics (1963, pp. 158-161) on three grounds. First, even if a priori reasoning suggests that a particular relation will not stay constant over time, only empirical observation can establish if this is so. Second, it is important to know just how stable or unstable any relation is. For example, if demand curves shifted in location and slope drastically and capriciously over short periods of time, none of the comparative statics of price theory and their policy applications would be of much use. Third, even if there are substantial variations in the relation under consideration, only empirical observations can show if these variations appear random, in which case we have done everything we can by way of explanation, or systematic, in which case the presence of an as-yet-unobserved causal variable is suggested.

In the early days of the M²T seminar, I think we were naïve enough to believe that one theory replaced another in the history of the subject when evidence that conflicted with the incumbent could be better explained by the challenger. Soon, however, we came to accept Imre Lakatos’s more subtle view of how scientific paradigms are related to evidence and how one replaces the other. (I have stated my current understanding of this issue in Lipsey (2000).) Although naïve falsification is open to serious criticism, and
although cases where theories have been rejected due to tests of their predictions are not
common in economics, two Popperian methodological messages seem to stand up and to
be something that all students of economics should be taught. First, economic theories
that are consistent with all possible states of the world are empirically empty and, as
such, they tell us nothing about the real world whose behaviour we seek to understand
and predict. They may be very general devices that can be used as receptacles for further
empirically based assumptions that specify lower-level theories that are not empirically
empty but their usefulness depends on this being so. Second, even if direct tests of
theories that do have empirical content are not all that frequent, such theories are in
principle testable by looking for the observations that they rule out. (As Popper stressed,
these are statements about the logic of scientific statements not necessarily about the
process by which scientific advance occurs.)

In arguing this view in the article “Positive Economics” in the forthcoming revision of
the New Palgrave Dictionary, I put it this way:

‘First, if an economic theory is to be about the real world, it must be possible
to imagine observations that would conflict with it. If conflicting observations
cannot even be imagined, the theory is compatible with all states of the world
and hence empirically empty. A great advance in making theory more
relevant would be achieved if today’s editors insisted that each author state
what factual observations would conflict with his or her theory, and, if there
were none, to state the theory’s purpose. Second, a new theory should be
compatible with (‘explain’) some existing facts and suggest some new
one(s).’

Instead, all too many articles follow the Robbinsian use of facts as mere illustrations of
the applicability of theory. The ‘test’ then is: “Can the theory or model be made to track
already known facts?” In doing so, the authors are applying what Popper criticised as
“sunrise tests”: predicting what we already know and nothing else. From this we learn
something about the ingenuity and technical skills of the authors but not much more.

The Robbinsian view on the relative unimportance of facts as controls on theorising
persists in a surprising amount of modern economics. The approach gives rise to what I
call “internally driven research programs” (IDRP s), programs that are driven by attempts
to understand problems created by the programs’ models rather than problems arising
from empirical observations related to the models. An IDRP often begins with a factual
question. Typically a simple model is developed yielding strong answers. Investigators
then ask if these prediction would stand up if we altered the model to make it more
realistic. What, for example, if we went from one, to two, to three sectors? What if we
made saving endogenous rather than exogenous? What if we let technology change in
response to the market signals? — and so on and so on. If empirical observations are
used in the program’s later stages, the question is usually of the sunrise variety: Can the
model be made to track the data? Many of the fads and fashions that sweep economics
are aspects of internally driven research programs. I am not arguing that internally driven
programs never produce interesting results, only that the probability of their producing
new empirically relevant results is quite low (even when the models are made to track already known data).

In Lipsey (2001) I gave a number of illustrations, the most dramatic being the burst of theorizing concerning economic growth that started in the 1940s and ran to about 1970. Harrod and Domar produced the then-famous knife edge result of a completely unstable growth path. Concern over this disturbing result was allayed by Solow’s famous neoclassical growth model in which the absence of a fixed capital/output ratio produced a stable, balanced growth path. Solow’s work led in two directions. One was the empirically based growth accounting exercise that looked for the sources of the residual that Solow had attributed to technical change—and that turned out to be due to a much more complex set of influences than just technical change. The other was the theoretical investigation of growth models. This second direction produced an IDRP as the profession embarked on a 15-year bout of balanced growth research. At the end of it all Amartya Sen (1970, p.33, italics added) had this to say:

"The policy issues related to economic growth are numerous and intricate. ... While the logical aspects involved in these exercises are much better understood now than they used to be, perhaps the weakest link in the chain is the set of empirical theories of growth that underlie the logical exercises. Possible improvement of policies towards growth that could be achieved through a better understanding of the actual process of growth remains substantially unexplored. It is partly a measure of the complexity of economic growth that the phenomenon of growth should remain, after three decades of intensive intellectual study, such an enigma. It is, however, also a reflection of our sense of values, particularly of the preoccupation with the brain-twisters. Part of the difficulty arises undoubtedly from the fact that the selection of topics for work in growth economics is guided much more by logical curiosity than by a taste for relevance. The character of the subject owes much to this fact."

Reviewing the same literature at a much later date I put the same point this way: “[I]nternally generated questions produce [only] internally directed answers.” (Lipsey, 2001, p 181). (For another example, this time with respect to the effects of rent control, see Lind (2007) and for more general discussion of the issue see Goldfarb and Ratner (2008).)

What I call “externally driven research programs” (EDRPs) contrast sharply with IDRPCs. These are programs that are driven by, and constrained by, observed facts. The growth accounting search to explain the Solow residual is one good example. Another is the evolution of theories concerning monetary and fiscal policy through the long set of debates between Keynesians and old fashioned monetarists that raged through the 1950s, ‘60s, and ‘70s. The conclusion can be dated at 1980 when the Keynesian, James Tobin, joined the monetarist, David Laidler, in a debate that was subsequently published in the Economic Journal (Tobin 1981 and Laidler 1981). Tobin and Ladler disagreed on some matters of judgement about speeds of reaction and the precise slopes of some curves.
They revealed, however, no discernible differences of underlying models or of fundamental assessment of what were the key relations that governed the economy’s behaviour and what were the key policy conclusions regarding fiscal and monetary policy. This 30 year debate generated much heat in its time, but the end result was much light. Empirical evidence about such things as the income and the interest elasticities of the demand for money, and wage and price flexibility, was amassed. The extreme position of each of the two schools was moderated in the light of the accumulating evidence, until their differences were slight compared with their agreements.136

4. Absence of Context Specificity

This is a pervasive characteristic of much modern economics. I discuss it in four parts, raising issues on which economists are still strongly divided.

Universally applicable generalisations

Robbins was certainly a major link in the chain of How Economists Forgot History (Hodgson 2002). Economists once actively discussed what Hodgson calls historical specificity, but which my co-authors and I prefer to call context-specificity: there is a trade off between the generality of a theory and its empirical content. Robbins clearly lies at one extreme in this issue. He argues that the generalisations of economics are universal, applying to all times and all places. For example (1935, p. 80): “It has sometimes been asserted that the generalisations of Economics are essentially “historico-relative” in character, that their validity is limited to certain historical conditions, and that outside these they have no relevance to the analysis of social phenomena. This view is a dangerous misapprehension”. The reason, of course, is that in Robbins’s view the main assumptions of economics are not historical relative.

This takes a very strong line on a debate that still rages among economists, both explicitly and implicitly. There are many illustrations of the non-Robbinsian side, of which the following are only a few examples.

- The preference functions that drive behaviour do appear to vary across societies, at least to some extent. For example, Michael Porter (1990) has noted the differences in risk-taking behaviour among business persons in societies such as Japan and Germany where business failure is regarded with strong disapproval, and societies, such as the US, where a failure or two on the road to final success is taken as acceptable, even normal. For some applications, therefore, the preference functions need to be specific to certain geographical and/or historical circumstances.

136 Just as that apparently satisfactory situation was being reached, a challenge arose in the form of the more basic critique of the Keynesian-monetarist synthesis, made by the new classical economists led by Robert Lucas. Although this new classical macro economics dominates modern advanced macro textbooks, Keynesian income-flow models still dominate the analysis of practical policy problems by central banks and departments of finance and these utilize much of the knowledge amassed during this long Keynesian Monetarist debate. I have discussed and assessed these developments in more detail in Lipsey (2000, p.69-80).
• The serious problems encountered in the rush to marketise the former USSR’s command economy stemmed partly from a view that economic theory of markets showed them working as long as all impediments were removed, irrespective of institutions and other factors that are not modelled in canonical neoclassical market theory, but which distinguish one economy from another. Appropriate policies for marketising a command economy, therefore, depend on the specific national context of such things as existing institutions, learned behavioural modes, and levels of development.

• The IMF and World Bank’s one-size-fits-all view on correct economic policy for all times and places, as enshrined in the policy of ‘structural reform,’ has proven to be a failure, at least when it had gone beyond removing the most extreme non-market policies. This is now recognized even by those two institutions and the discredited view is being replaced by an understanding that different policies may be relevant for the different conditions facing various developing nations. (For examples see Griffiths (2003), Hira (2007), Rodrik (2006) and Stiglitz (2002).)

One of the reasons why many neoclassical economists hated second best theory\(^\text{137}\) is because it showed that since a “distortion-free economy” is an impossibility, all policy advice has to take place in second best situations where context specificity is all important. Because different societies face different “distortions,” making second best improvements depend on the specifics of each set of market conditions. For example, removing ‘distorting’ subsidies on a poor country’s production of a product heavily subsidised by the U.S. may eliminate the local industry, add to unemployment, and accentuate balance of payments problems. (I have dealt with these issues in more detail in Lipsey (2007a).)

No economic analysis of historical processes

At one point in his analysis Robbins asks (1935, p. 131) “…:can we not frame a complete theory of economic development?”. Having answered in the negative, he goes on to argue (1935, p. 133) “Nor are the prospects improved when we turn to the sphere of technical change and invention…. What technique of analysis could predict the trends of inventions leading on the one hand to the coming of the railway, on the other to the internal combustion engine?”

Robbins’s negative answer is partly because he recognised comparative statics as the only valid tool of economic theorising and could not see this dealing with dynamic issues of economic growth and technical change. Of course, modern macro-growth models with either exogenous or endogenous technical change do theorise about economic development, even if they cannot deal with such specific questions as the coming of the railways or the invention of the internal combustion engine.

\(^{137}\) As Lionel said to me over lunch shortly after our original second best article had been published: “But my dear Dick it is so nihilistic!”
Even among modern growth theorists, however, a major difference in approach reveals a conflict between Robbinsian universal applicability and non-Robbinsian context specificity. Growth theories that use an aggregate production function, with either exogenous or endogenous technical change, are devoid of detailed specifications of technology - which usually appears as a scalar either pre-multiplying the aggregate production function or as a variable in that function - of institutions, or of anything else that distinguishes one economy from another. Thus, they yield the universal prediction that when certain basic conditions are fulfilled with respect to such things as saving and investment, growth will inevitably follow in all countries.

The few existing formal growth models that use a more structured representation of technology are either explicitly or implicitly context specific. For example, after presenting our three-sector model of sustained GPT-driven growth with endogenous technological change, we conclude (Lipsey et al. 2005, p.467):

“Our models implicitly assume the institutional circumstances that underpin modern market economies, such as private property, limited liability, and the rule of law. They also assume the specific institutions involved in the West’s invention of how to invent…[that] made the West’s growth process self-sustaining…. Because of their structure, they apply only to countries whose growth depends to a significant extent on developing from their own resources new technologies, both fundamental and derivative. Thus, they are not meant to apply to countries whose growth processes are more or less completely driven by the diffusion of technologies developed elsewhere. Nor are they meant to apply to those whose GPTs are currently static and who seek conditions that would allow them to begin a period of sustained growth.

All these qualifications illustrate once again the issue of historical specificity: the richer the explanatory power of a theory and the more predictions that it makes, the more restricted is its range of applicability in both time and space. Finally, we observe that there is no single ‘correct’ way to make the historical specificity trade-off. All growth processes have things in common, and to deal with these, very general theories are helpful. But all growth processes also have many aspects that are more specific in time and place. To deal with these, and, therefore, to get to deeper levels of explanation requires less generality and more specificity.”

**Explaining the emergence of sustained growth**

The conflict between Robbinsian generality and historical specificity can also be seen in a big way in theories of how the West turned the episodic growth of earlier eras into the sustained growth that was initiated by the two Industrial Revolutions. The highly popular unified growth theories (UGTs) that were introduced some time ago by Galor and Weil (2000) are fully in the Robbinsian tradition. They model an economy developing from a long period of extensive growth in which all increases in output are taken up by increases in population, through a transition period, and then into a modern period of intensive growth in which the rate of population growth falls well below the rate of output growth.
UGT models are general, containing nothing that would distinguish one economy from another. As a result, they predict that any country could have endogenously generated its own industrial revolution and the resulting transition to sustained growth; all that was needed was a sufficient passage of time.

In contrast, most economic historians argue that local conditions were important in generating the West’s Industrial Revolution and the sustained growth that it ushered in. There is debate about the proximate causes but most of these are to be found in things that distinguished the West from the rest - i.e., they are context specific.\(^{138}\) My co-authors and I emphasise Western science as an obvious necessary condition for the Second Industrial Revolution in the later part of the 19th century - a body of knowledge that was absent everywhere outside of the West. Somewhat more controversially, we argue that Western science in the form of Newtonian mechanics, was a necessary condition for the First Industrial Revolution - a body of knowledge that was also absent outside of Europe and best established in Britain.

**No interest in technological details**

Robbins forcefully argued what is still a commonly accepted view about the irrelevance of any detailed knowledge of technologies for understanding the growth process. “The technique of cotton manufacture, as such, is no part of the subject-matter of Economics…” (1935, p.33). “Economists are not interested in technique as such” (1935, p.37-8). “The precise shape of the early steam engine and the physical principles upon which it rested are no concern of the economic historian as economic historian — although economic historians in the past have sometimes displayed a quite inordinate interest in such matters” (1935, p.41).

This is still a common view so that most students and many theorists of economic growth do not see any need to have the kind of knowledge found, for example, in Usher’s *History of Mechanical Inventions* (which is the book I suspect Robbins had in mind in labelling such interest “inordinate”). The opposing view is that technical change lies at the heart of long term economic growth and that to understand such growth sufficiently to develop policies to influence its magnitude and direction requires a detailed knowledge of technologies and of how they change such as is found in the writings of Nathan Rosenberg and Alfred Chandler Jr. (See for example Rosenberg (1982 and 1994) and Chandler (1977 and 2001).)

Here are just two examples of this non-Robbinsian view. We argue in Bekar and Lipsey (2004) and Lipsey et al (2005) that the transition to sustained growth brought about by the two Industrial Revolutions was to a great extent the result of the culmination of three trajectories of technological advance that combined scientific and technological developments over several centuries. The first was the steam engine whose modern trajectory began in the 16th century with investigations into the nature of steam and of

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\(^{138}\) One exception is Kenneth Pomeranz (2000) who argues that there was at least one other country, China, that could have endogenously generated its own industrial revolution and sustained growth. Our reasons for dissenting from this view are spelled out in detail in Lipsey et al (2005, Chapter 8).
vacuums and culminated with the development of the high pressure engine at the beginning of the 19th. The second was automated textile machinery whose research program was charted and begun by Leonardo di Vinci late in the 15th century and culminated when the centuries-long trajectory of inventions and improvements produced machines that it paid to transfer from cottages to proto-factories in latter part of the 18th century. The third was electricity whose modern development began with the publication of Gilbert’s *De Magnete* in 1600. This put the West decisively ahead of China in understanding magnetism and electricity by making it a science rather than a piecemeal collection of individual observations. (For full discussion see Pumfrey (2002).) The trajectory evolved through countless discoveries and applications and culminated with the invention of the dynamo in 1867, which ushered in the electronic age in which we are still living. We describe these three critical trajectories in detail in Lipsey et al (2005), pages 243-4 for mechanized textile machinery, 249-52 for the steam engine and 254-5 for electricity. To understand them fully, why, how, and when, they occurred, why they did not occur outside of the West, and why they turned episodic into sustained growth, one needs to know a lot about technologies, including much of what is in Usher’s great book.

For a second example, Vernon Ruttan (2006) has argued that recent changes in US institutions make it increasingly difficult for the US to develop of new general purpose technologies (GPTs), which are the main engine of long term growth. One cannot assess the strength of his argument, or its policy implications if it is true, unless one knows a lot about the technical details of GPTs—not just the kind of abstract models that are found in Helpman (1998), useful through they are; one needs to know the engineering details of GPTs and how they evolve over decades to become highly efficient, and universally used for multiple purposes. (For a discussion of Ruttan’s argument see Lipsey 2007b.)

5. Conclusion

Although most modern economists would reject Robbins’s belief that economic theory can develop propositions that are simultaneously certain and empirically relevant, many accept, either explicitly or implicitly, other of Robbins’s beliefs, some of which I have criticised in this paper. Many assumptions, such as maximising behaviour, are treated as self-evident propositions on which theory can be safely erected. Facts are often used to illustrate rather than test theories, as when tracking known facts is the only test that a new theory or model is asked to pass. Some theoretical propositions are taken as universally correct in the absence of any context-specificity, such as that removing a ‘market distortion’ will inevitably increase efficiency and/or economic welfare. Theories of growth devoid of any characteristics that would distinguish one economy from another are often thought to be satisfactory and generally applicable, as for example is the case with unified growth theories (UGTs) and those models of growth that are based on a neoclassical aggregate production function either with exogenous or endogenous technical change. Debates about the causes of, and policies to affect, long term growth driven by technical change are often thought capable of being dealt with satisfactorily without any knowledge of the details of either the nature of technologies or the processes by which they change.
References


Defining Economics: the Long Road to Acceptance of the Robbins Definition

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Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses.
(Robbins 1932, p. 15)

1. Introduction

The sentence in which Robbins defined economics as dealing with the relationship between ends and scarce means is central to the arguments he made in his Essay on the Nature and Significance of Economic Science. Though Robbins minimized the novelty of his definition, stressing its roots in continental traditions, it had radical implications. It influenced both what economists believed they could and could not say in their role as economists and it figured prominently in discussions of the role of theory in economics. The reason the Robbins definition has such radical implications was that, in contrast to previous “classificatory” definitions, such as that economics is the study of the production and use of wealth, or the study of what contributes to economic welfare, his definition was “analytical”: it identified an aspect of behaviour (Robbins 1932, p. 16). This had the implication that, insofar as it deals with the influence of scarcity, “any kind of human behaviour falls within the scope of Economic Generalisations. ... There are no limitations on the subject-matter of Economic Science save this” (ibid.). This laid a foundation that could be seen as justifying not only the narrowing of economic theory to the theory of constrained maximization or rational choice but also the “imperialism” of economists’ ventures into the other social sciences. Given the importance of these issues, it is not surprising that scholars have paid attention to the origin of the definition (see Howson 2004) and to the methodological conclusions that Robbins drew from it. However, the question of how Robbins definition was received has been completely neglected. Here, we seek to fill that gap.

• Though Robbins’s definition is often presented as self-evidently correct, whether as a depiction of the economic problem faced by individuals or societies, both the definition and the developments that it has been used to support were keenly contested. This is perhaps not surprising given the irony

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139 This wording is chosen to avoid commitment on whether maximizing behavior and rational choice are or are not synonymous, or whether Robbins definition was in fact used to justify such approaches. The latter is, in part, the subject of this essay; the former is not relevant for our current concerns.
of the definition having been proposed when the world was at the deepest point of the worst depression ever encountered in the capitalist world. In 1932, it may have seemed counter-intuitive (to put it mildly) to argue that economics involved working out the implications of scarcity, at least at the societal level, where the pressing economic problem was a glut of capital and labor.140 It should, therefore, not be surprising to find that the notion that economists instantly recognized the Robbins definition as an appropriate summary of their discipline is a myth. We argue that it was not until the 1960s that the definition came to be accepted, and even then endorsement of it was far from universal.

• Our approach is to focus on explicit discussions of the Robbins definition. We start by analyzing the academic journals, considering first the initial reception of the definition and then discussions from the late 1930s to the 1950s, the period when economists began to see themselves as modelers. We next turn to textbooks, on the grounds that this is where economists typically encounter definitions of their subject. Finally we examine the 1960s and after, the period when economists began to apply their methods to topics traditionally considered to like outside economics. Conclusions are then drawn.

2. The Definition and its Initial Reception

Most discussions of the definition of economics comprise a few paragraphs in an elementary textbook. The Robbins definition was different in that the definition was explained and its implications worked out in an argument that extended over 141 pages (158 in the second edition) drawing on material that Robbins had developed over several years (see Howson 2004). This gave his definition an importance that other definitions did not have. The definition assumed a clear demarcation between ends and means, making it clear that economic science dealt only with the latter; the policy maker might need to consider ends, but they were not part of economics. Economics was concerned with the implications of scarcity, which had implications for the meaning of economic laws and how they should be derived. The definition therefore provided the basis for a discussion of methodology that extended ran to three chapters. It also enabled Robbins to examine the bearing of economic science on practice: what it was and was not legitimate for economists to say in their role as economists. Thus although others had offered definitions of the subject, and there were already some classic texts on methodology (notably those of John Stuart Mill and John Neville Keynes) no one had attempted to provide such a tightly-argued analysis of the subject: all seemed to follow from this simple, one-sentence definition of economics.

140 But see the discussion of Samuelson’s *Economics*, as well as note 14, below. This also raises the issue of who is the “agent” facing scarcity. In introducing his definition, Robbins (1932, pp. 12-16) focused entirely on individuals, yet elsewhere (1935, p. 155) he could write about decisions facing society under conditions of scarcity. As we shall see, definitions of economics that are used in the literature after Robbins refer sometimes to society and sometimes to individuals. In the period we are considering, views on the relationship between the individual and the society were were far from homogenous and changed significantly.
Robbins (1932, p. viii) said that he made “no claim whatever to originality” and that his definition did no more than sum up the way economists thought about their discipline. He claimed that his propositions were based “on the actual practice of the best modern works on the subject” (ibid.). That of course rested on a judgement about what those “best modern works” were, for he was influenced very strongly by, among others, Ludwig von Mises and Philip Wicksteed. In an age when Anglo-American economics was still very strongly influenced by Marshallian economics and, at least in the United States, institutionalism, this was a strong assumption. Shortly after the publication of the Essay, against the background of the Great Depression and then the Second World War, economics went through the upheavals associated with the Keynesian revolution and the rise of the econometric movement. Thus whilst the book was welcomed, it was controversial and acceptance of the Robbins definition was much more circumscribed than is commonly believed. This is hardly surprising, for the book came at a time when the subject was in turmoil.

The reviews in the major academic periodicals indicate some of these differences very clearly. It is hardly surprising that Edwin Cannan (1932), in the Economic Journal, should defend his own definition of economics as dealing with the production of wealth—the definition that, in one form or another, had dominated the subject since the late eighteenth century. A review in the American Economic Review had to wait until the second edition, when Harvey Peck (1936) observed that Robbins’s view of economics (“the individual exchange variety”) was too narrow and needed supplementing. Interestingly, the strongest praise for the book came from outside economics. George Catlin (1933), in the Political Science Quarterly, described the Essay as a “brilliant book,” protesting against empiricism, and he commended it to students of both economics and politics. Interestingly, in view of the “economics imperialism” that came many decades later, his objection to Robbins’s definition was that it included politics! To find support for the conventional view that the book contained little that was new, we have to go to an unsigned review in the Journal of the Royal Statistical Society (M 1933), where the reviewer explained that Robbins was presenting the view of the Austrian school and that this view was not universally accepted.

Robbins’s Essay came early in a decade during which economic methodology was widely discussed. Many of these discussions took up issues raised by Robbins, of which three are relevant here: economic theory versus empirical analysis; how economic theory is to be conceived; and the role of ethics in economics. These

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141 Cannan (1914). For a survey of the evolution of the definition of economics, see Backhouse and Medema (2008).
142 It has been suggested that the reviewer was H. W. Macrosty.
143 Book-length treatments in the years following the publication of Robbins's Essay include Souter (1933b), MacFie (1936), Beveridge (1937), Fraser (1937), Wooton (1938), and Hutchison (1938). See also the articles by Fraser (1932, 1938), Souter (1933a), Spengler (1934), Knight (1934), Parsons (1934), Hutchison (1935), Machlup (1936), Leontief (1937), Ayres (1938), Durbin (1938), Harrod (1938), and Bye (1939). We leave open the extent to which this literature was a response to Robbins, though all of this did lead Robbins (1938) to seek to disentangle what he called the “Live and dead issues in the methodology of economics.” Note that there is a significant literature on such problems before the Essay. On the other hand, Machlup (1936, p. 39) for one sees at least parts of this literature as a response by economists who, whatever their attitude towards laissez-faire in economic policy, want laissez-faire to apply to their own practices, and object to the “regimentation” of their subject offered by Robbins.
themes reflect important developments in inter-war economics. This was the period when institutionalism and neoclassical economics presented different ways of doing “scientific” economics (cf. Rutherford 1999). It was also a period when mathematical theory, though still a minority activity, was rapidly developing on both sides of the Atlantic, and economics was on the verge of coming to be seen as social engineering. There was also significant disagreement, going well beyond any disagreement over inter-personal utility comparisons, about the role of ethics in economics.

Robbins was seen by many as defending economic theory against empiricism, leaving little room for the historical and statistical work favoured by the institutionalists (Catlin 1933, p. 463; Fraser 1932). Knight (1934, p. 237-8, 225) shared this aversion to quantification, but saw Robbins as being too mechanical and argued for a more organic conception of economics. Others argued that Robbins had failed to acknowledge sufficiently the importance of empirical work, as when Hutchison (1935) criticized him for claiming that it was a virtue that the propositions of economic theory must be true: if they were tautologies, theory could have no empirical content and induction was needed.

The charge that Robbins’s definition reduced economic theory to something purely formal was expressed most forcefully by Dobb (1933, p. 590):

“Professor Robbins, who has carried this contemporary fashion to its logical conclusion, explicitly emphasises the purely formal character of economic theory, without, however, seizing the full implications of this statement. Economics, as a theory of equilibrium, he points out is unconcerned with norms and ends: it is concerned solely with constructing patterns for the appropriate adaptation of scarce means to given purposes. The corollaries of economic theory do not depend on facts or experience of history, but “are implicit in our definition of the subject-matter of Economic Science as a whole.”

Thus, although some writers saw merit in formalism as defined here (M 1933, Parsons 1934), others (Souter 1933b, p. 377 et seq., Janes 1933, Parsons 1934, p. 536-7) were critical. Knight, as has already been pointed out, considered that it reduced economic theory to something mechanical, while Harrod (1938, p. 407) argued that models hid, among other things, how little economists knew about causal sequences, implying that economic theory did not justify the claims to certainty that Robbins made.

Even stronger were the criticisms of Robbins’s attempt to exclude ethics from economic science. Fraser (1932, p. 557) argued that Robbins’ view implied that rationality was an end in itself. Others (e.g., Spengler 1934, p. 315) pointed out that ethical judgments were needed if policy conclusions were to be drawn, and to claim that conclusions could be derived from pure theory was to smuggle in ethical judgments in a way that was misleading. Harrod (1938, p. 396) challenged economists to accept what common sense told them – that the marginal utility of income to the rich and the poor was different, irrespective of whether such judgments

\[144\] Dobb is quoting from Robbins (1932, p. 75).

\[145\] His criticism of mechanical theories came out even more forcefully later in his review of Hutchison (Knight 1940).
were thought “unscientific.” Economics, for Harrod, was not so well developed that it could afford to dispense with such common-sense judgments.

In light of these controversies, it is not surprising that, in 1939, Bye could observe that there was still little agreement on the definition of economics. Indeed, some economists went so far as to question the notion that there could or should be a single definition of the subject (see Fraser 1932).

3. Modelling and the Acceptance of the “Received View”

The debates over Robbins’s definition in the 1930s took place against a background that was very different from that prevailing after the Second World War. After the war, many more economists thought of themselves as modelers. Though this shift was due in part to the wartime association of economics with operations research and economic engineering, there was no sudden transformation. Economics in the “old” style carried on in the post-war period, and this was reflected in discussions of Robbins’s *Essay* in the late 1940s. Joseph Spengler observed in 1948 that there was still no agreement on the subject matter of economics. He cited four definitions: that economics is what economics does; Robbins’s definition; Fraser’s definition in terms of wealth, but seen as consistent with belief in scarcity; and Parsons’s definition as concerned with the ramifications of economic rationality (1948, p. 2-3).

The implications of the Robbins definition for pure theory versus empirical analysis were still being debated in the post-war period. Gruchy (1949) argued that Robbins’s definition implied pure theory, and contended that, though pure theory was needed, economics was broader than this. He found such a broader view of the subject in the Cambridge tradition represented by Marshall and Keynes: in their hands economics was both abstract and humanistic. Keynes had managed to achieve a perspective on economics that came between Robbins’s formalism and Veblen’s cultural perspective. The methodological narrowing at work here was called in for some particularly strong criticism by Schuller (1949, p. 440):

> This “oyster” view of the field of economic inquiry, in the form of a school centrisim, unfortunately is not confined to the [Marxist] Soviet Empire and its representatives abroad. In the western world today there is another school of economic theory which has been so sanctified by traditional acceptance and has remained so impervious to attack by its rivals here (including the Marxists), that it is identified, in the vocabulary of most of our economists, with economic theory as such. The members of this school regard themselves not as a school but as the only practitioners of the economic profession; ... they dismiss critics ... for being non economists – i.e., “sociologists,” “psychologists,” or “historians.”

The example that he selects is Robbins, who “solves problems and enunciates verities not from the viewpoint of himself or his school but ‘from the point of view of Economic Science’.” This was hardly intended as a complement, as Schuller went on...

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146 See Backhouse (2002: ch. 11); Morgan (2003).

147 A twist on Viner, who reputedly defined economics by saying “economics is what economists do.”
to opine that, “Such passages by orthodox economists or Marxists often sound like variations on the ancient theme: ‘It is not I, a human sinner, that addresses you, but God, speaking through the mouth of His prophet.’” The strength of this attack is worth noting, for it suggests that the author felt under threat.

Evidence that the Robbins definition may by this time have been gaining wider acceptance in the journals is found in a piece by Gerhard Tintner (1953), a member of the Cowles Commission, who, in the course of trying to define economics, simply gives two definitions of economics—one from Robbins and the other from Lange (1945, p. 19), whose definition was the “science of the administration of scarce resources in human society.”

The theme that the choice of ends could not be ignored was carried through into the post-war period. Howard Ellis, editor of the AEA Surveys of Economics, saw economics as “concerned with the processes and results of free choice on the market,” deliberately defining it in such a way that choice of end fell within the subject (1950, p. 3). The implication of this line of argument was that economics should be broader than the Robbins definition allowed. Thus McConnell (1955, p. 160-1) argued that economics is about finding patterns but also about evaluating rules, institutional and ideological contexts, and the desirability of changes. As such, it includes not just “theoretical or analytical economics” but also welfare economics and other branches that took it beyond what Robbins considered economic science.

This period also saw a significant shift towards concern with prediction, testability and choice of assumptions. This change was hastened by Paul Samuelson’s *Foundations of Economic Analysis* (1947) and became even more marked after Friedman’s (1953) forceful advocacy of prediction as the ultimate test of theory. Samuelson’s focus was on the importance of deriving “operationally meaningful theorems.” Not only did he not define economics (though the reader would infer that it related to maximization), he argued that “logically there is nothing fundamental about the traditional boundaries of economic science” (Samuelson 1947, p. 9). His argument ran in terms of what should be taken as exogenous or endogenous; government, for example, might be in either category depending on the needs of the problem in hand. Gruchy (1949, p. 249) portrayed Samuelson as adopting the Robbins view. Others, however, saw a clear difference between them. Papandreou (1950), adopting Parsonian terminology, argued that *Foundations* showed that the “action” framework could be used in a different way: where Robbins ended up with pure theory, Samuelson used the framework to derive empirical propositions. Robbins had confined himself to the “back room” of pure theory, whereas Samuelson was started on the process of integrating the “front room” with concepts from the back room. Such a view agreed with Samuelson’s own remark that Robbins’s claims for deductive theory were “exaggerated” (1964, p. 736).

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148 Lange's article is notable for the absence of any citations, including to Robbins. Rothbard (1957, p. 314) is the only economist outside the Cowles Commission who supported Robbins unconditionally. He welcomed him as a fellow praxeologist.

149 See also Streeten (1950), Lachmann (1951), and Shahan (1952).

150 Hicks (1960) had also argued that because economics involved evaluating alternative institutions, welfare economics must fall within the boundaries of the subject.

151 Friedman focused exclusively on the meaning of “positive” and not of “economics.”
The way discussions of Robbins had changed by the 1950s is best illustrated in Machlup’s (1955) article on verificationism. Robbins was listed as an advocate of “extreme a priorism,” this being contrasted with Terence Hutchison’s “ultra-empiricism”: the former regarded economic theories as certain, whereas the latter required that assumptions as well as implications be tested. The assumptions of a theory should be understandable, but induction was useful in giving confidence in hypotheses. At the same time, it was going too far to claim that all assumptions needed to be verified, for theories and models were analytical devices used for generating testable predictions. What Machlup was doing was combining an Austrian element with what had become the so-called “Received View” in philosophy of science, centered on the hypothetico-deductive model. It offered a defensible position between Robbins and his empirical critics that many economists found attractive. Broadly interpreted, it could encompass both Friedman’s (1953) methodology and that of his one-time rivals at Chicago, the Cowles Commission, exemplified by Koopmans (1957).

By the early 1960s, though economists might still question whether the Robbins definition was adequate, it had come to be widely accepted. The general tone was to refer to it as being accepted and not to argue the case for it. Thus Johnson (1960, p. 552) started a very sympathetic review article on J.K. Galbraith’s *The Affluent Society* (1958) with the concession that most economists “would probably accept” the Robbins definition, “at least as a description of their workaday activities.” Its resonance with modern mathematical methods was noted by John Hicks (1960, p. 707) in his review of linear theory, where he argued that these techniques provide the tools necessary to cover economics as defined by Robbins—implying, without stating so explicitly, that this definition is the appropriate one to use. At the opposite pole, we even find institutionalist William Kapp (1968, p. 2) allowing that, in his estimation, Robbins’s definition “characterizes very well the prevailing preoccupations of many economists.” Finally, when Albert Rees penned his entry on “economics” for the *International Encyclopedia of the Social Sciences* (Rees 1968, p. 472), he gave a definition that was pure Robbins, calling it “widely accepted.” And so, a definition that very few economists had been prepared to endorse unequivocally in print had apparently come to be generally accepted.

4. Textbooks

Robbins may have denied that his ideas were novel, but there was little trace of them in the leading Principles texts of the day. The oldest, and most influential, was Marshall’s *Principles of Economics*, renowned for defining economics simply as “a study of mankind in the ordinary business of life” (1920, p. 1). The next oldest, by R. T. Ely, the first President of the AEA, assisted by a series of co-authors, of whom

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152 Despite his view that scarcity permeated economic theory, Johnson went on to review Galbraith’s argument that society’s problems concerned its opposite—opulence—with considerable sympathy. This opulence argument was thought by some to be a legitimate critique of the Robbins definition, or to imply that it was at the very least outmoded. (See also Beckerman 1956.) Yet, as Johnson and Bronfenbrenner (1962, p. 255), and others pointed out, the issue is not the size of the physical stock of goods at some time or place, but rather the amount of the good that would be demanded at a zero price. By this definition, the problem of scarcity had been no more overcome by societal affluence than it was absent during the mass unemployment during the Great Depression.

153 Marshall was extensively cited in the literature discussed in this paper. That is true of no other textbook writer.
the most prominent was Allyn Young, of Harvard, offered a more traditional definition: “economics is the science which treats of those social phenomena that are due to the wealth-getting and wealth-using activities of Man” (Ely et al., 1926). Sumner Slichter offers an institutionalist definition: “The subject matter of economics is industry ... [studied as] a complex of human practices and relationships” (Slichter 1931, p. 11). Other textbooks offered no definition, either providing an illustrative list of economic problems (Garver and Hansen 1928) or arguing that none was required (Taussig 1927). The closest thing to Robbins in the English-language textbook literature of the time seems to be the definition offered by Fairchild, Furniss, and Buck (1926), who, having identified “the insatiability of man and the niggardliness of nature” as “the foundation stones upon which rests the structure of economics” (1926, p. 8), define economics as “the science of man’s activities devoted to obtaining the material means for the satisfaction of his wants” (p. 8). Some of these texts were revised after Robbins’s Essay appeared, but none adopted his definition.\footnote{For example, Ely’s text (now become Ely and Hess 1937), is interesting as one of very few to provide a bibliography, dropped Mill, Cairnes and John Neville Keynes from its bibliography, and brought in Robbins’s Essay. However, it offered exactly the same definition as the 1926 edition. By the fourth edition of their Economic Problems of Modern Life, Patterson and Scholz (1948, p. 3) had expanded their “study of business” definition to also include “the social study of wealth and welfare” and “the study of pecuniary values” (supply and demand), but still had gotten no closer to Robbins.}

If the older generation cannot be expected to have taken to Robbins with enthusiasm, the same cannot be said of those writing new textbooks after 1932. Frederic Benham, a fellow student and later a colleague of Robbins at LSE, defined the subject implicitly in his first edition, saying that “the rationale of economic activity is to satisfy human wants by producing consumers’ goods” (1938, p. 5). In his third edition, he came even closer to Robbins, referring to economic decisions as choices that involve opportunity costs (1943, p. 5), but he neither reproduced the Robbins definition nor cited the Essay. Alec Cairncross (1944, p. 8) explicitly adopted a definition in terms of scarcity, but qualified it by saying that economics was concerned only with the social aspect of scarcity: when one person’s decisions impinge on other people. Furthermore, his wording, “Economics is a social science studying how people attempt to accommodate scarcity to their wants and how these attempts interact through exchange,” restricts the subject to an exchange economy. We see a similar pairing of scarcity and exchange from Nevin (1958, p. 6), while Thomas (1952) reverted to the materialist definition in terms of wealth. The Robbins definition clearly influenced this generation but it was heavily qualified.

If anything, North American textbooks remained further from Robbins in this period. In The Economic Organization, Frank Knight says that economics “deals with the social organization of economic activity,” lately via the price system or under free enterprise (1933, p. 4). Knight found both the traditional definitions, such as Marshall’s and the choice-based ones, over-broad and, as a result, “useless and misleading” (1933, p. 2).\footnote{“Many definitions of economics found in text books fall into this error of including virtually all intelligent behavior. One writer has actually given as his definition of economics the ‘science of rational activity.’ Others find its subject matter is ‘man’s activity in making a living,’ or ‘the ordinary business of life.’ Such definitions come too near to saying that economics is the science of things generally, of everything that men are for practical reasons interested in. Such a definition is useless and misleading” (1933, pp. 1-2).} In sharp contrast to the view that later became associated with the Chicago school, Knight insisted that “economizing … does not include all...
human interests” and that it is both “error” and “vice” to “look upon life too exclusively under this aspect of scientific rationality” (1933, p. 2).\footnote{Knight goes on to say that, “Life must be more than economics, or rational conduct, or the intelligent accurate manipulation of materials and use of power in achieving results. Such a view is too narrow” (1933, p. 2).} Kenneth Boulding’s (1941, p. 3) approach to the issue was not unlike Knight’s: he told his readers that, in terms of subject matter, defining economics “as the study of human valuation and choice” is probably “too wide.”\footnote{He also considered Marshall’s definition over-broad, but “the study of material wealth” and “the study of that part of human activity subject to the measuring rod of money” (Pigou’s definition) too narrow (1941, p. 3). Boulding carried these characterizations through subsequent editions of his book. See, e.g., Boulding (1955).} Perhaps reflecting the sort of frustration hinted at in Knight and Boulding, Tarshis (1947) explained that a formal definition is not appropriate and instead lists economic problems.

There was a move towards a Robbinsian perspective in the most influential text to appear in the late 1940s, Samuelson’s Economics (1948) in that Samuelson argued that economics deals with the relationship between means and ends, and ends are not part of the “science.” He also used the argument that the “American way of life” requires more resources than are available (1948, p. 16-17) to counter the claim that the existence of unemployed resources showed that resources were bit scarce. But he did not cite the Robbins definition and his focus, “What? How? For Whom?” resonated equally with more traditional definitions in terms of the production and distribution of wealth.

Some other texts explicitly used scarcity definitions. Most prominent was George Stigler’s The Theory of Competitive Price, which was unusual in actually citing Robbins’s Essay (1932, p. 13, 20). Stigler defined economics as “the study of the principles governing the allocation of scarce means among competing ends when the objective of the allocation is to maximize the attainment of the ends” (1942, p. 12). Stigler was not alone here. Gemmill and Blodgett (1937, p. 22) defined economics as “the social science that describes man’s efforts to satisfy his wants by utilizing the scarce means provided by nature,” and Bowman and Bach (1946, p. 3) cited “economizing,” arising from scarcity, as the “central problem” of economics.

Scarcity definitions of economics became more prominent in both countries from the late 1950s onward, though in the UK it was not uncommon for authors to broaden this to emphasize the social character of the subject and relate it specifically to exchange. Given the way Becker was at this time extending the boundaries of economic reasoning (not to mention later Feminist critiques of economics), it is significant to note the example Nevin (1967, p. 5-7) gave of the hazards of moving beyond the exchange context: “But who can assess the value of a mother’s services to her family in the home? The economist, at least, is too sensible to try.”\footnote{We leave it to others to unpack the gendered stereotypes involved. Note that though Becker’s work on the family came later, his analyses of discrimination, education, and the allocation of time had appeared by this time.}

In the USA, on the other hand, economics came to be defined in terms of scarcity, with fewer qualifications\footnote{See, e.g., Snider (1962); Keiser (1965); Fels (1966); Leftwich (1969); McConnell (1969).}—to the point where Bronfenbrenner could state already in 1962 that “Most of the current crop of textbook definitions of the subject stress...
the scarcity hypothesis (p. 266). McConnell, in a textbook that went through four editions during the decade, offered a definition that is worth citing in full:

“Recalling that wants are unlimited and resources are scarce, economics can be defined as the social science concerned with the problem of using or administering scarce resources (the means of producing) so as to attain the greatest or maximum fulfilment of society’s unlimited wants (the goal of producing)”. (McConnell 1969, p. 23)

Though it is tempting to ascribe this to the influence of the Robbins view, it is worth noting the emphasis on social science and the use of the word “administering,” echoing Lange’s definition, cited above.

The two most influential texts of the 1960s and 1970s were Samuelson’s *Economics* and Richard Lipsey’s *Introduction to Positive Economics*. Samuelson, whose *Economics* continued to go through new editions, remained the most important, not least because of its use internationally. By the fifth edition (1961), the comment on the Great Depression not being a counter-example to the prevalence of scarcity was gone, and there was now a section on the “Law” of scarcity. And when we come to the tenth edition (1976), written with Peter Temin, Samuelson was offering what can only be described as an expanded version of the Robbins definition, which comes after five alternatives have been considered:

“Economics is the study of how people and society end up choosing, with or without the use of money, to employ scarce productive resources that could have alternative uses, to produce various commodities and distribute them for consumption, now or in the future, among various persons and groups in society. It analyzes the costs and benefits of improving patterns of resource allocation”. (Samuelson and Temin 1976, p. 3)

Much of the expansion is purely expository, not changing the force of the definition at all (with or without the use of money; now or in the future), though the last sentence begs the question of how, if ends are not part of economics, one distinguishes between those changes in patterns of resource allocation that are “improvements” and those that are not. If economics is broader than “economic science,” this is not explicit.

The main new textbook, certainly judged in terms of sales, to emerge in the 1960s was Lipsey’s *Introduction to Positive Economics* (1963). Lipsey is an important figure because, though he came out of LSE, he was part of a group of young economists committed to replacing the Robbinsian emphasis on deductive theory with an economics based on measurement and testing. This was the meaning of the “Positive economics” in his title. In the first three editions, he defined economics not with a single definition but by listing six economic questions, the closest he came to the Robbins definition being to refer to “one of the basic problems encountered in most aspects of economics, the problem of SCARCITY” (Lipsey 1971 (3rd edition):

Though the use of capital letters implies that scarcity was a fundamental concept that the student must understand, the words “one of” and “most” make it clear that not the whole of economics. The view here was that economics cannot be deduced, as Robbins had claimed, from one basic postulate, but has to be empirical. In the fourth edition (1975), this rejection of Robbins was even more explicit. After discussing six economic questions, he went on:

“Economics today is regarded much more broadly than it was even half a century ago. Earlier definitions stressed the alternative and competing use of resources. Such definitions focused on choices between alternative points on a stationary production-possibility boundary. Important additional problems concern failure to achieve the boundary ... and the outward movement of the boundary over time” (1975, p. 59).

Lipsey distanced himself from Robbins (without naming him) by associating him with the past, and implicitly criticizing other textbook writers who were using the Robbins definition. Unemployment and growth are signalled as important economic questions from which an exclusive focus on scarcity deflects attention.

Subsequent textbooks, too numerous to survey, offer variations on the themes encountered up to now. Scarcity is clearly considered central to economics, though the emphasis placed on it has varied; it is not always clear whether this is because authors see that it is limited in scope, or whether it is to make what might be a very abstract definition of the subject more digestible to newcomers to the subject. The Robbins definition appears to be in the background but to have been blended with other ideas. Significantly, it did not become universally accepted, Lipsey’s textbook being the clearest example. Even today, Krugman and Wells (2004, p. 2) define economics as “the study of economies,” which is decidedly un-Robbinsian.

5. Economics Spread Its Wings

By the 1960s, the Robbins definition was becoming established in the textbooks, but there was little discussion of it in the journal literature, suggesting either that it had become accepted, or that it was simply something that was irrelevant to practicing economists. What changed in the 1960s and 70s was a progressive expansion of the boundaries of the field—an expansion that was at once consistent with Robbins’s definition and yet reflected a view of the discipline that was likely far beyond anything Robbins might have imagined in 1932. As Susan Howson (2004, p. 417)

161 The page number is significant. Unlike all the other texts, discussion of economics was preceded by a lengthy but non-technical explanation of the principles of modeling, including statistical methods, and the philosophy of science. The latter was Popperian rather than the Received View cited by Machlup (see above).

162 It was not as though older definitions had been eradicated, however. Peach (1965, p. 15), for example ascribes his definition, that “Economics is the study of, and embraces all knowledge relevant to, the production of goods and services,” to Keynes. Yet, he cannot resist pointing out a few pages later that “Economics has long been connected in people’s minds with the notion of scarcity.” (21) And Hailstones, writing in 1968, defines economics as “a science that is concerned with the production, distribution, and consumption of goods and services” (1968, p. 2).

163 Robbins returned to the Essay in his 1980 address to the American Economic Association, but he did not comment on the expanding boundaries of economics, though he did reiterate his views on the appropriateness of his definition of the subject (1981, pp. 2, 9).
has shown, Robbins was concerned to distinguish economics from the other social sciences—to show where the boundary between them properly lay. More recent work, in contrast, has been focused on showing that such a boundary does not exist. When Milton Friedman wrote in 1962 that “An economic problem exists whenever scarce means are used to satisfy alternative ends,” he went on to note that this is “a very general” conception, one that “goes beyond matters obviously thought of as belonging to economics.” But such is the breadth of economics today that his example of the allocation of leisure time seems rather quaint and leads one to wonder why there would ever be any fuss over excessive generality (Friedman 1962, p. 6). We also find Harry Johnson—like Friedman, a professor at Chicago—stating in his LSE inaugural lecture that he considered “scarcity” and “choice” to be the two basic conceptual ideas underlying economics (1968, p. 3-4)—invoking Robbins in the process—and going on to argue that economics had become more useful because of the new work being done within this framework in areas including human capital theory, the economic analysis of time, the economics of information, and the economic theory of democratic political processes. And already in 1968, Kenneth Boulding was referring in his AEA Presidential Address to “the attempt on the part of economics to take over all the other social sciences,” a movement that he labeled “economics imperialism” (1969, p. 8).

While several scholars contributed to these early forays into areas previously considered non-economic, the process was led by Gary Becker, who, in the late 1950s and 1960s had begun to advance the case that economic theory could be used to understand phenomena such as discrimination, irrational behavior, human capital, and issues in crime and punishment. Becker did not feel compelled to justify these early excursions by appealing to any sort of definition. In fact, in his classic article on crime and punishment, Becker suggested that economists may have avoided discussing illegal activity, not because it did not fall within the boundaries of economics, but because it was “too immoral to merit any systematic scientific attention” (1968, p. 170n1).

When Becker defined economics as “the study of the allocation of scarce means to satisfy competing ends” in his Economic Theory (1971), he immediately remarked on the breadth of this definition, noting that “It includes the choice of a car, a marriage mate, and a religion; the allocation of scarce resources within a family; and political discussions about how much to spend on education or on fighting a Vietnam war” (1971, p. 1). He later pointed out that this definition was so general that economists often found it an “embarrassment,” needing to be qualified “to exclude most nonmarket behavior” (1976, p.4). Becker acknowledged that economists tend

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164 This definition is repeated verbatim in his 1976 revision of the book.
165 Friedman was providing this definition in his lectures at least as far back as January 1947. See Friedman (2008/1947).
166 Johnson was referring to the work of economists such as T.W. Schultz, Gary Becker, George Stigler, and Anthony Downs.
167 This was not a new phrase, having been used, by Souter already in the 1930s: “The salvation of Economic Science in the twentieth century lies in an enlightened and democratic ‘economic imperialism’, which invades the territories of its neighbors, not to enslave them or to swallow them up, but to aid and enrich them and promote their autonomous growth in the very process of aiding and enriching itself” (1933b, p. 94).
168 In his The Economic Approach to Human Behavior (1976, p. 3), Becker mentioned three extant definitions of economics: “the study of (1) the allocation of material goods to satisfy material wants,
to study the market sector, but he argued that economic principles developed for this purpose were “essential” for understanding much of what had been studied by sociologists, anthropologists and other social scientists (1971, p. 2).

Becker suggested that Viner’s “what economists do” definition was emblematic of the problems with defining “a subject matter that has changed so much over time,” and, as if conceding that his definition did not seem to define a field called economics, argued that any definition of a discipline is inadequate. What is more important, he said, is “the economic approach” to human behavior:

“The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it”. (Becker 1976, p. 5)

Although this applied to all the social sciences, it could be called the economic approach because it was economists, not other social scientists, who had adopted it (something he wished to change).

Interestingly, not everyone associated with the expansion of the boundaries of economics was favourably disposed toward the Robbins definition. Both James Buchanan and Ronald Coase protested against defining the field apart from its subject matter, with Coase (1977, p. 487) suggesting that the field involves the study of “the social institutions that bind together the economic system,” and Buchanan preferring “the study of the whole system of exchange relationships” (1964, p. 220). Buchanan thought this subject of enough importance to devote his 1963 Presidential Address to the Southern Economic Association to it, and he brought the Robbins definition in for strong criticism, calling it “all too pervasive,” and suggesting that it “served to retard … scientific progress” (1964, p. 214). Robbins’s definition, he said, made economics about “a problem or set of problems” rather than “a characteristic form of human activity” (1964, p. 214). While accepting that the Robbins definition had become standard in the profession, Buchanan argued that economics should be conceived of as the study of markets, not of resource allocation, and thus that the Robbins definition should be replaced by one that emphasized market exchange, or catallactics (1964, p. 214).

Buchanan was by no means the only prominent economist to argue the case for a market exchange definition. Boulding, who came from a very different position on the political spectrum, offered a definition very similar to Buchanan’s in his AEA Presidential Address:

(2) the market sector, and (3) the allocation of scarce means to satisfy competing ends,” noting that the last of these is the most general.

169 Coase includes here firms, input and output markets, the banking system, etc.

170 Buchanan also suggests that this definition did not accurately describe the state of the field in 1930: “Only since The Nature and Significance of Economic Science have economists so exclusively devoted their energies to the problems raised by scarcity, broadly considered, and to the necessity for the making of allocative decisions” (1964, p. 214).

171 “In so far as individuals exchange, trade, as freely-contracting units, the predominant characteristic of their behavior is ‘economic’” (p. 220).

172 Both, though, had seriously engaged with political science.
Economics specializes in the study of that part of the total social system which is organized through exchange and which deals with exchangables. This to my mind is a better definition of economics than those which define it as relating to scarcity or allocation, for the allocation of scarce resources is a universal problem which applies to political decisions and political structures through coercion, threat, and even to love and community, just as it does to exchange. (1969, p. 4)\textsuperscript{173}

Unlike Boulding, of course, Buchanan favored the extension of economic reasoning to political structures (1964, p. 220), and in this sense did, like Becker, have an “outward-looking” streak. But Buchanan did not see the Robbins definition as necessary for the expansion of economics to other fields; the exchange definition could do the same, since, for Buchanan, politics is, at its heart, a collective exchange process.

Thus, while the Robbins definition may have been widely accepted, there was still significant dissent.

6. Conclusions

Tracing the influence of a definition of economics is problematic because, most of the time, economists have little reason to cite any definition of what they are doing. There is the further problem that, though it was by no stretch of the imagination a generally accepted definition of the subject before Robbins’s \textit{Essay}, the idea that economics is about scarcity was, as Robbins pointed out, far from novel. Even if an economist’s acceptance of the idea beneath the definition could be demonstrated (and this is often difficult), it would typically be hard to establish the definition’s role in his work. This problem is exacerbated by the fact that in the main place where one would expect to find discussions of the definition of economics—in introductory textbooks—sources are rarely cited. Had the initial reaction been one of, “Robbins has brilliantly expressed what we all know to be the essence of economics, but had never been able to articulate,” and had textbooks suddenly started adopting analytical definitions in terms of scarcity and choice between alternative uses of scarce resources soon after 1932, the circumstantial evidence would be strong indeed. \textit{But it was not like that at all}. Robbins’s definition of economics was challenged from the start. In the journals it was frequently attacked, and it was hardly ever accepted without qualification. Then around 1960, economists started to refer to the definition as generally accepted. What had happened was that it had gradually come to be accepted in the textbooks, though, even there, there was hardly unanimity.

If one thinks of knowledge being created in the journals and then finding its way into textbooks, this pattern is strange. It arises because one can do research in economics without being concerned with how the subject is defined, which means it is generally not discussed in journal articles. Evidence that economists were starting to accept the Robbins definition first appears in the textbooks because that is where the definition of the subject is generally discussed. However, this does not mean that it did not have

\textsuperscript{173} Peck (1936) actually said that Robbins’s definition was exchange-based. See p. 215, above. Kirzner (1965, p. 258) argues that Buchanan’s definition can be subsumed under Robbins’s definition, because the exchange process is a part of the struggle to deal with the problem of scarcity. One sees this reflected in the Cairncross (1944) and Nevin (1958) definitions, cited on p. 13, above.
significant consequences for research. The grounds on which economists objected to the Robbins definition were that it served to narrow economics, or to constrain its methods in ways that were thought unjustifiable. This was precisely because it was an analytical definition, which appeared to define a specific way to set about doing the subject. Because of this, it simultaneously both narrowed and broadened the scope of the subject. It narrowed it through suggesting that deduction could achieve more than many economists believed it could and it broadened it through freeing economists from being constrained to analyze a particular subject matter. Till around the 1970s, economists attached great importance to the latter, and many of the qualifications that were attached to the Robbins definition served effectively to confine an analytical definition to what was considered the subject matter of economics. From the 1970s, as economic methods came to be applied to social and other traditionally non-economic problems, economists became less concerned about this and the qualifications were widely dropped.

There may, however, have been another factor at work. This was that, though he did not formulate it in these terms, Robbins’ definition fitted well with acceptance of a rational choice model of behaviour. As economists learned to apply rational choice to an ever widening range of problems, the Robbins definition came to be used more prominently in textbooks. There remained division over whether economics was defined by a method or a subject matter but both sides in that debate could increasingly accept some version of the Robbins definition.

174 In Backhouse and Medema (2007) we argue that this narrowing contributed to the rise of axiomatic methods in economics, even though, and perhaps because, Robbins did not endorse such methods.

175 Support for this can be found in Stigler’s essay on economic imperialism, where he suggests that the increasing abstraction of economics, particularly via the widespread adoption of “the machine of maximizing behavior,” brought on the “imperialistic age” of economics (1984, p. 312). Coase (1977) argues along similar lines, although he, unlike Stigler, was very critical of and pessimistic about these forays into other disciplines.
References


What was “It” that Robbins was Defining?

David Colander*

Abstract

This paper argues that Robbins’s famous definition of economics was of “economic science” which he saw as only a narrow branch of the field of economics. Moreover, it was descriptive, not prescriptive, and was simply a statement that that was what economists were then doing in the science of economics. His prescriptive message was that policy belonged in the “political economy” branch of economics. He believed that while the science of economics should avoid value judgments, the political economy (applied policy) branch of economics should, and must, include value judgments. That prescriptive message has been lost.

Keywords: definition of economics, political economy, science of economics, Robbins, value judgments.

1. Introduction

There has been a renewed interest in Lionel Robbins’s famous Essay on the nature and significance of economic science (Robbins, 1932). An important reason for this interest is that currently economics is in a state of flux. The rational agent maximizing model is no longer the glue that holds the profession together, as the economics trinity of greed, rationality and equilibrium is giving way to a new trinity of enlightened self interest, bounded rationality, and sustainability (Kreps, 1997). Behavioural economics is flourishing, and new branches of economics, such as neuroeconomics, experimental economics, econophysics, evolutionary game theory, and complexity economics are developing and changing the face of economics theory (Colander, Holt and Rosser, 2004).

Today, economists distinguish themselves from other social scientists more by their ability to bring sophisticated statistical and mathematical methods to the analysis of social issues than by the model they use or the subject matter they study (Colander, 2007). If the study of “the allocation of scarce resources among alternative ends” ever was the defining nature of what economic science was, it no longer is (Colander, 2005). Because economics is changing, it needs a new definition. In thinking about this new definition, it is only natural that economists reflect those articles that have been central in shaping economists’ image of what it is that they do. Robbins’s essay is clearly one such article.

As discussed by Denis O’Brien (1988), and more recently by Roger Backhouse and Steven Medema (2007), Robbins’s 1932 Essay provoked much discussion when it was published and afterwards. Much of this discussion has focused on Robbins’s definition of economics, even though, in responding to critics, Robbins argued that his definition was not all that novel or important to the point he wanted to make. He states this explicitly in his paper "Live and Dead Issues in the Methodology of Economics” (Robbins, 1938). He writes

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“Economics, we have suggested, is essentially the study of the disposal of scarce goods and services. This suggestion has not met with universal acceptance. But the difference between this and other definitions now current is not a very serious matter.” (p. 344)

Backhouse and Medema see this downplaying as misplaced. I believe it was not, and that his downplaying the differences reflected his sense that his definition was descriptive, not prescriptive. It was not meant to tell economists that this is what you are to do when you do economics; it was what he believed that economic theorists were doing when they did the “it” that he was defining.

If providing a definition of economics was not the central point of Robbins’s article, what was? Robbins is also clear about what he thought the central prescriptive message was. In that same article he writes

“The only question with regard to the scope of economics which can be said to be in any sense alive, is the question whether economics, as such, can be said to include judgments of what is good and bad in the world of relative scarcities.” (p. 345)

Robbins’s prescriptive answer to whether value judgments belong in the “it” that he was defining is also clear; in Robbins’s mind value judgments had no place in the “it” that he was defining, no matter how “it” is defined. If you think you are doing “it” and doing “it” involves value judgments, then you are not doing “it.”

Let me now turn to the question of what “it” is. The argument in this paper is that the “it” being defined was not economics inclusive of all that economists did in their role as economists. It was a more narrowly defined “it” that included only the “economic science” portion of what economists did. That’s why he entitled the paper “The Scope and Method of Economic Science.” (my emphasis) For Robbins, the science of economics and the entire field of economics were quite different. So the “it” being defined was “the pure science of economics” which Robbins, following Keynes (1891) interpreted very narrowly. He saw this pure science of economics as only a small sub branch of economics—a branch, which in his view, almost by definition, had nothing to do with policy. He specifically saw another branch of what economists do—political economy, as the branch primarily concerned with applied policy, not with science. Here, he wanted value judgments to have free rein, and to be allowed into the analysis.

So the prescriptive message he hoped to convey was that value judgments and policy analysis belonged in the political economy branch of economics, not in the economic science branch of economics. This prescriptive message was misinterpreted by many in the profession as implying that we need to eliminate value judgments from the field of economics. That was not Robbins’s intent. Robbins was quite explicit about the need for this separate branch of economics to deal with applied policy, and he stated it repeatedly. For example, in his Ely Lecture, (Robbins 1981) he states that he favors the revival of political economy as a branch of economics. He writes

176 They write “Though Robbins portrayed himself as saying nothing original, the idea that his definition did nothing more than sum up the way economists thought about their discipline is a myth.” (Backhouse and Medema, pg 3)

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“My suggestion here, as in the Introduction to my Political Economy: Past and Present, is that its (political economy) use should be revived as now covering that part of our sphere of interest which essentially involves judgments of value. Political Economy, thus conceived, is quite unashamedly concerned with the assumptions of policy and the results flowing from them. I may say that this is not (repeat not) a recent habit of mine. In the Preface to my Economic Planning and International Order, published in 1937, I describe it as “essentially an essay in what may be called Political economy as distinct from Economics in the stricter sense of the word. It depends upon the technical apparatus of analytical Economics; but it applies this apparatus to the examination of schemes for the realization of aims whose formulation lies outside Economics; and it does not abstain from appeal to the probabilities of political practice when such an appeal has seemed relevant.

It should be clear then that Political economy in this sense involves all the models of analysis and explicit or implicit judgments of value that are usually involved when economists discuss assessments of benefits and the reverse or recommendations for policy.” (Robbins, 1981, 8)

As he states in that quote, his position that there should be two branches of economics and that policy belonged in political economy sub branch was not a position that he came to late in life. In his 1938 discussion of methodology he writes

“those who adopt this latter attitude (the view that his definition will preclude economists from being interested in policy) are concerned not so much with problems of definition as with problems of conduct or deportment. They think that the economist will sacrifice opportunities for usefulness if he abstains from excursions into social philosophy and (apparently) they think that if such excursions are not dignified by the title, economics science, he may feel precluded from making them. The warning may be necessary. But the psychology may be questioned. To me at least, it seems difficult to believe that recognition of the distinction between the two kinds of propositions will prevent any man of spirit from being interested in both.” (Robbins, 1938, 345)

2. The Context for Robbins's View

Robbins’s view of economics as consisting of both an economic science and an applied policy branch has a long history in economics. As Denis O'Brien makes clear in his retrospective essay, Robbins’s position relates back to David Hume, Max Weber, the Austrians, and Phillip Wicksteed. (O'Brien, 1988) The division was part of classical economist’s way of thinking about what they did. For example, in J. Neville Keynes’s famous Scope and Method of Political Economy (Keynes, 1891), which was the standard work summarizing classical methodology in the late 19th century, Keynes argued that it was necessary to separate out the art of economics (his name for political economy) from positive economics (his name for the science of
Like Robbins, Keynes argued that maintaining a separate positive science, which avoided value-judgments, quite distinct from applied policy, which included value-judgments, was necessary to avoid confusions about the relation between theory and policy.

Up until the late 19th century, the pure science of economics was quite small, and economists called what they did political economy, not economics. The term “economics” was reserved for economic theory, which consisted primarily of logical deductive models. Economic science for Classical economists, and for Robbins, focused on abstract theory and deductive logic, not on empirical work or applied policy. It was a very small branch.

With the dawn of the neoclassical era, the economic science branch of the study of economics started growing, as the marginalists and Austrian economists started framing the economic question in calculus and exploring the nature of constrained optimization mathematically for the whole economy through general equilibrium models. But even in the 1920s and 1930s, this formalization into a clear model was still at a very early stage. In terms of its insight for policy, it was speculation, with few conclusions. Robbins is quite clear about this and in his review of Hawtrey (Robbins, 1927), a review that included many of the ideas that would later become embodied in his 1932 essay, Robbins wrote

“What precision economists can claim at this stage is largely a sham precision. In the present state of knowledge, the man who can claim for economic science much exactitude is a quack. The problems of human motive we have to analyse with the “vast amorphous phantoms” of psychology at their back, are nebulous enough in all conscience. It is not because we believe that our science is exact that we wish to exclude ethics from our analysis, but because we wish to confine our investigations to a subject about which positive statement of any kind is conceivable.” (Robbins, 1927, 176)

One reason Robbins believed that economic science included primarily logical deductive work and not empirical work was at the time the tools and statistical methods available to do empirical work were too crude to allow scientific conclusions. Before any of the logical-deductive propositions moved from mathematic speculation to science they would have be empirically tested in a scientifically acceptable way. For Robbins, science consisted of both theoretical exploration and empirical testing of those theoretical explorations. Robbins’s prescriptive point was that propositions that were not even, in theory, empirically testable did not belong in the science of economics. Applied policy questions fit this category since they interrelated with so many non-economic issues that developing formal applied policy models that captured them were far beyond the scope of economics. So he opposed including any of that work under the mantel of economic science.

177 Keynes also distinguished a normative branch of economics where the goals of policy were to be discussed. See Colander (1999)
178 Nassau Senior (1836) was probably clearest about this when he limited the scope of economic science to purely deductive reasoning, and explicitly excluded empirical work.
In the 1930s when Robbins wrote, constrained optimization was what pure economic theorists were working on, which meant that Robbins defined the science of economics as constrained optimization. But if I am correct in saying that he meant it as a descriptive not a prescriptive definition, and if one had a time machine, and moved current economic research in neuroeconomics, econophysics, or behavioural economics back to the 1930s, based on his descriptive definition of economic science he would have not only included this work, he would have embraced this work in his definition, because it met the requirements of his definition-it is attempting to understand economic issues in a way that is in principle subject to empirical verification. (He was writing before Popper moved the debate from verification to falsification.) In Robbins (1930) he stated his strong support for empirical work writing “Clearly, quantitative exactitude is the object of all scientific inquiry, and it is only by continually testing our theories by reference to the facts of the situation that we can discover how far they proceed from assumptions that are appropriate.” (Robbins, 1930, 21).

3. Economics, Political Economy, and the Teaching of Economics

Debates about method and definitions have little direct effect on economists; they do what they do; practising economists don’t worry about methodologist’s prescriptions. Where debates about method and definitions have an effect is in the teaching of economics, or at least the way economics is presented to students. Through the texts, definitions and discussion of method they indirectly influence future economists. Thus, much of the initial interest in Robbins’s Essay and definition reflected a debate about what economists should be teaching students.

Through the 1800s what was taught was political economy-a set of ideas as they related to policy. Theory was embedded in policy discussions. With the rise of marginalist theories, and a mathematical structure for economics, there was a pedagogical debate about what part of economics to present to students. The classical position was to present a combination of theory and policy analysis under the heading, political economy. Some marginal theorists were pushing for a more formal presentation of economic theory.

One such economist was Maffeo Pantaleoni. He wrote a text, Pure Economics, (published in 1887 and translated into English in 1898) that was consciously about economic science rather than political economy. He states this explicitly in the preface, writing: “This manual is intended as a succinct statement of the fundamental definitions, theorems and classifications that constitute economic science, properly so called, or Pure Economics. Thus all questions pertaining to economic art, or Political Economy, are beyond its scope” (ivi, p. vii). He continues: “This is a departure from the lines on which textbooks of economic science are usually prepared, their authors’ objects being to equip the reader forthwith for the discussion of the most important economic problems presented by everyday life.”

179 This helps explain how he could choose the constrained optimization definition for economics even as the economy was in a depression. In Robbins’s view the work in macroeconomics that was being done did not measure up to the standards of science.
Pantaleoni’s usage of the term “economics” as being about theory was consistent with the Classical usage of the terms. However, that usage was changing, and the use of the term, “political economy” as a separate branch of economics was declining. Part of the reason for this was Alfred Marshall’s text. In 1890 Alfred Marshall faced the same problem as did Pantaleoni, with his principles textbook. But unlike Pantaleoni, he did not believe that what should be taught was only the deductive logic of the marginalist model. Marshall straddled the fence between the marginalists and the historical/institutional approach to teaching economics. He believed that economists could teach both political economy, which involved lessons for policy, and the marginalist model.

This desire to teach both left him with a problem of whether to call his book Principles of Economics, or Principles of Political Economy. While he chose the term “economics” to designate what he was teaching, he was very clear that what he called “economics” was not pure theory separate from policy, but rather a set of tools, and a “method of analysis that helps its user arrive at reasonable conclusions.” Under the older use of the term “economics”, Marshall’s “economics” would have belonged under the heading political economy.

Marshall’s approach to teaching principles blended in theoretical constructs with discussions of real world issues, showing how those theoretical constructs could shed light on economic issues. He gave little discussion of general equilibrium, with it showing up only in Note 21; he concentrated on developing partial equilibrium tools that could be used to analyze real-world policy problems. His tools embodied value judgments, as they had to, if they were to be applicable to policy, but he attempted to be clear about what these value judgments were. For him, and for many other economists of the time, the blend was science—not a pure science, but an applied, or moral science as J.M Keynes called it. (Wright, 1989, 473) Marshall was clear that he was using economic tools as rough and ready tools, not for providing definitive results, but for guiding thinking about policy issues. Consider the concept of consumer surplus, which Marshall developed as a theoretical tool to shed light on policy questions. It integrated all individuals’ welfare into an area under a curve, and thereby included the implicit value judgment that individuals’ welfare were comparable and interchangeable. Thus, Marshall’s “economics” had one foot in the science of economics and one foot in political economy.

AC Pigou, Marshall’s follower at Cambridge, was more explicit than Marshall about the methodology he was using, and that the approach he and Marshall used did not belong in the pure science of economics. He states explicitly that he was doing realistic theory. He writes, “Hence it must be the realistic, not the pure, type of

180 In making this choice he faced a problem; he was pushing for a separate trypos in economics at Cambridge. If he chose the term political economy, it would have been harder to justify the separate trypos. I do not know whether this influenced his choice or not, but it seems possible that it did.

181 Even with his limited interpretation of theory, Marshall was extremely hesitant to draw policy conclusions from his analysis. Policy was too complicated, and involved too many non-economic variables. For example, when he discussed the art of economics in the fourth edition he wrote: “Of course an economist retains the liberty, common to all the world, of expressing his opinion that a certain course of action is the right one under given circumstances; and if the difficulties of the problem are chiefly economic, he may speak with a certain authority. But on the whole, though the matter is one on which opinions differ, it seems best that he should do so rather in his private capacity, than as claiming to speak with the authority of economic science.” (Marshall, 1898, Vol. II, pg. 154)
science that constitutes the object of our search.” (Pigou, 1920, p. 6) To make this point even clearer, Pigou distinguishes between **fruit-bearing theory** and **light-bearing theory** (Pigou, 1920, p. 3). Fruit-bearing theory (realistic theory) is essentially political economy; it is theoretical apparatus that is designed to solve particular policy problems. It allows value judgments to be built into the analysis, and it makes no attempt to be pure; it was a type of engineering science, not a pure science. It can still be objective, in the sense of being open about the value judgments, and having the value judgments reflect economists’ estimate of society’s value judgments rather than their own, but it accepts value judgments as a necessary part of the analysis, necessary to make the analysis relevant to policy. Light-bearing theory is pure theory, or theory belonging in Robbins’s economic science branch of economics, and it was not a branch of economics that Marshall or Pigou had anything to say about.

Marshall’s broad use of the term, economics, to include work that was previously included only under the term political economy is likely to be, in part, responsible for the misinterpretation of Robbins’s methodological point, and about what “it” he was defining. Robbins, using the terminology of the classical tradition, was referring only to the pure science of economics when he used the term “economics”. Many of the critics, thinking of the term in the Marshallian/Pigovian tradition, were referring to the more engineering branch of economics that was designed to answer policy questions. Because Robbins thought of “economics” as a pure science, he opposed this usage. The problem he saw with that Marhsallian/Pigovian tradition was that it made it seem that the applied policy work had the imprimatur of science on policy conclusions. He felt doing so was inappropriate.

The economics profession did not follow Robbins’s prescription. Its applied policy work followed Marshall and Pigou’s approach and was classified under the name welfare economics. Over time, applied policy work became more and more theoretical and was integrated into a general Walrasian, not partial equilibrium Marshallian, framework. Applied policy work gave up the “engine of analysis” approach that was the Marshallian hallmark. It worked toward eliminating value judgments inherent in the Marshallian tools, as it tried to meet Robbins’s concerns about keeping value judgments out of the analysis.

In many ways, the approach taken by the profession in its applied policy work was the worst of both worlds. It combined Marshall and Robbins, but it did so in a way that undermined both of their positions, and left one with an approach that satisfied no-one. Theoretical and applied welfare work became completely separated. The result was bad for both sides. On the theoretical side, as J. de V. Graaff, concluded in his famous consideration of welfare economics, *Theoretical Welfare Economics*, without making some interpersonal utility comparisons “the possibility of building a useful and interesting theory of welfare economics-i.e. one which consists of something more than the barren formalisms typified by the marginal equivalences of conventional theory-is exceedingly small” (V. Graaff, 1959, p. 169). On the applied side, economics went on making interpersonal welfare comparisons in applied policy by hiding the interpersonal comparisons that embody hidden value judgments in the assumptions of the model. (Slesnick, 1998)

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182 As Richard Wright (Wright, 1989, 472) points out, the impetus to Robbins’s concerns was the Committee of Economists Report, which he believed had inappropriately intertwined political and ethical concerns under the guise of science.
In his Ely Lecture (Robbins, 1981) Robbins states his dissatisfaction with the direction that applied policy took. In it he argues that the name “welfare economics” is inappropriate. He writes “the raison d’être of welfare economics is to be “able to pronounce as a matter of scientific demonstration that such and such a policy was good or bad” (his emphasis). (p. 4) He states: “In the great work of Marshall and, still more, Pigou, we are assuming comparisons...(that are)...not warranted by anything which is legitimately assumed by scientific economics.” (pp. 4-5) Given his pure science conception of economics, this argument is understandable, but so too is Pigou’s argument, as long as one interpreted welfare economics as belonging in Robbins’s “political economy,” which is where Pigou believed it belonged, as is demonstrated by his carefully specifying that he was not doing pure science, but realistic science.

Marshall’s and Pigou’s “applied policy” work was too formal for Robbins’s tastes to be an appropriate methodology for political economy; it did not take adequate account of the many non-quantifiable issues that impinged upon policy. Robbins saw the analysis of applied policy more in the J.N. Keynes’s framework, where it involves “all modes of analysis and explicit or implicit judgments of value.”183 For Robbins, as for Keynes, the science of economics was to be used as a backdrop for thinking about policy problems, useful to help organize one’s thoughts, but was not to be directly applied to real-world problems.

What’s clear is that the last thing that Robbins wanted was for applied policy work to further restrict the value judgments used in them. Thus, welfare economics’ retreat away from the interpersonal utility comparisons that characterized the evolution of welfare economics as it retreated to a narrow focus on Pareto optimality, was specifically not what Robbins had advocated. Those changes were retrograde changes, and did nothing to achieve his prescriptive message. They just replaced one set of value judgments with another. In Robbins’s view welfare economics could not get around Hume’s dictum that “you cannot derive a "should" from an "is" no matter how welfare economics was formulated. He writes “the old or the new welfare economics are unlikely to be helpful and may well miss the main point entirely.” The reason they are unlikely to be helpful is that the name, “Welfare economics’ conveys an impression of value-free theory which it should be just our intention to avoid” (Robbins 1981, 7).184

4. Implications of Robbins’s Essay for Modern Economics

As I stated at the beginning of this article, Robbins’s Essay is of interest today because, as was the case in the 1930s when Robbins wrote, economics is currently in a state of flux. This is true in both the applied policy (political economy) branch of economics, and the pure theory branch of economics. What economists do now is

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183 The approach to applied policy work suggested by J.N. Keynes and Robbins would be more like that suggested by Swann (2006) than the type of applied work that economists typically do.

184 The problem of not dealing with the value judgments in applied empirical work has continued to exist in applied economics, just as Robbins thought it would. As Daniel Slesnick notes, the standard applied economist’s approach of assuming a representative consumer in applied policy work is “unappealing both because distributional issues are ignored and because much evidence shows that aggregate demands are inconsistent with the behaviour of a single representative agent.” (Slesnick, 1998; p. 219)
fundamentally different from what is presented in the texts as the definition of economics. Also, as I stated above, this is of no concern to most economists; they do what they do. Definitions and discussions of methods are for students, not for economists. So, in conclusion, let me consider the implications of Robbins’s *Essay* for the textbook presentation of economics.

The first implication is that, since, as a description of what economists do, even referring to the pure science of economics, Robbins’s definition is no longer applicable; it needs to change. The most important change in the way economic science is done is that it is no longer solely deductive and no longer tied to the constrained maximization model. It is also empirical. Theorists today collect and organize data, try to pull information from data using the latest econometric techniques to see what the data are telling them. They use natural, laboratory, and field experiments to provide insights into how the economy works and to test theories. They also use simulations and game theory constructs to attempt to gain insight into economic problems. The science of economics today has made enormous strides from the science of economics in Robbins’s time, and is essentially about finding robust patterns in data and finding explanations for those patterns.

Even that part of economic theory that is primarily deductive studies much more than constrained optimization models. There is a burgeoning branch of behavioural economic theory, while other theorists study non-linear dynamic models, evolutionary game theory models, biological generation models, statistical mechanics models, and a whole lot more. Any descriptive definition of economic science needs to be sufficiently encompassing to include all this work.

My suspicion is that Robbins would be the first to push for a redefinition. My reasoning here, as I stated above, is that Robbins’s definition was descriptive, not prescriptive. Because it was descriptive, Robbins would have wanted the definition of economics to include all the recent developments in theory. So the lesson I take from Robbins’s *Essay* concerning the definition of economic science is that it be sufficiently wide to include what it is that economic theorists do, as long as what they are doing falls within the confines of science.

But, there is a more important prescriptive lesson that I believe comes from a reconsideration of Robbins’s essay. While Robbins would not have been concerned about the change in the definition of economics, he would have been very concerned that the textbooks make clear the separation of the pure science of economics from the political economy branch of economics. He would stand by his prescriptive advice about the need for *two clearly distinguished separate tracks* in economics—one a science track designed for questions of understanding the economy, and one an applied policy track designed for guiding policy. Thus, to meet Robbins’s prescriptive message, the definition in the texts of the pure science of economics, will need to be accompanied by a supplemental definition in the texts of another applied policy track.

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185 It remained primarily deductive through the 1980s (Rosenberg, 1991) but more recently it has changed and become more empirical.

186 Robbins emphasized the changing nature of economics in his inaugural lecture at LSE (Robbins, 1930) where he noted Ricardo’s remark that economics was still in its infancy, and Marshall’s remark that economists had enough to occupy them for the next three thousand years, and he explicitly states that “No one pretends that what is being done to-day is anything but provisional. (Robbins, 1930. pg 15)
Whether you call both of these tracks science, but distinguish a realistic science-the applied policy branch-from the pure science branch, as Pigou and Marshall would do, or call one track the science of economics and the other track, political economy, as Robbins would, or call one track positive economics and the other track the art of economics as J. N Keynes would do, is less important than the fact that these two tracks be explicitly distinguished. The current texts do not do this, and thus miss the prescriptive lesson Robbins wanted to convey in his essay.

I believe that this applied policy track is also most usefully defined descriptively, not prescriptively. Considering what economists do when they do applied policy, we see an enormous change in what economists do compared to what economists did in Robbins day. Whereas in Robbins’s day, what he called political economy was largely heuristic, today it has become much more applied mathematical and statistical in nature. Economists see themselves as bringing technical expertise and modeling expertise to applied policy. Applied economists use experiments, they use game theory, and they use statistical methods extensively.

The evolution of method means that today the two branches of economics have come closer in approach, and less distinct than they were in Robbins’s time. But that does not mean that the two can be combined into one. In fact, I believe that Robbins would continue to insist on the need to separation these two branches, and that his line of demarcation would be whether the work was designed to establish a fact or come up with a theorem, or whether it is designed to arrive at a precept, which were the terms J.N. Keynes used to separate the output from the science of economics and political economy.

A fact is an empirically agreed upon observation. A theorem is a conclusion that follows from economic theory; theorems concerns the way the economy works. It does not concern policy questions. A theorem is not debatable by serious economic scientists. A precept is a rule of thumb that concerns policy that follows from political economy. Precepts are derived from economic theory, introspection, induction, educated common sense, the standard ethics of the day, and judgments on normative issues. A precept reflects the conventional wisdom of the profession, and while based on theory, is debatable by serious economic scientists. In Robbins’s mind if the goal of research is discovering a fact or a theorem, then it is part of the science of economics (or whatever one is calling that branch.) If the goal of the research is a precept, then it is part of political economy (or whatever one is calling that branch). Making that separation explicit in the principles of economics would go a long way toward meeting Robbins’s prescriptive goal of his Essay.

Whether it was possible or not to teach principles students the science of economics in Panteleoni’s and Robbins’s day, I do not believe it is today. The modern science of economics is far too sophisticated for principles students to understand without extensive study. We can teach students about the scientific economic process, and some of the findings; we can try to give them an appreciation of the science, but that is quite different from teaching them the science of economics. In reality, most principles texts don’t try to give students an appreciation of the science of economics,

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187 Robbins predicted that it would change in this way, but he also noted that as it did change, it would become more useful. He wrote “The theory becomes more complex, but its application becomes more practicable.” (Robbins, 1930, 21)
(as Robbins would define it); they try to give students an appreciation of political economy, as Robbins would define it. But because they don’t separate two branches of economics, and don’t distinguish precepts from theorems, they fail to meet Robbins’s prescriptive message, which was in many ways simply a restatement of Hume’s Dictum.\footnote{To differentiate a precept from a theorem, students could take what might be called the Hume/Robbins test, where the teacher asks a question about the policy implications of a theory. For example, he or she could ask: Does economic theory prove that international trade makes countries better off? If the student answer the question yes, then he or she fails the Hume/Robbins test, and the teacher would explain to them that to come to a conclusion about “better off” one needs to make value judgments, and that while it is a precept of political economy that international trade makes countries better off, it is not a theorem. To move from the theorem of economic science about free trade, to the precept of political economy about free trade, one must specify what one means by “better off” which requires value judgments for which we do not know how to develop a scientific foundation. 

Then, to see if the students understood the argument, students might be given the Hume/Robbins/Sen test, which would consist of asking them the following question: If by better off we mean achieving a Pareto Optimal improvement where everyone is made better off and no one is made worse off, can we then say that economic theory states that international trade makes countries better off? Again, if they answer yes, they fail, because as Sen (1970) has shown, Pareto optimality does not get one around Hume’s Dictum.} Reminding economists that the “it” that they are studying encompasses two branches of economics, and that the science of economics branch is subject to Hume’s Dictum, was Robbins’s goal, and hopefully, this time around, his message will be heard.
References


Cost and the ‘Means-Ends’ Definition of Economics in Lionel Robbins’s Essay: Analysis and Contemporary Implications

Andrew Brown and David A. Spencer

Abstract

This paper considers Lionel Robbins’s 1932 Essay from the perspective of the ‘cost controversies’. Robbins upheld the Austrian concept of opportunity cost. This concept fitted with Robbins’s underlying definition of economics as the relationship between means and ends, and with his overall emphasis on the system-wide import of economic science. The paper compares and contrasts Robbins’s Essay with two high profile additions to modern mainstream economics – behavioural economics and the economics of happiness. Both these approaches are shown to provide important challenges to Robbins’s Essay.

Keywords: Robbins; Cost; System; Economics-Psychology; Happiness
JEL Classification: B20, D5

1. Introduction

Lionel Robbins’s An Essay on the Nature and Significance of Economic Science (henceforth, Essay), first published in 1932, is well known for its influential advocacy of a ‘means–ends’ definition of economics, for denying interpersonal comparison of utility, and for championing of an ordinal as opposed to cardinal utility theory of value. Given recent encroachments of psychology into economics, the Essay is also notable for conceptually barring psychological science from economic science (though there is some dispute in the interpretive literature on this aspect of the Essay). Less commented upon is the role that Robbins’s engagement with the ‘cost controversies’ plays in the Essay. Robbins’s own views on cost were in fact integral to his means–ends definition of economics. They reflect his underlying conception of the economy and the role of economics in explaining it.

This paper re-examines the Essay from the perspective of Robbins’s engagement with the cost controversies, emphasising their importance for understanding the nature and implications of Robbins’s means–ends definition of economics. Robbins’s affinity with the Austrian theory of opportunity cost is established. This theory, it will be argued below, was important to Robbins in reflecting the system wide nature of the process of resource allocation. His resistance to any role for psychology within economics, in a similar way, can be accounted for by his desire to apply economic science to the explanation of the real economic system.

The latter part of the paper examines the relation between Robbins’s Essay and two recent high profile developments in mainstream economics – namely ‘behavioural...
economics’ and the ‘economics of happiness’. To the extent that contemporary
behavioural economics pursues psychological reality without detailed focus upon the
economic system of which individual psyches are part, it elides central questions
regarding the nature and theory of the real economic system as a whole and so would
have been anathema to Robbins. On the other hand, the economics of happiness
resurrects the idea of cardinally and interpersonally measurable utility. This is not
only radically at odds with hitherto broadly accepted arguments of Robbins’s Essay
on value and psychology (a fact that is not lost on the prominent advocate of the
economics of happiness and a successor to Robbins at the LSE, Richard Layard) but
also implicitly returns the sphere of production into the foreground of the mainstream
economic ontology by making the activity of work a direct source of utility and
disutility to workers. The economics of happiness (and the concomitant ‘hedonic’
psychology on which it draws) thus appears as the very antithesis of Robbins’s
arguments in the Essay, being wholly incompatible with the famous means–ends
definition of economics.

The paper is organised as follows. Section 2 reviews the impact of the cost
controversies on Robbins’s Essay. In terms of his contribution to the cost
controversies, as reflected in the Essay, Robbins combines (i) an instrumental view of
work and productive activity with (ii) an emphasis on the system-wide scope of
economic science. Section 3 focuses on the complex way in which the Essay denies a
role for psychological science in economic science. It is argued that whereas the
existence of a preference ordering for each individual is taken to be indubitable by
Robbins, the perfect rationality (transitivity) of that ordering is not taken by him to be
realistic but, to the contrary, is an unrealistic, ‘first step’ assumption made in the
theorisation of the economic system by neoclassical economics. Section 4 draws out
the relevant implications for behavioural economics and for the economics of
happiness. Section 5 concludes.

2. The Influence of the ‘Cost Controversies’ on Robbins’s Essay

Before the 1930s, there had been controversy in neoclassical economics over the
correct definition of cost and the ‘cost controversies’ as they became known had an
important influence on Robbins. At the centre of these controversies was the question
of the nature and extent of the utility theory of value. Could non-consumption
activities such as work be considered a source of utility and disutility? Or was utility
to be understood in terms of consumption alone? As we shall see below, there were
two sides in this debate: firstly, economists connected to the Austrian school believed
that utility was solely a product of consumption and thus promoted the concept of
‘opportunity cost’; secondly, English economists such as Jevons and Marshall argued
that work itself could be experienced as pain as well as pleasure and supported the
doctrine of ‘real cost’. Robbins (1930, p.207) believed that the ‘celebrated disputes’
over cost had been resolved in favour of the Austrian side and he approved of this
outcome. This section shows how the Austrian view of opportunity cost fitted with the
Essay’s underlying definition of economics as the relationship between means and
ends, and with the Essay’s overall emphasis on the system-wide import of economic
science.

As explained by Robbins (1932, p.78), it had been commonplace in classical
economics to look upon cost of production as the ultimate determinant of the value of
reproducible commodities. Although it was admitted that the value of non-reproducible goods was determined by their scarcity, there was a general reluctance to give any importance to the role of demand in the determination of value. Even after the marginalist revolution, there were still some economists who had sought to emphasise the role of cost of production as an independent determinant of value alongside the marginal utility of consumption. Jevons, and later Marshall, thus had asserted that the supply of labour would be limited by the marginal disutility of work and had assumed that the subjective cost of work itself would impact upon relative prices. Marshall (1910, p.348), indeed, had spoken of cost of production and marginal utility determining the value of commodities like ‘two blades of a pair of scissors’.

Robbins’s aim, in contrast, was to remove a direct role for cost of production in the conception of value. Specifically, he sought to promote a ‘pure’ (subjective) theory of value that gave exclusive emphasis to the role of consumption. To this extent, he was drawn to the ideas of Austrian writers. Robbins (1932, p.79) celebrated the achievement of Wieser and his successors in coming up with the concept of ‘alternative cost’. This concept, unlike the notion of ‘real cost’ supported by Jevons and Marshall, did not require any separate consideration of the direct cost of production measured by tired muscles and the like. Rather it related the cost of production to forgone opportunities for consumption (see also Robbins, 1934, pp.2-6). Costs were incurred in production, in effect, because the factor inputs used to create output were few in number and had alternative uses (see Wieser, 1892, pp.41-2). Costs of production were thus ultimately traceable to the value of output that might have been produced if factor inputs had been used differently in production and factor inputs gained their prices from the value of forgone consumption goods:

“What is it which causes the price paid for a given factor of production in a given line of production to be what it is and not something else? Clearly the demand in that line of production in relation to the supply. But why is the supply of the factors in that line limited to what it is? Why is it not the whole supply devoted to this line of production? Clearly because there is demand for the scarce products which it can produce elsewhere. Its price in one line therefore depends upon the price which is put upon it in others. In the end, subjective valuations govern costs equally with product prices.” (Robbins, 1932, pp.79-80)

Now, Robbins was aware that the Austrian approach to cost had been criticised by English writers such as Edgeworth for being applicable only to the case where the supply of factor inputs was fixed. Such writers had insisted that where the supply of factor inputs was flexible attention must be paid to the concept of ‘real cost’. Robbins conceded that this view had some plausibility:

“it may be admitted that, as against the form in which the arguments of the early Austrians were cast, it has a certain degree of validity. It was clear that, for expository purposes, both Wieser and Böhm-Bawerk had recourse to the hypothesis of fixed total factor supplies, while their
opponents, Marshall and Edgeworth, always proceeded from the assumption that factor supplies were flexible.” (Robbins, 1932, p.80)\(^{189}\)

But the compromise reached in the early debate in neoclassical economics over the role of cost, namely that the alternative cost doctrine applied to situations of fixed factor supplies and the real cost doctrine applied to situations of flexible factor supplies, was questioned by Robbins (1932, p.81). This compromise gave too much ground to the real cost doctrine. The key message of the Austrian approach was that the concept of disutility was redundant (see O’Brien, 1988, p.89). In all cases, costs were to be understood in terms of losses of opportunity for consumption. For example, the cost of labour supply was not to be identified with the irksomeness of work itself, but instead was to be linked to the opportunity cost of work time (see Robbins, 1930, pp.207-8). Even where the supply of labour was flexible, the cost of work derived from the alternative uses of work time (represented by the consumption of the good, ‘leisure’), as opposed to the marginal disutility of work (see Spencer, 2004).

Robbins took inspiration, too, from Wicksteed. While often seen as an advocate of Jevons, Wicksteed rejected Jevons’s notion of real cost, in favour of the Austrian conception of opportunity cost. Following the Austrians, he stated that factor inputs bore a price only because they created valuable consumption goods when put to alternative uses. Wicksteed thus was led to deny that cost of production had any direct bearing on the value of output. Hence, he wrote that:

“The only sense, … in which cost of production can affect the value of one thing, is the sense in which it is itself the value of another thing. Thus, what has been variously termed ‘utility, ‘ophelimity’, or ‘desiredness’, is the sole and determinant of all exchange values.” (Wicksteed, 1910, p.391)

Likewise, in respect to the supply of labour, Wicksteed (1910, p.624) emphasised the role of the marginal utility of leisure as the source of the cost of labour, rather than the direct pain of work itself.\(^{190}\)

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189 In the 1890s, Böhm-Bawerk (1894a,b) had been drawn into debate with Edgeworth over the nature of cost and its relation to value. For Böhm-Bawerk, the disutility concept of Jevons and Marshall required the assumption of flexible factor supplies, since it presupposed that workers could adjust their labour supply in line with the marginal disutility of labour. However, he stated that because of institutional constraints on the supply of labour workers in modern society were not able to make effective choices over the work they do. Consequently, he argued that the real cost of work could not be regarded as a separate determinant of value, Edgeworth (1894a,b), as a devotee of the real cost doctrine, in reply, had asserted that the flexibility of labour supply was much more apparent than Böhm-Bawerk had admitted. Workers, for example, had the capacity to vary their work effort as well as to change their jobs. The marginal disutility of labour, hence, could not be ruled out as a factor determining the cost of production and hence the value of output. A compromise thus was reached in neoclassical economics that the application of the real cost doctrine depended on the freedom of workers to alter their labour supply (Pagano, 1985, pp.84-88: see also Spencer, 2004).

190 O’Brien describes the connection between Robbins and Wicksteed as follows: ‘The main thing which Robbins took from Wicksteed was the concept of the omnipresent, choosing, maximising, individual, approaching allocation problems in terms of opportunity cost rather real cost, within the framework of general rather than partial analysis. Wicksteed may also have influenced Robbins in the rejection of inter-personal comparisons and in his view of the limits of what could be said scientifically, although it is likely that to some extent his views coincided with ones which Robbins
Several specific aspects of Robbins’s means–ends definition of economics (of enduring significance to this day) can be brought out, in light of the cost controversies. It is clear from Robbins’s position on these controversies that, for him, the ‘ends’ of economic activity were exclusively associated with final consumption. The costs of factors of production were not a separate source of value; rather they derived solely from the foregone alternative consumption goods, the foregone ends that their application entails. Accordingly, Robbins’s means–ends definition of economics required an instrumental conception of work activity. Work must be considered to be but a means to the end of final consumption, rather than a fundamental end in its own right. Such an instrumental conception of work activity was not held by the ‘English’ side in the cost controversies. The most striking example is Marshall. As Parsons (1931; see also 1932 and 1934) has made plain, for Marshall, work activities and human activities more broadly were a direct and fundamental source of creative development and hence well-being. Work activity, which appeared solely as a mere ‘means’ in Robbins’s definition, was also, and more fundamentally, an end in its own right for Marshall. Marshall therefore could not adopt the Austrian position on cost, nor, following the logic of this argument, could he have adopted Robbins’s means–ends definition economics.

A second route to a non-instrumental conception of work activity was taken by the other main opponents of the Austrians in the cost controversies – Jevons and Edgeworth. This route was arguably one that was also taken in Marshall’s writings (see Parsons, 1931, p.121). For Jevons and Edgeworth, ‘utility’ or ‘happiness’ was a distinct psychological object, an affective state of individuals that was cardinally measurable through the methods of psychological science. Jevons and Edgeworth drew this view from the psychological science of their time. It was only subsequent developments in psychological science such as behaviourism that turned away from it (see Giocoli, 2003; Bruni and Sugden, 2007). As the modern day revival of ‘hedonic psychology’ has affirmed (see Section 4, below), work activity appears just as much an influence on utility, once conceived as an affective state, as does the act of consumption. By contrast, an ordinal utility theory eschews the postulation of any psychological object termed ‘utility’ and focuses solely on individual choice in the market. Given the prevalence of an instrumental attitude to work activity in the labour market, the ordinal approach thereby supports an instrumental theorisation of work activity. Hence Robbins’s insistence in the Essay upon an ordinal conception of had arrived at from other directions’ (O’Brien, 1988, p.25). ‘Other directions’ might be seen to include the writings of the Austrians.

191 Consider, for example, the following quote from Marshall’s Principles where direct stress is placed on the formative aspects of work activity: ‘The truth seems to be that as human nature is constituted, man rapidly degenerates unless he has some hard work to do, some difficulties to overcome; and that some strenuous exertion is necessary for physical and moral health. The fullness of life lies in the development and activity of as many and as high faculties as possible’ (Marshall, 1910, p.136).

192 The above expressed coincidence of means and ends in work activity must be sharply distinguished from the coincidence that occurs in any chain of production where intermediate goods are both ends of previous production processes and means of future ones (see Parsons, 1934, and the discussion in Kirzner, 1960). The nominal coincidence of ‘intermediate’ means and ends perfectly accords with the key premise of Robbins’s definition, namely that final consumption is the end to which intermediate production processes are ultimately oriented. By contrast, the view that work itself is a final end of economic activity (making human activity self-reflexive), such as held by Marshall, contradicts Robbins’s key premise and so is truly incompatible with his means–ends definition.
utility can be seen as integral to his position on costs and to his means–ends definition of economics.

Robbins’s approach to costs reflected, more broadly, his distinct individualistic and consumption-centred picture of the economic system as a whole. Robbins rejected the social and class-based account of the economic system offered by classical political economy. For Robbins and the Austrians, it was subjective individual valuations concerning final consumption (‘ends’), not social and class-based costs of production (‘means’), which were the ultimate driving forces of this system. As he put it:

“So long as the theory of value was expounded in terms of costs, it was possible to regard the subject-matter of economics as something social and collective and to discuss price relationships simply as market phenomena. With the realisation that these market phenomena were, in fact, dependent on the interplay of individual choice, and that the very social phenomena in terms of which they were explained – costs – were in the last analysis the reflex of individual choice – the valuation of alternative opportunities (Wieser, Davenport) – this approach became less and less convenient.” (Robbins, 1935, p.69n)

Elsewhere in the *Essay*, Robbins sometimes wrote as if the definition of economics offered by Marshall and other English economists of the time, in terms of the production of material wealth, was a mere hangover from classical and pre-classical economic thought. Hence, for example, it was identified by Robbins (1935, p.9) as ‘the last vestige of Physiocratic influence’. However, as Robbins implicitly recognised in the above quote, the materialist definition in fact cohered with its supporters’ underlying conception of cost. Hence it was *only* given acceptance of the Austrian side in the cost controversies that Robbins could (again following the Austrians and Wicksteed) decisively change the conception of the economic system to one that foregrounds individual consumption as the ultimate end of the system as a whole, with general exchange equilibrium as the appropriate initial focus of analysis of that system.193

In terms of methodology, the cost controversies reflected Robbins’s overriding concern (and that of the Austrians and Wicksteed) with the real economic system as a whole. Thus, opportunity cost was (and remains) a general equilibrium concept, embracing the myriad competing demands for factors of production across the economic system, in contrast to Marshall’s partial equilibrium cost curves. Robbins’s means–ends definition implanted a system-wide scope into the essence of economic science. It was, as Robbins (1932, p.16) put it, an ‘analytical’ definition of economics that ‘does not attempt to pick out certain kinds of behaviour but focuses attention on a particular aspect of behaviour, the form imposed by the influence of scarcity’. Rather than being restricted to any specific kind of activity, such as rational activity,

193 Robbins outlines his alternative (consumption-centred) conception of the economic system in the following quote: ‘Instead of regarding the economic system as a gigantic machine for turning out an aggregate product and proceeding to enquire what causes make this product greater or less, and in what proportions this product is divided, we regard it as a series of interdependent but conceptually discrete relationships between men and economic goods; and we ask under what conditions these relationships are constant and what are the effects of changes in either the ends or the means between which they mediate and how such changes may be expected to take place through time’ (Robbins, 1932, pp.67-8).
economics was concerned with the system as a whole, in Robbins’s view. That this was a fully integrated system was often affirmed by Robbins. For example, in referring to what he saw as the unique ability of neoclassical economic theory to comprehend resource allocation within an exchange economy, Robbins wrote that:

“The implications of individual decisions reach beyond the repercussions on the individual. One may realise completely the implication for oneself of a decision to spend money in this way rather than that way. But it is not so easy to trace the effects of this decision on the whole complex of “scarcity relationships” – on wages, on profits, on prices, on rates of capitalisation, and the organisation of production. On the contrary, the utmost effort of abstract thought is required to devise generalisations which enable us to grasp them. For this reason economic analysis has most utility in the exchange economy.” (Robbins, 1932, p.18)

In other words, the system-wide scope of economic science implied, for Robbins, a methodological need to transcend individual psychology through abstract theorisation. In the next section, we consider this point in greater detail as part of an examination of Robbins’s arguments in the Essay on the relationship between economics and psychology.

3. Economics and Psychology in Robbins’s Essay

It is well-known that Robbins decided to distance economics from psychology. It will be argued in this section that Robbins’s disavowal of psychology drew strength from his concern to bring a system-wide focus to economic science. Robbins knew that economics was based on unrealistic psychological assumptions but he argued that such assumptions had a vital role to play in deriving important insights about the economy and hence had to be retained and defended.

In keeping with his position within the cost controversies, Robbins based his jettisoning of psychology from economics on a rejection of cardinal utility theory. He argued that the ability of individuals to arrange their ends in order of preference was the key assumption of value theory and hence of economic science. The psychological reasons underlying the preference ordering were not a matter for economics – exploration of such reasons was a matter for psychology alone. As he put it:

“all that is assumed in the idea of scales of valuation is that different goods have different uses and that these different uses have different significances for action, such that in a given situation one use will be preferred before another and one good before another. Why the human animal attaches particular values in this behaviouristic sense to particular things, is a question which we [economists] do not discuss. That may be quite properly a question for psychologists or perhaps even physiologists.” (Robbins, 1932, p.86)

Thus, according to Robbins, neither hedonic psychology, nor any other specific psychological doctrine, was requisite to economic science. For economics, the mere
existence of a preference ordering, to be treated as a given datum, was all that counted.

Whereas Robbins’s separation of psychology from economics via ordinal utility was similar to Pareto’s arguments, and to those of other pioneers of ordinal utility theory, his characterisation of the nature of the ordinalist assumptions was distinctive. According to Robbins (1935), the fundamental assumption that individuals are able to order their preferences was an ‘elementary fact of experience’ (p.75), in part known by ‘immediate acquaintance’ (p.105), i.e. by introspection. Robbins contended that the scales of preferences concept was thereby revealed to be an absolutely indubitable fact. Given that this key economic concept in part refers to a mental domain of introspection, Robbins was led to dismiss behaviourism, which denies any reference to such concepts, writing that:

“It is really not possible to understand the concepts of choice, of the relationship of means and ends, the central concepts of our science, in terms of observation of external data ... [These concepts] … involve links in the chain of causal explanation which are psychical, not physical, and which are, for that reason, not necessarily susceptible of observation by behaviourist methods.” (Robbins, 1935, pp.89–90)

However, the indubitable or ‘obvious’ (p.86) status of the fundamental psychological assumption of the existence of a preference ordering neither required nor implied the presence of ‘psychology’, in the sense of an academic discipline, within economic science. In this regard, it is important to stress the distinction between ‘psychology’ as referring to an academic discipline and ‘psychology’ as referring to everyday concepts of thought, feeling etc. For Robbins, economics could be seen to rest upon a partly ‘psychological’ foundation in the latter sense of the term, but it neither depended on nor required any particular theory from psychological science.

Relative scales of valuation alone are not, of course, enough for neoclassical economic theory to get off the ground. Though sometimes leaving the point implicit (e.g. 1935, p.75), Robbins recognised in the second edition of the Essay that the key generalisations of neoclassical economic theory required, inter alia, the assumption of a transitive or ‘consistent’ preference ordering. Thus he wrote:

“The celebrated generalisation that in a state of equilibrium the relative significance of divisible commodities is equal to their prices, does involve the assumption that each final choice is consistent with every other, in the sense that if I prefer A to B and B to C, I also prefer A to C.” (Robbins, 1935, pp.91–2)

However, Robbins also acknowledged that in reality people were not perfectly consistent in their preference ordering. He stated that perfect consistency is among the class of assumptions whose purpose ‘is not to foster the belief that the world of reality corresponds to the constructions in which they figure’ (p.94). This is very different, even opposite, to the class into which the assumption of the mere existence of a relative scale of preferences falls (the class that ‘have only to be stated to be recognised as obvious’, p.79). Having gone from the ‘indisputable’ (1935, p.78) postulate (in Robbins’s view) of a preference ordering, to an assumption of perfect
consistency that is admitted to be unrealistic, one might expect a lengthy explanation. In the event, Robbins’s main relevant argument was compressed into one heroic paragraph:

“The fact is, of course, that the assumption of perfect rationality in the sense of complete consistency is simply one of a number of assumptions of a psychological nature which are introduced into economic analysis at various stages of approximation to reality. The perfect foresight, which it is sometimes convenient to postulate, is an assumption of a similar nature. The purpose of these assumptions is not to foster the belief that the world of reality corresponds to the constructions in which they figure, but rather to enable us to study, in isolation, tendencies which, in the world of reality, operate only in conjunction with many others, and then, by contrast as much as by comparison, to turn back to apply the knowledge thus gained to the explanation of more complicated situations. In this respect, at least, the procedure of pure economics has its counterpart in the procedure of all physical sciences which have gone beyond the stage of collection and classification”.

(Robbins, 1935, pp.93-4)

The key points that Robbins makes can be broken down as follows: (1) perfect consistency is not an actual property of the psyche of real individuals but (2) the economic theory which assumes perfect consistency is nevertheless in some way applicable to the real economy; (3) economic theory employs the assumption of perfect consistency at appropriate stages of ‘approximation’; (4) further ‘complications’ can be introduced at later stages in economic analysis without rendering analyses based upon the assumption of perfect rationality redundant; (5) the assumption of perfect consistency enables theory to isolate real ‘tendencies’. Given their brevity, the precise meaning (and the individual and joint validity) of points 1–5 is open to interpretation. The pertinent question here is what these points imply, if anything, for Robbins’s conception of the economics–psychology relationship. Consideration of the system-wide focus attributed by Robbins to economic science offers help in interpreting Robbins’s stance.

Section 2 above relayed how, for Robbins, the system-wide interconnection of the exchange economy required the ‘utmost effort of abstract thought’ on the part of the economist. The assumption of perfect rationality is in fact an example of the ‘abstract thought’ required to begin to theorise the system as an interconnected whole. In his discussions of statics and dynamics, Robbins included the assumption of perfect foresight and more generally the abstraction from time as further examples of such ‘abstract thought’ (see, for example, Robbins 1935, pp.102–103). Future development of theory, dropping these unrealistic assumptions, and so introducing dynamics, was certainly required according to Robbins but he retained absolute faith that such development would not contradict (though it may modify) the basic propositions of static equilibrium theory. The point was summarised in the Preface to the second edition where Robbins affirmed the ‘desirability of transcending the rather trite generalisations of elementary statics’, i.e. of the existing static theory of system-wide resource allocation but he stressed at the same time that existing theory, though it

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194 This paragraph was inserted into the second edition of the Essay. Chapter 4 in which this paragraph appears was extensively revised by Robbins. Despite the revisions to this chapter, however, Robbins admitted to being ‘acutely aware of its imperfections’ (1935, p.xi).
may be ‘trite’, in fact offered ‘essential static foundations’ for further theoretical developments (1935, p.xii).

It thereby appears that Robbins’s discussion of the perfect consistency assumption was integral to his case against the encroachment of psychological science into economic science. Robbins insisted that, on dropping the assumption of perfect consistency, the theorist had to show (or at least recognise) that the core economic theory remains generally valid (if approximate and subject to modification). The key apparent danger was that the encroachment of psychological science into economic science would lead to a replacement of the perfect consistency assumption by a realistic psychological theory, without regard to the validity of the core economic theory of system-wide resource allocation. Given that such core theory was the raison d’être of economic science (as reflected in Robbins’s means–ends definition of that science), then its disregard in the name of psychological realism would be a disaster, on Robbins’s view. In other words, the necessity of the perfect consistency assumption as a ‘first step’ in core economic theory of system-wide resource allocation implied that economic science must be fenced-off from any encroachment from psychological theory, since the latter could not admit of such an unrealistic psychological postulate.

The interpretation of perfect consistency as a ‘first step’ assumption in a theory with system-wide scope is one that reflects the views at that time of several of Robbins’s contemporaries, such as Knight and Hayek (see Giocoli, 2003, and Knight, 1934, cited in the Preface to the second edition of the Essay). However, it should be recalled (and the Essay in fact stresses) that the prior assumption of the existence of a preference ordering was not, unlike the assumption of perfect consistency of the preference ordering, a ‘first step’ assumption, not an ‘approximation’ or mere ‘tendency’, rather it was an actual fact of virtually all human activity, according to Robbins. Thus, there was, within Robbins’s conception of economic science, a conjunction of ubiquitous system-wide actualities (the indubitable existence of a preference ordering for every individual in the system) and non-actual, psychologically unrealistic assumptions (the perfect consistency of the preference ordering of all individuals). This conjunction served as an insurmountable barrier to the coalescence of psychological science and economic science, in Robbins’s view.

There was one further key aspect to Robbins’s jettisoning of psychology from economics. This aspect concerns the notion of psychological hedonism as entailing a purely egotistical, self-interested individual. Robbins argued that economic theory by no means needed to assume such an individual. When such an assumption was made it was a mere ‘expository device’ which could be dropped at any stage (Robbins, 1932, p.90; 1935, p.94). In fact, any motives at all could underlie the preference orderings of individuals, from egotism to altruism, according to Robbins. All that mattered was that such preference orderings existed, such that they could be taken as given data by the economist. Since there appears to be some confusion in the

195 Methodologically, the argument above views Robbins as a realist, in believing in the existence of a real economic system to be explained. Again there is a link here to the Austrian school, which can also be argued to be realist (e.g. Mäki, 1990).
literature on this point\textsuperscript{196}, it is worth noting that these arguments resonate very strongly with those of Wicksteed and that, elsewhere, Robbins commented:

“Before Wicksteed wrote, it was still possible for intelligent men to give countenance to the belief that the whole structure of Economics depends upon the assumption of a world of economic men, each actuated by egocentric or hedonistic motives. For anyone who has read the \textit{Common Sense}, the expression of such a view is no longer consistent with intellectual honesty, Wicksteed shattered this misconception once and for all.” (Robbins, 1933)

To summarise, according to Robbins, the use of psychological concepts and introspection made by economic science was limited, at the most fundamental level, to recognition of the existence of a relative scale of preferences, the existence of which Robbins believed to be obvious. Further assumptions regarding mental processes were made (for example, perfect consistency) but these were required for the purposes of building an abstract economic theory of the system of resource allocation as a whole, they did not and should not stem from psychological science as such. Given the sense of ‘psychology’ as psychological science, then there was no compromise at any stage in the \textit{Essay} made as regards Robbins’s argument that:

“The borderlands of Economics are the happy hunting ground of the charlatan and the quack, and, in these ambiguous regions, in recent years, endless time has been devoted to the acquisition of cheap notoriety by attacks on the alleged psychological assumptions of Economic Science.” (Robbins, 1932, pp.83-4)

The ‘charlatan’ and the ‘quack’, who promoted an alliance between economic science and psychological science, in short, were to be repelled at every turn. The next section will consider some developments in modern mainstream economics and look to compare and contrast them with Robbins’s \textit{Essay}.

4. Consideration of Contemporary Developments in Economics

Several commentators (e.g. Davis, 2008) have suggested that mainstream economics research is in a state of flux as a number of approaches that would once have been marginalised have entered into the mainstream research agenda in the past two decades. Davis, for example, includes the following approaches: classical game theory, evolutionary game theory, behavioural game theory, evolutionary economics, behavioural economics, experimental economics, neuroeconomics, and agent-based complexity economics. Of obvious interest given Robbins’s arguments is that perhaps the most prominent new approach is ‘behavioural economics’ which is sometimes simply defined as the application of the theories and methods of psychological science to economics (e.g. Bruni and Sugden 2007, p.146), and as such could be said

\textsuperscript{196} Giocoli (2003, p.88), for example, argues that the assumption of perfect consistency of preference is included under the term ‘expository device’ by Robbins. In fact, the phrase is more appropriate for the assumption of egotistical behaviour, since this latter assumption was purely one of convenience which could, for Robbins, be dropped at any stage of analysis (as reference to Wicksteed helps to show) whereas the assumption perfect consistency could not be dropped at the initial stage of analysis, reflecting its status as ‘first step’ assumption.
to embrace several other new approaches such as the ‘economics of happiness’ (not included in Davis’s list). The ‘economics of happiness’ is itself an eye-catching development given Robbins’s strictures against any infiltration of hedonic psychology into economics. This section will compare and contrast Robbins’s arguments, as interpreted above, firstly with behavioural economics and secondly with the economics of happiness. The discussion below will develop in more detail our interpretation of Robbins’s Essay and will interpret in this light the significance of the new developments in mainstream economics.

**Behavioural economics**

Behavioural economics has become renowned for a sustained development of a psychologically realistic alternative to the rational choice theoretic model of human behaviour found in neoclassical economics. Particular criticism has been directed at the assumptions of expected utility theory (EUT). Departures from EUT, such as loss aversion and endowment effects, have been unearthed using the experimental methods of psychology (in particular, cognitive psychology) and psychologically based alternative theories to EUT have been accordingly developed, such as Kahneman and Tversky’s (1979) ‘prospect theory’. There have been numerous applications of the ideas of behavioural economics. To take one prominent example, in the area of financial economics, ‘behavioural finance’ is now a recognised term denoting the application of behavioural economics to finance, challenging the standard efficient market hypothesis.

The exposition of Robbins’s Essay in Section 3 above suggests strongly that the infiltration of psychological science into economics represented by behavioural economics would have dismayed Robbins. In particular, had he lived to see its present day success, he would likely have made several predictions regarding behavioural economics, insofar as it can be considered to be the coalescence of economics and psychology. Firstly, he would have predicted that the assumption that individuals are able to order their preferences would not have been challenged by behavioural economics. For Robbins this assumption is an indubitable fact, beyond challenge from any quarter. Secondly, he would have predicted that behavioural economics would focus upon replacing the unrealistic assumptions about human choice made by core economic theory with realistic psychological alternatives. To take the example discussed in Section 3 above, he would have expected that the perfect consistency assumption would have been replaced. Thirdly, he would have expected that, rather than developing from or adding to core economic theory, behavioural economics would gravely threaten the existing neoclassical theory of the economic system, whether through open hostility towards it, or through a silence on the essential question or raison d’etre of economic science: system-wide resource allocation. Fourthly, he would have predicted that behavioural economics would criticise the assumption of self-interest (despite the ‘fact’, as Robbins saw it, that no such motive is necessary to economic science).

Rather than initially attempt an assessment of Robbins’s views in relation to ‘behavioural economics’ as a whole, it is instructive to perform the simpler task of considering the seminal *Econometrica* paper of Kahnemen and Tversky (1979) on ‘prospect theory’. Three of Robbins’s four ‘predictions’ unambiguously are borne out: (1) the central aim of prospect theory is to replace the unrealistic assumptions of
economic theory (such as the assumption of perfect consistency) with psychologically realistic alternatives; (2) the context for prospect theory is one individual bet given two possible outcomes; this context could not be further removed from that of theorising resource allocation across a complex economic system of many millions of individuals. Later developments of prospect theory have broadened its scope in terms of the number of outcomes (Kahneman and Tversky, 1992) but it remains a theory of the single individual decision-maker, devised without reference to system-wide resource allocation; (3) the self-interest motive is criticised and rejected by prospect theory. As regards the predicted existence of a preference ordering, prospect theory could be viewed as arguing for a different preference ordering to that of EUT but as thereby endorsing Robbins’s indubitable postulate of its existence. However, such a view would ignore the question of the stability of preferences. Prospect theory is compatible with the view of the individual as ‘a decision maker who chooses reluctantly and with difficulty … and who constructs preferences in the context and in the format required by a particular situation …’, and so with the evidence that, ‘the image of a decision maker who makes choices by consulting a pre-existing preference order appears increasingly implausible’ (Kahneman and Tversky, 2000, p.xvi). However, in light of his successful ‘predictions’ above, especially (2), then Robbins could reasonably argue that the assumption of relative stability of preferences is amply warranted as a simplification made for the purpose of theorising the system as a whole.

It is surely striking that, overall, Robbins’s predictions seem to characterise very well the actual nature of Kahneman and Tversky’s seminal paper. However, it remains to assess Robbins’s negative judgement of the coalescence of economics and psychology, in light of contemporary behavioural economics as a whole. Consider an application of behavioural economics that would seem to offer the strongest rebuttal to Robbins’s charge that the coalescence of economics and psychology must inevitably divert attention from the fundamental question of system-wide resource allocation, namely behavioural macroeconomics (e.g. Akerlof, 2002). Surely, it could be argued, the use of behavioural economics to develop (for example, New Keynesian) macroeconomic propositions, propositions therefore concerning the economic system as a whole, directly refutes Robbins’s strictures against psychology within economics?

The issue depends upon what would count as a proper ‘development’ of abstract theory, from static to dynamics, dropping the perfect consistency and perfect foresight assumptions. For Robbins, and many of his contemporaries, the broad outlines were clear (see, for example, Giocoli, 2003, pp.135-199): there was a need to develop (disaggregated) general equilibrium theory into a dynamic rather than static theory. The basic realist premise was that the economic system in fact existed and involved the coordination through the market mechanism of myriad individuals and firms. The goal was to explain this system, i.e. to successfully grasp or depict the real processes or mechanisms, the real ‘system of forces’ (Giocoli, 2003, pp.3-6) whereby this coordination was (imperfectly) achieved. Perfect foresight was not a realistic assumption, not a real force. Therefore the step of dropping the assumptions of perfect foresight and perfect consistency entailed a requirement to successfully depict real dynamic tendencies towards the results that static general equilibrium theory had achieved, thereby developing and modifying the static theory.
It is well known that the difficulties of developing a dynamic and disaggregated general equilibrium model were greater than economists at the time of the publication of the Essay were expecting. Thus at least insofar as what became mainstream economics is concerned, the assumptions of perfect foresight and perfect consistency were not dropped and the depiction of real tendencies towards equilibrium is not present in the Arrow-Debreu model. However, from the point of view of Robbins’s argument in the Essay then two crucial points should be emphasised. Firstly, the real world relevance of the static propositions is not threatened simply because they are difficult to develop further. Secondly, the requirement for theory to realistically depict the system-wide allocation process across myriad markets, individuals and firms remains. For Robbins’s argument in the Essay, therefore, the nature of macroeconomics, as it developed from Keynes would have to be seen as deficient in so far as it does not explicitly theorise (depict in thought) the process, the actual mechanisms, of system-wide resource allocation, but instead models but a few markets with macroeconomic aggregates as variables (see Chapter 3 of the Essay regarding the merely ‘conventional’ status of economic aggregates). In this sense, the characteristically Austrian nature of Robbins’s perspective comes into sharp relief, and appears radical in relation to the mainstream economics of today.

Thus, it can be seen that contemporary behavioural macroeconomics is an entirely different animal to that which Robbins argued should characterise economics: from his perspective, behavioural macroeconomics, in keeping with mainstream macroeconomics more generally, abstracts from the real process of resource allocation, reaching resource allocation outcomes solely through abstracting from, rather than theorising, the myriad markets that truly constitute the real economic system. Thus, it is only within a framework that is alien to the disaggregated general equilibrium framework insisted upon by Robbins that the psychological postulates of behavioural economics can be employed.

The economics of happiness

Another recent development within mainstream economics can unambiguously be characterised as the very antithesis of Robbins’s separation of economics and psychology. A core tenet of the ‘economics of happiness’ is that economics can be enriched by insights from a new and rapidly expanding approach within psychology, namely ‘positive’ or ‘hedonic’ psychology. According to hedonic psychology the happiness of individuals can be directly measured and compared across individuals (see Kahneman, Diener and Schwarz, 1999). Thus contra Robbins, proponents of the economics of happiness have claimed that utility and happiness are interpersonally and cardinally measurable entities.

One key intervention has been made by Layard, with obvious pertinence for the reconsideration of Robbins’s Essay. Layard (2005) proclaims the ‘new science’ of happiness as requiring a ‘revolution’ in economics and in academia more broadly. Economists and other social scientists are implored to enquire into the determinants of happiness, and at the policy level, support is given to the search for and implementation of measures aimed at raising happiness levels. Layard (2005) specifically challenges the position taken by Robbins and others, claiming that it took economics down the wrong analytical track. He, thus, refers to the way in which economics was ‘hijacked’ in the 1930s by the doctrine of behaviourism and how it
came to reject psychology (Layard, 2005, p.128). He refers to Robbins as one of the
chief culprits of the turn away from psychology in mainstream economics (Layard,
2005, p.262). In fact, Layard’s story is not wholly accurate, since as we have seen
above, Robbins was a critic of behaviourism and wanted to exclude it from economic
science. Nonetheless, in respect of Robbins’s rejection of hedonic psychology,
Layard’s interpretation is correct.

Drawing on Section 2 above, it is possible to bring out certain broad implications of
the economics of happiness, as they sharply contrast to Robbins’s conception of the
economic system. Within the economics of happiness, the study of utility experienced
directly from different activities has once again assumed importance. Among the
activities considered to impact on utility is work: hence the increased interest in
mainstream economics of measures of job satisfaction. It is argued that work itself,
not just its monetary and temporal aspects, affects the utility of workers. Layard
(2005, pp.55-75), for example, reviews the relevant evidence showing that work itself
ranks third in causal influence upon well-being (the top ranked variable being denoted
‘family relationships’). Thus, the economics of happiness has effectively returned
economics to earlier concerns with the direct utility and disutility of work. It thereby
goes against the conventional opportunity cost conception of the labour supply
decision, championed by Robbins, in which worker welfare is related solely to work
hours and wages.

Now, it should be stressed that the economics of happiness is very broad in scope and
that the issue of happiness at work is but one facet of this approach. In terms of the
comparison with established mainstream economics, it has tended to focus on the
comparison with standard welfare economics. However, given the argument of
Section 2 that an instrumental conception of work is central to the means–ends
definition and to the individualistic, consumption-centred economic ontology
promoted by Robbins, the implications of the economics of happiness approach to
happiness at work are deep. The economics of happiness effectively requires a return
to a conception according to which production, an inherently social activity, is
accorded a key role in the economy. Hence the economics of happiness would seem
to implicitly herald a return to a pre-Robbinsian conception of the scope and nature of
economic science.

It is evident that Robbins would have treated the economics of happiness with a
mixture of shock and dismay. On the one hand, he would have been bemused that
hedonic psychology could have made a comeback. Hence, for much of the period
before, during and after the time of writing the Essay, hedonic psychology was
considered to have been effectively buried by psychologists. On the other hand,
Robbins would have been critical of the economics of happiness. The attempt to
measure and compare utility and happiness would have struck Robbins as a hopeless
and futile exercise that ultimately was detrimental to the progress of economics. In
addition, the consideration of activities (including work) as ends in themselves would
have been actively resisted by Robbins, because of the direct threat it posed to the
means-ends definition of economics.
5. Conclusion

This paper has revealed the links between Robbins’s position on the ‘cost controversies’ and his conception of the real economic system as set out in the Essay. By adopting the Austrian concept of opportunity cost, Robbins was able to assume that work is a means only and that final consumption is the sole end of economic activity. In doing so, Robbins broke free from the conception of the economy promoted by earlier writers such as Jevons and Marshall. Whereas Marshall’s (and Jevons’s) definition of cost sustained a key place within their conception of the economic system for the sphere of social production (offering some continuity with classical political economy), the opportunity cost approach adopted by Robbins brought individual exchange and final consumption into the foreground, whilst relegating production to the background, of the conception of the economic system.

The concern to explain the economic system also helps to understand why Robbins distanced economics from psychology. Robbins was not, as some critics have contended, an enthusiast of behaviourism. On the contrary, he recognised that individual psychology played a part in individual choice. The point, however, was that economic science should take for granted the detailed psychology of the individual. Critically, there was no necessity for economic science to seek any kind of alliance with psychological science.

At one level, psychological data such as the existence of an order of preference was a matter of commonsense that required no formal support from psychological theory. At another level, the assumption that the preference ordering of individuals is always and everywhere perfectly rational (‘consistent’, i.e. transitive) was an example of an unrealistic assumption made by neoclassical economics for the purpose of enabling, through simplification, at an early step in analysis, the theorisation of resource allocation at a system-wide level. Psychological science could not admit of such unrealistic psychological assumptions because it did not focus on the economic system. Again this focus on system-wide results led Robbins to jettison psychological science from economics.

Based on the above, it is unlikely that Robbins would have welcomed some contemporary developments in mainstream economics. Indeed, he would have been dismayed at the rise of behavioural economics and the economics of happiness. Behavioural economics risked a loss of focus on system-wide resource allocation. The economics of happiness, by elevating work as an end in itself, removed the separation between means and ends in the definition of economics. Seventy-five years after its original publication, it seems that Robbins’s Essay fits uneasily with mainstream economics.
References


Lionel Robbins: a Methodological Reappraisal

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Abstract

Lionel Robbins contributions are often discussed in terms of two main aspects. First, the delineation of the scope of economics in terms of decision making conditional on scarcity. Second, a more methodological concern with respect to scientific neutrality and the possibility of meaningful separation between positive and normative statements in economics. The related demarcation issue is subject to intense debate and Robbins is often associated with a strong neutrality view [see e.g. Davis (2005), Mongin (2006)]. This paper attempts to situate Robbins’s aprioristic point of view in terms of posterior methodological developments. In particular, the methodology of scientific research programmes (MSRP) advanced by Lakatos (1968, 1970) has been subject to adaptations in the context of economics by Latsis (1976) in an attempt to accommodate different degrees of apriorism, falsificationism and conventionalism as scientific criteria in economics.

The historical path towards Robbins’s (1932, 1935) Essay appears to be well documented [see e.g. Howson (2004)]. The paper aims at clarifying the role of Robbins’s Essay in shaping the dominant research programme in Economics, and contends that the author’s definition of economics is central to the main elements of the hard core of contemporary research programmes in line with the neoclassical research programme.

Keywords: Lionel Robbins, Scientific Research Programmes, Falsificationism.

JEL Classification: B4.

1. Introduction

Reappraisal of scientific theories constitutes a central issue in the history of science, particularly during the last few decades. Although one can classify the appraisal of scientific criteria in economics in different ways, at least one approach stands out as specially germane to the work of an economics theorist as important as Lionel Robbins: the gradualist approach of the Methodology of Scientific Research Programmes (MSRP) first put forward by Lakatos.

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The *Essay* reveals two essential issues, namely, Robbins’s delineation of the scope of Economics as going well beyond the assessment of the causes of material welfare that were embodied in previous characterizations and the methodological issues involved in Robbins’s position on neutrality. The historical path towards the essay is well documented [see e.g. Howson (2004)], and was summarized by Kirzner (1975, p. 117) “These considerations (on price and exchange at the margin) thus clearly set Robbins’s definition apart from the earlier definitions of economic activity in terms of maximization, despite the undoubtedly important part that the latter conception, in conjunction with the literature on scarcity, played in the emergence of Robbins’s view of economics.”

Indeed, the definition of economics as being about the study of decision-making conditional on scarcity has become a standard. The neoclassical dichotomy associated with the optimal allocation of scarce resources across possible alternative ends reinforces just how widespread Robbins’s definition did in fact become.

The second issue, pertaining to a more methodological concern with respect to scientific neutrality, involved the possibility of meaningful separation between positive and normative statements in Economics. The demarcation issue has of course been subject to intense debate and Robbins is often associated with a strong neutrality view [see e.g. Davis (2005)] and its aprioristic view followed a tradition that could be traced back to Nassau Senior [see e.g. Latsis (1976)].

By focusing on methodological aspects, this paper situates Robbins’s aprioristic point of view in terms of posterior methodological developments. In particular, the methodology of scientific research programmes (MSRP) advanced by Lakatos (1968, 1970) has been subject to adaptations in the context of economics by Latsis (1976) and attempts to accommodate different degrees of apriorism, falsificationism and conventionalism as scientific criteria in economics. In particular, Mongin (2006) has considered the possibility of a weaker form of non-neutrality, which is pertinent to establish the logical status of Robbins’s neutrality view within the context of this broader conceptual setting.

Hence, two research questions drive this paper: the applicability of the MSRP in economics in general and the place of Robbins’s seminal *Essay* of 1932 in particular. However, before getting to the main objective of our paper, certain preliminary issues must first be sorted-out. For that purpose, we analyse the development of the MSRP and how it relates to economics per se, turning to several recent methodological developments (e.g. Mongin, 2006) as well as a reconsideration of old concepts such as situational determinism (Nightingale, 1994).

Focusing on the neoclassical research programme, we then proceed to review methodological developments by Robbins himself that are relevant to the earlier analysis. With these building blocks in mind, we can next evaluate the place of Robbins’s *Essay* in the context of the neoclassical research programme. Hence, the first section explores the role of research programmes in economics. The second section overviews most influential methodological approaches for scientific assessment with reference to Economics. The third section explores Robbins’s methodological contributions found in the Essay. The fourth section analyses the role
of Robbins’s *Essay* and its historical place in the neoclassical research programme in economics. The fifth and last section brings some final comments.

2. Scientific Appraisal in Economics and the Methodology of Scientific Research Programmes

2.1 General aspects

Even though the MSRP is based on falsification criteria to appraise scientific theories, it operates quite differently in economics that in the natural sciences since the assessment of falsification criteria in economics is destined to be more complex. In fact, despite the growth of experimental economics, one has to concede that it is often a daunting task to obtain testable hypotheses in the context of complex social systems.

It is also true that distinct views on the evolution of economics as a falsifiable domain can be discerned in the history of economic thought literature. In fact, if one seeks to stay within the strictures advanced by Canguilhem (1970), the absence of value judgments in theoretical construction in the social sciences would not be possible at all since the object of study is mutable and the choice of the associated analytical categories cannot be completely neutral. Indeed, the research agenda in economics may in part possess historical conditioners that may themselves facilitate conviction strategies at a given moment.

With the previously mentioned caveats in mind, it is relevant to consider how certain influential methodological developments are related to Robbins’s contributions. Although, Kuhn (1970) argued that the forward thrust of science occurred in terms of revolutions constituted by disruptive changes in paradigms that did not reveal completely rational behaviour on the part of the scientific community, Lakatos (1968, 1970) advanced a counterclaim concerning the methodology of scientific research programmes (MSRP). In the gradualist view of the MSRP, there may be progress or its converse, which implies a much more sophisticated falsificationism in contrast to earlier dogmatic (or naive) falsificationism. Moreover, the concept of a research programme, positioned in a middle ground between Popper’s falsificationism and Kuhn’s paradigmatic shift, concerned itself with “normal science”, with the latter conceived primarily for the natural sciences. Experiments and testability were assumed as given in the developing of arguments regarding the research programme.

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197 Arida (1996) even argues for the existence of a contrast between the American and European views on the progress of economic thought. In the former, knowledge would essentially be cumulative and evolves progressively according to a Popperian conjecture/refutation sequence and therefore somewhat mimics the trajectory of natural sciences where recent theories incorporate the current temporary ‘truth’. The latter, in contrast, conceives the existence of distinct theoretical matrices that cannot trivially suppress each other and must, at least, co-exist at some key points. It is important to stress that beyond highlighting those polar cases, it is possible to detect a non-negligible content of rhetoric in explaining the growing dominance of a particular research framework over time. McCloskey (1998) also pursues similar arguments.

198 Robbins (1979) remarks that Popper should not be included in the latter category. For a discussion of Lakatos’ views on Popper see Lakatos (1974)
Lakatos’ research programme concept is broader than Popper’s in the sense that it accounts for what Popper observed as irrational behaviour of scientists. An example of the latter would be a continuing insistence on working with theories where evidence shows them to be of limited value or even, by a Popperian definition, false. In this vein, Caldwell199 (1994, p.86) traces the discussion back to the development of Popper’s ideas by Lakatos, showing that “Lakatos’ positive contribution is to complete the program begun by Popper by proposing a methodology of scientific research programs that contains the best of Popper’s insights (some of which, incidentally, agree with ideas propounded by Kuhn and Feyerabend) and that enables a rational (as opposed to a sociological or irrational) reconstruction of methodology and of the growth of scientific knowledge.”200

The difficulty in empirically falsifying a theoretical hypothesis is also related to the so-called Duhem-Quine problem, i.e., the claim that empirical, testable implications incorporate several interconnected auxiliary hypotheses. In other words, one never faces an isolated individual hypothesis, but rather a set of hypotheses, what is sometimes referred to as Quine’s “holistic” thesis of scientific theorizing. In economics, for example, ceteris paribus assumptions are often considered and not easily disentangled from a string of hypotheses. Ceteris paribus is so central to many economic models that “the requirement of ceteris paribus, despite all sorts of ingenious techniques, is very exacting.” (Robbins, 1979, p. 999)201.

The MSRP addresses the previously mentioned concerns through delineating the scope of a research program into the hard core and the protective belt as the essential categories. The former refers to assumptions of a more axiomatic nature that are not directly testable but that define the primitives of the framework. The latter, on other hand, characterize the set of auxiliary hypotheses that are prone to empirical falsification. This means that specific violations do not necessarily jeopardize the existence of the research programme. In fact, the progressive or degenerative nature of a research programme has to do with robustness to empirical refutation and the ability to explain or not new empirical facts, that is, to improve the empirical content/scope of the model with comparable robustness properties.

Although criticisms of falsificationism abound, with many claiming a kind of methodological pluralism202, economists in general203 still consider falsificationism as the method to appraise economics research. Blaug204 (1992, p. xiii) even contends

199 Caldwell is one of the most important authors in tracing the recent developments in the methodology of economics, and his restrained methodological pluralism (instead of the complete relativistic pluralism of Feyerabend) and positive heuristics are relevant contributions.
200 Caldwell (1994, p. 86) continues the argument, showing the ultimate goal of Lakatos: “His sophisticated methodological falsificationism, then, not only lays down prescriptions by which science can proceed, it also provides a basis for a descriptive rational reconstruction of how scientific disciplines often evolve”.
201 Cross (1982) investigates Quine’s thesis, arriving at an interesting result on grouping hypotheses for testing in macroeconomics. Boyle and Gorman (2003) also revisit the subject of Duhem-Quine, with the goal of disputing the original thesis. Even if they do not match their ultimate goal, it is a work that delineates many important issues regarding Duhem-Quine in economics.
202 see e.g. Dow (2007), where the author argues for some form of rigour in the face of methodological pluralism to avoid a disinteresting relativism of the form anything goes.
203 The word economists here is used in the same vein as Mongin’s (2006) economist qua economist.
204 Blaug has been at times a supporter and a critic of the MSRP, and his works have been central to the discussion of the applicability of the MSRP in economics.
that “modern economists do in fact subscribe to the methodology of falsificationism: despite some differences of opinion, particularly about the direct testing of fundamental assumptions, mainstream economists refuse to take any economic theory seriously if it does not venture to make definite predictions about economic events, and they ultimately judge economic theories in terms of their success in making accurate predictions” 205. In terms of pragmatics, it seems plausible to hold that modern economics subscribes to falsificationism, although some controversy still surrounds the applicability of MSRP into economics. Thus, the original admonition by Leijonhufvud (Latsis, 1976) that, since the MSRP was created to apply to natural sciences, a lot of caution is necessary on its transposition to economics, is still valid.

The major critiques to the applicability of the MSRP in economics can be divided into three kinds 206: the conceptualization of a research programme; the search for empirical content; and the rhetorical content of alternative programmes. The first point refers to the fact that it is not a trivial quest to define the hard core or the auxiliary belts of alternative competing research programmes. The second one alludes to how Lakatos emphasized the role of new empirical content as necessary to validate or not the hypothesis brought forward by the programmes. The last point has its roots in Kuhn’s and others’ work in which the commensurability of different programmes is considered a necessary condition for appraisal of scientific programmes.

Although several arguments for all three critiques are advanced in this paper, the main point to be emphasized is that the neoclassical research programme is by its very nature broadly defined. Logical foundations for the applicability of MSRP in economics then may be based on the same foundations for its applicability in natural sciences: it is based on sophisticated falsificationism and presupposes an ontology of economics that may or may not be acceptable to the interested researcher. Lakatos’ work in economics is unfashionable (Backhouse, 1998) and when used is accompanied by several caveats. Based on the former analysis, and considering the validity of some form of methodological pluralism that allows different valid choices, we argue that even if taking into account its limitations, the MSRP is a valid and impartial way to appraise scientific theories. It may not be the furthest a researcher could go into analysing the development of science, especially regarding a subject as problematic as economics, but it is at the very least a concrete and sound foundation on which to build the appraisal of alternative economic theories. Maybe it is a useful starting point (Backhouse, 1998) after all, but one that should not be hidden behind curtains of caveats. However, we want to try arguments based on a unashamed view of Lakatos’ ideas, with the goal to arrive at a semi-rigorous definition of the neoclassical research programme on which to base some analytical construct useful in economic methodology, and apply it to Robbins’s Essay.

Latsis (1976) illustrates MSRP in economics with four different examples. The most emblematic of the MSRP in economics is that of the perfect competition neoclassical research programme. In that case, the hard core would be characterized by hypotheses concerning profit maximization, independence of decisions, and complete relevant

205 Blaug (1992), however, shows that there is a strong difference between the discourse of economist regarding falsificationism and their practice.
knowledge. Additionally, complementary assumptions regarding homogeneity of the product, the number of competitors and perfect mobility establish the familiar competitive environment. It seems clear that even when one departs from this benchmark, other research programmes in mainstream economics share some common elements with the neoclassical hard core.

2.2 The neoclassical research programme and situational determinism

The object of analysis of the MSRP is a scientific theory. To better situate Robbins’s contributions in the Essay in terms of the MSRP, it is necessary to understand the specific programme that is consonant with Robbins’s work, viz., the neoclassical research programme.

Backhouse (1998, p. 41) summarizes one of the difficulties in defining a research programme as to “whether SRPs are to be defined on a large or small scale”\(^{207}\). We contend that both levels of analysis yield interesting results and are not mutually excludent. We only choose the macro scale because we intend to criticize some aspects of previous definitions of the neoclassical research programme.

Even after choosing the scale on which the analysis is based one is not free from critiques. (Backhouse, 1998, p. 41): “programmes may overlap, with some theories apparently fitting into two different programmes; different programmes may be related to each other; it is sometimes difficult to identify a hard core that is unchanged over the life of a research programme”. To provide a counter argument against the two points we use the metaphor of the definition of sets in abstract algebra in mathematics. Different sets are defined by slight changes on its properties – two different sets can have almost the same properties and still possess unique features. The same situation happens in economics - different economic theories arise from slight changes in the hard core of a research programme, maybe generating alternative research programmes – and this happens on different scales. This may complicate the development of a typology of research programmes in economics, but is not a major problem per se since appraising scientific theories imply a search for the specific features of alternative research programmes. The search cost may be high, and one may discard the MSRP on the grounds that it is unfeasible and does not yield useful content if the search cost involved is taken into account, but there is no major problem in using the MSRP if the proper scale is defined.

Having defined the scale of analysis and its subject – the neoclassical research programme, we look into how this programme has been defined in the past. A central concept to the definition of the neoclassical research programme is situational determinism as advanced by Latsis (1972, 1976).\(^{208}\) The concept of situational determinism\(^{209}\) states that the typical neoclassical agent behaves as part of a single-exit game. In that polar case the agent’s decision is uniquely determined by

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\(^{207}\) The author continues: “At one extreme we can view neoclassical economics as one SRP, ranged against various heterodox programmes.”

\(^{208}\) The first version of Latsis’s argument dates from 1972, but the often cited reference is 1976, where the author analysis situational determinism alongside the application of MSRP in economics.

\(^{209}\) The concept received immediate criticism by Machlup (1974), and was revisited by Nightingale (1994), and Szenberg and Ramrattan (2004).
situational considerations. In contrast, in multiple-exit situations non-situational aspects also become relevant for decision-making.

To be sure, there is no claim that psychology is involved and Latsis (1976) argues that single-exit problems define the neoclassical programme. His famous example is that the monopolistic theory is methodologically no different than perfect competition models, since the monopolistic firm would face a single-exit game - the firm’s “choice” would logically have to be the one predicted by the model, with no rational deviation. Alternative research programmes in economics would then be those that had multiple-exit situations. Latsis (1976) advances some alternative programmes in economics, and his examples include economic behaviourism and organizational approaches. Later alternative programme propositions include the work of Lavoie (1992), who argues for a postclassical programme defined as a synthesis of post-Keynesianism and Neo-Ricardianism. Lavoie (1992) is even more stringent when defining an alternative from the neoclassical programme, deriving his synthesis as one that abandons scarcity analysis in favour of alternative foundations.

Machlup (1974) criticizes Latsis (1972) on many grounds, but his specific criticism regarding situational determinism is based on three arguments, what Machlup (1974, p. 276) calls confusion regarding single-exit situations: the confusion between action and reaction; the confusion between reactions and effects of reactions; the confusion between the effects of reactions of particular persons and the effects of mass reactions or, more correctly, of assumed typical reactions of imaginary persons. Although Machlup’s (1974) arguments can be amended to allow the possibility for maintaining situational determinism as the neoclassical programme, there is another argument that is even stronger: if one introduces any kind of probability distribution to a neoclassical model, the single-exit situation vanishes, since a researcher would not be able to identify the only course of action of the representative agent. Even more so, a single agent would be able to take different, mutually exclusive actions, and maintain the rationality necessary in neoclassical models, and such a model would then still be characterized by the hard core of the programme.

A similar argument is found in Runde (1996) where he analyzes Popper in the context of probabilities, and what Popper defined as propensities, a prima causa of probability in social sciences. Runde (1996) argues that Popper’s view is incompatible with that of situational determinism, since (Popper, 1990, p. 17): “with the introduction of propensities, the ideology of determinism evaporates. Past situations, whether physical or psychological or mixed, do not determine the future situation.”

Game-theory would also be a source of multiple-exit situations, and game-theoretical models thrive in the context of uncertainty. Indeed, Runde’s (1996) analysis of Popper’s work has also shown how probability plays a role in undermining single-exit
We contend that the neoclassical programme envelopes single-exit situational determinism; maybe all such situations as they happen economically. However, the neoclassical programme is broader than that, with an auxiliary belt that allows multiple-exit situations. The hard core of the neoclassical programme would then not necessarily be identified with situational determinism, but decision-making conditional on scarcity, based on rational behaviour.

Alternatives research programmes would have to be based on a different hard core than the neoclassical programme. A compelling new programme (certainly not the only one as the aforementioned work by Lavoie (1992)) is particularly interesting as well, that of complexity theory. An argument can be made that the study of complex adaptive systems is not a theory when applied to social sciences, but enough work has been done on these kinds of dynamical systems for complexity to warrant the moniker of an alternative research programme, if not a proper theory. Thus, Colander et al (2004, p. 485) observes that “this article argues that economics is currently undergoing a fundamental shift in its method, away from neoclassical economics and into something new. Although that something new has not been fully developed, it is beginning to take form and is centered on dynamics, recursive methods and complexity theory.”

It is in fact not particularly difficult to describe complexity theory and evolutionary economics as an alternative research programme, since no main characteristic of the neoclassical research programme appears to be included in it. For instance, evolutionary economics has incorporated Simon’s idea of satisficing instead of the usual decision making concept. Satisficing is a strategy where agents attempt to meet some adequate criteria (for instance, have to satisfy some constraint) for its decision, instead of identifying an optimal solution. Although some of the new terminology brought by complexity theory is definitely noise and could be explained through orthodox economics theory (see Zeidan and Fonseca, 2005), enough new concepts are brought that make complexity theory an alternative theory to neoclassical and other research programmes in economics.

Nevertheless, we also contend that despite the existence of different research programmes in economics, some relative consensus has been attained with respect to central elements. Latsis (1976) outlined the implicit positive heuristics that would indicate appealing features to be on the look-out for. Apart from analytical tractability that largely justified more static formulations and formulations with well defined

\footnote{As Runde (1996, p. 478) puts it, “Of course there may be more than one action for each agent that follows from the "logic" of his or her situation, particularly in game-theoretic situations in which the payoffs to any one agent of taking some course of action depend on the actions of other agents. Popper does not have much to say on situations of this kind.” Although trying to specifically analyse situational determinism in the context of Popper’s work, Runde’s point is easily generalized to reinforce the notion that single-exit situations are not found in more developed models. We argue that those developed models are still part of the neoclassical programme, since they do not violate the hard core but bring more empirical content to the programme, constituting novel facts, one of the conditions for the applicability of the MSRP.}

\footnote{Nightingale (1994) applies the Lakatosian framework to evolutionary economics and arrives at a hard core of evolutionary economics composed of four assumptions, including the differentiation of individuals in a population (no representative agent), and the obvious mechanism of selection.}

\footnote{See Markose (2005), for an important survey and contribution on the analysis of the relationship between complexity and economics.
equilibrium, one faces aspects that relate to the rationality of agents’ decision making. In fact, Latsis (1976) contends that the neoclassical research programme embodies a rationalistic view more along the lines of a single-exit situation. Nevertheless, it is important to stress that the reshaping and broadening of the protective belt of the neoclassical research programme makes multiple-exit situations worth discussing and related issues will be further addressed in the text.\footnote{Sabooglu and Villet (1992) also criticise Latsis for the excessive identification of the neoclassical research program with single-exit situations.}

The next section will further discuss to what extent one can relate Robbins’s contributions to those methodological developments.

3. Lionel Robbins: Some Methodological Remarks

Taking as reference modern methodological tools, one can reassess Robbins’s \textit{Essay} to put it in the context of MSRP and other modern lines of research, as delineated in the previous section. With historical hindsight, the main transforming ideas put forward in the Essay accomplish a great deal to mould modern economics.

The classic definition by Robbins (1935, p. 16) that “Economics is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” is still the standard definition of the field. But the impact of his other contributions in the Essay can be considered more important for the shaping of modern economics – first, the differentiation between positive and normative economics, and second, the idea that economics can be expressed as a system of logical deductions from axiomatic principles.

The impact of Robbins’s ideas was immediate and of course much criticism has been aimed at his work. Three major contributions summarize the historical background and impact, and offer a thorough criticism of Robbins’s \textit{Essay} - Kirzner (1960, 1975), Blaug (1980, 1992), and Caldwell (1982, 1994). In the realm of current economic methodology Robbins’s positivism is considered dead and plural methodological approaches are advocated (see, for instance, Dow, 2004).

One good example of the current methodological debate is Mongin (2006), who analyzes the value judgment problem through economic evaluations, by trying to distinguish evaluative statements from actual value judgments. For Mongin (2006), the value neutrality problem has received three solutions in modern economics, with Robbins’s position being central to one of them. Those three solutions are classified as strong neutrality, weak neutrality, and complete non-neutrality. The author dismisses the two polar extremes that were respectively defended by Robbins and Myrdal and aims at establishing a compromise in terms of a weak version of non-neutrality.\footnote{The third category would refer to the acceptance of normative statements in very narrow specific contexts.}

The proposed fourth category defines weak non-neutrality, that (Mongin, 2006, p.) “starts with the broad claim that the question of making value judgments does arise for the economist qua economist, and that he might, might and should, or might not, make these judgments, depending on the case at hand. This claim clashes with the strong neutrality thesis and fits in with the weak neutrality thesis. The line is drawn

\textit{...}
with the latter by rejecting its containment claim.” In any case, the main departure from strong neutrality is associated with the excessive simplicity involved in the dichotomy between evaluative or ethical predicates that embodies the usual separation between normative and positive analyses as motivated by Hume.

The *Essay* can easily be classified as adherent to the strong neutrality position. The work was fundamental to the view of economics as a “quasi-hard” science, in line with the Austrian school of economics. Robbins (1979, p. 999) recognizes this and reiterates: “but on the positive analysis of the implications for behaviour of the fact of scarcity – Economics – I see no reason to recognize any difference between such generalizations and the generalizations of Physics or of Biology”.

Although the modern discussion of methodological issues in economics considers the strong neutrality position as naïve, its strength is pervasive in modern economics. First of all, Robbins’s (1932, 1935) position stems from the original problem of the demarcation of economics as a science, a problem very much unresolved then, as summarized by the author (Robbins, 1935, p. 2): “indeed, it follows from the very nature of a science that until it has reached a certain stage of development, definition of its scope is necessarily impossible. For the unity of a science only shows itself in the unity of the problems it is able to solve, and such unity is not discovered until the interconnection of its explanatory principles has been established.” This argument is hardly ever brought up when criticism of Robbins’s positivism arises.

Even Robbins (1979) found it easy to defend his earlier canonical work, subscribing, with some clarifications, to the same position held over 45 years before. For instance, regarding his definition of economics as based on scarcity, Robbins (1979, p.997) expands on his earlier work by affirming that: “as regards the accusation of narrowness, I suspect this rests on misapprehension due perhaps to undue preoccupation with the theory of exchange. In fact, explanation of the influence of scarcity extends far beyond the immediate incidence of catallactics: it covers questions of incentive, institutions, and indeed much of the legal framework of society, not to mention matters of indiscriminate, as well as of discriminate, benefit.”

The positivism subscribed by Robbins, in his Essay and later work, is based on the original demarcation problem, where his preoccupation is with an analysis that resonates with the work of his contemporaries, e.g., Austrian authors such as Mises and Hayek. Much has been written on the influence of Austrian authors on Robbins’s works and Robbins (1979) remark on cattalactics, above, is a return to a concept first developed by Mises, and later used also by Hayek.

In Mongin (2006), the position of positivism in modern economics methodology is subsumed in the strong neutrality view. In this sense Robbins (1932,1935) is still ingrained in mainstream economics. But Robbins (1932,1935) also advances further

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217 For a particularly sharp critique, see Davis (2005).
218 See, for instance, Kirzner (1960,1975), or Robbins’s biography by O’Brien (1988).
219 Cattalactics is nowadays a footnote to the history of political economy, but is an interesting expression of the desire of political economists to clearly and unambiguously define their craft. Its definition is the economics of market society, and Mises used it to try to define the scope of economics from his more general study of human action principles (*praxeology*), since he was dissatisfied with then current economics terminology, which he did not consider rigorous enough.
methodological issues in his search for unifying principles of economics thoughts. Robbins’s *apriorism* is a tentative search for the definition of economics as a logical system derived from basic principles.

Even though the prevalence of strategic interdependence in non-ideal settings is largely explored with the development of Game Theory and other important tools in economics, the explicit optimization assumption is recurring. It is important therefore to characterize Robbins’s *Essay* contributions in later delineating the central issues on MSRP in Economics. The aprioristic view presented in the *Essay* is often referred to as embodying a strong rationality assumption. Nevertheless, as indicated by Robbins (1979, p. 998): “But if ‘rational action’ means, as I think it should mean, *consistent* action, in the sense that, if one prefers A to B and B to C, then it is consistent to prefer A to C and inconsistent to prefer the contrary, I certainly do not hold that all action that is not vegetative must be regarded as rational in the sense that mutual contradictory preferences and policies on the part of single individuals or collection of individuals are ruled out.”

He does not necessarily champion an extreme rationality view but rather the prevalence of consistent constructions at the logical level and it therefore would be open to different configurations of the protective belt of the research programme. In that sense, some form of flexible *apriorism* as given by conventionalism is accommodated. In that case creativity is allowed and one is not hostage to a very limited set of *apriori* categories. Influential examples are given in terms as the ‘as if’ approach considered by Machlup (1955) or most notably the instrumentalism defended by Friedman (1953) that emphasizes predictive power of the theoretical construct rather than realism. Examples of unrealistic frameworks proliferate in economics as for example the Real Business Cycle research that emphasizes the role of technological shocks in explaining economic activity fluctuations. The lack of closed analytical closed form solutions for those dynamic general equilibrium models were later made feasible by the use of calibration methods that became widespread in macroeconomics.

At any rate, however, even when computational improvements provide an additional capacity for refutation (or rather generic consistency), it is important to emphasize that the route towards a progressive scientific research programme in economics is likely to be less smooth than in the natural sciences and diverge from a conjecture/refutation path, since it is more difficult to generate testable empirical hypotheses in economics than in natural sciences. Indeed, that should be the case even when it is not a matter of theory being ahead of measurement.

4. Lionel Robbins and Research Programmes in Economics

The argument for the usefulness of the MSRP as a tool to explain the development of economics was advanced in the first section. Robbins (1979) is an important contribution to the debate regarding Robbins (1932,1935) and its links with the work on MRSP being advanced by Latsis (1976). It is worth mentioning that Robbins (1979) is sympathetic to the MSRP approach, but with some caveats, especially regarding the possibilities of accruing true generality with MSRP. In this section, we follow that direction and attempt to articulate Robbins’s contributions when one regards economics in terms of the MRSP.
Where does Robbins’s *Essay* rank if one is to analyze the evolution of economics through the prism of MSRP? To answer this research question, which is the ultimate goal of the paper, we first take the neoclassical programme as a benchmark.

The neoclassical research programme and other mainstream research programmes are often criticized for their static character and reliance on a strong informational assumption. It is important to stress, however, that at least with respect to this claim the scope of neoclassical economics has greatly expanded to encompass different forms of asymmetric information. There are in fact progressive research programmes in mainstream economics and even anomalies detected in the realm of Economic Psychology which have not imposed serious wounds in what concerns the hard core of mainstream research programmes. One example is the issue of self-control and conflicts between short-run and long-run that are addressed with hyperbolic discounting in contrast to exponential discounting without, nevertheless, abandoning an optimization approach.

We propose that Robbins’s definition of economics in terms of decision-making conditionalities on scarcity and the associated optimal allocation of resources highlights the essential element of the hard core of research programmes in economics, namely, that objective functions and constraints as defining an optimization problem characterizes economic analysis. This does not mean that those elements remain as simple as in initial neoclassical formulations. In fact, the protective belt is gradually reshaping itself, but the essential optimization notion remains central in the hard core of mainstream research programmes, and the explicit consideration of it in the delineation of economics presented in the *Essay* is important.

The modern neoclassical research programme may or may not still be classified as progressive since many research questions are still open and many models are still being carried out in the grand tradition of this research programme. The hard core is mostly constant, as would be expected, and the nature of the programme, its definition as progressive, depends on the formulation and research being done in the auxiliary belt axis. Heterodox theory, of course, assumes that the neoclassical model does not hold. Appraisal of current alternative research programmes is unusual, since philosophy of science is regarded as a historical discipline – one major issue is the identification problem, i.e., it is very difficult to rigorously account for alternative research programmes while they are developing. Contemporary economics presents an interesting case, however, where complexity theory is clearly an alternative research programme to all other programmes in economics, be it orthodox or heterodox, since the hard core of the complexity theory research programme is almost completely (but not completely) incongruent with mainstream economic research programmes.

An alternative to Robbins’s (1932, 1935) famous definition is given by him in the same essay (Robbins, 1935, p. 83): “In pure Economics we examine the implication of the existence of scarce means with alternative uses. As we have seen, the assumption of relative valuations is the foundation of all subsequent complications.” That the points raised above are in the center of the neoclassical research programme is hardly controversial. Coupling that with the strong neutrality position expressed in
the rest of the work, we contend that Robbins’s *Essay* is one of the central pieces of the neoclassical research programme. We argue that situational determinism, although not incongruent with Robbins’s decision-making conditional on scarcity, presents more problems to the definition of the neoclassical research programme than Robbins’s and others authors’ contributions. A straightforward axiomatic set with decision-making under scarcity as one of the axioms would better characterize the neoclassical research programme.220

The neoclassical research programme has an in-built strong aversion to value judgments, for better or worse. Monguin’s (2006) version of the weak non-neutrality, as plausible and interesting as it is, is clearly incongruent with the *praxis* of neoclassical economics. Models that strive for pure impartiality are the norm in modern micro- and even macroeconomics, and are judged, in theory, by falsificationism, while value judgments as observed by Monguin (2006) are strange to its core. In this sense, an epistemology of science that incorporates Monguin’s concept of weak non-neutrality would certainly be an alternative to the neoclassical research programme.

Not every argument, however, found in the *Essay* has permeated the neoclassical research programme. Robbins’s distrust of empirical studies is expressed when he argued against the incautious use of empirical studies (Robbins, 1935, p. 107): “we are here entering upon a field of investigation where there is no reason to suppose that uniformities are to be discovered. The "causes" which bring it about that the ultimate valuations prevailing at any moment are what they are, are heterogeneous in nature: there is no ground for supposing that the resultant effects should exhibit significant uniformity over time and space.” The same argument is echoed in Robbins (1979, p. 1003): “in my judgment current appreciation of the real value of economic science has been too much influenced by excessive focus on its power to predict to the neglect of its wider power to explain.” Although an interesting argument, it is a battle that Robbins ultimately lost since prediction models that use real data are currently widespread in all areas of economics, be they neoclassical or not.

5. **Final Comments**

The paper aimed at assessing Lionel Robbins’s impacts on methodological developments that were later advanced to appraise scientific method in economics. For that purpose we revisited important issues in economics methodology. In particular, we discussed the methodology of scientific research programmes (MSRP) as advanced by Lakatos (1968, 1970) and further discussed by Latsis (1976).

We tried to highlight the limitations of the MSRP utilisation in Economics, but also how it can lead to some interesting insights, especially since economists still regard themselves as practitioners of falsificationism in their craft. Recent developments in the methodology of economics lead to possible increased interest in the applicability of MSRP in economics since we showed that some arguments show promise in dealing with the limitations of the applicability of the MSRP in economics. Using

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220 Although an interesting and maybe herculean research question in itself, a complete hard core definition of the neoclassical research programme is not the goal of this paper. Here we merely argue that points raised in the *Essay* are part of it.
these arguments as a logical foundation, we then proceeded to use the MSRP to assess Robbins’s Essay. We emphasized the role of situational determinism as the definition of the neoclassical research programme and concluded that the concept is insufficient to broadly define this particular programme.

We contend that Robbins’s dichotomy between scarce resources and pressing necessities that require optimal allocations define optimization as a central element in the hard core of different research programmes, especially the neoclassical research programme. We also argue that this definition, alongside the aprioristic view – now regarded as strong neutrality - found in the Essay is central to the neoclassical research programme.

In summary, we concur about the seminal character of Robbins’s Essay in explicitly setting the basis of the neoclassical research programme. Even though the programme might have not attained the stability it strived for and has maybe entered its degenerative phase, the notion of optimization remains central to it.

An issue that deserves further investigation refers to the reconfiguration of the protective belt of the neoclassical research programme to assess if the programme has entered its degenerative phase. In fact, the particular new forms of optimization problems that arise as new research questions merit further discussion. However, those considerations extrapolate Robbins’s more general considerations that characterise the essential elements of the hard core of the neoclassical research programme.
References


Robbins on the Stationary State:  
an Early Attempt to Distinguish Idealization from Abstraction  

Menno Rol∗

Abstract

In 1930, Lionel Robbins warned economists not to confuse the Classical stationary state of an economy with fixed endowments of an economy as conceived by John Bates Clark. The first way of modelling assumes that economies produce a stationary state as a result of balancing forces, the second way of modelling assumes that endowments are stable by hypothesis and aims to show that equilibrium can be attained under such conditions. Robbins said (1930, pp.206-7) that ‘[b]oth rule out inventions and fundamental changes in nature and human beings. But the one admits the possibility of variations of labour and capital, the other excludes these by definition’.

With Heinz Kurz, I hold that Knut Wicksell (1954) used the Clarkian and not the Classical approach in his interest - and distribution theory. For his theory of capital and distribution in the long run Wicksell dropped the assumption of a stationary state and used a comparative static economic model of what actually is a long run dynamic situation. However, there is a further important distinction involved, not observed by Robbins, which highlights the difference between classical and Clarkian approaches.

I shall first show that the two strategies used in Wicksell (1954) and (1951) are in fact opposed in precisely the way Robbins indicated. Secondly, I shall claim that these two strategies essentially make the difference between idealization and abstraction. Also, as this distinction is often misunderstood, we can see how a rereading of Robbins’s methodological publications helps clarify economists’ reasoning strategies even today.

1. Introduction

In 1893 Knut Wicksell published his Über Wert, Kapital und Rente (see his (1954) for the English translation Value, Capital and Rent) in which he formalized and extended Böhm-Bawerk’s interest theory. In this work, he praised Böhm-Bawerk’s insightful concept of interest, which allows for a positive rate of interest even in a stationary economy. Böhm-Bawerk’s concept was new. The dominating idea was that, for the stock of capital to have a determinable price, it had to grow in size. For instance, Wicksell observed that Walras had said:

“[I]n order to determine the level of interest, it is necessary to turn from the investigation of a stationary economy to the investigation of a progressive one, where new interest-bearing capital goods are produced, whose capital can be determined from the production costs.” (Wicksell 1954, 167)

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It is true that, if the production costs of a capital good are known, one can calculate interest by focussing on its marginal productivity. However, due to the heterogeneity of the macroeconomic capital stock, this is not feasible for an entire economy. Böhm-Bawerk used a version of the classical wages fund instead. Wicksell continues his observations, saying:

“In the stationary economy too […] a rate of interest of the circulating capital will undoubtedly establish itself, precisely because the lengthier methods of production prove more profitable. […] The merit of having taken the decisive step forward belongs in this field to Jevons and, above all, to Böhm-Bawerk.” (Wicksell 1954, 167-8.)

Indeed, the Austrian Böhm-Bawerk is the first economist to perceive the importance of time as a productive factor in a complete theory of distribution. In this theory, interest does not come forth as the surplus of the value of new capital over old.

Wicksell wrote his work on value, capital and rent (henceforth VCR) as both a summary and an improvement of Böhm-Bawerk’s magnum opus. It is a mathematical interpretation of Böhm-Bawerk’s theory in which capital is conceived as a wages fund enabling entrepreneurs to invest in more roundabout – i.e. more capital intensive – ways of production. In the course of intermediary goods’ maturing into finished consumer goods in production, they gain in value as a consequence of the technical datum that ‘past’ goods satisfy more needs than ‘present’ goods (or, as Böhm-Bawerk alternatively put it, that present goods satisfy more needs than future goods; known as Böhm-Bawerk’s ‘third cause’). This technical fact has nothing to do with issues of liquidity preference (which come close to Böhm-Bawerk’s ‘first cause’) or with irrationality of agents on the market (i.e. the underestimation of future needs, the ‘second cause’). It has to do with the phenomenon that fishing rods help catch more fish than our bare hands do and that the more sophisticated ways of fishing require investment and, hence, more total labour input. We could say that what the wage workers receive is a present good, but the revenues for the investor, who lends from the wages fund to sustain the workers, are ‘future goods’221. The mere fact that labour hours mature in the production process gives rise to interest, which is the marginal productivity of roundabout production. It is this Böhm-Bawerkian concept of interest that Wicksell fitted into a general theory of production, income, and distribution in a stationary economy. It constitutes the body of VCR.

This work, originally from 1893, marks what I would call Wicksell’s first phase. In his later work, Wicksell tried to develop a broader theory with growing capital, laid down in his Lectures on political economy (Swedish original 1901). The stationary state of Böhm-Bawerk had instead assumed inelastic supply of steadily circulating

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221 ‘Future goods’ is a translation of Zukunftsgüter (or −ware). These are goods now in existence but the expected revenues of which are the object of valuation. They are not to be confused with goods that do not exist yet. In note 1 on p.323 of the Positieve Theorie des Kapitales (1889) (henceforth referred to as PTK), Böhm-Bawerk remarks that that no fine translation exists in either English or French to indicate the subtle distinction between the revenues of goods that are and of those that are not existent now. But ‘future goods’ must be taken to exist in the present, for otherwise they cannot be exchanged against present goods. The possibility of physical exchange is crucial to explain the source of interest. I shall invariably use the term ‘future good’ for Böhm-Bawerk’s Zukunftsgüter.
capital. Thus, Lionel Robbins wrote in his introduction to Wicksell’s *Lectures on Political Economy* that Böhm-Bawerk was “able to present an account of equilibrium of capitalistic production which combined all the best features of these apparently divergent theories, and, by invoking the methods of Walrasian analysis, he was able to present it in a much more general setting […] It is true that this theory itself is not complete. It was fully developed in [Wicksell’s] *Lectures* only for the case of circulating capital.”

The move from a stationary state to a growing economy proved very difficult for Wicksell. The difference of – what I call – the first and the second phase of Wicksell’s development is marked by two important distinctions. The first distinction, between the stationary state and a static analysis, has been made by Lionel Robbins. To appreciate Robbins’s elucidation in full, I believe that there is yet another distinction at stake, that between idealization and abstraction.

Below I shall depict the project Wicksell engaged in, after VCR, when he tried to extend the model to capture a growing economy (section 2). Next, I shall point to the importance for such an enterprise of understanding the differences between a static analysis and a theory of the stationary state and discuss the distinction as Robbins demarcated the two (section 3). In order to apply the concepts of idealization and abstraction to the distinction, I first have to explain the structure of idealizational reasoning strategies in comparison with abstraction in economic theory making (in section 4). It will be maintained that Robbins’s conceptual clarification can be applied for an understanding of Wicksell’s strategies in his first and second phase. With the help also of my distinction between abstraction and idealization we can see that Wicksell’s strategies turn out to be orthogonal to each other (section 5).

2. **Long Run Dynamics: Capital Accumulation**

A stationary economy can be conceived as one with long run inelastic supply of land and labour. It is feasible to model capital as a homogeneous fund of purchasing power, as Böhm-Bawerk had done. But a growing economy feeds on a growing stock of capital, and to produce capital goods, capital is needed. There is, then, a loop structure of investment. Capital has to be modelled as heterogeneous machines and unfinished products and, hence, cannot be valued in its own technical units. The valuation of capital requires prior information about the interest rate. The so-called Cambridge controversies arose out of the problem that the determination of interest by the marginal product of capital induced the economist to reason in a circle: marginal productivity theory cannot explain the rate of interest without mixing up explanandum and explanans.

Böhm-Bawerk’s assumption, meanwhile, had been that land could be treated as a special case of durable capital goods. He hoped that this would simplify the model; something he was in need of because he was incapable of doing what Wicksell managed to do: developing the model mathematically. However, even in a stationary

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222 Wicksell 1951, p.xvi.
economy, the loop structure of investment already enters at the level of land itself. Böhm-Bawerk had defined capital as piled up (or frozen) land and labour\textsuperscript{223}, but land, in turn, was taken to be a sort of perpetual capital good. He had calculated the value of land simply as the sum of an infinite geometric series of its future product. This, then, implies that land – as a form of capital – partly consists of labour, because that is the one original factor. This contradicts another of Böhm-Bawerk’s claims, i.e. that land is an original factor together with labour. Due to Wicksell land was returned its autonomous status in the general theory as he proposed to treat durable capital as a rent-earning good.

The assumption of a stationary economy had allowed Böhm-Bawerk to calculate the average investment period as a measure of the quantity of capital: in PTK more capital is synonymous with longer roundaboutness. But with two original factors, land and labour, some capital may consist of more frozen land, other of more frozen labour. The heterogeneity of capital then props up more pressingly. If the project is to also allow long run growth, tracking down the quantity of capital becomes a real problem. This is the challenge Wicksell faced.

Heinz Kurz discusses the attempts by economists to adjust Wicksell’s model, which have turned into the debate on Wicksell’s ‘missing equation’ (Kurz 2000). I am interested in Kurz’s analysis for his treatment of the difference between being in a stationary state (what I would call a property of the world) and the method of a static analysis (what I would call a property of our strategies to know about the world), based as it is on Robbins’s much earlier distinction. Some passages from Wicksell’s work seem to imply strictly stationary conditions, numerous others cause ‘this impression [to be] quickly dispelled’\textsuperscript{224}. Heinz Kurz concludes that Wicksell did not want to describe stationary economies.

“There is additional evidence that Wicksell did not intend to study the problem of distribution in terms of a strictly stationary state of the economy.”\textsuperscript{225}

But does Kurz really prove that Wicksell was not interested in the stationary state at all? I shall deny this below. Kurz indicates two ways for Wicksell to choose from: either capital is measured by some value unit, or capital – heterogeneous as it is – is represented by the average period of production in the economy (which is in fact Böhm-Bawerk’s solution), or $\frac{1}{2}t$ (‘t’ referring to the roundaboutness of production). Kurz says that Wicksell started off using the average period of production as an indicator for the capital intensity of the economy. Wicksell, Kurz goes on, wanted to explain capital accumulation. But it requires calculating compound, not simple interest to deal with growing capital stock. So Wicksell allegedly did not see the importance of the use of compound interest for the analysis he ultimately was after.

The whole idea was not to stick to the analysis of a stationary state, Kurz claims, but to a comparative static analysis. He says:

\textsuperscript{223} Note that this definition applies both to durable capital goods with their key role in the capital theory and for the subsistence fund that was assumed in the interest theory.

\textsuperscript{224} Kurz (2000), p.774.

\textsuperscript{225} Ibidem, p.776.
“While he saw that compound interest was necessitated by the assumption of free competition, he seemed to think that using simple interest involved an admissible simplification and no ‘essential alteration’. As we know, this presumption cannot be sustained.”

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For a comparative static analysis, the use of simple interest is not harmless at all, let alone essentially harmless. But Kurz’s analysis, I believe, is not correct! Another quote can show this. Wicksell contended that assuming a stationary state makes the use of simple interest harmless, even ‘essentially’ so:

“The Product $\varepsilon t$, that is to say, the investment period of the capital, can here, however, be conceived as a single variable, so that the expressions undergo no essential alteration, at least when calculating simple interest.”

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Clearly, given simple interest, and only under that condition, the quoted factor $\varepsilon t$ can as well be taken as exogenous. Wicksell did not claim at all that the use of simple interest is of no matter for the essence of the analysis. He knew that the use of simple interest was not harmless in general, i.e. in case one wants to study growing economies. This is also suggested by the fact that, in his VCR, Wicksell takes into account compound interest and provides the appropriate equation.

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Both in VCR and in the major part of his 1951 publication he explicitly assumes the stationary state. (It must be admitted that, if the investment period and the rate of interest both are large, the deviation between calculating simple compared to compound interest becomes very large too. But the simplification is not so serious in principle and this is what Wicksell contended when he judged it as admissible).

Kurz’s claim, that Wicksell was not really interested in the stationary state, is untenable if it is taken to describe all phases of Wicksell’s career. (Strangely, Kurz admits elsewhere in the text that ‘[I]n a first stage he took the capital endowment of the economy as given’.) Wicksell initially was interested in the properties of Böhm-Bawerk’s idealized model with time as the essential factor, even if – or rather, especially if – it described a stationary state. In the last chapter of his 1951 publication on capital accumulation, the stationary state is finally abandoned. But this precisely marks the next step in Wicksell’s research.

227 Wicksell (1954), pp.125-6. He refers here to the fact that the average production period need not be Böhm-Bawerk’s $\frac{1}{2}t$, as the distribution of labour and land over the process of production can be subject to choice by the entrepreneur, or to technical conditions, giving rise to a period $\varepsilon t$ instead. The coefficient $\varepsilon$ supposedly had to be empirically determined.
3. Robbins on the Difference Between Static Analysis and the Stationary State

There is a potential confusion over the terms ‘stationary’ and ‘static’. It is possible in principle to treat a stationary state dynamically, viz. to bring to light the short run market events in an economy at long run standstill. Also, it is viable to discuss growth by a static view if one is interested in the properties of a growing economy synchronically and not diachronically. Kurz’s project is meant to show that much of the research on Wicksell’s failure to develop a complete distribution theory for the case of modern, growing, economies is misguided due to its confusing the two concepts. This has induced the researchers to look for some missing equation in Wicksell’s work, which is, he says, not missing at all. He draws on the clarification of (and the warning to observe) the distinction of the concepts already expressed by Lionel Robbins (1930):

> “we must recognise not one general class of “static states” and “static laws”, but two: the classical conception in which the condition of stationariness is the resultant of the balancing forces tending to change, and the Clarkian in which the factors of production are stationary by hypothesis, and equilibrium is attained within these conditions. Both rule out inventions and fundamental changes in nature and human beings. But the one admits the possibility of variations of labour and capital, the other excludes these by definition. […] The modern economist […] will recognise in the two constructions we have been examining, not competing abstractions, but successive stages of exposition.”

The Classical economists held a view of the economy as a system that produces long run equilibrium and, hence, of a stationary state until interventions or spontaneous changes in initial conditions reset it; and they investigated the conditions for such a stationary end state. John Bates Clark looked upon it as a system that produces long run equilibrium only under conditions of fixed endowments: he studied the economy under the given axiomatic condition of a stationary state. Thus, Clark’s approach is a limiting case of the picture seen by the Classical economists. Kurz, then, notes that Wicksell did not assume a strictly stationary economy (in the sense of Clark), but maintained a static point of view ‘designed to throw some light on the actual, growing economy in terms of a comparative static analysis of consecutive states of the economy characterized, inter alia, by different “quantities of capital” in existence’. Wicksell ultimately did not aim for describing the limiting case. Above I have made clear that I agree with Kurz only insofar as this is concerned with Wicksell’s second phase.

It is important to note that it is not useful, perhaps not even intelligible, to speak of ‘a static economy’. This is not just because economies are essentially dynamic systems, even if temporarily in a stationary state, but because it is only the method of analysis by which we try to explain such an economy, which can be static in kind. In other

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231 Kurz (2000), p.777. I endorse this interpretation, but I repeat that I differ in opinion as concerns Wicksell’s first phase, the early period in which he wrote VCR.
words, ‘static’ is a property of a method, not of an economic system. The latter can be ‘stationary’, at least if we refer to a hypothetical economic system. To speak of a ‘static economy’ amounts to making a category mistake.

4. Abstraction and Idealization

The distinction between the assumption of stationary conditions and a static mode of analysis is not futile. No economy is in a stationary state for any significant period of time and, hence, it may be asked what any model economy resting on such a false assumption has to do with real economies. A static analysis of a growing economy, conversely, does not impose any deformation on the model that alienates it from our ultimate interest, the functioning of real economies. A static approach only affects the mode of analysis, not the model economy itself. The distinction has consequences for the issue of realism and truth.

The ubiquitous use of the infamous ceteris paribus clause in economics has triggered a fountain of literature discussing how economists could pretend to truthfully explain an aspect of reality that is to form the domain of economic scrutiny, while making use of unrealistic assumptions such as those packed in the ceteris paribus clause. The most widely known literature on this subject forms the debate around Milton Friedman’s essay ‘The methodology of Positive Economics’ (Friedman, 1953). In my paper ([anonymized], 2008) I have set out to explain the different consequences for the function of scientific claims of on the one hand idealization and on the other abstraction. The use of a clause is a case of *idealization*. I qualify idealization as an epistemic mode by which true propositions can be formulated even though it is by the insertion of a clause which, taken as an (approximate) description of reality, is false. Meanwhile, *abstraction* is a mode of analysis due to which true theoretical hypotheses may be generated without the deliberate insertion of a false claim, like a ceteris paribus clause. For the assessment of Robbins’s distinction, it is worth while to briefly highlight the core idea of the distinction between idealization and abstraction.

When idealizing, the scientist mentally or materially creates a model world where, for instance, friction has been reduced to zero or where countries’ current accounts are balanced. In the first example it is materially possible to create an almost frictionless plane or a vacuum; in the second case it is not possible to minimize net trade surpluses in goods and factors. The notorious lack of laboratory situations in economics – and in fundamental social science in general – forces scientists to rely on some form of thought experiment: ‘suppose it were the case that this variable decreased to a limit of zero, then it would be the case that the other variable …’. However, conditional sentences of this sort can be very instructive. In fact, many if not all claims underlying policy relevant talk have this fundamental form. The same ‘if-then’ form can be acknowledged in the everyday sentence ‘if I let go of my cup of tea it will drop’. It is the very truth of this sentence, which induces us to be careful and tighten our grip. Note that the sentence is not only of a conditional form, but that it has an antecedent which is false as long as I do not let go of my tea: it is a *counterfactual*. The debate over the use of deliberately false descriptions of reality in purportedly true theories can be clarified if it is noticed that idealizational propositions are potentially true counterfactuals – or so I wish to uphold.
Counterfactual sentences have an antecedent which is conditional for the consequent in a subjunctive way. This means that there is a reference to an as-if world. The reader may think this is a long shot, but I think it isn’t. In daily life we regularly imagine such hypothetical worlds, as we imagine what might happen if we choose one out of two or more options. Let me make this clear. Counterfactual propositions have the following subjunctive form:

\[ \text{If } X \text{ were the case, then } Y \text{ would be the case} \]

This is a conditional proposition, which leaves open that the antecedent (the part before the comma) is true, or will become true. Actually making a choice between two options is an example of one antecedent coming true and the other not. However, idealizational claims have nothing directly to do with choice making. The point with idealization is that the antecedent of the subjunctive conditional is false as it is formed by a ceteris paribus clause – this is when we have an idealizational counterfactual:

\[ \text{If variables } x_1, x_2, \ldots, x_n \text{ had (limiting) values } v_1, v_2, \ldots, v_n, \text{ respectively, then } Y \text{ would be the case} \]

In natural science an example of such a variable might be friction, its value being zero. In economics the example could be an income change of demanders, and its value zero. Idealizations, then, are counterfactuals and the falsity involved in idealization lies with the idealizational ceteris paribus clause in the antecedent of the counterfactuals (not with not choosing a particular option). The idealized claim is expressed by the consequent of the counterfactual.

As indicated, counterfactuals – although superficially seen it appears not to fit their name – can be true. Why? Because some claims of the form “if it were the case that …” are allowed according to the lawlike behaviour of the world, and some other of these claims are not. To say that in some hypothetical world a particular phenomenon is present is to say that we know what goes on in such a world although it differs from the actual world. We are often well informed – even if perhaps only locally – about the relatively regular make-up of the world we live in. We understand which things will change into what direction and how much if some input variable changes. We imagine hypothetical worlds when we give policy recommendations. If we didn’t, we couldn’t predict the near future even approximately or give any advice at all.

For example, our awareness of gravity makes us believe that my tea will drop under the condition specified. Likewise, our awareness of the influence of substitutes and complements on our demand for particular goods makes us believe that price stability of substitutes and complements helps fix our individual demand for the good in question. Hence the textbook condition ‘no change in the price of substitutes and complements’ for demand functions.

Now, what I phrase (to some perhaps hyperbolically) as ‘the insertion of falsity’ is often coined ‘abstraction’, e.g. by Milton Friedman (1953). Wrongly so, I believe. The mathematical idea of a circle is an abstraction of the circle I can draw in the sand in that it is somehow free of many concrete details, such as the thickness of the line or the imperfection of the curve. *Abstraction* removes *concrete* aspects mentioned in a proposition by definition – why else the name? In abstraction there is no reference to
these concrete aspects. In the other hand, the insertion of a finite clause that sums up which variables are to counterfactually remain unaltered, or zero, or infinite, does involve a clear reference to those variables. And as the variables do represent concrete aspects of the world, it follows that such a clause cannot amount to abstraction.

So what is a good example of abstraction? An everyday example of abstraction would be to infer that an apple drops from my bag from the assertion ‘a red apple drops from my bag’. Here we can see what leaving out concrete detail (redness) from propositions leads to higher levels of abstraction in a discourse. The second law of Newtonian mechanics is extremely abstract in this sense. Of course, lots of prior knowledge is employed in such abstract claims, as in ‘larger groups have a lower change of realization of a public good’ or, to refer to natural science again, ‘both burning and rusting are processes of oxidation’. In scientific abstraction, conjectures regarding structural aspects of a research object are proposed. Nevertheless in such cases concrete details, like the particular public good (is it a dike or is it trade union membership?) or phenomena pertaining to the two types of oxidation (is it heat or is it corrosion?), are left out of the scientific description. As a result, abstract descriptions are logically weaker than their concrete counterparts. This is an important difference with idealization, which clearly bears on issues of truth.

I shall not dwell with further contemplations about realism and epistemic strategies here. Only the fundamental difference, explained above, between the use of false clauses and of varying levels of abstraction are of interest for Robbins’s distinction. To sum up, idealization amounts to the insertion of false clauses into a proposition (an hypothesis, a theory) but leaves the level of abstraction untouched. This is because clauses sum up the variables to be set at a limiting value. Conversely, abstraction is a particular way of generalizing.\(^{232}\) The full description of a concrete phenomenon includes the details taken to be fundamental to it (although the judgement about what is fundamental or not is meaningful only relative to some context, such as the research interests of the theorizer).

5. Abstraction and Idealization in Robbins and Wicksell

In the *Lectures on Political Economy*, Kurz says, Wicksell gave rise to later misunderstandings due to his ambiguous treatment of equilibrium. At one time Wicksell said that:

“we shall content ourselves with what has been called the static aspect of the problem of equilibrium, i.e. the *conditions necessary* for the maintenance, or the periodic renewal, of a stationary state of economic relations.”\(^{233}\)

But at another time he referred to:

\(^{232}\) To be sure, it is an instance of existential generalization. Claims about the properties of red apples (that they are round, or falling) imply an existential claim that there *are* apples with such properties.

\(^{233}\) Wicksell (1951 [1901]), p.105. Italics are mine.
“a fundamental – and simplest – hypothesis [of] the stationary economy in which capital and the other economic factors can be thought of as an approximately unalterable sum.”

The first quotation talks of the conditions for the possibility of a stationary state. The second quotation involves a stationary economy as the simplest hypothesis and as a mode of thought. In fact, however, in his second phase Wicksell looked for a way to treat growing economies by a comparative static method, although he alluded to the ‘stationary state’ to refer to that method.

The stationary state analysis assumes stable endowments, reflected by fixed factor supply. But no such long run standstill economy was like what Böhm-Bawerk and Wicksell observed. In his VCR, Wicksell introduced the assumption of a stationary state as an isolative strategy. He studied a hypothetical economy. This model economy in perpetual stationary state equilibrium, gave him an interesting research object. It allowed him to learn about the sources and level of interest and about the determinants of income distribution, without any form of capital growth. But it did not take him to his final goal. Kurz’s main point shows that it is wrong to take Wicksell as trying to present a stationary state analysis ‘stricto sensu’. Indeed, for a theory of capital and distribution in the long run Wicksell dropped the assumption of a stationary state, changed his strategy, so to say, and started of with a static economic model of what actually is a long run dynamic situation.

Robbins (1930) locates the view on an economy as stationary in Mill’s Principles of Economics. He paraphrases Mill by the interpretation that “[w]e have studied [in the preceding part of the Principles] what happens when the factors of production are constant. Now we must proceed to ask what causes their numbers to change” and, next, quotes Mill: ‘we have still to consider what these changes are and what are their laws’. In conclusion:

“[t]he statics should deal with what happens when the factors are given. The dynamics, with the laws of change in the quantity of the factors.”

Robbins then observes that Clark is indebted to Mill in his conception of (‘what he calls’) a static state by abstracting the forces of social progress. Clearly, here it is worth while to consider my distinction between abstraction and idealization to assess this paraphrasing. ‘To study this state’, Robbins says, ‘we must consider changes of this sort absent.’ To consider changes absent, I proposed above, is best rendered idealization, not abstraction. We first explicitly take them into consideration, hence we do not abstract from them; next we give them a limiting value. This limiting value does not accord to the facts, so we idealize by the insertion of a false clause (in the antecedent of the counterfactual conditional). If it were the case that capital does not

234 Wicksell (1954 [1883]), p.22
236 Ibidem, p.203.
237 Ibidem.
238 Ibidem.
accumulate, that populations do not grow, etcetera, then we would have the following stationary state results. That is what the theory can teach us.

Robbins does not mark the epistemic labels this way, but he does distinguish the two very different modes of analysis.

“this is the fundamental difference which is desired here to exhibit – in the one, this constancy is the condition of equilibrium; in the other, it is simply one of the resultants of the equilibrating process. In the Clarkian state, population and capital are to be constant – they are not allowed to vary. In the classical constructions, population and capital are constant, but this is because, together with wages and interest, etc., they have reached a position of rest.”

Let us express all this in terms of the conceptual apparatus briefly developed in the previous section. In his research after VCR, Wicksell dropped the clause of inelastic supply of endowments so as to further approach what he thought was the kernel of actual economies: growth. Dropping the clause about vertical supply lines is an act of de-idealization: the false clause of a stationary state was relaxed.

But the more sophisticated model was not strictly dynamic, it was an instance of comparative statics. Each step of the analysis was studied under the assumption of an already accumulated stock of capital. The result of every such step was an abstraction in the sense that Wicksell disregarded the concrete flow of phenomena over time. Comparative static analysis is a repeated static analysis. The use of the instrument of successive synchronic views, however, does not imply the assumption of a world in long run arrest. There is no clause involved that does a claim about a hypothetical world without growth. Instead, each synchronic slice of economic reality from its diachronic flux is an abstraction in the sense that the dynamics of the system are ignored, not denied. Nevertheless, the aim of it all is not to ignore but to study it. That is why the mode of analysis is repeated. Wicksell ignored the dynamics but was nevertheless interested in (long run) economic development: comparative statics came in the place of real dynamics.

6. Conclusion

When Robbins said of the classical and the Clarkian ways of talking about the absence of long run growth that ‘the one admits the possibility of variations of labour and capital, the other excludes these by definition’, he traced a difference in a mode of analysis that seems to have puzzled many commentators of Wicksell. When Heinz Kurz notes that historians, who tried to find a missing equation in Wicksell’s work, were mistaken because of their failure to distinguish two modes of analysis, he points to the distinction Robbins warned us to observe: stable equilibrium versus stationary capital endowments.

My interpretation is that the first requires abstraction, the second is an act of idealization. I disagree with Kurz that Wicksell did not analyse the stationary state (in my terms: that Wicksell only tried to abstract and did not idealize as well). Wicksell

239 Ibidem, p.204. Italics by Robbins.
did, be it only in his first phase; he later used a mode of analysis, which seems more similar to the Classical approach. What is of much more importance, however, is that *idealization* characterizes Robbins’s idea of given endowments and *abstraction* characterizes the idea of ignoring the possibility of fluctuations affecting the state of equilibrium.

As idealization and abstraction are recurrent and well respected modes of analysis in modern economic thought, Robbins’s distinction elucidates also what economists are doing today, not only what the Classical economists and Clark did. They both abstract and idealize (and of course concretize and de-idealize, when needed). If they abstract, they engage in a mode of reasoning and the abstractive qualities involved designate *a property of this reasoning*, not of the (model) world they study. Alternatively, if they idealize, they have *the model world* assume a set of particular properties. Failing to distinguish this properly amounts to making a category mistake. Robbins, without today’s epistemological toolbox designed to analyze isolative reasoning, already made the distinction at a very early stage.
References


Wicksell, K. (1954, [1883]) *Value, capital, and rent*, translated from German by S.H. Frowein, London: Allen & Unwin. (Referred to in this paper as VCR.)
The Continuing Muddles of Monetary Theory: A Steadfast Refusal to Face Facts

C.A.E. Goodhart∗

Abstract

Lionel Robbins was much concerned about the methodology of economic science. When he discussed the desirable relationship between theory and ‘reality’, two of the three examples that he presented where the theoretical analysis was not sufficiently based on a knowledge of historical fact were taken from monetary economics. Indeed, monetary theory has remained prone to such shortcomings ever since.

Amongst the worst are:-

1. IS/LM: the monetary authorities set the monetary base, and the interest rate is determined in the market;

2. The monetary base multiplier of bank deposits, and the role of reserve ratios;

3. The current three equation neo-classical consensus, which not only assumes perfect creditworthiness for all agents, but also an essentially non-monetary system, e.g. no need for banks;

4. The standard theory of the evolution of money.

Monetary economics can only get better, but it has a long way yet to go.

Keywords: Monetary theory; IS/LM; Monetary base multiplier; Default; Evolution of money
JEL Classification: B22, E40, E42, E51, E59

1. Economic Generalisations and Reality

Lionel Robbins was much concerned with the methodology of economic science, and wrote several books on this subject.240 For the purposes of this paper, I shall focus on the relationship between theory and factual knowledge, or as Robbins put it, between ‘Economic Generalisations and Reality’, which was the subject of Chapter V of his book entitled, An Essay on the Nature and Significance of Economic Science (3rd Edition, 1984).

On this relationship, I have selected the following statements as representing the core of Robbins’s position:-

∗ Financial Markets Group, London School of Economics.
240 I am grateful to Amos Witztum for pointing me in this direction.
“It is a characteristic of scientific generalisations that they refer to reality. Whether they are cast in hypothetical or categorical form, they are distinguished from the propositions of pure logic and mathematics by the fact that in some sense their reference is to that which exists, or that which may exist, rather than to purely formal relations.” …p. 104.

“It follows, too, that it is a complete mistake to regard the economist, whatever his degree of "purity", as concerned merely with pure deduction. It is quite true that much of his work is in the nature of elaborate processes of inference. But it is quite untrue to suppose that it is only, or indeed mainly, thus. The concern of the economist is the interpretation of reality.”…p. 105.

“The fruitful conduct of realistic investigations can only be undertaken by those who have a firm grasp of analytical principle and some notion of what can and what cannot legitimately be expected from activities of this sort.

But what, then, are legitimate expectations in this respect? We may group them under three headings.

The first and the most obvious is the provision of a check on the applicability to given situations of different types of theoretical constructions. As we have seen already, the validity of a particular theory is a matter of its logical derivation from the general assumptions which it makes. But its applicability to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation. Now the concrete manifestations of scarcity are various and changing; and, unless there is continuous check on the words which are used to describe them, there is always a danger that the area of application of a particular principle may be misconceived. The terminology of theory and the terminology of practice, although apparently identical, may, in fact, cover different areas.”…pp 116-177

"Secondly, and closely connected with this first function of realistic studies, we may expect the suggestion of those auxiliary postulates whose part in the structure of analysis was discussed in the last chapter. By inspection of different fields of economic activity we may expect to discover types of the configuration of the data suitable for further analytical study…..

And, thirdly, we may expect of realistic studies not merely a knowledge of the application of particular theories, and the assumptions which make them appropriate to particular situations, but also the exposure of areas where pure theory needs to be reformulated and extended. They bring to light new problems.”…p. 118
When Robbins comes to give illustrations, it is notable that his examples of failures to take facts, ‘reality’, into consideration in both his first and second heading were taken from monetary theory. Thus his first example relates to the need to identify what is used as money in order to test the quantity theory of money. His second example related to the relationship between the reserve base available to banks and the size of the money stock, on which I shall have more to say later.

2. The IS/LM Basic Model

Most economics undergraduates still get their initial exposure to macro-economics in the guise of the IS/LM model, and it sits at the centre of most introductory textbooks, even today; it certainly did so in 1957, when Robin Matthews taught me at Cambridge.

You will recall that,

\[ y = I + C \quad \text{(expenditure)} \]
\[ y = S + C \quad \text{(use of income)} \]

So in equilibrium I must equal S;

\[ I = f(i), \quad f' < 0 \]
\[ S = f(y), \quad f' > 0 \]

where \( y \) is output, I investment, C Consumption, S Saving, i the interest rate. When this model was first put together, in the late 1930s, ‘the rate of interest’ was more commonly taken to be the long-term rate of interest. Now it is usually taken to be the

\[ ^{241} \text{“A simple illustration will make this clear. According to pure monetary theory, if the quantity of money in circulation is increased and other things remain the same, the value of money must fall. This proposition is deducible from the most elementary facts of experience of the science, and its truth is independent of further inductive test. But its applicability to a given situation depends upon a correct understanding of what things are to be regarded as money; and this is a matter which can only be discovered by reference back to the facts. It may well be that over a period of time the concrete significance of the term “money” has altered. If then, while retaining the original term, we proceed to interpret a new situation in terms of the original content, we may be led into serious misapprehension. We may even conclude that the theory is fallacious. It is indeed well known that this has happened again and again in the course of the history of theory. The failure of the Currency School to secure permanent acceptance for their theory of Banking and the Exchanges, in other respects so greatly superior to that of their opponents, was notoriously due to their failure to perceive the importance of including Bank Credit in their conception of money. Only by continuously sifting and scrutiny of the changing body of facts can such misapprehensions be avoided.”…pp 117/118} \]

\[ ^{242} \text{“Again, we may take an example from the theory of money. It will be clear from an inspection of the actual procedure of banks of issue that the effect upon the supply of money in the widest sense of given additions to the reserve of precious metals will depend upon the exact nature of the law and practice concerning reserve requirements. It follows, therefore, that in the full elaboration of the theory of money we must introduce alternative assumptions, taking account of the various possibilities in this respect. It is clear that these are not possibilities which are necessarily easily exhausted by general reflections on the nature of banks of issue. Only close study of the facts is likely to reveal which assumptions are most likely to have a counterpart in reality, which assumptions, therefore, it is most convenient to make.”…p. 118} \]

\[ ^{243} \text{See Begg et al. (2005); Lipsey and Chrystal (2007).} \]
short-term rate\textsuperscript{244}, to which the long-term rate is related by an expectations-based, no-arbitrage, yield curve.

Also the demand for money must equal the supply of money, which is assumed to be set by the Central Bank, so $M_S$ is given, and in equilibrium

$$M_S = M_D \quad (5)$$

Since,

$$M_D = f(Y, i), \; f'_y > 0, \; f'_i < 0 \quad (6)$$

Which gives us:-

This is probably the second most famous diagram in economics.

The basic problem with this formulation was that no Central Bank has ever operated in this way.\textsuperscript{245} Instead they set the short-term official policy rate, or maintain a fixed exchange rate peg against the currency of another country, which in turn has a Central Bank which sets a policy rate. This means that at any point of time the LM curve is horizontal.

\textsuperscript{244} Tim Congdon has frequently noted how the meaning of economic concepts, such as the output gap, tends to migrate over time.

\textsuperscript{245} There can be a few historical qualifications to this dictum, but they are sufficiently rare and, under careful analysis, doubtful, enough to be ignored.
This means that there was a discrepancy between discussions and proposals about current policy, which were naturally couched in terms of how the Central Bank should vary its policy rate, and theoretical analysis of how it should allow the monetary base to vary. Admittedly in a given context\textsuperscript{246}, there is a dual relationship so that a given interest rate implies a certain stock of monetary base, and vice versa, but, under conditions of uncertainty, the Central Bank would not know what level of interest rates would be associated with what level of monetary base, and vice versa. That, of course, led on to the famous Poole article (1970), which suggested that the case for choosing to set \( M \) or \( i \) depended on the relative stability (predictability) of the demand for money and investment functions. It is the case that the instability (unpredictability) of the demand for money functions did help to bring about the demise of pragmatic monetary targetry in the mid 1980s. But none of the monetary target mechanisms, including Volcker’s famous non-borrowed reserve target, ever denied commercial banks access to cash, at a predictable interest rate, though in the above case via borrowing at the discount window which involved some small non-pecuniary cost.

The real reason why Central Banks set interest rates, rather than a monetary aggregate, relates to its financial stability objective, not to its macro-monetary price control aim (though the two are, of course, intertwined). Commercial banks cannot operate a fractional reserve system, with relatively low levels of cash and liquid assets, without assured recourse, at a predictable interest rate, to cash on demand, see Goodhart, Sunirand and Tsomocos (2008). Of course, one could run a free-banking system, but this would simultaneously raise the cost of intermediation (as more capital and liquid assets would have to be held by the banks), and, most likely\textsuperscript{247}, the probability and severity of financial crises.

In another famous article, Sargent and Wallace (1975) demonstrated that, if the policy interest rate was exogenously set, then the macro-economic system, especially the price level, would become totally unstable and would explode. Whereas if the Central Bank set the money stock, the macro-monetary system would be stable, (though, as I have asserted, the financial system would become unstable, with panics and collapses). This seemed to overlook the historical fact that Central Banks had been setting interest rates on a regular basis, and only on some rare occasions did macro-economic price instability ensue.

The resolution of this conflict between reality and theory was, as is now well known, resolved by the realisation that Central Banks did not set interest rates exogenously, but endogenously in response to current, and expected, macro-economic developments, especially to forecasts of inflation. This was encapsulated in the Taylor reaction function,

\[
i = a + b_1(\pi - \pi^*) + b_2(y)
\]  

\textsuperscript{246} Making a strong assumption about the existence of a single unique equilibrium.

\textsuperscript{247} In view of the Fed’s failure to mitigate the 1929-33 great depression in the USA, this latter claim is debatable.
where $\pi =$ inflation, $\pi^*$ = the inflation target, $y$ is the output gap. To this is added the Taylor principle that stability will be achieved so long as $b_1 > 1$.\textsuperscript{248}

So, at least, this division between reality (Central Banks set interest rates, not monetary quantities) and theory has at long last\textsuperscript{249} been resolved, as it must eventually be, in favour of reality.

While the question of what the Central Bank is trying to do has now been settled, the subsidiary issue of exactly how it goes about doing this remains open. The Taylor reaction function relates the present choice of interest rates to the current deviations of inflation from target and output from potential. Because of the long and variable lags in the transmission mechanism from monetary policy to controlling inflation, Central Banks in practice decide on present changes in interest rates on the basis of their forecasts of future deviations of inflation from target, (and of output from potential). Such forecasts are not always easily available, and those that are published by Central Banks are usually ex post, i.e. after the interest rate decision has been taken, not ex ante, i.e. the forecasts that triggered the decision. This can make quite a difference to the econometric results (Goodhart, 2005). While it can be argued that current deviations are an important input into forecasts of future deviations, nevertheless the discrepancy between the way that the Taylor reaction function assumes that Central Banks behave and the way that they actually do so has distorted much research and analysis in this area.

3. The Base Multiplier

Analysis of the determination of the money stock is frequently undertaken via the base money multiplier, e.g. Friedman and Schwartz (1963).

$$M = H \left(1 + \frac{C}{D}\right) \left(\frac{R}{D} + \frac{C}{D}\right)$$

(8)

Whereas this is frequently misinterpreted as a behavioural equation, it is in fact a definitional identity, derived from the two identities,

$$M = D + C$$

(9)

(the money stock is defined as deposits plus currency in the hands of the public), and

$$H = R + C$$

(10)

(the high powered money stock is defined as the reserves of the banking system and currency outstanding; to get from (9) and (10) to (8) divide throughout by D and then divide (9) by (10)).

\textsuperscript{248} Actually the stability condition is somewhat more complicated than this, but the simple form will do, and is widely used.

\textsuperscript{249} Taylor’s first article on this did not appear until 1993.
Since equation (8) above is a definitional identity it gives no clues at all to the direction of causation. If, however, one should assume that the Central Bank operates by fixing the monetary base (H), then that, (plus variations in the two ratios, which may be influenced by policy (R/D), and by confidence in the banking system (C/D), and other economic factors, e.g. relative interest rates), determines M, the money stock. But, if, as we have now seen, it is agreed that the Central Bank sets a policy interest rate, then given the demand for money and credit, and the factors affecting the two ratios, the so-called multiplier simply determines the quantity of high-powered money (H) and bank reserves (R) that the Central Bank has to create in order to maintain its desired rate of interest. The base multiplier in reality works in reverse, determining H, not M. Economists, and others, often fail to appreciate this. It is not uncommon to find textbooks incorporating both a Taylor reaction function and a standard base multiplier, wherein the CB is supposed to control H in order to determine M! See, for example, Blanchard (2006), Dornbusch, Fischer and Startz (2001).

This misunderstanding has caused numerous policy errors. It leads people to believe that raising the reserve ratio, e.g. by calls for Special Deposits, will have a significant direct effect in reducing the money stock. In practice, in order to maintain the chosen interest rate, the Central Bank has to provide the extra reserves required, after the minimum reserve ratio has been raised, in order to maintain the given interest rate. It usually does so in effect by buying short-dated liquid assets from the banks. Since such reserves are required to be held, and generally offer a zero or lower interest rate, the net effect is to make banks both less liquid and less profitable. The latter may induce the banks to widen the spread between deposit and loan rates, which will tend to reduce money (and credit) expansion slightly, but also to shift bank portfolios towards riskier, but higher yielding, loans. Reserve requirements are, therefore, best seen as a tax on banks, slightly reducing their growth rate and making them both less liquid and less risk averse. In so far as taxes can be avoided by shifting location, they will be.250

Next, it is often stated that Central Banks have a choice whether to sterilise, or not, intervention in the foreign exchange market. In fact, so long as they seek to maintain some given policy-determined domestic interest rate (greater than zero), they have no such choice. Such intervention will automatically be sterilised.

The failure to appreciate this mechanism has also complicated discussion of monetary policy during the 2007 financial crisis. When banks wanted more cash, they were automatically given it by all Central Banks. Because of counterparty risk, and projections of future calls for extra bank funding, (to replace asset-backed commercial paper not being rolled-over), banks would not lend to each other in the three-month interbank market, so three month Libor rates rose relative to overnight rates. To reduce this latter rate, Central Banks either had to lower the short-term policy rate, or try to undertake an ‘operation twist’, in which they buy (lend on) 3 month paper and offset this by net sales (borrowing) overnight in order to keep overnight rates close to the policy rate. In the past such an operation twist has rarely

250 There were many policy discussions about whether, and how, to impose reserve requirements on the euro-$ international markets in the 1970s and 1980s. These were made more difficult since many of the participants misunderstood the base multiplier analysis.
been successful, but it may well have been worth attempting in the recent crisis, (what can one lose from it?).

4. The Current Consensus Model

Besides the shift from assuming that the Central Bank sets the monetary base, to the realisation that it sets a policy interest rate, recent decades have seen two revolutions, the adoption of rational expectations and quest for optimising micro-foundations, both connected with the work of Lucas, (e.g. 1972, 1976). This has led the initial two equation model to morph into the current consensus three equation model, whose domination of analysis is stronger than ever. As is well-known, this takes the form:-

\[ y_t = E(y) + b_1(i_t - E(\pi)), \quad b_1 < 0 \]  \hspace{1cm} (11)

\[ \pi_t = E(\pi) + b_2(y), \quad b_2 > 0 \] \hspace{1cm} (12)

\[ i_t = b_3(\pi - \pi^*) + b_4(y), \quad b_3 > 1, \quad b_4 > 0 \]  \hspace{1cm} (13)

where E, the expectations operator, is some combination of backwards and forwards looking elements, y is the estimated output gap, and equation (13) is the Taylor reaction function.

Equations 11 (the old I/S curve) and 12 (the old Phillips curve) are, in turn, derived from an underlying optimising DSGE model, plus a (rather dodgy) assumption/estimate of temporary wage/price frictions/rigidities (e.g. Calvo pricing) (Calvo, 1983). Amongst the several problems/disadvantages of this current consensus is that, in order to make a rational expectations, micro-founded model mathematically and analytically tractable it has been necessary in general to impose some (absurdly) simplifying assumptions, notably the existence of representative agents, who never default. This latter (nonsensical) assumption goes under the jargon term as the transversality condition.

This makes all agents perfectly creditworthy. Over any horizon there is only one interest rate facing all agents, i.e. no risk premia. All transactions can be undertaken in capital markets; there is no role for banks. Since all IOUs are perfectly creditworthy, there is no need for money. There are no credit constraints, (everyone is angelic; there is no fraud; and this is supposed to be properly micro-founded!). Money is generally introduced into the model by auxiliary ad hoc frictions, e.g. cash in advance requirements or limited participation, both of which are totally internally inconsistent with a world without any default. Essentially, therefore, the consensus three equation model assumes a non-monetary, non-banking, system, so it is no surprise that most theoretical adherents of it tend to down-play attention to, or concern with, purely monetary variables, e.g. the monetary aggregates, (see for example Woodford, Svensson, (Woodford, 2003, 2007; Svensson, 2003, 2008)

Under normal circumstances risk premia remain, more or less, steady and defaults are low. In these (fair weather) circumstances, the main driving force affecting financial conditions is the change in the official policy rate, and expectations of future developments to inflation, the output gap and policy rates. In such usual
circumstances the consensus model and its background DSGE representations will work well.

But every now and again, and 2007 has become an example, risk premia shift sharply, as do credit constraints. Defaults, and fear of future defaults, can rise sharply. DSGE, and the consensus, models have no capacity (at present) to incorporate such effects. A variety of, ad hoc, auxiliary data (on credit conditions) and subjective add-ons have to be bolted on to forecasting models. The modellers’ hope is that the monetary authorities can restore calm (normal conditions) quickly enough to make the standard model usable again. But the truth is that such models can neither forecast financial disturbances, nor the scale of their effect while a crisis persists. This is hardly surprising since the models abstract from the possibility of any such crisis by definition.

A further implication of this is that the basic analytical paradigms of the macro-monetary side of a Central Bank and of its financial stability wing are mutually inconsistent, and rarely interconnect. The former (macro-monetary side) uses a model that abstracts from default. The financial stability department cannot do so, but struggles to find a theoretical underpinning.²⁵¹

Hy Minsky (e.g. 1982) gave a verbal description of financial processes, but this has been generally dismissed as insufficiently rigorous, non-mathematical and not based on rational expectations or micro-foundations. Martin Shubik (e.g. 1973, 1977, 1999) provided a much more rigorous and well-founded account of a monetary/banking system in which default plays a central role, but his work has also been largely bypassed, for reasons that elude me, by the mainstream. D. Tsomocos and I have been trying to build on Shubik’s work to develop practical, yet rigorous, models of the interaction between risk aversion, default probabilities and the real economy, (e.g. 2004, 2005, 2006a and b, 2007; also see Aspachs, et al, (2007) and Goodhart and Zicchino (2005)). There is a long way to go, but a good starting point would be to recognise the inherent lack of realism, and deficiency, of any model, such as the current consensus model which fails to have a central role for default.

5. The Evolution of Money

Kiyotaki and Moore (2002) wittily and correctly coined the phrase ‘Evil is the root of all money’. I described in the last section how human failings in the shape of refusals, and/or inability, to honour promises to repay debts (i.e. defaults) was central to the need for, and shape of, our monetary system. Another key failing of our human society is the predilection of the strong to prey (often violently) on the weak. In order to prevent society falling into Hobbesian chaos, there is a need for government, (often in the guise of the strongest power, see Mancur Olson (2000); ‘power grows out of the barrel of a gun’).

Besides the pure rents that government can levy, they do have expenditures, on the army, police, justice system, etc. In some early governments, e.g. in early Egypt,

²⁵¹ I have a soft-spot for the old ‘real bills’ doctrine. It was analytically flawed, but it did unify the macro-monetary and the financial stability objectives. The idea was that, if a Central Bank limited its discounts to commercial bills based on real trading activity, it would simultaneously stabilise both inflation and the banking/financial system.
these were financed in kind by transfers of labour services or goods (a set proportion of the harvest) to government. But this was highly inefficient. Payment in kind did not provide the government with the proportions of goods and (labour) services that it needed. A solution to this was for the government to issue claims on itself, (supported by, but not entirely dependent on, the intrinsic value of metallic coins in many cases), which it promised to accept in payment of taxes (in lieu of goods and services). Such promises were generally credible, (they were backed by the power of the state), so long as,

(i) the purchasing power of money was not debauched by over-issue and devaluation; and
(ii) the power of the state was not threatened.252

Violence is endemic in human societies, and can lead to debilitating and persistent feuds that disrupt the social framework. ‘An eye for an eye, and a tooth for a tooth’ is a natural, but not a welfare enhancing, response. Another key factor leading to a monetary system is the need for a common tariff whereby the wrong done by X on Y can be settled and expurgated by the transfer of a predetermined number of units of some object from the transgressor (or his clan) to the victim. That object will evolve into a monetary unit. Indeed many societal relationships, such as the bride price, involve transfers of monetary type objects.

Money was invented as a social, and governmental, phenomenon253, not as a means of reducing transactions costs in markets. The invention of money probably predated the development of formal markets; thus money facilitated the rise of markets, rather than vice versa. One piece of evidence of this is that many early money forms, notably cattle (the word pecuniary derives from the later Latin word ‘pecus’), are highly unsuitable for ordinary transactions (being neither standardised, easily portable nor divisible). Even gold coins, the prototype of early metallic money, were so expensive relative to regular wages/goods prices that they would very rarely be usable in day-to-day transactions.

Our knowledge of the monetary systems in primitive and early societies is necessarily somewhat sketchy. Nevertheless I believe that the consensus among historians and anthropologists is that money developed as a social (and governmental) artefact, rather than as a mechanism for reducing transactions costs in private-sector markets. But such a viewpoint is somewhat woolly and socio-logical, and has not, in the past254, lent itself to mathematical modelling. So, economists have tended to ignore historical reality, to establish formal mathematical models of how private agents (with no government), transacting amongst themselves, might jointly adopt an equilibrium in which they all settle on a common monetary instrument.

252 If the state collapsed, the value of its outstanding money would fall back to its intrinsic value as a pure commodity, whether of gold or as art-work, as in defaulted government bonds.
253 Though money did reduce the transactions costs of government.
254 There is an excellent paper by Dror Goldberg of Texas A&M on ‘The Tax-Foundation Theory of Fiat Money’, which uses a dynamic mathematical model. Perhaps once economists see that the realistic approach can be rigorously expressed in abstract theory, they will become more willing to accept its historical validity.
Does such a misconception matter? I have argued that it does, particularly in the case of the euro-zone, in my paper on ‘The Two Concepts of Money’ (2003). The concept, originally developed by Menger (1892), that money emerged as a private-sector initiative (to cut transactions costs), implied that you could change the monetary regime within the EU without worrying much about the need for associated adjustments to the fiscal regime. On the other hand, if money is a social artefact, then a key feature of any monetary regime change must be to design the appropriate accompanying fiscal measures.

Let me take a current concern. The adoption of a single currency is being accompanied, as intended, by the emergence of pan-European banks. That has led to proposals for a common pan-European system of banking supervision and of crisis management and resolution for such banks, in order to handle cross-border coordination problems. Crisis resolution is, however, potentially very expensive. There is no current fiscal mechanism to provide funds for crisis management at the federal level; that can only be done at national level. So long as the fiscal funding remains the responsibility of the constituent nation states, it is difficult to see how banking (financial) supervision and crisis management could be moved to a federal pan-European level.

6. Conclusions

John Hicks (1969), at least in his later years, argued that monetary economics needed to be firmly grounded on a knowledge of historical and institutional fact. Yet in recent decades the suggestion that Prof. X took an institutional approach to monetary analysis was sufficient to cast his/her reputation into outer darkness. Only small groups of mainly heterodox (and of various hues of post-Keynesian views) economists have bothered much to relate theory to reality. Why this has been so, I do not know. That it has been so, as I have sought to document, is not a good advertisement for this sub-sector of our profession.

In particular, Lionel Robbins argued that the applicability of a theory “to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation”. I have argued, above, that the current dominant consensus money/macroe model, the standard DSGE model, abstracts from (or ‘excludes’ as Brian Loasby (2008) would put it) any possibility of failure, or default, and thereby largely eliminates any rationale for banks, financial intermediaries, or even money. That this is strictly insufficient and inappropriate has been all too clearly illustrated by the events of 2007/8. Robbins admitted that he had been mistaken in his analysis of the Great Depression. I hope that the mainstream money/macro theorists will similarly admit the shortcomings of their own current models in the current context.
References


Abstract

In my paper I would like to contrast Lionel Robbins’s approach to monetary reform and to the ideas – public works, etc. – that henceforth would be associated with Keynes’s name. Robbins’s gradual acceptance of the latter, which is widely recognised, is seen against a doctrinaire opposition to the former, which is rarely discussed. The context of these manoeuvres included of course the Great Depression, the collapse of the gold standard and the increasing prominence of Keynes. His attacks on gold and his goal of money managed by public authority had been prominent since the publication of the *Tract on Monetary Reform*. But Keynes was moving quickly in a policy and theoretical direction that members of the economics profession were unwilling to endorse. Perhaps more than any other, Robbins’s work exemplified the nature of the opposition.

Up until the mid-1930s, classical doctrine was re-asserted. And it was to this period that the *Essay* belonged. In practice Robbins opposed tariffs and public works policies (most famously in the LSE response to the 1932 initiative by Pigou). Equally he opposed any monetary palliatives and continued to support the gold standard.

But in the 1940s, Robbins emerged as a leading instigator of the ‘Keynesian’ approach in Britain. He led the Economic Section of the Cabinet Office, from where the celebrated *Employment White Paper* would emerge. But again, the *White Paper* remained lukewarm to monetary reform.

In the post-war era, with Robbins in a powerful position, an invigorated profession would define its subject as concerned with the ‘allocation of scarce resources’ and debate only the Classical versus ‘Keynesian’ perspectives over which it had shown such flexibility in practice. Monetary considerations remained relegated as of no significance and unworthy of study. The instruments of monetary reform that had been constructed under Keynes’s influence and guidance were quietly dismantled.

1. Introduction

Lionel Robbins was a central figure in economic and wider policy debate of the 1930s and 1940s, a time of the most profound change to global economic policy. The gold standard, established first in Britain in 1717, was under challenge. It had proven inadequate during WWI; and the post-war return to gold in Britain was strongly contested. For some, gold was the root cause of the high unemployment of Britain in the 1920s and then finally the global collapse of prosperity in the Great Depression. Britain came off gold in 1931 and the world-wide system gradually unravelled over the next four years. At the centre of these charges and subsequent policy
developments was John Maynard Keynes. Following Keynes, I have attempted to characterise these events as the fourth grand monetary discussion. In most general terms, the result was the transfer of the management of money from private authority – under the Gold Standard – to public authority.

LSE economists opposed each and every one of Keynes’s monetary initiatives; section two traces Keynes’s early skirmishes with Edwin Cannan. But Keynes’s policies began to prevail. Robbins wholeheartedly entered the fray on Cannan’s retirement; he and his colleagues began a relentless opposition to Keynes’s programme for monetary reform and a defence of the monetary status quo, of the gold standard and dear money, that is detailed in section three. The Essay on the Nature and Significance of Economic Science was a public manifesto of his school.

With monetary reform taking hold across the world, Robbins, and a large part of the economics profession, began to change tack. Section four argues that, during the 1930s, the international community sought to develop an alternative theory that was first a rival to Keynes’s and second would become identified with Keynes. In parallel there were calls for greatly increased planning of economic activity. Robbins engaged with the planning agenda on his own terms; he called for a new and planned, yet liberal, internationalism (including the creation of a European Federation) that he set against a monetary reform portrayed as a central feature of economic ‘nationalism’ (section five). But, with the coming of war, Robbins played a central role in redefining the role of the state in national economic activity in a manner that has become known as Keynesianism (section six).

But this redefinition did not go as far as endorsing the policies of monetary reform that were increasingly established. The Employment White Paper was lukewarm to monetary reform, and, after the war, Robbins joined in the great condemnation of the cheap money policy of the majority Labour Government (section seven). He remained a proponent of the gold standard and of dear money until the end (section eight).

Sadly, the economics profession has been unwilling to put monetary policy developments in this era at the front place of discussion. The Robbins story is therefore polarised as state intervention versus free market capitalism (exemplified by Richard Wright’s (1989) response to O’Brien’s (1988) obituary essay on Robbins). But the reality is much more subtle and sophisticated than that.

2. Keynes, Monetary Policy and the LSE before Robbins

Keynes argued from very early on that the gold standard was an inappropriate mechanism for economies based on bank credit; as he later put it: “The confusion lay in the futile attempt to ignore the existence of bank money and consequently the inter-relationships of money and bank credit, and to make representative money behave exactly as though it were commodity money” (CW V, p. 15).

1 Keynes set out this characterisation in his lectures as follows (dating is mine): first, the bullionist controversy, of 1790s-1820s, second, the Californian and Australian gold discoveries of the 1840s/50s and third, the bimetallic controversy of the 1880s/90s (see CW XII, pp. 772-3).
His own mentor, Alfred Marshall, had studied the nature of bank credit and had taken tentative steps towards bimetallic arguments. Keynes’s own early arguments focused on monetary developments in India, and led up to his *Indian Currency and Finance*. India had moved from the classical-gold-standard mechanism, reliant on interest rate changes, to a system where exchange rates were preserved or *managed*, through the buying and selling of currency by its central bank. Even this early policy debate encapsulated his central pre-occupation: that exchange considerations should not inhibit the setting of appropriate interest rates for domestic policy.

Britain moved to similar arrangements during WWI, with Keynes a prime mover as a senior civil servant in HM Treasury. Britain (as well as other countries) modified its internal gold standard, and the foreign exchange policy became a form of exchange management. From 1915, J. P. Morgan was instructed to buy and sell sterling to preserve an exchange rate of $4.76. The arrangements meant that the short-term rate of interest was freed from its role in preserving the exchange parity and could, in theory at least, be operated more in accord with the requirements of domestic/wartime policy.

The development of these policies brought Keynes into conflict with Professor Edwin Cannan of the LSE. Skidelsky (1992, p. 163) provides a helpful biographical sketch:

… Cannan had done his economics at Oxford, not Cambridge, and was equally suspicious of Marshall, mathematics and monetary reform. He was … a ‘Johnsonian debunker’ of all new-fangled theories, who ‘oversimplified and probably ridiculed too much’. ²⁵⁶ Cannan was both a socialist and an orthodox economist, a quite usual combination at the Fabian-inspired LSE of the 1920s … Both his economics and his socialism made him suspicious of Keynes’s monetary therapy. …

The central point of his monetary theory was his denial that banks can create credit.

Skidelsky goes on to discuss their clashes. He characterises their WWI dispute as follows (with perhaps too much emphasis on the nature of inflation and too little on the underlying fundamental policy issue of the suspension of gold):

Cannan had argued that it [inflation] was due to the oversupply of notes; Keynes that it was ‘more scientific … to attribute the inflation to the excessive expenditure by the Government and to hold that it can only be cured by the diminution of expenditure public and private’. … [For] Cannan, … the only way to control expenditure was by a ‘stoppage of notes’. The virtue of a currency convertible into gold was that it

²⁵⁶ Skidelsky cites Robbins (1971), who recorded “the younger generation … refer to him … somewhat disparagingly. This is almost entirely due to an error in his theory of money and credit and to his advocacy of a mistaken policy, deflation to restore the old parity of sterling after the First World War … At the time of which I am writing, however, at any rate at L.S.E. and within its sphere of contacts, his ascendancy was paramount. We revered him. We hung on his words. We conned over his every piece of writing. He represented for us archetypal mature wisdom in his subject” (p. 83).
automatically limited the issue of notes, which politics alone could not achieve. \textit{(ibid.)}^{257}

The more public clash came with the publication of Keynes’s \textit{Tract on Monetary Reform}. On the one hand, the work was a polemic against the gold standard. On the other, Keynes propounded a positive agenda for domestic and international monetary policy in the light of monetary developments before, during and after the war. The Preface set out his manifesto for monetary reform:

Nowhere do conservative notions consider themselves more in place than in currency; yet nowhere is the need of innovation more urgent. One is often warned that a scientific treatment of currency questions is impossible because the banking world is intellectually incapable of understanding its own problems. If this is true, the order of society, which they stand for, will decay. But I do not believe it. What we have lacked is a clear analysis of the real facts, rather than ability to understand an analysis already given. If the new ideas, now developing in many quarters, are sound and right, I do not doubt that sooner or later they will prevail. \textit{… (CW IV, pp. xiv–xv)}

Keynes dismissed the gold standard as a ‘barbarous relic’, inappropriate for preserving domestic stability. Rather than control cash, as the gold standard purported to do, he promoted the control of credit. For international policy, he promoted currency management, whereby all central banks would mimic India and British wartime actions, according to fixed buying and selling prices for gold and hence exchange rates. Cannan (1924) reviewed Keynes’s book in the \textit{Economic Journal}, under the title ‘Limitation of Currency or Limitation of Credit?’. He rejected Keynes’s central proposition: “I hold that while the control of prices by controlling currency and letting credit follow is perfectly real and effectual, the control of prices by controlling credit and letting currency follow is altogether chimerical” (p. 54).

Keynes responded in the same issue, summing up colourfully:

Professor Cannan is unsympathetic with nearly everything worth reading – as it seems to me – which has been written on monetary theory in the last ten years. Yet the almost revolutionary improvement in our understanding of the mechanism of money and credit and of the analysis of the trade cycle, recently effected by the united efforts of many thinkers, may prove to be one of the most important advances in economic thought ever made. The ideas are new. They are only just beginning to be capable of complete or clear expression. It is natural that middle-aged bankers should feel shy. But it is not natural that Professor Cannan should write as though none of all this existed, as though his own subject were incapable of development and progress, and as though the last word had been said years ago in elementary text-books. \textit{(ibid., p. 68)}

But Cannan’s views prevailed. Around the world the gold standard was restored. In Britain, special emphasis has been accorded to the overvaluation of sterling and the consequent high unemployment of the 1920s, but this has served to detract from

\footnote{257 This dialogue (from October 1917 and January 1918) is not published in the \textit{Collected Writings}, but see Skidelsky (1995).}
Keynes’s view of the more fundamental inadequacy of the system as a method for regulating economic activity. Keynes retreated to write his Treatise on Money.

The new book on Monetary Theory which I have in preparation will, I am hopeful, throw much new light on my fundamental arguments in favour of the dogmas to which I have rashly given utterance without sufficiently substantiating them.258

It was published on 31 October 1930, with the Great Depression beginning and the collapse of the gold standard a short way off. In parallel he was engaged with the Macmillan ‘Committee on Finance and Industry’ and the newly instituted Council of Economic Advisers and Committee of Economic Information as well as speaking around the world.

In the course of these deliberations he began to accord a fundamental role to the rate of interest. A memorandum to his fellow Macmillan Committee member Robert Brand led to an important response by Keynes (dated 7 April 1931) – perhaps offering his sharpest and most concise analysis of the Economic Problem to date:

This memorandum brings home to me what I was beginning to forget, namely that I have nowhere introduced into my draft chapters in any clear or emphatic form what I believe to be the fundamental explanation of the present position. I felt, I think, when I was drafting that this was rather a personal theory of my own and that some members of the Committee might be more inclined to agree with my practical conclusions than with my fundamental reasons. Hence, what has resulted is a semi-suppression of the latter. But Brand points out in effect how lame the result is. For unless my general explanation is more or less on the right lines, it is not clear that it is feasible for central banks to do what I am asking them to do …

My fundamental explanation is, of course, that the rate of interest is too high, – meaning by the ‘rate of interest’ the complex of interest rates for all kinds of borrowing, long and short, safe and risky. A good many of Brand’s factors I should accept as part of the explanation why interest rates are high, e.g. effects of the War, post-war instability, reparations, return to gold, mal-distribution of gold, want of confidence in debtor countries etc., etc. …

Next comes the question of how far central banks can remedy this. In ordinary times the equilibrium rate of interest does not change quickly, so long as slump and boom conditions can be prevented from developing; and I see no insuperable difficulty in central banks controlling the position … The drastic reduction of the whole complex of market-rates of interest presents central banks with a problem which I do not expect them to solve unless they are prepared to employ drastic and even direct

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258 Skidelsky (1992, p. 164); he gives the reference: “KP: NS/1/1. JMK to Prof. Kurt Singer, 23 April 1925”. The item is not included in the Collected Writings.
methods of influencing long-term investments which, I agree with Brand, they had better leave alone in more normal times. … But I should not be surprised if five years were to pass by before hard experience teaches us to get hold of the right end of the stick. (CW XX, pp. 272–3)

Published in July 1931, the Macmillan Report too offered a clear general direction, in spite of much inevitable compromise:

[In] the case of our financial, as in the case of our political and social, institutions we may well have reached the stage when an era of conscious and deliberate management must succeed the era of undirected natural evolution. (Cmd. 3897, p. 5, para. 9)

Matters turned rapidly in favour of Keynes. On 21 September 1931 Britain suspended membership of the gold standard.

Keynes and Cannan would clash one last time. In December 1931, an Economic Journal article by H. Somerville hailed Keynes’s Treatise as “a vindication of the Canonist attitude to interest and usury!” and argued that “interest is the villain of the economic piece” (Somerville, 1931, p. 647, my emphasis). The piece prompted a symposium on ‘Savings and Usury’ in the next issue. Three of the four contributions, including Cannan’s, rejected any condemnation of usury. Keynes’s contribution, while agreeing some theoretical points, “on one main issue [came] to the support of Mr Somerville” (CW XXIX, p. 16). His concluding words foreshadowed his future work:

Personally I have come to believe that interest – or, rather, too high a rate of interest – is the ‘villain of the piece’ in a more far-reaching sense than appears from the above. But to justify this belief would lead me into a longer story than would be appropriate in this place. (CW XXIX, p. 16)

And Keynes’s policies had already begun to prevail. His advice was sought as soon as Britain left gold. As a result of this advice and the associated deliberations, there were four major lines of policy change (see Annex 1):

- exchange management policy was introduced with the April 1932 implementation of the Exchange Equalisation Account (EEA), so that fixed exchange rates were maintained by the buying and selling of currency rather than through manipulations of the discount rate;

- cheap money on short-term interest rates was set by rapidly cutting the discount rate and then maintaining it at 2 per cent;

- cheap money on long-term interest rates was initiated with the conversion of the War Loan from 5 to 3 ½ per cent in July 1932; and

- capital control was implemented to ensure autonomy over the domestic interest rate environment.
The combined effect was a shift in the control of monetary policy from the Bank of England to HM Treasury and hence from private to public control.

3. Robbins and LSE Oppose Monetary Reform

After some to-ing and fro-ing between Oxford and London, Robbins took the Senior Professorship of Economics at LSE in 1930. He appears to have galvanised the institution and presided over a renaissance of a continental brand of classical economics just as economic activity across the world was collapsing in the Great Depression. His early and most enduring contribution was of course the subject of this symposium, *An Essay on the Nature and Significance of Economic Science* (1932).259

The work was one of three significant contributions from LSE economists, with Friedrich von Hayek and John R. Hicks emerging into the public eye at about the same time. Robbins has “Fritz Hayek” “first appearing at L. S. E. in January 1931” when he delivered his lectures that were a “revelation of an aspect of classical monetary theory that had been forgotten” (Robbins, 1971, p. 127).260 This theory, most fully articulated in his 1931 *Prices and Production*, allowed the LSE to challenge Keynes on his own – monetary – ground. Robbins celebrates Hicks’s role as a “leading agent in making us aware of the relevance of other schools of thought. If Hayek must be credited with bringing Austrian and Wicksellian thought to the School, the introduction of Walras and Pareto must be chiefly attributed to Hicks” (*ibid.*, p. 129). This was perhaps foreshadowed in his 1932 *Theory of Wages*, which was “bursting with ideas” according to Robbins (*ibid.*, p. 130).

Robbins’s *Essay* challenged those who sought to resolve the Economic Problem of the world by disregarding or rejecting the strictures of classical economics, which he presented simply as truth. “False ideas are prevalent with regard to the preoccupations of the economist and the nature and the extent of his competence” (Robbins, 1932, p. vii.). He re-asserted the general theory of value, and the famous statement that continues to define economics as “the science which studies human behaviour as a relationship between ends and scarce means which have alterative uses” (*ibid.*, p. 15) seemingly sought to confine the competence of economics to a more abstract plane.

Towards the end of the book he reached some conclusions. “We have seen that it provides, within its own structure of generalisations, no norms which are binding in practice. It is incapable of deciding between the desirability of different ends” (*ibid.*, p. 135). But this did not make economics irrelevant to practical questions: “… it enables us to choose with full awareness of the implications of what we are choosing” (*ibid.*, pp. 135-6). To illustrate, he offered brief remarks on the two prominent policy issues of the moment: tariffs and monetary reform. In the case of the “protective tariff”, economics could see to the “further repercussions” (*ibid.*, p. 137) that went beyond the naïve benefit to any specific industry. In the case of monetary policy, Robbins offered an explicit challenge to Keynes’s *Tract*:

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259 O’Brien’s (1989, p. 106) discussion of Robbins’s earlier contributions is of interest: “The first publication I have traced was a review which appeared in 1925 (Robbins, 1925) – and interestingly, in the light of subsequent events, it contained an attack (quite unprovoked it would seem) on Keynes for wishing to have monetary policy influenced by popular sentiment”.

260 Corry (1987, p. 207) has Robbins bringing Hayek to LSE in 1928.
It is an inescapable deduction from the first principles of monetary theory that, in a world in which conditions are changing at different rates in different monetary areas, it is impossible to achieve at once stable prices and stable exchanges. [footnote:] See Keynes, *A Tract on Monetary Reform*, pp. 154-155; also an interesting paper by Mr. Dennis Robertson, *How do We Want Gold to Behave?* reprinted in the *International Gold Problem*, pp. 14-46 [end footnote]. The two ends – in this case the ‘ends’ are quite obviously subordinate to other major norms of economic policy – are logically incompatible. … Yet without some analytical apparatus how few of us would perceive the incompatibility of the ends in question! (*ibid.*, 138)

In closing, he made his plea:

> If irrationality, if the surrender to the blind force of external stimuli and uncoordinated impulse at every moment is a good to be preferred above all others, then it is true the *raison d'être* of Economics disappears. … But for all those who still affirm more positive values, that branch of knowledge which, above all others, is the symbol and safeguard of rationality in social arrangements, must, in the anxious days which are to come, by very reason of this menace to that for which it stands, possess a peculiar and a heightened significance. (*ibid.*, p. 141)

And this is the significance of economics: to protect against the hasty implementation of dangerous policies in the wake of the world crisis.

At the time, there were some who took a dim view of this retrenchment. In 1936, a review of the *General Theory* observed:

> There has been of late years a general trend among economists, led by what might be described as the London School, away from the real world of economic affairs. A certain scorn even that economists should concern themselves with ‘realistic economics’ breathes in the whole tone of the manifesto of this school, Professor Robbins’s *Nature and Significance of Economic Science*. (A. L. Rowse, September 1936, re-printed in Backhouse, 1999, p. 108)

Robbins rejected this charge. Indeed, with their foundations secure in classical theory, the LSE economists engaged themselves in a substantial and very public policy intervention, opposing those who sought to move away from the truths of classical policy.

Before the *Essay* had been published, and before Britain’s departure from gold, the LSE economists came into the open against protectionism. Calls had come from city bankers (see Kynaston, 1999, p. 204) and the Rothermere and Beaverbrook media, but matters came to a head at the 1930 Committee of Economic Outlook, when Keynes and others advocated protective tariffs given ongoing adherence to the gold standard or devaluation. In spite of Keynes’s furious opposition, Robbins insisted on
producing a dissenting minority report of one, which was included at the end of the main report. The economists’ report was initialled on 24 October 1930.

Robbins persevered. In his autobiography, he tells how he convinced William Beveridge, the Director of LSE, of the justness of his argument, and that, as a consequence, Beveridge “propos[ed] that we should form a committee with himself as chairman and draftsman to prepare a considered treatment of the whole subject” (Robbins, 1971, p. 157). Beveridge appears to have marshalled all LSE economists to the cause. Their report, published in August 1931, *Tariffs, The Case Examined, by a committee of economists under the chairmanship of Sir William Beveridge*, featured contributions by F. C. Benham, W. H. Beveridge, A. L. Bowley, T. E. Gregory, J. R. Hicks, W. T. Layton, A. Plant, L. C. Robbins and G. L. Schwartz. The Preface summed up the context as “economic troubles of all kinds crowd[ing] thick and fast on the world and on Britain”; it concluded:

… we should all think it a disaster, if the policy of Free Trade which has served Britain so well materially, as through her it has served as an inspiration to all in any land have worked for good understanding among nations, were to-day to be sacrificed to ignorance or panic or jealously or specious calculations of a moments gain. (Beveridge, 1932, p. vi).

A month after publication, sterling came off gold. Keynes’s rationale for protectionism was gone. Nevertheless, in February 1933 the new (Conservative) Chancellor Neville Chamberlain announced a 10 per cent general tariff.


But Robbins’s attention was fixed mainly on monetary reform. He later conceded the existence of genuine rivalry between LSE and Cambridge, on this “general policy of financial expansion to counter the depression” (Robbins, 1971, p. 133). Portraying himself as a “slave” of Mises’s and Hayek’s theory, “in which the coming into existence of this ‘real’ disproportionality was explained in terms of a failure of money

261 See for example, Skidelsky (1992, pp. 368-78) and Robbins (1971, p. 151).

262 Though Robbins was deeply dissatisfied with the results, noting how “Dennis Robertson, who in a detached position, had sat with us at the beginning, found ways of disengaging himself gracefully …” (ibid., p. 158).

263 Salter and Layton would be important advocates of ‘planning’ (see section 5) and played leading roles in setting the agenda for the post-war world. Salter was variously a bureaucrat (in the British civil service and as a League of Nations official), academic (the Gladstone Professor of Political Theory and Institutions at Oxford and a fellow of All Souls) and a politician (Member of Parliament for Oxford University, 1937–50, and as a member of the Conservative Party, 1951–53). He became a Baron in 1951. Layton was an academic (economics at Cambridge University and then University College London) and then the editor of *The Economist* from 1922 to 1938; *The Dictionary of National Biography* adds that Layton ‘worked for Anglo-American understanding, European Unity, and the United Nations’. He became a Baron in 1947.
rates of interest to reflect adequately the relation between the disposition to save and the disposition to invest” (*ibid.*, pp. 153-4), he saw the cause of the Great Depression in “excessive financial ease and mistaken real investment”, a diagnosis that led to a prescription for tighter money.

He set out his case in his next substantial work, *The Great Depression* (1934), a rival to Keynes’s *Treatise* and forthcoming *General Theory*. As Hubert Henderson (1935, p. 122) put it in his review, the “villain of Professor Robbins’s story” was “the monetary reformer”.

Robbins told a story of an ineptly-managed gold standard. The Great Depression in the US was the result of over-production, caused by a credit “‘reflation’ on the part of the Federal Reserve authorities, which produced the worst phase of this stupendous fluctuation” (Robbins, 1934, p. 54). The situation was exacerbated by “… the element of elasticity and uncertainty introduced by the existence of the various pools and restriction schemes, the rigidities of the labour market and cartel prices …. ” (*ibid.*, p. 60).

In the case of Britain, Robbins pointed his finger at the early 1920s debate about whether to return to gold: “[f]rom the point of view of the historian of the recent crisis, nothing can be more important than the propaganda for a managed currency. … It created an attitude of mind on the part of the educated public which in subsequent years made it more and more difficult to work the Gold Standard successfully” (*ibid.*, p. 77). Although he conceded that the return may have been effected at too high a sterling parity, he concluded: “[i]n so far as the disaster of this period is to be attributed to monetary causes, it was not conformity to the logic of the Gold Standard, but rather disregard of this logic, which was at the root of the trouble” (*ibid.*, p. 97).

Concerning immediate monetary policy matters, he judged:

> … if recovery gets going at all … one of the main tasks of the monetary authorities will be to prevent it flaring up into a wild boom whose collapse might well be associated with consequences even more disastrous than anything which has happened in the present depression. (*ibid.*, p. 164)

And finally, despite its limitations, Robbins held that the Gold Standard was as “a worthy object of policy in our own life” (*ibid.*, p. 169).

> A stabilisation of exchanges and an eventual restoration of an international monetary system, run on the lines indicated above, would probably afford the basis for a considerable recovery of business if political conditions were favourable… (*ibid.*, p. 182)

But Robbins also began to engage with the emerging debate on the necessity of ‘planning’ economic activity, which had gained political impetus, most notably through the future Conservative Prime Minister, Harold Macmillan (see next section). “But is this a tendency we wish to avoid? It is not clear that this is the attitude of the
present leaders of opinion. Socialism is a term which is not universally popular. But ‘planning’ – ah! Magic word – who would not plan?” (ibid., p. 145). The agenda was not rejected out of hand, but he raised practical as well as libertarian objections. For the time being he concluded: “A world of national planning is not a world which offers high hopes of political stability of economic progress” (ibid., p. 159). But his conversion had begun.264

4. Developments Beyond the L.S.E. and Keynes’s Cambridge

The fourth grand monetary discussion fostered a renaissance in academic economics. Across the world, new journals were published, the Econometric Society was founded, the scope of teaching was expanded and economists’ views became increasingly prominent in public policy debate. The most important centres, from the perspective of the story here, were Cambridge, the LSE, Oxford, Harvard, Chicago and the Stockholm School of Economics. The League of Nations would also play a central role, with an explicit mandate to develop a consensus of emerging views. The resulting agenda was founded on loanable-funds theory. The specific notion of ‘planning’ was advocated in Britain, with, as noted, Macmillan in a leading role (though, plainly, variants of ‘planning’ were already in effect in the totalitarian economies).

Keynes by no means dominated academic economics in Cambridge. Pigou, in spite of his advocacy of fiscal initiatives (see above), would emerge as a vigorous critic of the General Theory and sought to develop his own mathematical statement of theory. But it was Robertson, again, in spite of their celebrated theoretical partnership, who was Keynes’s most persistent and robust critic. Robertson may be regarded as a monetary economist, but as Fletcher has emphasised, his aim was always to “to deny the power of money to subvert established classical relationships” (Fletcher, 2007, p. 80) and hence was wholly opposed to Keynes’s view. From his review of Treatise of Money onwards, Robertson came out in the open as at loggerheads with Keynes’s economic theory and as a distinct sceptic about his policies. However, he tended to present his views in more moderate terms. He reviewed Robbins’s Great Depression, summing it up as “an exhilarating convey of thunderbolts” (Robertson, 1935, p. 106).265 But he promoted a slightly more moderate policy, willing to countenance a degree of cheap money and fiscal policy but only to counter slump (see also footnote 19).

The advocates of energetic State action against developed depression have had in all countries a hard fight to wage against the forces of apathy and despair. Let us salute them everywhere, in their victories or in their honourable defeats: but let us beg them, whether flushed with success or saddened with failure, to think again before concluding that cheap money and Government deficits, still less trade restriction and exchange manipulation, are the right diet for all phases of the trade cycle or the right remedy for all the economic ills of the world. (September 1936, speech at Harvard University, reproduced in Robertson, 1966, p. 94)

264 Robbins had not always been so enamoured of the free market cause; he was originally “greatly impressed by the literature of Guild Socialism; it was under that banner that I hoped to participate in what I believed to be the forthcoming transformation of society” (Robbins, 1971, p. 56).

265 Henderson’s (1935) review was more sceptical, but still accorded to Robbins high praise.
As Keynes moved from his *Treatise* to the *General Theory*, Robertson, like Pigou, sought to develop his own theory of the economy (Robertson, 1934 & 1936); central to his interpretation was the loanable funds theory of interest. I have argued that this theory, not Keynes’s theory, was the precursor to IS-LM (Tily, 2007, Chapter 4).

The League of Nations’ major theoretical initiative would bring matters together. A resolution had been adopted in September 1930, with funding provided by the Rockefeller Foundation, according to the following mandate:

> The key word was ‘consensus,’ since the Rockefeller Foundation expected as one of the results of its five-year grant to the League of Nations that ‘the divergence of views among economists as to the nature and the means of controlling the business cycle will be appreciably lessened,’ which would be an ‘essential preliminary to the unification of national policies for dealing with the business cycle’. (Boianovsky and Trautwein, 2006, pp. 73-4, citing a memorandum by a Rockefeller Foundation employee)

The Director of the Financial Section of the League of Nations had originally wanted Robertson to take the lead, but Robertson declined. Instead Gottfried Haberler, an economist originally of the Austrian school, took the lead in developing the ‘consensus’. But Robertson’s ‘framework’ would be adopted in the eventual synthesis (ibid., p. 51).

The first milestone was reached in 1934, when Haberler produced the first of his coming two-part work under the title, *Systematic Analysis of the Theories of the Business Cycle*. He circulated the report to and entered into a number of correspondences with leading members of the economics profession from across the world (including Robbins).

Meanwhile, momentum was building behind a British campaign for ‘planning’ economic activity. In 1933 Harold Macmillan had set himself up as figurehead for the campaign for what he referred to as ‘economic nationalism’. The preface of his book, *Reconstruction: A Plea for a National Policy*, explained cause, motive and means:

> We must realise the essential contradictions of laissez-faire even while we may appreciate the energy and drive of a rugged individualism. The policy we are seeking will only be satisfactory if it goes deep enough to correct the maladjustments and reconcile the disharmonies from which our problems arise. But, if revolutionary violence is to be avoided, it must also make its appeal to a sufficiently broad strip of public opinion to secure the support for its adoption. It must be at once radical and popular. (Macmillan, 1933, pp. 6–7; his italics)

Macmillan then emerged alongside Clifford Allen as leaders of the ‘Next Five Years Group’ (NFG) (Skidelsky, 1992, p. 438). Reflecting the cross-party nature of the initiative, Allen was a leading Fabian and member of the Labour Party (he took MacDonald’s side when the Labour Party split in 1931). After issuing two pamphlets,

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266 He had favourably reviewed the LSE tariffs book (Haberler, 1932).
a fuller manifesto was published as The Next Five Years: An Essay in Political Agreement (1935).267 The manifesto foreshadowed much of what was to become the post-war agenda. Notably, monetary reform was rejected:

The importance of monetary policy, though great, can easily be exaggerated.

… But money is not all-powerful. Many years ago Jevons wrote: “There are men who spend their time and fortunes in endeavouring to convince a dull world that poverty can be abolished by the issue of printed bits of paper. I know one gentleman who holds that exchequer bills are the panacea for the evils of humanity. Other philanthropists wish to make us all rich by coining the national debt, or coining the lands of the country, or coining everything.” The forms of these beliefs have grown more subtle, and more plausible in the process, since Jevons’ day. But the root-belief is still the same: that by a few simple book-keeping transactions a flood of wealth hitherto pent up by an imperfect monetary system can be released to sweep poverty from the face of the earth. We do not share these beliefs. (NFYG, 1935, pp. 97–9)

The most promising suggestion is that currencies should be re-linked to gold, but at parities which could be changed from time to time. (ibid., p. 111)

In February 1936, as the planning and fiscal agendas and associated theories were emerging, the General Theory was published to instant and almost unanimous opposition from the academic community (see Backhouse, 1999, p. 12). Its fortunes would contrast sharply with the papers that would come to define the ‘Keynesian’ approach. A critical moment seems to have been the 26 September 1936 Econometric Society symposium on the General Theory held at Oxford University. As Young (1987) has emphasised, Hicks, Roy Harrod, James Meade and Brian Reddaway each prepared algebraic formulations of macroeconomic theory with a good deal of common ground. All of the papers were published, but Hicks, of course, carried the day; his IS-LM model would become the textbook Keynesian model even to the present day. IS-LM was a loanable funds theory, with a good deal of overlap with Robertson’s preceding contributions and with no role for bank money or uncertainty. Monetary policy was confined to determining the quantity of money.

The Oxford Keynesians now included Hubert Henderson, who in 1934 had left his HM Treasury post as assistant and later joint secretary to the Economic Advisory Council for All Souls. In 1937 he was joined by Beveridge, who resigned as director of LSE to become Master of University College. In 1935 Hicks left LSE for Manchester; he arrived at All Souls in 1946.

267 The ‘Foreword’ includes a list of 152 signatories drawn from across the British Establishment (‘drawn from different parties and schools of thought’). The list also indicated those signatories that were members of the ‘drafting Committee’: Allen, W. Arnold-Forster, A. Barratt Brown (the Principal of Ruskin College, Oxford), Geoffrey Crowther (soon to succeed Layton as editor of The Economist), Macmillan and Salter.
The League of Nations initiative reached its next milestone in March 1936, when a group of leading economists were invited to participate in discussions of the second part of Haberler’s work in Geneva.268 Robbins and Robertson were present alongside many who would become leading lights of the profession in the post-war era (Boianovsky and Trautwein, 2006, pp. 62-3):

- Dennis Robertson (Cambridge),
- Otto Anderson (Economic Research Institute, Sofia),
- John Maurice Clark (Columbia University),
- Leon Dupriez (Catholic University of Leuven),
- Alvin Hansen (Department of State, Washington DC),
- Oskar Morgenstern (Austrian Business Cycle Institute),
- Bertil Ohlin (Stockholm School of Economics),
- Charles Rist (University of Paris),
- Lionel Robbins (London School of Economics),
- Wilhelm Röpke (University of Istanbul) and
- Jan Tinbergen (Dutch Institute of Economics).

The verbatim note of the meetings reflects the desire for coherence with Robertson’s contributions, as well as an interest in public works:

Ohlin suggested that a more detailed discussion of the saving–investment mechanism should be provided, one that takes into account Robertson’s ‘Industrial Fluctuations’ paper (1934). According to Ohlin, this could show how a higher level of expenditure in public works brings about ‘unintentional savings’ able to finance the public deficit (pp. 30–1). There was general agreement that “we must have period analysis in the way indicated by Mr. Robertson,” … (ibid., p. 64)

Moreover, a memorandum by a Rockefeller Foundation employee (John Van Sickle) reported the success of the conference:

A number of those in attendance told me privately that they had come with considerable scepticism as to the usefulness of such a conference. At the end there was not a single one who did not feel that the meeting had been definitely worthwhile. (ibid., p. 73).

The work was published in June 1937 under the title *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements*. The first of the two parts set out summary categorisations of existing theories of the cycle.269 In the second part, Haberler set out his ‘synthesis’. There is perhaps an implicit message that Keynes’s work presented no great challenge to academic economic theory.

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268 Robbins does not mention his participation in this work in his autobiography.

269 Keynes’s work was included as a ‘psychological’ theory, with elaboration only in a footnote: “In recent years, it has become fashionable to lay stress on the element of expectation. Keynes’ “General Theory of Employment, Interest and Money” is conceived in terms of expectation; and, at an earlier date, the conception of economic expectation was interpreted and developed by the Swedish school …” (Haberler, 1937, p. 135).
In the course of this analysis of existing theories, it became apparent that many of the seeming differences of doctrine were due rather to the use of different terminologies than to any more fundamental causes. The measure of agreement which appeared to exist between those who have devoted special attention to the problem of the trade cycle seemed to justify an attempt to make from their theories even at this early stage of the work the general synthesis which constitutes the second part of this volume. This synthesis, however, is more than a simple patching together of the theorems of others: it is an attempt to create a living and coherent, if incomplete, theory on the basis of the knowledge at present available. (Haberler, 1937, pp. iv–v)

In specific theoretical terms, the book set out a loanable-funds theory. *Prosperity and Depression* was reviewed in the main economic journals “with largely positive reactions to its synthetic approach” (Boianovsky and Trautwein, 2006, p. 77). Earlier correspondence with Haberler indicated that Keynes was not impressed with the initiative.270 On publication, he got Kahn to review it. The review was highly critical and the subsequent dialogue was acrimonious (Kahn, 1937 & 1938, Haberler, 1938).271

Haberler then moved to Harvard, where he had been appointed professor in 1936, and from where United States Keynesianism would emerge. He joined Paul Samuelson, who had arrived on a fellowship from Chicago in 1935 (staying until 1940), and Alvin Hansen, who in 1936 was appointed the first Lucius S. Littauer Professor of Political Economy.

Hansen’s review of the *General Theory* advocated Robertson’s loanable-funds approach. In 1941 he set out the ‘Keynesian’ policy agenda in his *Fiscal Policy and Business Cycle*:

> It is at this point that depression policy emerges in a new role as an important element in a positive governmental program. But an economic minimum cannot be insured by reliance exclusively upon monetary policy. (Hansen, 1941, p. 74)

> There is thus emerging a new aim of fiscal policy, vigorously assailed by some and staunchly defended by others – the aim of ensuring full employment of the factors of production. This policy involves greatly enlarged governmental expenditures. (ibid., p. 117)

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270 Boianovsky and Trautwein (2006, p. 49) illustrate: “Keynes (30 August 1934) reacted negatively to the project, writing [to] Haberler that ‘I cannot think that you have gone the right way to work. The method of taking various propositions in isolation is to bring authors into the same pigeon-hole who are really leagues apart and have very little in common’.”

271 Keynes himself was involved in a number of controversies; with matters coming to a head – to some extent at least – in the ‘alternative theories of interest’ debate, when he challenged directly those who adhered to the loanable-funds theory (explicitly identifying Hicks and Robertson; however, Ohlin presented the rival case (Ohlin, 1937a&b)). He also openly challenged the League of Nations’ subsequent empirical initiative in the famous dialogue that followed the publication of Tinbergen’s *Statistical Testing of Business-Cycle Theories* (1938).
The public debt is an instrument of public policy. It is a means to control the national income and, in conjunction with the tax structure, to regulate the distribution of income. (*ibid.*, p. 185)

A planning agenda and theory had been constructed. Despite the extent of the shift from classical economics, little or no controversy followed the publication of the various contributions. On board were a good number of the big names of the future of the profession.

5. International Planning *versus* Monetary Nationalism

In the years between the publication of the *General Theory* and the start of the war, Robbins began to take a new approach to Keynes’s work, but one that remained underpinned by his doctrinaire rejection of monetary reform.

In 1937 Robbins responded to Keynes’s pronouncements on ‘How to avoid a slump’, published between 12 and 14 January in *The Times*. Keynes re-iterated his view that the authorities had control over the spectrum of interest rates, and he warned:

Unquestionably in past experience dear money has accompanied recovery; and has also heralded a slump. If we play with dear money on the ground that it is ‘healthy’ or ‘natural’, then, I have no doubt, the inevitable slump will ensue. We must avoid it, therefore, as we would hell-fire. … A low enough long-term rate of interest cannot be achieved if we allow it to be believed that better terms will be obtainable from time to time by those who keep their resources liquid. The long-term rate of interest must be kept *continuously* as near as possible to what we believe to be the long-term optimum. (*CW* XXI, p. 389)

In May, Robbins published a short article, ‘How to Mitigate the Next Slump’, in the *Lloyds Bank Limited Monthly Review*. He argued: “It has long been a maxim of prudence that, to avert a slump, it is necessary to avert the boom” (Robbins, 1937a, p. 234). Foreshadowing today’s policies, he saw a need for the “expansive process to be restrained” at the point “at which prices begin to rise rapidly” (*ibid.*, p. 235).

Now this means quite definitely that, once this point has been reached, a continuance of the régime of cheap money which has accompanied the previous depression is dangerous. If money is kept cheap by deliberate manipulation in the face of a rising investment demand and rising costs and money incomes, then mal-investment of the kind which sooner or later leads to a bad crash is deliberately encouraged. … If such a collapse is to be avoided, the régime of cheap money must not be artificially prolonged.

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272 Moggridge (1992, p. 605) notes that the recommendations in this article then went on to ‘serve as the basis for the Committee of Economic Information’s 22nd report of February 1937, “Employment policy and the maintenance of trade activity”’. 
I hope that what I have just said will make it quite clear that I am no
friend of the view which holds that we can dispense with the use of the
discount rate as an instrument of stabilisation. (*ibid.*)

In the meantime, Robbins had begun to engage constructively with the planning
agenda. As he put it, “Reflecting … on current developments in the world of reality, I
decided upon an entirely new approach to the problem” (Robbins, 1971, p. 158). His
consequent major work *Economic Planning and International Order* originated in
lectures at the *Institut des Hautes Etudes Internationales* at Geneva in the summer of
1935. The Preface indicated how far he had already come: “It has not been
seriously suggested that there should be no plan, no order in society. The issue is not
between a plan and no plan, it is between different kinds of plan” (Robbins, 1937b, p.
6). The title of the book indicates the scale of his vision.

Robbins characterised ongoing political and economic developments as nationalistic,
in particular monetary reform and tariff policies. Keynes’s monetary nationalism was
a particular case in point; an internationalist gold standard had been replaced by
autonomous national policies: “of all forms of economic nationalism, monetary
nationalism is the worst” (*ibid.*, p. 290). In a chapter entitled ‘International Money’,
he was only partly inconclusive:

… [I]t is clear … that control of local policy should be removed as far as
possible from the influence of local governments; that, whatever their
ultimate destiny, the different reserve systems should cease to be the
instruments of monetary nationalism. The banking policy of the twenties,
which sought to remove the central banks from the interferences of
governments, was right. The banking policy of the thirties, which has
been to bring them once more under government control, is wrong. The
immediate objective of policy, therefore, must be to reverse this tendency.
In a world free from monetary nationalism, the solution of the remaining
problems of banking policy should not present insurmountable
difficulties. (*ibid.*, pp. 304-5)

According to Opie, he pleaded only for return to some kind of Gold Standard:

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273 Robertson’s comments on this paper are of interest: “There is nothing in Professor Robbins’s pages
about the rate of interest with which in practice I disagree – neither in his defence of low money rates
in the early phases of the present recovery, nor in his contention that the normal tendency of rates, both
short and long, to harden as expansion proceeds should not now be resisted and may have to be
actively reinforced. All the same, I am conscious of a certain difference of emphasis in my mind, both
from Professor Robbins and from those [unnamed!] who, diagnosing differently from himself, have
reached precisely the opposite conclusion that an increase in interest rates should be avoided like hell-
fire. For both these parties are convinced that the rate of interest is supremely important … The one
party finds the ultimate villain of the piece in the extravagance of the human race, the other in its
passion for keeping money safe; and their recipes for monetary policy diverge accordingly. ‘Let rates
rise now,’ says Professor Robbins, ‘to prevent unwise planning; for the desire to spend is sure to make
them rise later.’ ‘Keep rates down now,’ say his opponents, ‘lest if once you let them rise the desire to
hoard prevent you from ever getting them down again.’ My judgement is, on the whole, with Professor
Robbins; yet, even if he has his way, I hope for no great things” (Robertson, 1937, reproduced in 1966,
p. 98).

274 He was invited by William Rappard, “one of the truly great men of the inter-war period” (Robbins,
1937b, p. 159).
Events since the war have convinced Professor Robbins that efforts to construct an international monetary federation via monetary nationalism are doomed to failure, and therefore he pleads for a return to some kind of gold standard. He is not dogmatic: indeed, with admirable frankness he says that “if we are honest, we must confess that … our knowledge both of the desiderata and of the possible instruments of general monetary policy is so imperfect that, even within the context of the general liberal idea, it is not possible to speak with any certainty concerning the most desirable ultimate form of international banking institutions” (p. 304). (Opie, 1937, pp. 515-16)\(^{275}\)

This ‘nationalism’ was set against his own vision for an international plan. He advocated the deconstruction of nations and the development of the necessary structures, institutions and frameworks that would permit international liberalism. The same plan:

… proceeded to exhibit economic liberalism as essentially involving a plan, a plan of a framework of law and order canalizing the spontaneous activities of producers into patterns conducive to the satisfaction of members of the community in their capacities as spenders, both private and public – a policy, however, which tended to be frustrated in the international sphere by the absence of any such framework and the dangers of inter-group conflict. (Robbins, 1971, p. 160)

However, the following types of ‘interference’ or ‘planning’ within nations were rejected: protectionism, control of investment, monetary policy, restriction of migration, cartels and agreements affecting labour, wages and hours.

It has been argued that communism on an international scale must develop internal weaknesses which frustrate the achievements of its aims. But it has been argued, too, that liberalism exhibits none of these weaknesses and that an international liberal plan is conceivable which is technically workable and which is free from the manifest contradictions of other forms of planning. (Robbins, 1937b, p. 309)

In the final pages, Robbins warned that the nationalist vision, attributed explicitly to Keynes from his expressed desire to “… let goods be homespun whenever it is reasonably and conveniently possible, and, above all, let finance be primarily national”,\(^{276}\) would lead to war.

In 1939 he published two major works that built on these themes.\(^{277}\) The first, *The Economic Basis of Class Conflict*, had chapters that attacked protection, restriction, monopoly, agricultural planning and state planning of trade. He also reprinted ‘How

\(^{275}\) The review singled out this discussion as “the best chapter in the book” (Opie, 1937, p. 515); Knox (1938, p. 597) saw it as “a particularly valuable part of the book”.

\(^{276}\) Published in the June 1933 (Vol. 22, no. 4) edition of *The Yale Review* under the title ‘National Self-Sufficiency’ (*CW* XXI, pp. 233-46).

\(^{277}\) Curiously, his autobiography does not mention the title of the second work; but refers to the first as “the first part of a collection” (Robbins, 1971, p. 162).
to Mitigate the Next Slump’. But, the detailed point of most interest was his reproduction of a lecture delivered to the Stockholm Economics Society in *March 1936*. Perhaps for the first time, he indicated a more open-minded approach to public expenditure.

He conceded that

[i]n the real world, however, this assumption of continuous and full employment does not hold, and there is reason to suppose that, even in a system much less hampered by friction than our own, things would still not work out that way. … The question therefore arises whether variations of government expenditure may not be made to have, as it were, a damping effect on the variations of activity elsewhere. (Robbins, 1939a, pp. 214-15)

He proceeded to analyse the different effects of government expenditure on the ‘income account’ and the ‘capital account’. On the former he remained “not altogether convinced” (*ibid.*, p. 217), but it could be resorted to “only as an emergency measure” (*ibid.*, p. 218). But on the latter: “… if we are dealing with a period of slack employment … the borrowing is intended to have monetary repercussions favourable to the increase of general economic activity; and it may indeed have this effect” (*ibid.*, p. 219). However, in his closing summary he urged caution:

But I do not believe that there is usually a large place for obviously extraordinary expenditure. Of course, cases may arise where, in order to prevent complete deterioration of the position, in order to prevent complete cumulative deflation, measures of this sort may have to be adopted. … But in general I believe that the scope for this sort of thing is much smaller than usually supposed. (*ibid.*, p. 235)

Robbins’s discussion of the broader matter indicated by the title of the work, primarily in the opening essay, involved an implicit rejection of Marxist notions and his taking tentative steps to seeing class conflict arising from nationalistic struggles for economic ascendancy between nations.278 Workers in one nation were seen as pitted against workers in another:

In the present organization of the world into different national groups, with its almost total prohibition of international migration, the interests of labourers in different parts of the world are often violently opposed to each other” (*ibid.*, pp. 17-18).

In a later chapter, he was more specific:

A world in which the movement of goods, of money and of people is restrained and impeded by international organization, a world in which the national states separately organize economic processes which are

278 “The paper was first presented as part of a symposium on Class Conflict and Social Stratification, held under the auspices of the Institute of Sociology in the autumn of 1937” (*ibid.*, p. 3)
essentially anational, is a world in which the achievement of the international ideal, whether on socialist or liberal lines, is more distant even than it is at present. (ibid., pp. 208-9)

In *The Economic Causes of War*, these struggles were found to be at the root of not just the class conflict but of war. Again the book was based on lectures delivered in Geneva (in Spring 1939); he motivated the discussion using E. M. Forster:

“Bourgeois born and in my fifties”, writes Mr. E. M. Forester, “I feel that communism will submerge all the things I have learnt to love. But I am deeply impressed by the communist argument which ascribes war to the capitalist system and I feel that the hysteria which prevents people from examining this argument … is most discreditable” (*Time and Tide*, November 23rd, 1935). (Robbins, 1939b, p. 17)

In the final chapter, ‘the ultimate cause of international conflict’ was first identified as restrictive trade. But he moved on: “The Ultimate condition giving rise to those clashes of national economic interest which lead to international war is the existence of independent national sovereignties” (Robbins, 1939b, p. 99). The final section of the chapter looked to “The United States of Europe” as providing an appropriate institutional structure for the international planning that could bring an end to war. “There must be an international framework of law and order, supported by solid sanctions which prevent the emergence of those policies which are eventually responsible for conflict” (ibid., pp. 104-5).

6. The Emergence of ‘Keynesian Economics’ in W.W.II Britain

In war, Robbins would help adjudicate over the extent of the implementation of domestic planning and fiscal policy in Britain. According to Harrod (1972, p. 592), Robbins was recruited onto the Economic Section of the Cabinet Office by Mr Francis Hemming, assistant secretary to Arthur Greenwood, Minister without Portfolio and previously an official on the Economic Advisory Council. In Autumn 1941 Robbins succeeded John Jewkes as the head of the Economic Section.

The precise genesis of the Employment White Paper is not wholly clear. Robbins (1971, p. 186) had Jewkes as instigator, with James Meade making the early running (though Robbins adds that Meade had thought about the subject “at an earlier stage”). He portrayed his own role as follows: “… my part was a very modest one. I can claim no credit in the initiation of the Meade paper, which was due to Jewkes; nor did I contribute to the basic ideas as they evolved” (ibid., p. 188). Howson shows

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279 It should be emphasised that the lectures were presented in spring 1939. Robbins acknowledged that “We are fighting Germans” and that “The Nazis must be Exterminated” (ibid., p. 109). He closed the book: “What more appropriate outcome of our present agonies, therefore, what more fitting consecration of the blood which is being shed, than a peace into which this great people [the Germans], purged of its devils, shall be coerced into free and equal citizenship of the United States of Europe” (ibid.).

280 Robbins was not impressed by Hemming; see Robbins (1971, pp. 171-2).

281 Meade (1907-1995) was originally based in Oxford, but had worked quite closely with Keynes’s Cambridge colleagues over the 1930s (and made contributions to discussions as the *General Theory* was being developed). Before he moved to Whitehall, from 1937 to 1940, he worked at the League of Nations.
that Robbins re-drafted Meade’s early efforts (“after all that he has had to put up with [by way of criticism] from us, it would have been a last indignity to ask J.E.M. to do this”, Howson, 2000, p. F129). The Employment White Paper (EWP) was published in 1944.

Robbins had become a ‘Keynesian’; he explained the extent of his conversion in his autobiography as follows:

… if all that is involved by that description is a conviction that, in a free society, the fluctuations of aggregate demand must not be left to look after themselves and that it is an important function of government, national or international, to pay attention to such matters, that indeed was my position. (Robbins, 1971, p. 188)

He may have become a ‘Keynesian’, but he continued to oppose Keynes’s monetary policy. This was in spite of the further official consolidation of his interest rate policies. With the end of the ‘phony war’, Keynes had returned to HM Treasury and became directly involved in the implementation of the policies that he had long advocated. Over the next years the authorities developed the specific instruments and mechanisms that permitted the full control of interest that Keynes had long seen as possible, in particular (and see Annex 2):

- the ‘tap issue’ policy for government debt sales and extending the range of securities on offer; and
- the extension of floating debt facilities through the introduction of Treasury Deposit Receipts (TDRs).

Along with the ongoing freezing of the discount rate, capital control and full exchange control, these facilities enabled the authorities to manage the ‘three per cent war’ with ease. Keynes’s theory had been vindicated: it had been proven that the rate of interest could be controlled; moreover, that control had been achieved under the most severe financial conditions imaginable.

But the (Cabinet Office) Employment White Paper was decidedly lukewarm to these (HM Treasury) developments. In particular, cheap money was portrayed as a transitory policy. Keynes’s (mainly private) comments on these policy deliberations reveal some disagreement.282

The 1945 National Debt Enquiry (NDE) appears to have been set up to reconcile the views of the authors of the White Paper and HM Treasury economists and civil servants. Comprised of senior civil servants and Keynes, Robbins and Meade from

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282 In May 1943, on receiving Meade’s preliminary work, Keynes was perceptive: “I think you lay too much stress on cure and too little on prevention” (CW XXVII, p. 326). But when the paper was published, observations in his briefing for the Chancellor indicate that he did not appreciate the underlying rejection of monetary reform, though they do reveal a degree of bafflement: Criticism. Reference to interest rates in Paragraph 59 has been subject to criticism in some quarters of the Press. It is said that whilst we are promised a continuance of the cheap money policy for the time being, we are threatened with a reversal of it at some later date. Answer. I have never myself been able to make much sense of that paragraph … (CW XXVII, p. 375).
the economic side, it met in April and May 1945. Sir Richard Hopkins, the war-time Permanent Secretary to HM Treasury, drafted the Report. The remit of the Enquiry was presented as follows: “We were asked to define more closely an appropriate Treasury policy in regard to cheap money with particular reference to statements in the White Paper on Employment Policy” (NDE Report, para. 2). The relevant statements from the EWP were cited as follows:

3. Employment White Paper references. The principal reference in the White Paper is as follows:

“58. In ordinary times the volume of capital expenditure is influenced by movement in the rate of interest. If the cost of borrowing money is high, some projects which are not profitable at that rate will be held back. When it falls again, those projects will be brought forward and others will also be taken in hand.

59. For some time after the end of the war it will be necessary, as explained in paragraph 16, to maintain a policy of cheap money. Thereafter, the possibility of influencing capital expenditure by the variation of interest rates will be kept in view. The experience gained since 1931 of co-operation in this field between the Treasury and the Bank of England and the Joint Stock Banks will make it possible to operate a concerted and effective monetary policy designed to promote stable employment.

60. Monetary policy alone, however, will not be sufficient to defeat the inherent instability of capital expenditure. High interest rates are more effective in preventing excessive investment in periods of prosperity than are low interest rates in encouraging investment in periods of depression.”

4. Paragraph 16, referred to in paragraph 59, includes the following:

“(d) The use of capital will have to be controlled to the extent necessary to regulate the flow and direction of investment. Heavy arrears of capital expenditure on buildings, plant and equipment have to be overtaken, and construction on new development must begin. Without control, therefore, there would be a scramble to borrow, leading to a steep rise in rates of interest. The Government are determined to avoid dear money for these urgent reconstruction needs. In this period, therefore, access to the capital market will have to be controlled in order to ensure the proper priorities.”

The conclusions of the enquiry were summarised in the report as follows:

6. General desirability of low rates. There is a wide measure of agreement, though not complete unanimity, in the present Committee in the view that on the whole, subject to the qualification dealt with in paragraphs 11 to 15 below, the desirable
ideal for this country for a long time to come is not merely the continuance but even the reduction of the existing relatively low levels of interest rates both for long term and for short.

25. General Conclusion. We have been led to form a series of views not completely consistent with the brief references to the matter in the Employment White Paper. Rather we say that the White Paper ought to mean that, subject to uncertainties as to the extent to which and the conditions in which moderate fluctuations should be admitted, (which uncertainties need not be brought too much into the open), the object of Government should be to maintain low interest rates, long and short, for as far ahead as can reasonably be the subject of discussion – certainly far beyond the transitional period. …

The actual debate at the Enquiry (recorded in the minutes) brought the different views into sharper relief. On the one hand, most Enquiry members purported to agree with the theory and policy that Keynes had articulated:

Prof. Robbins … confirmed that the doctrinal analysis was one with which he (in common with most other professional economists at the present time) agreed. (PRO File T230/94, p. 220)

Mr Meade concluded by saying that he entirely agreed with the practical suggestions put forward by Lord Keynes. (ibid., p. 121)

On the other hand, a divergence of views about the implications for future policy remained apparent:

Mr. Meade, while agreeing with Lord Keynes in his general analysis, preferred to lay more emphasis on the importance of keeping interest rates down when we are entering a period of possible stagnation, with a view to lowering the cost of socially desirable public and semi-public investment. (PRO file T160/1408, p. 119)

But he [Prof. Robbins] felt that there should be a certain flexibility. … During the transitional period, if the physical controls were to crumble, there might be a case for a slight upward movement of rates; … [and] expressed some uncertainty about his proposals regarding short term rates … It might well happen that, if the leading nations were for political reasons to put the Trade Unions in a strong position, this would lead to a rise in wage-rates and in the general level of prices, resulting in an upward pressure on interest rates. (PRO File T230/94, p. 220)

While conceding non-unanimity, the Report was in line with Keynes’s views. And with the post-war general election victory of the Labour Government, the policy was brought into practical effect. On 7 November 1946 the Financial Times reported:

Cheap money policy, born of a depression, having survived a world war, is now regarded, not as a short lived device to overcome a temporary decline in prosperity, but as a permanent weapon for state regulation … So long as the government remains determined to push its cheap money
policy to the limit, long-term prospects for the rentier are anything but cheerful.

7. Post-War Domestic Policy

Robbins never abandoned his opposition to cheap money policy and his support for an active discount rate policy. Immediately after the war, he (alongside other key members of the academic economics profession) vigorously opposed Dalton’s cheap money policy. But he proclaimed his conversion to the necessity of managing demand through government expenditure (as well as interest rate manipulation).

The literature has tended to concentrate on the ‘failure’ of the Labour Government to go beyond the immediate conclusions of the NDE and to establish a long-term rate of interest of 2½ per cent. But, as noted, the opposition was more fundamental. It seemingly began with Robbins’s ‘Marshall Lecture’, delivered in May 1947, that has become more notorious for his “announc[ing] his conversion to full employment policies via the control of aggregate demand, although it is not clear that he became a Keynesian” (Corry, 1987, p. 207).

But, in the lecture, he raised inflation and monetary policy considerations. Seeing “the threat of inflation” (Robbins, 1947, p. 59) following from capacity constraints and the prevailing full employment, he argued “the first requirement of policy is that we should get this dropsy out of our system” (ibid., p. 61). He later regretted his slightly moderate stance, but listeners can have been in little doubt about the general direction in which he thought monetary policy should take:

How is this to be done? Contrary, perhaps, to your expectations, I am not prepared here and now to recommend a rise in interest rates. I say this, not because I believe that the interest structure, properly manipulated, cannot be a most potent instrument for regulating the rate of investment and for allocating supplies of capital, but because I fear the effect on the budget of a rise which, in present circumstances, would be sufficiently great to be effective.

… I would like to observe en passant that I do not think the situation has been made any easier by the policy of trying to force the rate of interest down – a policy which, I am clear, does not flow from Keynesian prescriptions. (ibid., pp. 61-2)

Instead he looked to the removal of some controls, the tightening of public expenditure, budgeting for surplus and an increase in purchase tax. His celebrated conversion to ‘Keynesianism’ was announced as follows:

Whatever we may think of the virtues of the price system as a mechanism of allocation, … I am quite clear that as an instrument for maintaining reasonable constancy of aggregate demand it has most profound limitations (ibid., p. 67).

284 E.g. Howson (1993), which rightly absolved Keynes from blame for this policy (p. 152).
285 “I did not urge they should go up. I now think that this was wrong and I missed an opportunity of recommending what, at that time, would have been a better policy” (Robbins, 1971, p. 225).
… I favour something which, if you like, you can call over-all financial planning. At the beginning of each appropriate period the government should make estimates of both the amount of expenditure (consumption plus investment) which is needed to maintain aggregate demand on a more or less even keel and of the amount of expenditure which is likely to be forthcoming. Then if there is a discrepancy between the two … it should seek, by whatever measures seem appropriate in that particular situation, to cause it to disappear. In the sector of public investment (which is henceforward likely to be large) it will have to plan in the current sense of the term … The sector of public consumption … is likewise susceptible to direct control. At the same time in the private sectors, both of investment and consumption, there are available a considerable number of indirect controls, chiefly of a fiscal nature, which can be used, at discretion, to supplement these more direct measures. (ibid., p. 68-9)

He claimed indebtedness to “Cambridge economists, particularly to Lord Keynes and Professor Robertson, for having awakened me from dogmatic slumbers” (ibid., p. 68).

Other economists quickly backed him, dwelling on monetary policy and moving towards a harder line. With confidence paramount to cheap money, their stance must have greatly undermined what the government was seeking to achieve – be it cheap or cheaper money.

In the June 1947 Economic Journal Hicks raised the spectre of inflation and looked to arguments for the re-assertion of interest-rate policy as a stabilisation tool. Henderson followed up with an alarmist piece in the next issue. In ‘Cheap Money and the Budget’, he maintained that the pressure of aggregate demand could not “be allowed to persist indefinitely without disaster” (Henderson, 1947, p. 265). He sought to undermine both the feasibility and the purpose of Dalton’s cheap-money policy:

A few months ago there was a disposition among financial experts to lay much if not most of the blame for this over-strong aggregate demand upon the cheap money policy of the Chancellor of the Exchequer. … Well, my personal opinion is that the cheap money policy has only been a very minor factor in the inflationary complex, so unimportant relatively to other factors as to be scarcely worth considering; and yet I am convinced that Mr. Dalton has carried this policy much too far. I do not believe that it will be possible to keep interest rates down over the next few years at anywhere near the low level of a few months ago; and I fear that it will prove that in trying to establish a long-term rate of 2½ %, or even less, Mr. Dalton may have missed the opportunity of turning a large part of what is now either floating or comparatively short-term debt into really long-dated securities on a 3% basis.

I see no good reason to suppose that the strength of demand in the general economic system would be materially reduced by somewhat tighter conditions in the money market, or by somewhat higher interest rates, whether short or long. I do not believe that a single industrialist or trader
would be deterred thereby from a single act of real investment, whether this be the purchase of additional stocks of materials or the renewal or extension of his plant. (ibid., pp. 265–6)

Then in December 1948 Robertson delivered a lecture at the Institut de Science Economique Appliquée in Paris that attempted to give – indeed, gave? – the coup de grâce to Keynes’s theory of interest. Under the title ‘What has Happened to the Rate of Interest?’, he re-asserted his own position with regard to the practical and theoretical importance of the rate of interest and to the appropriate theory of interest.

In the nineteen-thirties, under the first impulse of Keynes’s work, the rate of interest was elevated to a position of commanding theoretical importance. … it became, as never before, the keystone of the whole theoretical arch. But it also became the villain of the piece, and a very powerful villain. It was the dragon guarding the cave of “liquidity preference” – of the ineradicable urge of capitalist society to run for cover and to play for safety; it became the rock against which the waves of social improvement beat in vain. Nowadays – I am still talking about high-brow opinion – things seem to have altered in two ways. The rate of interest has come to be regarded as of less importance in the causal nexus, its high reclame of the nineteen-thirties savouring too much, to the modern taste, of an obsolescent economics of price. And at the same time it has come to be regarded as less powerful in practice and more vulnerable to attack …

… I think the rate of interest, in what Marshall once called its “strict sense”, is the price of the use of loanable or investable funds, and is governed like other prices by the interaction of schedules of supply and demand. (reprinted in Robertson, 1966, pp. 188–9)

In parallel, the mechanisms that Keynes put in place were gradually dismantled. Treasury Deposit Receipts were withdrawn, the tap issue mechanism was discontinued and in 1951 Bank rate was reactivated as an instrument of demand management.

Robbins remained a steadfast opponent of cheap money for the rest of his life. His autobiography gave an extreme characterisation of Dalton’s policy, portraying the “ultra-cheap money policy” as “disastrous” (Robbins, 1971, p. 211). In his 1981 Richard Ely Lecture to the American Economic Association, perhaps echoing his Essay, he reminded the economics profession of his long-held stance:

I should have thought that one of the main practical functions of economic science was to enable us to detect inconsistencies in plans, such as, for instance, simultaneous demands for low interest rates brought about by increases in the size of the credit base and a diminution of inflation. (Robbins, 1981, p. 2)
8. The International Monetary Environment

Robbins’s views of international financial architecture bring matters full circle. In his autobiography, he celebrated Keynes’s plan for an International Clearing Union (ICU):

For those of us who cherished the belief that the outcome of war might eventually permit an innovation of benevolent international institutions, it became as it were a banner of hope, an inspiration to the daily grind of war-time duties” (Robbins, 1971, p. 196).

But his other contributions suggest that he remained supportive of currency mechanisms that restricted the amount of international credit. O’Brien observed:

When this [the ICU] was rejected by the conference, he was disappointed, but he later became a strong supporter of the IMF system and accepted that Keynes’s plan would have been inflationary (Hansard, 29 March 1961, vols. 79–82). (O’Brien, 1988, p. 117)

Indeed, even in his 1947 Marshall Lecture, Robbins was looking back to a world of commodity standards:

And if I may admit to what may prove to be the pursuit of an entirely false scent, I will confess that at times I have felt very interested in the suggestions, thrown out by Messrs. Frank and Benjamin Graham, for stabilizing the price level of certain storable commodities by a device which is essentially an extension of Marshall’s symmetallism. If such a scheme were in fact administratively practicable on an international scale, I can see the possibilities of mitigating world booms and slumps which certainly would be very attractive. (Robbins, 1947, p. 73)

In Summer 1954, he presented a series of lectures on international financial policy at the Instituto Brazileiro de Economia in Rio de Janeiro, published as The Economist in the Twentieth Century. He re-asserted classical themes: any dollar shortage followed from

... disequilibrium in the balance of payments of non-dollar powers, the persistence of these influences is to be attributed in a very substantial measure to monetary influences operating on the same side – to the unwillingness of the deficit countries to curb their local inflations. (Robbins, 1954, p. 55)

For the control of inflation (which “ha[d] not been kept within desirable limits”, *ibid.*, p. 74), he was an early advocate of monetarist policies supporting fiscal policies. “The important thing is the control of the credit base – in other words, direct control of the quantity of money in the sense in which it is convenient to define it” (*ibid.*, p. 76, Robbins’s emphasis). And ultimately he did not abandon the Gold Standard:
The history of the Classical Gold Standard is punctuated by episodes of local breakdown; … Nevertheless, I venture to suggest that it is possible to exaggerate the difficulties and disadvantages. (ibid., p. 89)

He remained an opponent of free exchange rates as “the antithesis of a liberal economic arrangement” (O’Brien, 1988, p. 118).

9. Conclusion

There can be little doubt that Robbins’s position – or the position he lent support to – prevailed. Today, a sentence from his Essay defines economics and policy aims the discount rate at inflation. The European Union is a reality, with a monetarist stance and fiscal austerity. The Euro was forged in a shape similar to the gold standard; as Chick (1997) has pointed out, the ills of Britain’s flirtation with the Exchange Rate Mechanism were entirely predictable from the experience of the 1920s. All of these policies are based on theories presented as fact and not open to substantial challenge. All of them entirely discard what Keynes was trying to achieve and his theoretical conclusions about the operation of a free market economy. The intensity of the monetary policy debate through the 1920s-40s has been air-brushed out of economic and social history.

Paramount in this state of affairs has been the re-invention of Keynes as the great advocate of fiscal not monetary policies, even as rejecting any effectiveness of monetary policy. The conventional wisdom on Robbins is coherent with this fiction. Monetary policy is scarcely mentioned and the substance of the discussion concerns only his approach to state intervention given his generally undimmed support for liberalism.

My own pre-occupation is the recovery of Keynes as a monetary economist who was concerned primarily with monetary reform. The reconstruction of the nature of his relations with Robbins is very instructive. Robbins joined a long tradition of opposition to monetary reform. He asserted classical economics as fact, yet in practice, as with virtually the whole of the inter-war and post-war economics profession, he proved flexible over state intervention. He did not engage with Keynes on a theoretical level. Like Hayek, he did not rise to the technical challenge of the General Theory, but instead attacked it using political economy of a crude and surely highly contentious nature.

The General Theory led to certain practical conclusions, most fundamentally that nation states should aim monetary policy at low interest rates across the spectrum. Other policies should be subservient to this basic principle. Dear money was the cause of high unemployment and the economic cycle. If Keynes’s policy amounted to economic nationalism, then, so long as high and stable employment is the goal, so be it. If the theory too leads to a more restrictive trade policy, then likewise.

Robbins’s stance exemplifies the approach of the economics profession to Keynes’s theory: first to reject it out of hand and later to transform it into something they had the gall to call Keynesianism. In this way Keynes’s theory was simply set aside.
Robbins was one of many that built the alternative theory; and, despite his denials, seems highly likely to have been instrumental in its practical implementation in Britain through his wartime role. He then stood by as Keynes became identified with that ‘Keynesian’ theory and policy.

The post-war era, however, did have the virtue of relatively low interest rates. Policymakers may have quickly dismantled the recommendations of the NDE, but capital controls, permitted under the Bretton Woods agreement, did facilitate fairly cheap money and the golden age of economic activity. With financial liberalisation, effected between 1970 and 1980 (roughly), the world returned entirely to the doctrines of the gold standard world – to Robbins’s world. If my interpretation of economics is correct, this return has not been costless. Economic activity has been artificially restrained yet is increasingly exposed to the consequences of the waves of exuberance that are presently being described by Alan Greenspan.

I am grateful to the organisers of this conference for this opportunity to revive interest in these debates over monetary reform. I believe that the debt and capital-market inflations that characterise the world economy today are an indication of the highly dangerous course of not avoiding dear money “as we would hell-fire”. In the wake of recent credit market crises, tentative steps are being taken toward what I believe will become a fifth grand monetary discussion.
References


Hayek, Friedrich von (1931) *Prices and Production*, Routledge.


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Annex 1  The start of Monetary Reform

Keynes’s memorandum of 16 November 1931, ‘Notes on the Currency Question’, constituted a detailed discussion of exchange policy and formed the basis for the discussions of the sub-Committee on Financial Questions, which he joined on 26 November 1931. There were four lines of policy change, all shifting control of monetary policy to the Treasury from the Bank of England.

i. The Exchange Equalisation Account (EEA), which put into effect the currency management policies that Keynes had long advocated, was announced in the April 1932 Budget. Under its arrangements, the sterling exchange rate was to be managed by intervention in the foreign exchange markets rather than by manipulation of Bank rate. The arrangements involved a supplementary fund of £150,000,000.286

Over the next months the ‘Sterling Area’ came into existence, as country after country left the gold standard, adopted currency management arrangements and pegged their exchange rates to sterling. Even as early as June 1932, Richard Hopkins287 was observing how the ‘greater part of the world’ had ‘managed exchange currencies’.288

ii. Bank rate was increased sharply when Britain left gold, an operation perceived as necessary in the absence of the assurance of gold (Figure 1). The sub-committee took the opposite view and promoted a reduction in rates. On 18 February 1932, a cut of Bank rate to 5 from 6 per cent marked the start of the cheap-money policy and the start of Keynes’s material impact on economic policy. With the freedom of domestic monetary action permitted by the EEA, further Bank rate cuts followed rapidly. As Figure A1 shows, economic activity in the 1930s and 1940s was subsequently conducted at a Bank rate of 2 per cent.

286 Four per cent of 1931 GDP.
287 Richard V. N. Hopkins; at the time the Second Secretary to HM Treasury; Permanent Secretary to HM Treasury from 1942–45.
288 Source: The National Archives: Public Record Office (PRO) file T 175/157.
iii. As seen, Keynes’s developing theoretical perspective gave the greatest importance to the long-term rate of interest. In July 1932, the authorities’ faith in this notion was indicated by the conversion of the War Loan from 5 to 3½ per cent. The operation was announced in the House of Commons on 30 June and was accompanied by the final cut of Bank rate to 2 per cent and by the introduction of an embargo on overseas loans (see below). Keynes published an analysis of the operation in the September 1932 *Economic Journal*.

A reduction of the long-term rate of interest to a low level is probably the most necessary of all measures if we are to escape from the slump and secure a lasting revival of enterprise. The successful conversion of the War Loan to a 31/2 per cent basis is, therefore, a constructive measure of the very first rate importance. For it represents a direct attack upon the long-term rate, much more effective in present circumstances than the indirect attack of cheap short-term money, useful and necessary though the latter is. (*CW* XXI, p. 114)

He went on to recommend other measures aimed at ensuring that the effect of the conversion was not just a ‘flash in the pan’. In particular, he argued for the extension of the range of securities that the Government issued: ‘It is important that the market should be supplied with securities of different types and maturities in the proportions in which it prefers them’ (*CW* XXI, p. 115); specifically he advocated the issue of ‘… a shortdated bond at a lower rate of interest than 31/2 per cent some time in the autumn or winter’ (*CW* XXI, p. 116).
iv. Lastly, the manoeuvres on long-term rates of interest were supported by what was known at the time as the ‘embargo on overseas loans’. This capital control policy remained in force for roughly the next 50 years. The issue of a low interest rate long-term bond in Britain was aided by preventing British investors purchasing higher-rate instruments issued by other countries.

Over the next years much of the world was taking a similar approach to monetary policy: cheap money, supported by exchange management and capital control. In October 1936, Britain, the United States and France signed a ‘Tripartite Agreement’, committing mutual exchange support if necessary. The main exceptions to this approach were the totalitarian economies.
Annex 2  Debt management policy in W.W.II

The following monetary and debt management mechanisms operated during W.W.II:

the ‘tap issue’ policy for government debt sales and an extension of the range of securities on offer. Rates of interest and maturities were announced but no limits were set to the cash amount of any issue. The ‘tap’ of any bond was held open so that individuals and institutions could purchase when and whatever quantities they desired (a notice read ‘subscriptions will be received on Tuesday, 25th June, 1940, and thereafter until further notice …’). The technique was first introduced for the June 1940 wartime issue of 2 ½ per cent medium-term bonds (known as National War Bonds) and then for the subsequent issue of 3 per cent long-term bonds (known as Savings Bonds);

a change in the approach to the floating debt, including an extension of the instruments on offer. Setting long-term rates of interest meant accepting a large increase in the volume of floating debt (see Tily, 2006). Treasury Deposit Receipts (TDRs) were introduced, which had a six-month maturity and slightly higher interest rate than Treasury bills. These were not reservable against cash at the central bank, to help ensure that the increase in floating debt did not lead to excessive credit creation;

Bank rate remaining unused as an instrument of day-to-day monetary policy; and

from the perspective of the international environment, domestic monetary policy was supported by the ongoing embargo on overseas loans and full exchange control.
Robbins’s Epistemology and the Role of the Economist in Society

Fabio Masini*

Abstract

When Robbins’s Essay on the Nature and Significance of Economic Science was first published, it was mostly criticized for depriving economists of an important professional market (policy advice) and for detaching economics from policy considerations. Most critics considered Robbins’s Essay as the manifesto of a mechanical, “ivory-tower” concept of economics.

In fact, if we read through the lines of Robbins writings and Autobiography, we can see that such an accusation was misdirected. Economics is definitely an autonomous (from ethics and politics) science as a set of knowledge-methodology but it has a high-rank position for policy action. For him, economics is the tool box indispensable for responsible policy action.

The aim of the paper is to enquire into Robbins’s concept about the relationship between economics and politics and to underline how and why his message was misunderstood in economic literature. If we read the Essay beyond its crude language and as a step within a wider, longer, more complex but absolutely “time consistent” whole of considerations developed by Robbins from the Twenties until the Sixties, a completely different judgement emerges.

The paper goes back to earlier and further contributions to enquire into Robbins’s concept of the role of the economist in society and policy action, in the attempt to contribute to a more general reconsideration of the historiographical assessment of Robbins’s work.


1. Introduction.

When reviewing the second edition of An Essay on the Nature and Significance of Economic Science by Lionel Robbins (1935), Frank Knight (1936: 425) commented: “The sort of economic science which Professor Robbins advocates has no relation to the prediction or control of any concrete phenomena”, implying a critique of detachment from reality and from any policy consideration.

As it is known (Howson 2004: 416), Robbins started his studies at the London School of Economics in the early Twenties with the intention to specialize in the history of political ideas. Only a couple of years later he decided to abandon it as “it seemed so futile to go on studying it. […] economics seems more fruitful in practical results and

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capable of yielding greater intellectual satisfaction”\textsuperscript{289}. And the stress on “practical results”, as we shall see, would be restated in many others of his writings.

Several different interpretations may be attempted on this apparent contradiction. For example, Robbins might have completely changed his mind on the true nature of economics during the next years or learned how “it really was” after actively dedicating to it. Or Knight might have misinterpreted the book.

The economic literature and the historiography on Robbins have unanimously credited the first one: whatever he might have thought at twenty-five-years-old, ten years later Robbins should be considered the champion of a “technical”, ivory tower\textsuperscript{290} approach to economic science, detached from reality and from any involvement in social and political choices. The \textit{Essay} was considered the manifesto of a concept of the economist as a mere “engineer” of maximization procedures\textsuperscript{291}, and of neoclassical economics\textsuperscript{292}.

To reinforce this idea, Daly (1940) pretends Robbins has considered economic laws in analogy with mathematical and physical ones; some have pointed at his intransigent deductivism as if he had rejected inductive methods in economics\textsuperscript{293}, others (Fraser 1932: 563, Parsons 1934: 513, Knight 1934b: 227) have even hinted at him as a supporter of the \textit{homo oeconomicus}\textsuperscript{294}.

For these many remarks and “labels” he was probably ideologically rejected in many countries\textsuperscript{295}. But mainly it was because Robbins seemed to be against the stream of history. The \textit{Essay} was in fact written in the very moment when economists were required to give policy advice on public action from governments and public opinion, in the very heart of the doom years which followed the 1929 crisis.

As Joan Robinson wrote in (1972: 1) about Robbins’s (1932) \textit{Essay} and its definition of economics: “No doubt this was the expression of a long tradition but the date of publication was unlucky. By the time the book came out there were three million workers unemployed in Great Britain […] It was just a coincidence that the book appeared when means for any end at all had rarely been less scarce”.

\textsuperscript{289} Robbins to Iris Gardiner, 24 June 1924. In a previous letter to his future wife (10 April 1924) he confessed that he just “immersed himself in trade cycle literature, which he found ‘intensely interesting’ as well as of immediate practical relevance” (Howson 2004: 418-9)
\textsuperscript{290} Fraser (1932: 570) accuses Robbins not to consider that “the end of knowledge is action”.
\textsuperscript{291} Among the early critics we find Cannan (1932), Fraser (1932), Souter (1933), Knight (1934a, b). But still in later years we can cite at least Harrod (1938) and Buchanan (1979).
\textsuperscript{292} In this respect see Peck (1936: 496) and more recently Addleson (1984) and Giocoli (2003: 94). We are obviously aware of the enormous literature on the “true” meaning of “neoclassical economics” and this will be partly discussed later. On such a delicate question see, just to indicate some of the most interesting among recent contributions, Colander (2000), Hands (2001), Davis (2002).
\textsuperscript{293} Robbins in 1959 gave a conference at the University of Rome which was enthusiastically defined as a clear detachment from his “deductivistic” 1932 methodology and Talamona (1960) welcomed it as his “landing” on “positive economics”.
\textsuperscript{294} For an extensive and critical review of economists’ approaches to the \textit{homo oeconomicus}, see Machlup (1972).
\textsuperscript{295} Italy, for example, but this would deserve further attention.
At that time it was economists themselves that felt impatient to give some practical relevance to their work: while the Essay was being written, the first concrete steps were made by the Econometric Society which, as Schumpeter (1933: 10) wrote, was meant to gather the “most of us [who] undoubtedly do agree in finding the present state of our science disappointing […] in comparison with what our science could fairly be expected to perform”.

Furthermore, as Macrosty (1934: 343) noted, with his style and language in the Essay Robbins “runs the risk of being suspected of immaturity and damages his case by evoking irritation” and to “cause the reader to close the book prematurely”. Even those who did not close it might develop a violent defensive attitude against Robbins’s impolite arguments in a book that Knight (1934b: 225) labelled in the American Economic Review as “presumptuous”. This roughness, together with plenty of Austrian citations “scattered throughout the text” (Howson 2004: 441) just before publication probably did a good financial favour to the author (Robbins 1971: 147) but a poor job for his arguments.

The most astonishing aspect is that Robbins’s biography is itself a testimony of civil and political action. He was a man of action, not only academically as a vivid guide of the London School of Economics. He was a member of some Secret Committees for the Economic Advisory Council of the British Government; he was later appointed Head of the Economic Section of the War Cabinet; he was among the founders of the association Federal Union which gathered prominent intellectuals to promote the idea of a federal Union of Britain and France during the Second World War (Chaning Pearce 1940; Mayne, Pinder, Roberts 1990; Ransome 1991). After the war he was an outstanding figure of British institutional and political life. Also Robbins’s scientific production is a demonstration of his engagement, as an economist, in questions of political economy, economic policy and public management. It is true that this all came later, but did he change so much, or did he suffer from schizophrenia, from “split personality”? Are we sure there is no thread that coherently leads from his writings, also his early ones, to his personal life?

The answers to these questions need the 1932 Essay to be read more carefully and with a wider historical perspective. If Robbins’s epistemology is considered, looking at the Essay as only a step in a more general discourse which occupied all his scientific and public life, a different vision will result from the common one.

Robbins was not an intransigent deductivist, he rejected the idea of a rational, maximizing homo oeconomicus and was certainly against any determinism. But mainly, he was not at all for an “ivory tower” concept of economics, far from reality...
and isolated from the other social sciences. Quite the contrary. Our point is that Robbins, on the question of the role of the economist in society, has been absolutely “time consistent”. He always conceived economics as a technical knowledge indispensable for assisting policy choices, long before he wrote the Essay in 1932 and until he died. No major changes have influenced his epistemological attitude towards economics, especially on this aspect. Simply, his message was not properly understood during his life and was certainly completely misunderstood immediately after the publication of the Essay.

This is not the first time this thesis is being expressed. Thirty-five years ago, in the Preface to a collected volume of Essays in Honour of Lord Robbins, Peston and Corry (1972: vii) wrote, referring to the Essay, that “the purpose of that book is misunderstood as an ‘attack’ on economists indulging in political economy. Far from this being the case, as Lionel Robbins’s own contributions demonstrate, it is a clarification of the conditions under which economic science relates to political economy”. Robbins (1981) himself, apart from the Autobiography, returned to his attitude towards political economy, politics and the role of the economist in society just three years before dying. And, on the same questions, an interesting debate took place in The Economic Journal in 1988 and 1989 between O’Brien and Wright. But, apart from some interesting hints, confined to only some lines, an attempt to assemble a coherent puzzle on Robbins’s contribution to the relationship between economics and politics has not yet been made. The aim of this paper is twofold: to look for an explanation to the misunderstanding we have underlined and to provide an alternative interpretation of Robbins’s apparent ambiguities and of his attitude towards the relation between economics, ethics and politics.

In order to illustrate this, we will go back to illustrate the origins of Robbins’s interest in epistemological questions concerning economics and have a look at the main elements put forward in the Essay. We will then look through the review articles and essays written as a direct reply to Robbins’s Essay between the first and the second edition, underlining the difficulties to properly grasp Robbins’s attitude towards the role of economists in society and verify to what extent Robbins tried to clarify his views in the second (1935) edition.

We will finally follow Robbins’s epistemological ideas from the (1937a) book on National Planning and International Order to a few later contributions, before attempting some conclusions on the path followed and the material found.

2. A Never-Ending Story: the Boundaries of Economic Science

In the various editions of Principles, Marshall, engaged in the long and violent public quarrel with Cunningham, invited economists to avoid wasting time on the boundaries of economic science but concentrate on saying clever things. Such an appeal was hardly bound to exert any influence if from the first edition of Marshall’s Principles in 1890 to 1932, when the first edition of Robbins’s Essay was issued, several contributions were published in Britain on methodology and the epistemic foundations of economic science.300

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300 We should recall at least the following: Keynes (1890), Wicksteed (1910, 1914), Cannan (1914, 1927), Pigou (1920). In the list, we should also include Cassel (1925) as he exerted some important
Susan Howson (2004) has attempted a detailed reconstruction of the genesis of the 1932 work. She tried to demonstrate, as Hutchison (1981) and O’Brien (1988b) had already suggested, how the common idea (which emerges from the early writings reacting to it\(^{301}\)) of the *Essay* as a tribute paid to Austrian economics\(^{302}\), and mainly due to his meeting with Hayek, is misleading.

Although this judgement is to be better specified, the major contents of the *Essay* were undoubtedly in Robbins’s mind before he met Hayek in January 1931 and, in many respects, even before he came across Austrian literature\(^{303}\).

It is true that Robbins’s first contributions to economic literature were apparently far away\(^{304}\) from the themes of the *Essay* as they meant to challenge “the appropriateness of the assumption of the ‘representative firm’, ambiguities in the conception of stationary equilibrium and the like” (Robbins 1971: 145). But he also writes that the idea of the *Essay* came as he arrived at the *London School of Economics* (Robbins 1971, 146); and this is a quite controversial time reference. Robbins was in fact a student at the LSE from the end of 1920, then Lecturer there from 1925 to 1927 and only in 1930 he became Professor of Economics. As there is no exact reference in Robbins’s *Autobiography*, we have to guess which date he had in mind.

We should remember that the first lesson he attended at the LSE on 8 October 1920 by Hugh Dalton was on the definition of economics. In that lesson Dalton criticized Cannan’s definition (“The aim of economics is the explanation of the general causes on what the material welfare of human beings depend”, Howson 2004: 416) as too much linked to material welfare, a criticism Robbins was definitely bound to make his own and reinforce (even though only after 1926).

Dalton also cited Marshall and Clay; Robbins again seemed (Howson 2004: 416) to be sceptical towards their insistence on “ordinary business of life” (Marshall) or “business in its social aspect” (Clay). It is presumably not in those years when he began to think of the book, but this shows that he very soon started wondering about the epistemology of economic science. This interest might be attributed to several elements.

The first is a “personal” one. Having crossed the line from political science to economics he probably needed a deeper reflection on the different contents of both fields and a stronger consciousness supporting his choice\(^{305}\). Thirty years later, in a

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\(^{301}\) See in particular Souter (1933: 377): “Professor Robbins’s recent book under the above title is important, not merely because it provides English and American students with an able, scholarly and succinct account of the main tenets of the ‘Austrian School’ (it is Professor Robbins’s _credo_ as an adherent to that school)…”.

\(^{302}\) Even recently, Addleson (1984) gave new impetus on these early opinions, although with a different perspective on the exact meaning of “Austrian economics”.

\(^{303}\) This was between 1925 and 1927 but apparently only Mises (Howson 2004: 422).

\(^{304}\) When one looks at them with a careful eye, they appear to be very close to such themes (Robbins 1926; 1930a, b).

\(^{305}\) On these questions he profited from life-long epistemological debates with his friend Isaacs, member of the Aristotelian Society.
letter to John Maurice Clark dated 1 March 1951, Robbins wrote: “that I eventually
crossed over and made Economics my chief interest was due directly to the fact that I
felt it threw light on problems of politics I had been studying from the other side […]
*The Nature and Significance* was always intended to be a sort of preliminary
manifesto designed to forestall the criticism that I did not know where the borderline
between the different disciplines really lay”.

But this *personal* explanation of the interest towards the topic of the boundaries of
economic science is only part of the reason why he came to write the *Essay*. We
should not forget Wright’s (1989: 472) judgement, reminding us that in 1930 Robbins
entered the *Economists Committee* set up by Ramsey MacDonald “and it was in part
in response to his experience of its activities and the resulting policy
recommendations that his two most important works of this period were written:
first, and probably his most lasting contribution to economic scholarship, *An Essay
years later by *The Great Depression*”.

And probably there is a third reason for this interest. Later on, while writing the
*Essay*, he *de facto* took up the responsibility of the economics department at the
London School of Economics, and his *personal* problem was to become an *academic*
one: he felt the need to assert and defend a division of labour where all disciplines
have their peculiar role. Economics should therefore be underlined as a scientific
coherent system detachable from other research fields.

But academic strategy and public scientific motives should not be confused with a
refusal to consider the economist as part of a social body, with his peculiar role.
74) statement that “a person is not likely to be a good economist who is nothing else”,
as he himself recognized.

And he certainly had well in mind Marshall’s (1890: 73 ff) words against Comte’s
 presumeion that economics should not have a special role in social sciences306.
Marshall’s criticism of Comte was on the basis of complexity, of a sort of *ceteris
paribus* clause applied to scientific knowledge: “the whole range of man’s actions in
a society is too wide and too various to be analysed and explained by a single
intellectual effort” (1890: 73).

And on similar lines had been Wicksteed, who manifestly had a major influence on
Robbins (Howson 2004: 422). Wicksteed’s “Common Sense” suggested that
economics should be realistic, which mainly meant “complex” to him. According to
Comin (2004: 479), Wicksteed maintained that:

306 Marshall (1890: 72) writes: “The growing prominence of what has been called the biological view
of the sciences has tended to throw the notions of economic law and measurement into the background;
as though such notions were too hard and rigid to be applied to the living and ever-changing economic
organism. But biology itself teaches us that the vertebrate organisms are the most highly developed.
The modern economic organism is vertebrate; and the science which deals with it should not be
invertebrate. It should have that delicacy and sensitiveness of touch which are required for enabling it
to adapt itself closely to the real phenomena of the world; but none the less must it have a firm
backbone of careful reasoning and analysis”.
“economists should “take [man] as we find him”, examining individuals in all their complexities with all their characteristics, as we understand them complexities and characteristics) in ordinary life. Thus, economics should reject the narrow simplification of economic man and work with a comprehensive concept of rationality. Robbins (1933, xxi) has suggested that the conception of economic man was “shattered [...] once and for all” by Wicksteed because he rejected the egocentric or hedonistic aspects present in earlier formulations of this concept. Wicksteed criticized the attribution of egocentric or hedonistic qualities to economic man, but those qualities were not the only ones he criticized.

Also on the praxeological side, Robbins takes much out of Wicksteed:

“economists should extend the scope of economics, regarding it “not as a separate and detached region of activity, but as an organic part of our whole personal and social life” (3). According to Wicksteed, there are no special laws governing economic life and no particular motive behind economic actions. His objection to the term economic motive is that “it easily suggests a deliberately selected end or goal” (167), most often the wealth motive. This is a statement he refused to accept because he thought that “to regard the ‘economic’ man . . . as actuated solely by the desire to possess wealth is to think of him as only desiring to collect tools and never desiring to do or to make anything with them” (163).

This detachment from an objective, measurable “wealth” brought Robbins to replace its central, explanatory role with “choice”, derived certainly from the Austrian School - Menger in particular, as Robbins (1981: 2) himself recognizes - but also from Aristotle307, Hume308 and Wicksteed again. And from the British classical tradition he derived the question of demarcation between economics and moral sciences. This can be seen in the review article he wrote in 1927 to Hawtrey’s *The Economic Problem* published the year before. In the review, Robbins tries to defend the classical economists from Hawtrey’s critique on the very question of the boundaries of economic science. As he puts it: “In the past, economists have generally agreed that ethical criticism was not part of their business as economists”309. Hawtrey (1926: 184), on the contrary, pretends that “economics cannot be dissociated from ethics”.

In order to cut the difference between the economic and the ethical problems, Robbins (1927: 177) uses a funny paradox310:

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307 As Comin (2004: 476) writes: “Aristotle’s view of wealth as something instrumental. Wealth, he argued, ‘is evidently not the good we are seeking; for it is merely useful and for the sake of something else’ (367). The view of wealth as a tool, so important to Wicksteed’s argument on ‘economic nexus’, altruism and ‘economic relations’ is an intrinsic part of Aristotle’s view that practical reason without moral excellence is not possible. Choice, the origin of action for Aristotle, ‘cannot exist either without thought and intellect or without a moral state; for good action and its opposite cannot exist without a combination of intellect and character’ (418).”

308 It is again Robbins (1981: 2) himself underlining the contribution of “David Hume’s *Treatise of Human Nature* (pp. 261-62)” to his central idea of choice to the economic discourse.

309 Robbins (1927: 174). Please notice that as economists is in italics in the original.

310 The same paradox, with little differences, will also be used by Robbins in the *Essay* (1932: 134-5; 1935: 151), but Confucius, Mohammed and the Maya designer seem to have managed to escape the room in the meanwhile, substituted by Bentham and the Head of the United States Steel Corporation. In this second case the disagreement is supposed to emerge on “the ethics of usury”.

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“Shut Mr. Hawtrey into a room together with Lenin, Confucius, Mohammed, Buddha, and the designer of the Maya altar pieces […] No doubt they would agree that the good, the true, and the beautiful were everywhere desirable. But when they came to define these terms, a difference of opinion would be probable. On the other hand ask them to produce a report on the causes of change in the rate of interest, and it is possible that Mr. Hawtrey might succeed in eliciting no inconsiderable unanimity – Lenin perhaps dissenting”.

Through these lines, one can catch the role of the economist in relation to ethics and to policy action: he is the privileged advisor on technical questions which are relevant for making also political choices with full awareness of their consequences, as may be clearly understood from this passage that we think important to quote at full length.

“This is not to say that economists should not deliver themselves on ethical or normative questions, any more than an argument that botany is not concerned with aesthetics is to say that botanists should not have views of their own on the laying-out of gardens. Whatever may be our philosophical views as to the possibility of scientific ethics, as social beings we have to assume some such conception, and there is no reason why economists alone among their fellows should deny themselves the liberty of dogmatising. On the contrary, it is highly desirable that they should not do so. If we are to judge by results - and there could be no better example than this book of Mr. Hawtrey’s - there is apparently something in the study of economics which is peculiarly conducive to the formation of what men of the Anglo-Saxon tradition regard as right judgment in these matters. It is no accident that most of the great economists have also, as we conceive it, lived greatly—today what leadership in ethical opinion we do not find revolting to our sceptical tempers comes from men who have devoted time to the study of economic problems. In spite of the sneers of modern sciolists and the futile bellowings of soi-disant Victorian prophets, economics holds its own as a liberal education with any intellectual discipline. An understanding of the objective limitations of social action is no bad preliminary to a just appreciation of the possibilities of social development. All this is common ground between Mr. Hawtrey and the rest of us. All that I am pleading for here is that we should preserve that separation of science from what at best must remain pure opinion, which has emerged so hardly from the irrationality of the pre-scientific era. By all means let us be willing to spill our opinions on the public. By all means let us try to make our categorical imperative the categorical imperative”.

But for the repute of that little area of knowledge which we can fence off from the wilderness of velleity and dogma, do not let us pretend to be talking economic science. Some day perhaps we may persuade the world that we understand those phenomena we call economic. Let us beware lest
we jeopardise even this title to respect by claiming the same sanctions for judgments of value” (Robbins 1927: 177-178).

This is definitely one the most illuminating passages Robbins has ever written on his conception of the role of economists in society. They belong to a professional corporation that shares some tools of analysis which prove to be particularly suitable and helpful for understanding everyday processes. As citizens, that is as part of a wider community facing collective choices, economists have the opportunity (and maybe the duty) to use such technical knowledge when expressing value judgements.

Far from weakening the role of the economist in society, these arguments set the proper framework for gaining credibility as policy advisors. Only when it is “purified” from opinions, economics can be a scientific and credible domain of reflection at the service of policy action. These elements will be restated in the 1932 text, although within a wider and probably more ambiguous framework.

3. The Essay, from the First to the Second Edition

For the purpose of our work, the manifold and complex Essay will be regarded only by considering briefly four main aspects: positivism; atomism; rationalism and neutrality towards ends. The ivory-tower concept of economics attributed to Robbins would in fact be coherent with a social vision where individual sovereignty degenerates, at the aggregate level, in a pure atomistic collection of preferences, where man is an impersonal maximizing agent acting on purely rational calculus, where no one is entitled to express value judgements on each others behaviour. But this is not Robbins’s social vision. It is only a caricature of it.

The most famous part of the Essay is the definition of economics as the: “science which studies human behaviour as a relationship between ends and scarce means which have alternative uses” (1932: 15). But he then adds:

“the subject-matter of Economics is essentially a relationship – a relationship between ends conceived as tendencies to conduct, on the one hand, and the technical and social environment on the other. Ends as such do not form part of this environment. Nor does the technical and social environment. It is the relationship between these things and not the things in themselves which are important for the economist” (1932: 37)

And this is an ambiguous message. It gives rise to an idea of immobile ends and means, suggesting a positivist approach.

But the very stress on “choice” as a distinguishing character of economic phenomena should have dissipated any accusation of positivism, if with this word we mean “the tendency to emphasize the importance of the methods and general doctrines of the physical sciences” (Parsons 1934: 512).

Actually Robbins meant to distinguish economics as a science from a mere technique. We can discuss if the reference to Mayer was the most appropriate one: “To use Professor’s Mayer very elegant way of putting the distinction, the problem of technique arises when there is one end and a multiplicity of means, the problem of
economy when both ends and means are multiple” (1932: 34-35). But definitely Robins did not conceive of economics as a pure logical set of relations.

Physical sciences have no open choice; and their scholars try to find the “true” laws able to explain the mechanical and deterministic relationship among physical variables (Robbins 1932: 83). Economics is founded on human beings, each with his almost infinite needs, scarce means and different choices of satisfaction. The accusations of determinism should melt down in front of Robbins’s idea of individuals as perfectly sovereign in the choice process: “the [economic] problem is one of adjustment to the situation that is given” (1932: 52), in its time evolution. The economic problem is one of continuous adjustment to ever-changing conditions, not a mechanical, deterministic game. Robbins’s very critical attitude towards empiricism is that it pretends to extract general laws, always valid irrespective of a continuously changing environment. Historical data series might be useful to understand the past but certainly not to predict the future. At the same time, individuals cannot be said to be “rational” agents in the sense of maximizing utility function, although they should be considered “rational” for the capacity to adapt to changing environmental conditions: “conduct is the resultant of conflicting psychological pulls acting within an environment of given material and technical possibilities” (1932: 34)

But the main critical point to sustain the “castration” problem was the idea of “neutrality towards ends”. When Robbins says economics is independent of ends he may suggest he means that ends are unimportant to economics. But even on this point Robbins attempts a clarification. For him, ends first of all modify the economic discourse, as they influence the very attribute of scarcity. In an illuminating example that later was to be criticized by Cannan, Robbins explains that when Britain signed the Armistice that ended the First World War, what five minutes before was to be considered wealth, five minutes after was “not-wealth, an embarrassment, a source of social waste” (1932: 47) “The ends had changed. The scarcity of means was different” (1932: 47).

You may of course maintain, as Costabile (2007) has recently done, that Robbins’s arguments of the neutrality of economics towards ends is in itself a circular and antinomious reasoning as it implies a specific objective, that is the absolute sovereignty of the single agent facing his personal ends. But again this is not what Robbins meant to say, as is better shown by his contributions in 1937 and 1939.

The stress on the importance of individual sovereignty in the process of choice can be (and indeed has been) read as a behaviouristic approach (where each individual is not tolerant towards others’ choices; Parsons 1934, 512) and even the result of an atomistic concept of society where, therefore, no collective action is legitimate and a social body can only be conceived of merely as the sum of individual pulses. The decision to use the example of Robinson Crusoe might again induce misinterpretation. As Knight critically observed: “a society made up of such individuals is essentially an aggregation of Crusoes, each on a separate economic island”.

311 Or “atomistic individualism”, as Peck (1936: 496) has put it.
But Robinson-Crusoe-economics is for Robbins just a first-step “expository device”, exactly as the *homo oeconomicus* is “a first approximation used very cautiously at one stage in the development of arguments which, in their full development, neither employ any such assumption nor demand it in any way for the justification of the procedure” (1932: 90).

Actually, Robbins is even more critical towards the Crusoe economics for it helps him to criticize Cannan’s foundation of economics on wealth. Such a foundation, observes Robbins, applies to “the activities of a man isolated completely from society and enquiring what conditions will determine his wealth – that is to say his material welfare” but cannot be valid when social interaction takes places.

Actually Robbins, far from “individualistic atomism”, and anticipating the wider and deeper reflections of *Economic Planning and International Order* (1937a), underlines how the society needs a set of constitutional rules governing individual freedom:

“[…] it is clearly necessary to assume a social order within which the valuations based upon it may show themselves in tendencies to action. […] We assume a legal framework of economic activity. This framework, as it were, limits by exclusion the area within which the valuations of the economic subject may influence their action. It prescribes a region in which one is not free to adopt all possible expedients” (1932: 93).

The major question becomes, then, whether economists *as such* should contribute to the definition of such a framework. But Robbins did not neatly recognize the centrality of this problem until later.

Anyway, whatever was the stage of Robbins’s reflections, the importance of the topic and the way he laid it down on paper inevitably determined that the *Essay* could not help being felt of like a stone in the water. It immediately received very vivid review articles312 and in the next years it was to generate a huge debate on the epistemological foundations of economics (and even of other sciences).

The first reaction to the publication of Robbins’s *Essay* was by his betrayed master313. In September 1932 Edwin Cannan wrote a sharp three pages review article on the *Economic Journal* violently attacking Robbins’s work. Cannan could not bear the abandonment of a central role for “material wealth”. And he could not stand Robbins’s attitude towards the concept of “gigantic machine” that for Cannan represented the juridical, social, institutional superstructure within which economic relations take place. The economic system is part of this *machine* and influences are


313 Cannan was certainly betrayed as concerns the epistemology of economics but definitely not on other important aspects of Robbins’s future scientific production, which can be considered a further enquiry into pioneering contributions by his master, as is the case of *National Planning and International Order*, largely based on Cannan’s (1916), *International Anarchy from an Economic Point of View* and other later writings, as we shall see further.
reciprocal. It is therefore impossible to study economic relations without studying the
structure and functioning of the machine. According to Cannan, the role of the
economist was to study the structure and operation of the economic system and to
give hints to improve it. Robbins’s reasoning seemed to take economics far from this
ethical and civil commitment to “progress”.

A more subtle criticism the Essay received was that it deprived economists from an
important market: policy advices. Lindley Fraser concluded his long, critical review
article titled How do we want economists to behave? and published in The Economic
Journal in December 1932, writing: “Let us hope that economists in general will not
forget that in the social studies the end of knowledge is action” (Fraser 1932: 570).

Robbins (1971: 147) felt he was accused of “a sort of a general castration of the
subject”. And this was an accusation that hurt him much more than any other. He
collected all the material published on his Essay after its publication and decided he
had better clarify some points. Three years later the second edition was ready for
publication.

The critiques by Fraser had touched Robbins’s sensitiveness so much that the most
evident difference between the two editions (1932) and (1935) is “Chapter V” on
Economic Generalizations and Reality. Robbins opens it writing:

“It is a characteristic of scientific generalizations that they refer to reality. Whether they are cast in a hypothetical or categorical form, they are
distinguished from the propositions of pure logic and mathematics by the
fact that in some sense their reference is to that which exists, or that
which may exist, rather than to purely formal relations.” (1935: 104)

And on the following page he adds: “it is a complete mistake to regard the economist,
whatever his degree of purity, as concerned merely with pure deduction. […] The
concern of the economist is the interpretation of reality.” (1935: 105)

But, though much emphasis had been added on this point, his defence was not more
convincing than in the previous edition. Again, his lack of clearness brought Robbins
(1971: 148) to later reaffirm: “I definitely stated that it is only if one knows how the
machine runs or can run that one is entitled to say how it ought to run”. Only
economists can provide the necessary analysis tools for making policy choices. But
this still has to be better explained.

4. Economics and Politics in Later Robbins’s Writings

In order to understand this properly, it is important to follow Robbins’s reflections in
his later writings, although it would be probably enough to read the Preface and

314 Robbins (1971: 149) later commented on his attempt to change that Chapter saying that “in its
original form the chapter on the nature of economic generalization smacked too much of what
nowadays is called essentialism. That is to say, it seemed to suggest that the sanction of economic
generalizations lay in the conceptions from which they were deduced and these conceptions in some
ultimate way were part of the nature of the things […] In the second edition I tried to put this right and
to make it clear that the ultimate assumptions were elementary facts of experience whose
appropriateness was always subject to testing by reference back to reality”.  

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Introduction to Economic Planning and International Order. As Robbins observes (1971: 150), the 1937 book (which was actually written in the summer of 1935, as it collected some Lectures he gave at the Institut des Hautes Études Internationales in Geneva) was exactly the concrete translation of his own social vision.

In his Autobiography, Robbins (1971: 150) recalls how he underlined that the book was “essentially an essay in what may be called Political Economy, as distinct from Economics in the stricter sense of the word” (Robbins 1937a: vii).

In that book, in fact, Robbins makes use of the “technical apparatus of analytical Economics; but it applies this apparatus to the examination of schemes for the realization of aims whose formulation lies outside Economics; and it does not abstain from appeal to the probabilities of political practice when such an appeal has seemed relevant.” (Robbins 1937a: vii-viii). With that contribution, he tried to demonstrate also that political economy is within the domain of the economist’s role.

Economics is the founding basis of any political discourse, it is the shared language and knowledge which are required to express the intimate relationship between variables upon which political confrontation is possible with full awareness of ideological implications.

The book shows Robbins evolution on what were the important aspects which remained open in his reasoning and was meant therefore as a direct reply to those who accused him of social atomism, in the sense of lacking the idea of a collective body with its own problems of (collective) choice. His reply is based on an interesting concept of planning. Robbins writes: “To plan is to act with purpose, to choose; and choice is the essence of economic activity”. Planning here has the meaning of a path towards the achievement of particular goals: “The essence of a plan is that it is an attempt to shape means to ends” (1937a: 224). And each individual has its goals to pursue. But coordination problems may arise from the fact that choice implies the use of scarce means: “as soon as there is more than one individual planning, the different plans may not harmonize” (1937a: 4).

We would expect that the liberal Robbins would reassure the reader on the capability of spontaneous market relations to provide the necessary incentives and signals for such a compromise among individuals to emerge. But Robbins denies that markets alone have such a potential: “the result of our separate planning may be disorder and chaos. To avoid this, to secure that social relations involve a greater realization of individual plans rather than their mutual frustration, a co-ordinating apparatus, a social order, a social plan is necessary”. Like Hume, Robbins’s idea of society is one where homo homini lupus and where some sort of Leviathan is needed, “a co-ordinating apparatus with coercive power” (5). The Leviathan is embodied by a set of institutions: “For co-operation to be effective it must be restrained within suitable limits by a framework of institutions” (227).

The idea of a social order with coercive power implies a constitutional framework where market relations and individual plans are ruled. This also implies that a social collective identity exists and must be guaranteed together with (we might even dare
to say above) the individual ones. The federal\textsuperscript{315} structure guarantees that each governmental level has its independence and concurrent juridical powers over each branch for which it is responsible.

But society is not a mere aggregation of individuals. It is a complex system of interconnected and concentric groups\textsuperscript{316} (from single individuals to the world as a whole), each with his needs to be satisfied and collective choices to make and ruled according to a principle we would now call “subsidiarity”\textsuperscript{317}. In order to be sure that such independent centres of choices may not degenerate, a precise political arrangement is necessary, thus implying that economics (based on choices) and politics (based on the co-ordination of choices plans) are interconnected:

“The sentiment of public service may be best evoked by institutions which are most conducive to human goods. To find the ultimate goods of life in particular forms of political machinery, regardless of the suitability of that machinery to promote human happiness, is surely a delusion – a confusion of ends and means, of mechanism and purpose” (1937a: 325)

The proposal of a constitutional framework based on a federal structure as the founding social contract in a multilevel world, where nation states, just like single individuals, lose their exclusive sovereignty and legitimation to make collective choices, will be restated in the next couple of years (Robbins 1937b; 1939a, b)

But Robbins must have still felt frustrated by his incapacity to make himself understood on a question that he still considered of the utmost importance. In 1938 he tries again to clarify some points, stressing the relevance for economists to keep an eye on reality. In Live and Dead Issues in the Methodology of Economics he writes: “That the object of economics is to understand reality is not a proposition which is likely to be questioned by any economist” (Robbins 1938: 342) and he quotes some passages from the V Chapter of the second edition of the Essay. And again: “the economist who thinks that his subject is capable of being used for policy without further appeal to social philosophy is cherishing a naïve conclusion” (1938: 345). He therefore seems to have slightly changed his opinion.

But, in fact, he is merely contrasting the accusations of abstractness, determinism and social atomism. Furthermore, from the day he arrived at the London School and 1938 much had changed in the economics profession, from the academic point of view. If ten years before the top priority strategy was still to make some room for chairs in an autonomous subject-file called “economics”, now it was starting to expand it through internal specialization: “In a subject so wide as economics it is natural that there should develop some division of labour, that some should specialize on the more theoretical developments, some on description and verification”. For this reason he seems to be more indulgent towards a more catholic method: “we need both induction and deduction, observation and theoretical system; and that the important thing is to suit the method to the job.” (1938: 352)

\textsuperscript{315} On the role of federalism for Robbins (and vice versa, i.e. the contribution Robbins gave to the federalist thought) see Masini (1994).

\textsuperscript{316} Robbins derives many of his arguments by Edwin Cannan, who had extensively written on such matters (Cannan 1914, 1927).

\textsuperscript{317} For a more detailed reflection on this see Masini (2007).
One year later, with *The Economic Causes of War*, he comes again to the demarcation question. In a note of the *Appendix*, Robbins writes: “I have dealt with this problem at some length in my *Nature and Significance of Economic Science*, 2nd ed., chaps. I and II. […] What follows may therefore be regarded as a supplement to these chapters. The solution here offered is entirely consistent with the somewhat radical approach of the general methodology there presented” (1939: 117). Although the “solution” offered there looks even more ambiguous than in the *Essay*, it is clear that Robbins epistemology allows a wider range of possible methods and interests.

But this does not mean that things are mixed up. As he maintains again in a lecture at the LSE in 1939: “A Theory of economic policy, in the sense of a body of precepts for action, must take its ultimate criterion outside economics” (1961: 177)

And he will try to explain this again in a Lecture delivered in France in 1961: “The focus of economics on politics, at least in the more abstract sense, has remained very intimate […] it is clear that the majority of economists aspire to some influence on politics, both as a system of thought and in manifestations of practical action” (1963: 6-7)

But the proper domain for such aspirations has to be properly set. Economics is what pertains to individual and collective choices but it is also the design of a constitutional framework where such choices are not degenerating in conflict, thus endangering the whole society. What lies in between the two levels (rules supporting efficient choices by individuals and groups on the one side, constitutional provisions on the other) is a matter of politics. For this reason he so violently against “welfare economics”: because it pretends it is “a body of rules concerning the relation between welfare and the production and distribution of wealth which is independent of political valuations […] whereas] any claim that its generalizations and norms of measurement are independent of politics is surely based on illusion” (1963: 14).

In 1981 he will restate the need of a constitutional architecture: “the necessity of a framework of law and an apparatus of enforcement is an essential part of the conception of a free society” and “acts which are not free are not acts which are capable of having value in the ethical sense” (Robbins 1981: 8).

Economists can (and actually should) therefore devote themselves to this constitutional level of reflections, as freedom requires institutions enforcing rules: “one of the main practical functions of economic science was to enable us to detect inconsistencies in plans”. Plans, intended as the ways to move along a changing environment, where no persistence is possible: “In natural science, once causal connections have been established, the quantitative relationships can usually be assumed to persist […] this is not so in economics. […] The same absence of persistence applies also on the side of obstacles. The human beings […] are capable of learning” (Robbins 1981: 3).

But when we get out of the constitutional level an inevitable judgement of value is implicit: “in the application of Economic Science to problems of policy, I urge that we must acknowledge the introduction of assumptions of value essentially incapable of scientific proof”. That is why Robbins concludes: “I recommend what I call
Political Economy which, at each relevant point, declares all relevant non-scientific assumptions. [...] I venture to suggest that, as teachers of the subject, our instructions will be more fruitful if, side by side, they run parallel with suitable courses in Politics and History” because “excessive specialization in the first-degree stage, productive of one-eyed monsters, is too frequently the order of the day” (1981: 9). The three life-long beloved of Robbins are finally supposed to be walking hand in hand.

5. Progress in continuity

In the Preface to his Politics and Economics, written in 1961, Robbins confesses: “[on these matters] I have written a certain amount in the past, but […] I have never succeeded in making my views immune from misunderstanding” (Robbins 1963: vii).

Much of this misunderstanding came from his 1932 Essay on the Nature and Significance of Economic Science. Most of the critiques he received after its publication were directed to an epistemological construction where economics seems to be a pure mechanical science completely detached from reality and from policy action. The economist seems, accordingly, to be a pure discoverer of logical implications who has nothing to say to those who are responsible for collective choices, an intellectual closed in his ivory tower of pure formal relationships.

We have seen that some of these critiques were not misdirected, as Robbins’s discourse is often rude, sometimes ambiguous and apparently self-contradicting. In some passages, he gives the impression of considering the concept of given ends and given means as if they were stable, immobile, not undergoing profound changes in time and environment. Some sentences, if taken outside their wider context, sound as cutting definitions of this nature. But this was clearly not his intention, as his long record of writings on this topic, at least from 1927 to 1939, reminds us.

Robbins himself understood this misunderstanding due to his lack of clarity and tried to react. He did not change much in the 1935 second edition, although the way he rewrote Chapter Five on Economic Generalizations and Reality was a little step beyond.

But when read carefully, the Essay shows that Addleson was not wrong in confronting Robbins’s epistemology versus neoclassical economics on the point of choice versus determinism. Only when the choice is bounded by a maximizing end, the choice disappears and leaves room for a deterministic mechanical problem (the core of neoclassical orthodoxy). The homo oeconomicus is only a metaphor. And Robinson Crusoe is himself a metaphor bound to lose its universal significance when more individuals are aggregated and concentric groups are considered.

To explain this he wrote the 1937 book on Economic Planning and International Order, where he added important reflections on economics and political economy and tried to better clarify the relationship between the economist and policy choices, as he later recognized in his Autobiography. In that book, he tried to show a way to tackle both with atomistic agents and collective bodies, each of them facing ever-changing choices and needing a (federal) constitutional framework within which a coordination could emerge.
The economist emerges as a privileged advisor to design a constitutional architecture impacting on economic relations, that is the rules governing the process of choice within the social body. His successive writings until 1939 restated and further enquired on this.

Robbins’s scientific production, at least between 1927 and 1939, when the process of continuous reflections on this topic was virtually over, shows an interesting evolution on the role of the economist in society.

This might induce one to credit the idea of a progressive “correction” of his theses. An extreme explanation along this line is Talamona’s idea of a complete reversal of opinion which he (1960: 83 ff) attributes to the increasing abstractness of economic theory in fields of research such as “game theory”. Robbins, in his opinion, would have abandoned his youth deductivist credo when watching the results it later produced on economic theory, with various excesses of detachment from reality, auto-referential formalism, and so on.

A second possible explanation is that Robbins might have progressively changed his ideas but continued to repeat that he always thought the same way because he could not stand having been wrong in his early days. His evident anxiety, until he died, to restate and refine his assertions on the role of economics in social sciences might be supporting this idea of a continuous “expiation”.

But, reading the original writings by Robbins on questions of economics epistemology, we have found a common thread which cannot be said to have interruptions and which, although with some differences in language, shows an important evolution but only in terms of enrichment and clarification, and therefore a convincing continuity in time.

We can in fact argue that differences in emphasis are attributable to other causes. Strategy and tactic are two different levels which may sometimes seem self-contradicting; but the important thing is the former and in order to defend strategy you can even need to sacrifice direct coherence with tactic moves. Robbins has always shown the same attitude towards the question of the relationship between economics and politics, and even on the role of the economist in society, although the weights of the different arguments, of the various pieces of the puzzle change according to the audience and historical period in which he expressed them.

Tactical reasons may explain sharp, crude expressions, leading to misunderstandings. As Marshall had to fight against Cunningham and his Political Economy as a Moral Science, and was therefore forced to stress the detachment of economics from ethics, although rescuing it in several ways (and giving rise to accusations of ambiguity), similarly had Robbins to clear the market from Hawtrey and his renewed melting pot of economics and ethics, although convinced of the importance of value judgements to be assumed at a constitutional level.

318 And the persistence of such misunderstanding is probably attributable to the very debate that the Essay generated immediately after the publication of the two editions. Think, for example, of the impact which the review written by Harvey Peck (1936) and published in the authoritative American Economic Review must have had.
For Lionel Robbins, economics is the tool box of economic policy and of political action. No doubt economic reasoning can and should be detached methodologically from political (and ethical) matters but the former represents the basic training for the latter. No political action can be implemented without the perfect knowledge of all the implications suggested by a rigorous study of economics. Far from being “without” a professional market, the economist is then the fittest social scientist to be “demanded” in assisting political choices. He should be the privileged advisor of politicians. Although considered contingently against the stream of history, Robbins’s Essay was not at all against such a stream on a wider time-horizon. It was the necessary manifesto trying to define exactly what might be asked of economists when required to give policy advice, a professional market for economists that was just opening in those years.

As an economic advisor himself, he felt that it was necessary to set the proper expectations of government and public opinion on what economists’ contribution could be helpful for: individuals choose for themselves; politicians choose for collective groups. And in order to have such choices made with full awareness of their implications, the economist - a scientist used to work in and on settings characterized by changing choices - is the indispensable advisor.
References


Scratch a Would-Be Planner: 
Robbins, Neoclassical Economics and the End of Socialism

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Abstract

Robbins’s central contribution to the debate on market versus plan links with identification of economics as the science of how societies handle scarcity, a central contribution of the Essay. This was not a narrow focus on static efficiency; inflation was a key part of Robbins’s conception of (mis)handling scarcity. The irony that transition to the market led to movement away from the market in economics is analysed, highlighting the obscured role of macroeconomics, and questioning a new conventional wisdom that Russia should have followed the Chinese path of gradual and Pareto-improving institutional development. A conclusion is that the demise of the Washington Consensus should not lead to a new dogma: the neoclassical paradigm is not being replaced but extended.

Keywords: Transition, Lionel Robbins, socialist calculation debate, Russia, inflation
JEL Classification: P21, B31

1. Introduction

An exhilarating covey of thunderbolts ... (Robertson, 1935, p.106)

Robbins’s characteristically eloquent and powerful contribution to the intellectual battle over market versus plan (1934) executed with a style which, as Dennis Robertson observed (1935, p.104) possesses “the stirring quality of a work of art” permanently established him as a key figure in the great “socialist calculation debate,” of the twentieth century. This long discussion directly influenced events in Eastern Europe until it was settled by 1989. Remarkably, Robbins penned pointed paragraphs where Mises and Hayek wrote books, but the “forcible and eloquent … exposition of a case which is so often gravely mishandled by its adherents” (Robertson, 1935, p.106) led Robbins to be assigned a lasting central role. Thus Oskar Lange (1936, p.67) famously made his challenge that “Professor Hayek and Professor Robbins themselves ‘solve’ at least hundreds of equations daily, for instance, in buying a newspaper or in deciding to take a meal in a restaurant, and presumably they do not use determinants or Jacobians for that purpose.” In the eight-volume anthology on this debate assembled by Peter Boettke (2000) Robbins is woven throughout.

Lange was replying to Robbins’s (1934, p.151) observation on the computational impossibility of solving millions of equations to emulate a market solution for a planned economy, given that, importantly, the essence of the economic problem is that it is constantly subject to change. Lange’s proposal was then to do what Robbins

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called “playing, so to speak, as if real competition existed” (1976, p.144). Robbins’s response (1937b, p.210) to Lange’s supposedly practical solution appears both realistic and decisive, and indicates why the latter’s market socialism was pure chimera.

“The managers of the productive units are to sell against each other in the product markets. They are to bid against each other in the markets for factors of production. But it is scarcely to be supposed that they are to be free to change the general use of their capital. As the manager of a communal cotton factory, a government servant may be permitted - even ordered - to undersell his competitors in the market for cotton cloth and to overbid them if necessary in the markets for labour and materials. But can we assume that he is free to transform his business altogether, to become a merchant instead of a manufacturer, or a producer of some other commodity? Is he to be free to close down his works in Lancashire and to commence operations in Japan? The thing is not impossible to conceive. But it is really most improbable in practice.”

Thus in 1937 Robbins elegantly captured an issue of our own fin-de-siècle and the transition to the market: the importance of the vitality of de novo firms as the essential economic engine (e.g. Dabrowski, et al, 2001). Robbins thus long preceded Peter Murrell (1990:23) in positing a “Schumpeterian approach” to understanding the problems of socialist economies, that is to indentify their Achilles’ heel as lacking the capacity for “the rapid creation of new products…as well as the ability to promote these new products on the world market.”

This may seem to contradict the basic (static allocation/general equilibrium) thrust of neoclassical economics, and therefore be in contradiction with the Robbins of the two editions of the Essay on the Nature and Significance of Economic Science. That Robbins, in identifying how societies handle scarcity as crystallising the crux of economics, is sometimes considered to have been “not merely putting his finger on a central feature of the economics that he knew in his own time, but was also significantly shaping the character of microeconomics as it would develop during the remainder of the century.” (Kirzner 2000, p.19). The author of the last encomium himself provided the answer to the apparent contradiction, by defending Robbins against the sustained attack on economics defined as scarcity-conditioned behaviour delivered by James Buchanan (1965) in his presidential address to the Southern Economic Association. “Robbinsian economics”, observed Kirzner, (1965, p.258), “studies the activities of men engaged in solving their economic problems. It is not its task to compute efficient solutions to anybody's allocative problems; its task is to explain the phenomena (including interpersonal exchange processes) which result from the circumstance that men are, in fact, engaged in seeking efficient solutions to their allocative problems.” Buchanan's quarrel, Kirzner observed, ought not to be with Robbins.

Among these phenomena which Robbins repeatedly sought to explain as a priority was high inflation (1937a, 1973): it too arises from the way that a society handles or attempts to deny scarcity. It had a unique role in the death agony of communism.

There is a strong case to be made for a direct connection between Robbins’s own very early concerns, which brought him to economics and his homing in on the central question of scarcity. Robbins’s intense but short-lived attraction to socialism is well-
known from his autobiography (1971, p.54-71), where it is related with self-deprecating wit and insight. Robbins found Marxism and communism sectarian and unattractive, but was drawn to the leftist Guild Socialism of G.D.H. Cole (1971, p.57), later a leading Oxford don, rather than the more sedate collectivist ideas that (for example) attracted Friederich Hayek in the same period following the Great War. Hayek’s inaugural address at LSE (1933, p.135) may surprise with its note on the naturalness that “every warm-hearted person, as soon as he becomes conscious of the existing misery, should become a Socialist.”

The unsatisfying character of Cole’s doctrine of “syndicalism” or workers’ control, precisely as it was formulated at that point of Robbins’s adherence to Cole, is brilliantly captured by Dennis Robertson (1920, p.537) in an *Economic Journal* review: “Mr. Cole has made a gallant attempt to pitch his tent somewhere between the Trade Union Lodge and Cloud cuckoo-land; but now and again a sand-storm blows it away.”

A demonstration of the link posited here between Robbins’s socialism and the issue of how society addresses the scarcity of resources is wryly captured by Robbins in his autobiography:

“In all the literature of this sort … I do not recollect any serious discussion of allocation of resources, mobility of productive agents, the role of markets or the accumulation of capital. The fundamental assumption throughout was that the economic problem did not really exist… I began to find this a little unsatisfying. (1971, p.65).

Robbins (1971, p.67) goes on to describes how newly demobbed Lt. Robbins, personal assistant to the head of The Labour Campaign for the Nationalization of the Drink Trade, was drawn to the reading of academic textbooks. Although the texts did not, in fact, supply him with the answers in this regard, he noted that he was struck by the discussion of “economic problems which have to be faced by any society, recognised and discussed in a more or less realistic perspective.”

Any thought that stressing this link might be elevating this year and a bit of Robbins’s life to an exceptional status may be dispelled if one reads Robbins’s knowledgeable and irrepressibly Robbinsian *Economica* (1940) review of the biography of Stalin by French marxisant Boris Souvarine, written nearly two decades after his socialist episode. This excellent book, Robbins notes with irony, will have some disadvantages for the English reader, and we can expect he had in mind those like Sidney and Beatrice Webb, Fabian founders of the LSE, and their colleague George Bernard Shaw. For one thing, he observes with that identifiable impish style, there is no mention of how one cannot make an omelette without breaking eggs, no admiration of authoritarian will power. In his review Robbins reveals himself to be completely aware of developments among leftists, and to distinguish nuances amongst them. His capture of the essence of the dictatorship of the Plan must also have arisen from such close observation.

The focus on contrasts in the handling of scarcity turned out to be a powerful subsequent vision, east and west, in understanding the pathologies of what claimed to be a planned economy. This was not because microeconomics, for all its static,
timeless and institutionless nature (Lipsey, 2000) is at root an ideology or illusion, but because its basic (testable) propositions, along with modern macroeconomics, were powerfully seen to explain daily phenomena in the unusual economic turbulence and strain engulfing the failing Soviet-type systems from 1986. The particular way in which the Soviet-type economies functioned eventually served to undermine them, and to confirm Robbins’s basic point, first expressed in the Essay, “Scratch a would-be planner and you will usually find a would-be dictator.” A lighter version of this Robbinsian diagnosis is to be found in the Economist epitaph for the USSR: “Thus perished the Soviet Union. It promised the earth. It could not deliver a pair of blue jeans.”

1989 was only a brief moment of victory for Robbins’s focus on economics as the way in which systems handle scarcity and for this supporting role in the great debate on socialism. It was not an accidental discovery, as anyone (like Robbins) who had a serious episode as a radical socialist would have had to wrestle with this question.

A major irony of the briefly euphoric end of socialism has been that the pain of the transition to the market is strongly argued to have had the impact on economics of having “provided strong empirical support for this shift in focus from markets and price theory to contracting and the legal, social and political environment of contracting.” (Berglöf and Roland, 2007, p.5). The rest of this article explores this irony and its lessons for economics.

Section 2 provides a different framing for the lessons from some disappointments of transition, against the recent trend to reduce these erroneously to the sequencing and methods of privatisation. Robbins, we are clear, was more about inflation than questions of ownership, or of pricing at the margin. Section 3 asks if the rising tide of enthusiasm for the Chinese road away from socialism could have been at all applicable in Central and Eastern Europe and the Former Soviet Union, and concludes that it could not. The people of the region shared Robbins’s attitude towards freedom. Section 4 does ask what lessons to learn from the “failed brand” of the Washington Consensus, and finds that it was defeated above all because of the way it was applied, with an increasing ideological fervour and rigidity.

2. Hamlet without the Prince, the Transition without Macroeconomics

Concluding his influential Transition and Economics, Gerard Roland observes with evident satisfaction that “at the Fifth Nobel Symposium in Economics devoted to the economics of transition, out of six sessions, only one was devoted to macroeconomic developments, while there were five sessions on institutions, with three devoted to the organization of government and two to contracts.” (Roland, 2000, p.343). Even in that sole session, inflation failed to find any place, nor does it appear in Roland’s admirably detailed 22-page index.

Table I is perhaps all that is needed to make clear that this is an odd and very probably symptomatic silence.
Table I: Annual Inflation, per cent, Year on Year

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<tbody>
<tr>
<td>Russia</td>
<td>92.7</td>
<td>173.4</td>
<td>874.6</td>
<td>307.3</td>
<td>197.4</td>
<td>47.5</td>
<td>14.6</td>
<td>27.6</td>
<td>85.9</td>
<td>18.6</td>
<td>13.8</td>
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</tr>
<tr>
<td>Poland</td>
<td>585.7</td>
<td>70.3</td>
<td>43</td>
<td>35.3</td>
<td>32.2</td>
<td>27.8</td>
<td>19.9</td>
<td>14.9</td>
<td>11.8</td>
<td>7.3</td>
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Source World Economic Outlook (IMF) Database

Peter Murrell, who has emerged as one of the key proponents of institutional economics to study the lessons of the transition, sagely forecast in 1996 that “The victors in the theoretical and policy debate will be those who establish the salient facts through the murk of reform.” (1995:167-8). Note that for Murrell, victory would be the recognition of the case for gradualism, with a strong family similarity to the arguments advanced more loudly by Stiglitz.

As a result of the distraction of the very loud debate over the rapid Russian privatisation, another salient fact has recently been shrouded recently in a certain fog: the death agony of most of the communist states was marked by a progressive loss of fiscal and monetary control, which resulted first in repressed high inflation – massive shortages, loss of incentives to produce, lengthening queues – and then finally in open high inflation. (Granville, 1995, Ellman 1992, Rostowski 1998). Murrell (1994) concurs that in the period before the new Russian government took the helm of a state without borders or its own currency, “an economic crisis of vast proportions was setting in. The maldesign of Gorbachev’s economic reforms, particularly the lack of attention to macroeconomic balance, had left the economy in a parlous state by the end of the 1980s.”

As Table I indicates, in Poland the initial stabilization was successful, if with substantial inflationary inertia. In Russia the stabilisation was initially abandoned. The rapid post-Soviet growth of the money supply and the sizeable fiscal deficits made it bizarre to consider this “shock therapy.” (Granville, 1995).

An IMF programme for Russia in April 1995 was precipitated by enterprises and households’ increasingly systematic avoidance of the inflation tax, by holding US dollars. As a result, the feed through from money growth to an increase in the price level shrank to about 3 months. (Granville, 1995). Although there were grounds for cautious optimism at that time, the budget deficit from early in 1995 was financed not by central bank credit but by short-term treasury bills (“GKOs”). The floating exchange rate was replaced in July 1995 with a (crawling) peg regime. Seigniorage declined. This stabilisation drive resulted in low average monthly inflation, at least compared to the previous sub-period. (1.85% versus 13.18% per month). However, less than three years later, and with a worldwide bang, Russia was thrown back into a profound financial crisis.

Why did this happen? Substituting (very short term) bond financing for monetary financing of stubbornly high budget deficits papered over the cracks, but was ultimately doomed. The catalyst was the global financial market shock we call the East Asian crisis. The Russian default and massive devaluation can be seen as a brilliant and tragic confirmation of Sargent and Wallace (1981), who placed primary
emphasis on budget stabilisation rather than on money growth per se. They reached the conclusion that permanently higher government deficits must eventually be accommodated by increases in the monetary base. The budget deficit will be inflationary whether financed by the non-banking sector, the central bank or the banking system. Financing the budget deficit through bonds is only an alternative to inflation in the short to medium term, not in the long run.

This narrative is even less factually controversial than the leading stylised fact which presently dominates the discussion, that of the fall in output at the start of the transition, whose magnitude and household welfare effects, it is widely agreed, remain uncertain, but definitely not as high as the raw statistics would have tended to indicate.

The tendency, praised by some, to remove, or diminish macroeconomics in interpreting of post-Soviet transition may likewise be found wanting when it comes to the bounce-back in output and incomes from the recessions triggered by the collapse of central planning. In Poland, the recovery began two years after the start of transition, while in Russia it took eight years – and even longer in other cases like Armenia, which was held back by conflict but since the mid-2000s has enjoyed double-digit growth. The common thread is that recovery follows monetary stabilisation and discipline in the public finances. In Russia’s case the turnaround was of course facilitated by the rebound of world oil prices, but a much more important factor was the economic policy choice not to negate that helping hand by a return to the fiscal profligacy of the 1990s. An OECD study (Ahrend 2004) runs sensitivity analyses which indicate that post-1998 growth rates would have been very similar if oil prices had remained at their average 1990s level.

To be sure, the macroeconomic instability and chaos of the 1990s will have left a deep mark on the body politic and is a critical component in understanding present-day Russia. (See Ferguson and Granville, 2000). As Robbins (1937a:5) emphasised forcefully in the preface he was pleased to write for Bresciani-Turoni’s classic study of the German hyperinflation, whose English translation he arranged: “next probably to the Great War itself, it must bear responsibility for many of the political and economic difficulties of our generation….Hitler is the foster child of the inflation.”

This essential critical macroeconomic narrative has been crowded out by the skilfully told but arguably very wrong account of the lessons of transition presented most vigorously by Joseph Stiglitz, and then contested by defenders of the Russian privatisation. That Stiglitz completely lacks perspective and understanding of the conditions in Russia is demonstrated by his fundamentally mistaken account of the genesis of the initial high inflation:

“In the enthusiasm to get on with a market economy, most prices were freed overnight in 1992, setting in motion an inflation that wiped out savings, and moved the problem of macrostability to the top of the agenda. Everybody recognized that with hyperinflation (inflation at

319 Ferguson and Granville, “Weimar on the Volga” (2000, p. 1084) concludes with Stefan Zweig’s “Nothing ever embittered the German people so much . . .nothing made them so furious with hate and so ripe for Hitler as the inflation”, having explored the parallels with contemporary Russia.
double-digit rates *per month*) it would be difficult to have a successful transition. Thus the first round of shock therapy – instantaneous price liberalization – necessitated the second round - bringing inflation down. This entailed tightening monetary policy, raising interest rates.” (2002, p.142).

Anyone simply reading the *New York Times* or *Financial Times* in the late eighties could not have avoided becoming acquainted with dramatic tales of escalating shortages, kilometre long queues, introduction of ad hoc rationing schemes, and quite literally empty shelves. Gorbachev understood the situation better than Stiglitz, when he was quoted in *Pravda* (2 December 1990) explaining the worrisome phenomena as related to “the growth of the money supply, which is not linked with a corresponding increase in production.” As Ellman (1992) captured it, during the period of Gorbachev’s rule the country went from macroeconomic disequilibrium to collapse.

A recent OECD retrospective recorded that “By 1991, the polity was fragmenting and the economy was in free-fall. Real GDP that year dropped by somewhere between 8 and 17 per cent – estimates vary widely, owing to the chaotic economic and political situation of the time.” (Ahrend and Tompson, 2005, p.6, citing Granville 1995:37).

The additional important and unfamiliar feature for a market audience is that prices were relatively inflexible, and so the excess demand became hyper-shortage rather than inflation. This hyper-shortage was a caricature of the long-familiar Soviet system, with its pervasive *deficit* (short supply), economy of favours (Ledeneva, 1998).

It was in these stressed circumstances, in which “fewer than a dozen of the 130 goods considered by the statistical authorities to be basic necessities for everyday life were available through normal retail channels by late 1991” (Ahrend and Tompson, 2005, p.6) that prices were liberalised.

The initial price jump in Russia in January 1992 was much higher than had been forecast by the IMF, and probably reflects both inflationary expectations and the existence of more liquidity in the system than had been officially acknowledged. It did appear to be the act which drastically reduced the value of peoples’ savings, but essentially this had happened under the old system, when they accepted increasingly useless rubles. (Of course it is inevitable that the median saver did not take such a nuanced view, but economists should be able to appreciate it).

The only alternative to liberalisation would have been the continuation of the truly enormous queues of 1991, or of iron rationing and the reaction in black markets. There was no other way to get goods back on the shelves, and, with a certain slowness in comparison to entrepreneurial Poles, for example, this began to happen. Goods which had last been seen 2 or more years ago were back.

The first Russian government of 1992 held the line fiscally, although it was very difficult. They did so in the hope that if they demonstrated their credentials international assistance to their bankrupt new state might be forthcoming. When this did not happen, a critical compromise was made by the reform team, to abandon
stabilisation but to remain as ministers and to go for all out and fast privatisation, in
the hope of making the reforms irreversible. (Gaidar, 2003).

Leading former Soviet specialist Gur Ofer (2006) recently recalled conversations with
Anatoly Chubais, the reformer in charge of privatisation who considered that
“premature stabilisation, before enough privatisation had taken place might have
opened the door for the return of the old regime. Stabilisation might also slow the
privatisation process by making credit to potential buyers more expensive.”

This recollection is supported from another angle by the argument of Dabrowski et al,
(2001) against the most well-known and rather widely-accepted argument of Stiglitz
(1999) that transition failed because privatisation and marketization occurred before
the right institutions were in place. They argue that the Russian transition did not fail
for the reasons Stiglitz put forward. Rather the 1992 attempt at disinflation,
liberalisation and hardening the budget of the state owned enterprises failed because
of opposition in the Supreme Soviet (which had been elected under the old order and
Soviet constitution) and insufficient support from President Yeltsin. This led to
adopting the “privatisation first” approach, which these reformers had previously
opposed.

Dabrowski et al thus consider (2001: 318) that “the sequencing error which was made
in Russia was not so much, as Stiglitz claims, in placing privatisation before the
creation of a legal, competitive and regulatory framework for market processes, but
rather the much more important error of placing privatisation before liberalisation
cum hardening of budget constraints and disinflation.”

They continue with the persuasive and knowledgeable argument that:

“In an environment of near hyperinflation, massively distorted prices and
soft budget constraints, the best designed privatisation scheme, occurring
in an environment with the requisite legal structure …could not have
succeeded in allocating firms to efficient managers who would have
successfully restructured them. On the other hand, as the Slovak and
Croat experiences show, given free markets, predictable prices, hard
budget constraints, and the basics of commercial law … even outrageous
degrees of "crony privatisation" need not lead to the huge disorganisation
and social costs experienced in Russia.”

As these authors note, and we underscore, making a virtue out of the necessities
forced upon the new Russian government is a mistake. We have read Hayek too, the
authors assure Stiglitz, and by that they also mean this warning in the Road to
Serfdom:

“This is perhaps the place to emphasise that, however much one may wish
a speedy return to a free economy, this cannot mean the removal at one
stroke of most of the wartime restrictions. Nothing would discredit the
system of free enterprise more than the acute, though probably short-
lived, dislocation and instability such an attempt would produce.” (1944,
p.209).
No one quoting this powerful concern in the recent context has, however, has produced a reasonable proposal for the distribution of the necessities of life in 1992 Russia, with very different initial conditions from post-war European war economies. (The June 1948 German overnight monetary reform led by Erhard may further call Hayek 1944 fear into doubt). The degree of macroeconomic disequilibrium of post-communist Eastern Europe is, perhaps, beyond the comprehension of the commentators who offer advice that price liberalisation should have been postponed.

But at least these commentators could be expected to keep in mind the chronology of economic policy in that first decade of Russia’s transition which has been briefly reviewed here. To sum up, after the emergency measures in 1992 to get basic goods back into the shops, disinflation and hard budget constraints were put onto the back burner. Formal employment was maintained, even if non-payment and arrears of wages revealed millions of jobs to have been already lost, which gave some residual access to rudimentary social benefits provided by enterprises. Far from shock therapy, the pain of transition was thus administered gradually – until it finally burst into the open with the crash of 1998 which cut real incomes across the board by one third. The root cause of that crash – even if it was catalysed and intensified by external shocks – was a phoney macrostabilisation based on bond financing of persistent budget deficits. The political reaction to 1998 was a change of government, and it was the incoming team backed by the Communist majority in the legislature which administered for the first time hard budget constraints and disinflation. This much overlooked fact of shock therapy finally being presided over by Yevgeny Primakov and Yury Maslyukov has a revealing explanation. They explicitly renounced the option of monetary financing on the grounds that the public well understood that inflation was not the answer. The lessons of the high inflation of the early 1990s – especially its highly regressive distributional effects (Granville, Shapiro and Dynnikova, 1997) – had been learned by society as a whole.

“Macro is on the way out. It’s all micro now”, chortled Economics dissident Arjo Klamer recently, in comments in David Colander’s Making of an Economist, Redux, 2007, p.230), which surveyed graduate students in the leading US PhD programmes again, with riveting but “small and haphazard” samples of unknown selection bias (Solow in Colander 2007, p.234). Indeed, Solow did observe that:

“What struck me most, and horrified me most, was the widespread feeling among these elite students that the macroeconomics they were taught was the least “relevant”, least applicable, least enjoyable part of their curriculum.”

As Solow notes, thirty years ago, the opposite would be true. With the return of inflation it is not impossible to think that we may return to the condition described a bit ruefully by David Kreps just a decade before (1997, p.59): “To most people, economics means the (mis)behaviour of the macroeconomy, both national and global”

3. Trade Off Between Market and Freedom? Conclusions from China

From the time of his memorable barbed footnote “Scratch a would-be planner and planner and you usually find a would-be dictator, “(1932, p.113; 1935, p.115)
Robbins consistently linked his opposition to planning to his clearly stated personal values which gave a high priority to liberty. As he drew his 1947 Marshall lectures in Cambridge to a close (1947:82) he reminded his more etatishe audience of the virtues Keynes had cited for individualism in The General Theory, which went far beyond efficiency: “It is also the best safeguard of the variety of life, which emerges precisely from this extended field of personal choice, and the loss of which is the greatest of all the losses of the homogeneous or totalitarian state.”

In those Marshall lectures Robbins displays an evident glee in relating his encounter with a “leading collectivist” then occupying a high state position, who is a bit out of touch with developments in the socialist calculation debate. Robbins appreciates exactly how discomfiting the “market socialist” proposals of Abba Lerner will be, and presents a fine example of his special ability to understand the essence of this type of thinking, and why he sees the aspiring dictator lurking behind the yeartning planner. Robbins unmasks him as someone who wants to decide for others with a wave of this hand.

Robbins (1976, p.146) was even more emphatic and rather less cheerful, when he proposed of total collectivism that something in the logic of its action which must lead to a much greater curtailing of the liberties of the ordinary citizen. As recent discussion (e.g. Grosjean and Senik, 2007) emphasises, we know of no planned economy in a politically free system, though there are successful market economies without such a system.

It is quite clear that those who took part in the 1989 “Autumn of Nations” in Central and Eastern Europe would not have listened long to someone promising the trade-off of less output fall in exchange for the newly-won taste of freedom. As Witztum (1994) observed, it was less the economic aspects of the system than the political manifestations (so well described by Robbins) which were hated. Witztum’s view remains confirmed for 2006 in the Life in Transition Survey (LiTS) of 29,000 respondents - in 28 transition countries and Turkey - carried out by the EBRD and the World Bank, which indicates that in every country it is inevitably the market which is less popular than democracy. (Grosjean and Senik, 2007).

Following his own injunction to delineate value judgements from positive statements, Robbins added to his observation on the incompatibility of the plan with freedom: “Whether you regard that as a recommendation, whether you welcome the limitations of speculative thought, the regimentation of ignorant opinion by shy-making slogans the cultivation of hero-worship by ubiquitous giant photographs and a popular shrine is of course a matter of ultimate values,” whilst making it clear precisely where his own values lay.

However, as Grosjean and Senik observed, an unintended consequence of China’s “spectacular rise” has been a new conventional wisdom that a key sequencing for reforms is to allow political freedom only after economic changes. This part of a possible future “Beijing Consensus” is difficult for those who share Robbins’s attitude to the good society and good life.

This new conventional wisdom will quite likely be gaining momentum, as the new Chief Economist of the World Bank, Justin Yifu Lin, has been very clear on his view
(2004a, p.9): “Although the problems that China faced were similar to those in the EEFSU [Eastern Europe and Former Soviet Union] China adopted an approach very different from that at EEFSU. First, China pursued *perestroika* (economic restructuring) to stimulate the dynamism of the economy but avoid *glasnost* (political openness) to avoid the collapse of the Communist party.”

From the start of the East European transition the example of China, which began its reforms with the successful decollectivisation of agriculture from 1978 has beckoned as an example of a superior, smoother and more gradual route to the market. Despite the quite remarkable return to growth of the European accession countries which have successfully joined the EU, and also the often double-digit growth in the countries of the former Soviet Union since 1999, the memory of the shocks of the early 1990s is still vivid in both the populations affected and in the minds of Western economists. The advertisement of a Pareto-improving reform without losers, even if exaggerated to some degree, cannot but appeal. China’s rise has certainly represented, as Stanley Fischer remarked, quite probably the single largest increase in human welfare since the invention of fire. (Fischer, 1994, p.131).

In the growing literature on the irresistible exploration of the counterfactual “what if Russia had taken the Chinese road?” We have been able to identify at least 15 major obstacles to any Russian or Soviet adaptation, even by those most admiring of the Chinese reforms. They range from the macroeconomic balance and low debt of China at the start of its reform process, which allowed continued moderate subsidies to the intensely less complex and more decentralised (“M-form”, Qian and Xu, 1993) nature of the Chinese planning process, involving about 700 centrally “planned” commodities, rather than the 50,000 of Russia on the eve of perestroika. (Granick, 1990). The most thoughtful of proponents of Chinese lessons for Russia, Peter Murrell (2005) makes it very clear that he only urges adaptation of the *spirit* of the Chinese reforms, which he regards as experimental, bottom-up and trial-and-error, ignoring the key over-arching part of the structure which allows the experiments to be run: “This way of understanding matters contains an implicit riposte to those who make the observation that Chinese-type reform policies were not applicable in the circumstances of Eastern Europe and the former Soviet Union. I regard this observation as wholly uncontroversial.”

But it is doubtful even whether the gradual experimental method was applicable in countries where the system had collapsed. For experiment is easier in an environment like China, where people had so much less to lose than in the former Soviet Union and its satellites (if only perceived losses, which have the same political and social

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320 Macroeconomic balance allowed the Chinese to consider continuing subsidies for a period to enterprises which would not be viable in open international trade, a strategy, not an option open in Eastern Europe by 1989. A return to more economic autarky was also not feasible. Throughout this paper we have further abstracted from the additional difficulties facing Russia, which had neither borders nor its own currency at the start of 1992.

321 Recent work since the Soviet “archival revolution” (Gregory and Harrison, 2005) has demonstrated the view of Ledeneva (2006) that “The Soviet system was not a planned economy. It was meant to be, but those living within its borders found that they had to counteract its overcentralization and its ideological limitations through intricate schemes of informal exchange, regional and industrial lobbying, and a variety of practices for cheating the system.” Harrison and Kim (2006) propose an empirically-grounded alternate model of the Soviet firm under “partial centralization” in which the old stereotype of the enterprise as price-taker is replaced by bargaining.
impact as real welfare losses). China had nothing like the rudimentary but universal social safety net provided by the mature Soviet system. The productivity gains from gradually introducing incentives into the overwhelmingly rural economy followed by urbanisation (almost certainly less gradual than the Chinese Party intended or foresaw) amounted to the “low hanging fruit” of development which Soviet-style centrally planned economies had long since plucked – since the initial industrialisation drives of the 1930s, with all their appalling human costs and economic inefficiencies.

Although, as we have explained in the discussion on high inflation, the Chinese fork in the road was no longer open at all to the new Russian government of 1992, the characteristic innovation which has continued to intrigue Roland and his co-thinkers is the “dual-track liberalisation”, in which state-owned enterprises are allowed to sell above-plan production on the market, whilst retaining their state orders. This, as may be demonstrated in a variety of plausible models, would have prevented the Eastern European output fall, and allowed “growing out of the plan.”

In a tale of two reforms, Wei Li (2002) elaborates on this point, demonstrating that if plan quotas are not strictly enforced then, in the extreme case, it will be no different from a “big-bang” price liberalisation. Li, as does Young (1999), also points to the enhanced possibilities for corruption when planned and market prices diverge, corruption which has been a noted feature of the Chinese reforms. (It should be noted, as Li does, that this corruption is to some extent open, in comparison with the privileges of the old system, which was not seen in the same light). “The Chinese government in the 1980s was able to plug enough loopholes in the dual-track system to make it work.” (Li, 2002)

Li’s elegant model allows the drawing of a few other important conclusions on barriers to the dual-track in the Eastern European case. Not only was the Chinese economy decentralised, and thus much less inter-dependent on one planning input, but it was effectively then a closed, self-sufficient economy. No coordination with the equivalent of CMEA trading partners was necessary.

Other characteristic distinctions between the Chinese and the more developed command economy appear to include the fact that the Chinese plan was not the “taut” variety which had supplied what dynamism there had been in the classic Soviet-type system. Chinese enterprises frequently overfulfilled their very simple plans; there was apparently no ratchet mechanism which then punished them for this by an increase the following year. These enterprises also characteristically were not running at a loss at the start of the reforms, so major budget subsidies were not necessary.

Despite all these advantages, the need for intensive monitoring of the type only the Chinese Communist Party could mobilise was brilliantly painted by Alwyn Young in an article which was included in the Nobel Symposium papers (1999, 2007). Young, explaining the razor’s edge which this two-track strategy has imposed, describes the “silkworm cocoon war” of 1988, in which Shanghai, right next door to the major silkworm province, Zhejiang, received only 40 tons of their planned allocation of 2000 tons of raw silk. This was no small thing, Young observes. China accounted for 60% of world production of raw silk and 90% of its exports. As early as 1986, Young explains, each harvest season the government bureaucracy “from the provincial level
all the way down to village units, and including police and militia forces, was mobilized to ensure that Zhejiang’s silk farmers sold silk cocoons locally.” Justin Lin has christened China’s new strategy as Comparative Advantage Following (CAF) (2004) and indeed labour-rich China can perhaps afford the monitoring costs involved.

Without the authoritarian governance structures in place, it is difficult to see how in (say) Russian conditions it would have been possible to proceed when “incremental reform releases segments of the economy from centralized control, and the freed segments find it profitable to exploit the rent-seeking opportunities implicit in the remaining distortions of the economy.” (Young 2007, 2000). It was precisely these possibilities which were exploited during the final years of Soviet rule, and led to the implosion of supplies. Li (2002) describes the rise of corruption in China, one of the causes (along with inflation and a desire for more democracy) which led to the protests crushed in Tiananmen Square in 1989.

China may proceed to widen the sphere of civil liberties and political choice which are summed up as democracy or freedom. We admit of no expertise on this, and do not believe that there is a theoretical or empirical knowledge base for this at present. Whatever the outcome, it seems highly implausible that this could have been accepted in the 1980s by those looking west from Eastern Europe. It is a different question to imagine how history might have been different if “growing out of the plan” had been attempted in the Soviet Union decades earlier. At that point, however, with adequately high growth rates, it is difficult to see why the sheer inefficiency of the system should have led the Communist Party of the Soviet Union in this direction. In 1985 it appears most likely that the authority of the communist parties was too low to be restored.


How could the transition to the market and prices in the economy lead to a trend “away from markets and prices” in economic analysis? To understand this it is necessary to understand the end of the period of the Washington Consensus, and the new central role argued for institutions. In his masterly summing up of the retirement of the Washington Consensus as a “damaged brand,” Naim (2000, p.88) observes that the surprising popularity of this “wonkish moniker” arose from the “sudden collapse of the Soviet system” and the consequent worldwide disillusion with socialism and central planning. The Consensus filled an ideological vacuum, ideological in the sense of an intellectual labour-saving device in the face of a complex reality. The IMF and World Bank’s conditioning of stabilisation arrangements on the adoption of Consensus-compliant policy reforms reinforced its role at that moment. Naim stresses the positive role of the Consensus as well, notably in bringing in to the policy world mainstream economics commonsense that loose monetary and fiscal policy were actually at the root of inflation, which had been denounced as “monetarism.”

Naim traces the discovery of institutions in Washington to the January 1994 Zapatista uprising in the poor Mexican state of Chiapas, dispelling much hype about the marvel of that country’s reforms. In contrast, in the field of transition and Eastern Europe, the roots of the acceleration in passion for institutional analysis are widely believed to have been given a major stimulus by the transition, as we have noted. (Murrell, 2005;
Roland, 2000). However, although the “New Institutional Economics” (NIE) had been gathering increasing professional attention from 1985 (Richter, 2005), assisted by the Nobel Prizes to Coase (1991) and North (1993), both laureates in their acceptance speeches noted that we did not know enough to give recommendations on the transition, though they emphasised the importance of the transition to the market as evidence for the importance of studying institutions. Oliver Williamson, the third of the leaders of the NIE “movement” put it this way: “Broad reach notwithstanding, the NIE is not and does not pretend to be an all-purpose construction, as the reform of economies of Eastern Europe and the former Soviet Union illustrate.” (Williamson, 2000).

This refreshing absence of hubris here puts into perspective the excessive claims for pain-free transition by the alchemy of institutions and experiment. Nonetheless, it is to be regretted that Robbins (1932:94) “assumed” (that is abstracted from) both institutions and psychology, and even more that this assumption became implicit rather than explicit at times.

Naim is careful to emphasise that “the discovery of institutions” (2000, p.93) is also subject to the temptation of hubris or fashion hype and over swing which was so important in undermining the Washington Consensus. In an aside about the vitriolic character of the debates Naim (2000, p.95) quotes IMF chief economist Michael Mussa’s response that those who believe a loose monetary policy would have eased the plight of stricken countries are “smoking something that it is not entirely legal.” As it can be seen, we agree, believing the evidence supports the view that in attacking what was perceived as an overly rigid and increasingly doctrinaire set of recipes, it has been entirely possible to slip into a new ideology: the new dogma could turn out to be a rigid insistence on pragmatism.

The actual practice of the Washington Consensus did contribute to its own downfall. In the earliest (inside the Washington Beltway) debates on a “Post-Washington Consensus” an incisive contribution was made by Ravi Kanbur, an academic economist who has also served on the real frontlines of the World Bank, as head of operations for Ghana, and thus had a virtually unique vantage point for observation on what can go wrong in practice. Kanbur (1999) remarked, in a session led by Naim, that this is how the Consensus was carried out in practice: “So the negotiators from Washington always took a more purist stance, a more extreme stance than even their own intellectual framework permitted (they were all surely well schooled in the theory of the second best). ‘Give them an inch of nuance, and they'll take a mile of status quo", seemed to be the mindset.”

There may be important lessons for the translation of sound economics into policy in the history of Robbins’s evolution into an economic “statesman,” on which Hayek (1994) remarked in fond reminiscence: “I have been spared practical experience with government service. And watching, in the case of a man I so much admired like Lionel Robbins I’ve no doubt that corrupts the attitude of the economist. He becomes a statesman instead of an economist.” The Washington Consensus spirit, perhaps because of the mood of triumphalism of 1989, owed more to Hayek than Robbins.

Although the younger, pre-war Robbins was a passionate proponent of a rather exceptionally rigid orthodoxy, which denied the possibility of intervention in the
Great Depression (1934, 1971) both time, his experience in government during the war (1947) and close working contacts with others who had been seen as opponents, certainly mellowed him. Although Robbins attended the founding meeting of the Mont Pelerin Society, his contribution was to write the Statement of Aims. As Aaron Director recalled (Ebenstein, 2001, p.145) of this “nobody else at the meeting …could have reconciled the differences in politics among the participants …as well as Robbins. After we had spent days discussing these issues and tried to draft a statement, Lionel finally took it over and drafted the one we all signed.” That was Robbins’s last contribution to Mont Pelerin, although his resignation was linked to a painful personal falling out with Hayek (Ebenstein, 2001, p.155, citing Mont Pelerin archives). Indeed, Andrei Shleifer (1998, p.134) criticises Robbins for “not being too opposed to state ownership.” When one considers the damage done by an excess of ideology, however, including the damage to important components of neoclassical economics which went into the Washington consensus recipe, one wishes there had been a few more statesmen of Robbins’s breed.

Robbins as statesman continued to adhere to his neoclassical economics his antipathy to high inflation (1973), but he did not pose as one uniquely possessed of the sole truth. In attempting to moderate the fashion cycle in development policy, his example may be attractive. Possibly his view of himself (1971) as a synthesizer rather than a genius in economics helped him to strike a balance. Among those who are at the frontiers of economics today, where research in static microanalysis is scarcely needed, there are those who have made real contributions, like Joseph Stiglitz, but who insist that we have had a true paradigm shift in economics (2004, p.27), and that neoclassical economics is therefore dead. There are others, such as the pioneering behavioural economist Matthew Rabin, who are insistent that what they are doing is in large measure “nonradical” but rather see that their contributions will be integrated into the mainstream (2004:69). The leaders of the New Institutional Economics took that approach too, as noted.

It is much to be hoped that the Economics of Transition as a topic will soon be part of the sub-discipline of Economic History, but we accept that the demands of good history for cool and collected objectivity will be difficult whilst those who participated in the intense and world historic debates are still on the scene. This is a pity, as the narratives which are adopted will inevitably inform views on development and on reform policy for a generation. So far the victors in the theoretical and policy debate have tended to be the paradigm-shifters rather than the paradigm-extenders, pace Murrell. We have attempted to disperse a little of the murk he observed. We do not find that the economics of institutions would have allowed a change in the often painful tasks facing those who tried to move forward from the economic and social rubble of communism. Of course, much went wrong, and insights from institutions can add to our understanding of the causes. This will not happen if the new creators of a conventional wisdom yield to the inevitable temptations to the ego which occur when the outsiders replace the insiders. Robbins last critique of socialism in the course of a survey of the history of economic thought, singled out for condemnation (1976, p.146) the “limitation of speculative thought” and the regimentation of ignorant opinion.” He had in mind a Mao rather than a World Bank conference audience, but his value judgement on this is no less applicable.
5. Conclusion

Lionel Robbins made a central contribution to the long theoretical debate on market vs plan, both explicitly and by the identification of economics as the science of how societies handle scarcity which is a central contribution of the Essay. Robbins’s approach can and should be distinguished from a narrow focus on static microeconomic efficiency, a trap into which economics has fallen at times. For Robbins the study of inflation was certainly a critical part of how a society handled scarcity (sometimes by denial). This reading of Robbins is applied to the analysis of the irony that the transition to the market and prices in former centralised or "planned" economies has led to advocacy of an opposite transition from the market and prices in economic analysis. However, any account of the transitions, especially the Russian, without an understanding of the exceptional inflationary situation from 1990 will mislead: the widespread focus on the early privatisation has been misplaced. The argument that Russia should have followed the Chinese path of gradual and possibly Pareto-improving dual-track institutional development, falls on a very large number of counts, one of which that it required the retainment of a strong monopoly of authority by the Communist Party. This was almost certainly not realistic, not least of all because, for better or for worse, in Eastern Europe freedom and pluralism were highly valued as ends, as much as economic growth for East Europeans, as they were for Robbins. We conclude that the new enthusiasm for pragmatism in development policy, epitomised by the demise of the Washington Consensus, must not be allowed to harden into dogma, and that extensions of neoclassical economics are not a replacement paradigm.
References


Buchanan, J. (1965). What should economists do? Southern Economic Journal,


The Science of Things Generally?∗

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Abstract

In his celebrated Essay written 75 years ago Lionel Robbins dealt with the definition of the subject and method of economic science. He was also concerned with the demarcation between normative and positive economics. In this article we update the discussion of both issues and argue that they allow for a better understanding of the present status of our discipline and profession. We argue that economics is today too much centered on method and lacks concern with the economy and the solving of economic problems. We also claim that the demarcation recommended by Robbins still holds today and is recommended for the clarification of both the scope of economics and the objectives of economists.

1. Introduction

The purpose of this Essay is twofold. In the first place, it seeks to arrive at precise notions concerning the subject-matter of Economic Science and the nature of generalizations of which Economic Science consists. Secondly it attempts to explain the limitations and the significance of these generalizations, both as a guide to the interpretation of reality and as a basis for political practice (Robbins 1935, p. xiv).

Lionel Robbins wrote these words in the preface to his celebrated masterpiece, Essay on the Nature and Significance of Economic Science (ENSES), originally published in 1932. Writing three fourths of a century later we couldn't agree more that these are essential issues. It was not a coincidence that Robbins put them together, since they are closely related: it is not possible to assess the limitations and significance of economics without having "what economics is" clearly defined.

We use Robbins as a benchmark to think about these issues given the popular influence of his definition. We argue that Robbins’s famous definition of economics is less clear-cut than it is usually taken for and carried a normative agenda. As a result of this it was a good positive description of some parts of economic theory to the exclusion of other traditions in economics, both theoretical and empirical. The practical influence of Robbins’s definition on the practices of economics research may or may not have been important. But it is a fact that it is one of the most widely accepted definitions of economics. Here we look at some of the consequences of this.

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The optimum scope of economics depends on what are our objectives as a profession. There is no unique or correct answer to what should economics be about; it is an intrinsically normative choice. One way to present this choice is by defining the subject-matter: if we define economics as the science that studies material wealth we correspondingly throw many aspects of marriage outside the legitimate scope of economics; if we define it as the science that studies markets then much of the socialist calculation debate of the 1930s was not economics; if we define it as the science that studies how people achieve their ends given their (scarce) means, then much of old-style Keynesian theory was not economics; finally, if economics is an inquiry on everything, or it is not defined at all, then anything goes.

Before we look at the optimum, if it exists, let us look at the present. What is economics about right now? Given the remarkable scope of issues that economists have been dealing with in the last 20 or so years, the only currently agreed definition of economics seems to be that it is the science that studies things generally.322 It studies things generally but it does so in a particular way: its analytical methodology is well defined. It is a valid point that the method defines contemporary economics, which as such is not totally left undefined. Yet economics is still undefined regarding what things it is about, as opposed to how does it inquire into those things. Most academic fields have at least some agreement of what things form its subject-matter. Economists however seemingly don't. Maybe economics is not about anything in particular, in which case it is about things in general.

In fact it is undeniable that lately it has been. If it seems a fairly obvious statement to you that economics is the science that studies the economy, please bear in mind that the defining (positive) characteristic of economics as a scientific field today is not the object of analysis, but in fact the method of the analysis: it was indeed in this that Robbins was incredibly predictive, in fact beyond what he could reasonably expect. Economic theory today consists of little more than constrained optimization where the objective function is given.

Also, today an overwhelming share of the profession's resources, as measured in share of publications in professional journals, appointments in top departments, National Science Foundation grants, and so on is dedicated to the sui-generis field of "Applied Microeconomics". In the last twenty years, economists have been analyzing virtually everything: family, politics, crime, epidemiology, sexual behaviour, drug addiction, sports, music, religion, etc.

Thus victory to the Beckerian imperialist movement323. Like all colonizers, the profession is proud of its empire: more analytical competence than the "others" is the only seldom-heard justification. One wonders if the colonization reflects a lack of

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322 We borrow our title from Frank Knight, the first to make this warning (1933, p.2): "[Many] definitions come too near to saying that economics is the science of things generally, of everything that men are for practical reasons interested in". Knight didn't stop here: "Such a definition is useless and misleading" (same page).

323 In fact this may not be precise since Becker's early work had a strong theoretical content based in economic theory, for example applying consumer demand theory to families and international trade theory to discrimination, which contrasts to the modern, more empirical Levitt-style "black-box" version of economics imperialism.
expansion opportunities at home, and what shall we do when the "others" finally learn the math, but a secondary point still holds: much of this work is still good science. It is undeniable that the methods of economics are useful for shedding light on issues such as "Why do sumo wrestlers cheat" (Duggan and Levitt 2002) or "Is there a positive correlation between the use of the designated hitter in baseball and hit batters?" (Bradbury and Drinen 2007). However, despite the potential third-order general equilibrium implications on the economy, most of this work, which ranges from the economics of teen pregnancy to oenology (God knows they are related) has in practice the common aspect of having nothing to do with the behaviour of economies. If we define economics as the study of the economy, then it just ain't economics. From this perspective, it is not what society expects us and pays us to do, and more importantly it occurs at an expensive opportunity cost: that of distracting us from the real issues.

But who is in authority to decide what are those "real issues" anyway? No one is. What we would like is to persuade you that it is important for us all to think whether the cheating of sumo wrestlers and baseball are the questions we should be asking, financing, publishing in the top journals, giving the more prestigious prizes of the profession.

But to answer that, we need to make up our minds about what economics is about. The fact is that the recent choice of "everything" as a subject-matter, wherever it has happened by default or conscious choice, has consequences on the structure of the profession's research output and influence on the whole society. The current choice is a valid one, technically speaking. Economics may well be the science that studies things in general. But we should ask ourselves: Is this choice optimal?

To answer we need to know our objective function. Assuming for a moment that a representative professional economist exists, what are we trying to maximize? Economists are agents like all others if we are to be consistent with our own theories. The usual suspects are: personal income; attention from fellow economists; from other scholars; from the wider society. There is a possibility that it is genuine contributions to science, either pure or applied. Perhaps the objective is altruistic: many economists seem to have been attracted to the subject in the first place due to its promise as the systematic inquiry into how to improve society. But it may as well be the probability of winning a Nobel just for the sake of its glory.

The wise answer is of course that most economists gain utility from a combination of all the above. Some mainly want income, but will be happier if that is compatible with professional success as viewed by their peers; others are obsessed with society's improvement, and may or may not seek political positions; still others just want to make a living; a few are betting everything on the trip to Stockholm.

To some economists these objectives prove to be incompatible: John K. Galbraith enjoyed wide respect in the political world; he had influence on the general public and certainly saw himself as improving society. And his opinion was taken seriously by the academic world - if we exclude that of economists. Among his own his influence was embarrassing. Many converse cases exist.
The bread and butter of every economist however is not the object: it is the means. Economists seek their objectives through being experts in this particular field, economics. Our statement looks like a tautology, but we have transferred the question from what is the object of economists to what is the object of economics; from what do we want as individuals to what do we want economics to be? We have tried to establish these questions as equivalent. Hence this is the importance of having a clear definition of our subject-matter: If we define our science as the inquiry on things in general, then we shall have some influence in general on the society in general.

2. Friends and Foes of Lionel Robbins: Economics as a Method vs. Economics as an Object

“Economics is the science which studies human behaviour as a relationship between end and scarce means which have alternative uses” (Robbins 1935, p.6).

The definition of economics Robbins made famous was indeed methodological. It moved away from the traditional definition of economics based on the object of analysis: that of Alfred Marshall, "[The] study of mankind in the ordinary business of life ... that part of individual and social action ... most closely connected ... [with] the material requisites of wellbeing" (Marshall 1920, p.1), or that of Robbins’s teacher E. Cannan (1914), "[T]he explanation of the general causes on which the material welfare of human beings depend". But having moved away from a specific object, Robbins’s position was much less "anything goes except method" that taking his above often-quoted definition taken literally would suggest, and what accordingly is to him usually attributed.324

“It is the object of this essay to arrive at conclusions which are based on inspection of Economic Science as it actually exists. Its aim is not to discover how Economics should be pursued ... but rather what significance is to be attached to the results which it has already achieved” (Robbins 1935, p.72).

The position Robbins is selling us in the ENSES is that there was no prescriptive content in his definition. ENSES was not, Robbins argued, telling us what economics should (normatively) be about; he says he is just describing what he saw as the positive, descriptive characteristic of economics in practice.

It is very easy to misinterpret this statement by Robbins, and so it has been. The first thing to keep in mind is that what he calls economic science and economics is not what we usually associate with the word325. Robbins’s definition intended to be an

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324 The (largely Marshallian) "price theory" approach, until recently mostly associated with the University of Chicago but now prevalent elsewhere as well, would in fact be closer to the method-exclusive definition and statement of purpose of economics than Robbins’s.

325 More on this in the next section. But it is easy to see Robbins’s definition was not purely analytical. ENSES was very influenced by John Stuart Mill as well as by a number of Austrian economists, Carl Menger and Ludwig von Mises in particular. Robbins often acknowledged this, including in the footnote of the famous definition. In fact, Robbins’s approach was based in the notion that individuals act purposely, with designed intentions, a notion retained from his continental influences, in particular Carl Menger’s theory of human needs and economic goods. In defense of his early position in ENSES
objective description yes, but of what Robbins thought was legitimate positive economic research: a certain kind of economic theory, to the exclusion of other approaches. It has been commonly argued that Robbins’s definition was indeed descriptive as opposed to prescriptive, but a point which is usually missed is that it did have a normative element. This was his exclusion of other contemporary economics research traditions Robbins was perfectly acquainted with, but chose to ignore. It is essential to have in mind that the 1930s were much more methodologically diverse than today's Samuelsonian constraint-maximization paradigm may suggest; but as economists we cannot allow the present to conceal the past.

In the 1930s there was no such thing as methodologically unique mainstream; in fact there was no such thing as a mainstream at all. It is well understood that ENSES was a defense of one program competing to the orthodox position, but something the literature is not so clear about is that this was true not only for the later chapters dealing with the nature and significance of economic generalizations, but also that the usual definition was itself part of this intention from Robbins.

Examples of research programs Robbins intentionally excluded from his definition were: the a) Oxford-German Historical school326; b) American Institutionalism and its offspring the statistical business cycle school as exemplified by Mitchell's 1927 *Business Cycles*327; c) Frisch's emerging econometrics program (the Econometric Society being founded in 1932, after much publicity); d) Wicksell's and the Stockholm school macrodynamic approach; e) Sraffa and the critique of Marshallian economics; f) Marxism g) Fisher's 1911 *The Purchasing Power of Money*, which defined aggregate price determination through the quantity theory of money, and the similar tradition which existed in Cambridge, England as exemplified by Keynes's 1930 *A Treatise on Money* which with a number of other pieces of high theory made possible the fabrication of the "Keynesian Revolution" (Laidler 1999). The list could go on.

The point is that none of these traditions fit with the scarcity-optimization-choice approach of Robbins who was intentionally ignoring them in his definition. Conscientiously, Robbins was giving his enemies the worst possible treatment which can be given by an academic: indifference, to the point of defining them out of existence from the very definition of economic science.

To some extent this point would be recognized by some of his opposition: George Schuller (1949, p. 440), defending institutionalism, would point out: "[Lionel Robbins in ENSES] defends what he calls 'Economics,' 'Economic Science', 'Economic Laws', 'the propositions of Economics',... [H]e solves problems and enunciates verities not from the viewpoint of himself or his school but 'from the point of view of Economic Science". Schuller was right.
In our age where positivism and rules-based methodology are long gone (Feyerabend 1975, McCloskey 1994, Hands 2001), it is safe to say that Robbins’s definition was not a good representation of past economics, since he had no authority to exclude some traditions as non-science. Let us look if it makes a better job regarding the present:

“What is economics about? Answer: The optimal allocation of resources given ends. This catechism was promulgated in the 1930's ... [but] pause and reflect how poorly this captures the primary concerns of neoclassical economists nowadays: Nash equilibrium, strategic uncertainty, decision theory, path dependence, network externalities, evolutionary games, principal-agent dilemmas, no-trade theorems, asymmetric information, paradoxes of noncomputability”, (Mirowski 2002).

Despite the fact that Robbins’s definition was not objective, it was still descriptive, though restricted to the part of economics which Robbins considered deprived of ethical judgments, thus scientific. Robbins’s famous definition of economics aimed at describing the practical reality of this selected part of economics as it factually was - and in this endeavor he succeeded. But just because (an important part of) the present is remarkably similar to what Robbins considered to be legitimate, this does not mean we should take his definition as representative of the present. It is not, as it was never of the past. We should not be short sighted or pessimistic: just because we inhabit a Samuelsonian world it does not mean that we or our children always will. It is a safe bet that there will be more to mainstream economics, including economic theory, than constrained maximization and cost-benefit analysis.

Perhaps ironically, Robbins’s definition gained success in a period (from the early 1940's to the early 1970's) when Keynesian economics, which was based to a large extent in ad-hoc behavioural rules, thus not optimization, was part of the mainstream of the profession. But since Robbins "economics as a method" trend was not left unnoticed, and many other brilliant economists have fallen to the same appeal. The very J. M. Keynes writes in the introduction to the Cambridge Economic Handbook Series which he edits (1934, p.6): "The theory of economics … is a method rather than a doctrine, an apparatus of the mind, a technique of thinking, which helps its possessors to draw correct conclusions". At a very different methodological spectrum from both Robbins and Keynes, Gary Becker (1976, p. 5) is more focused on optimization, but nevertheless, perhaps surprisingly similar: "The combined assumptions of maximizing behaviour, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach".

But from early on a method-based definition was open to attack. Though the advance of economics imperialism has been steady, since Robbins a number of economists have shown some concern about these advances. For instance James M. Buchanan wrote: "Economics, as a well defined subject of scholarship, seems to be disintegrating" (Buchanan 1964, p.222, our emphasis). For Buchanan Robbins’s definition had become "all too pervasive." and it "served to retard...economic progress" (p. 214). Buchanan was criticizing the Robbinsian standard view at the time that the economic problem was one of allocating scarce resources among competing ends. According to Buchanan, economists should be studying exchange relationships, not allocation problems, which he dubbed "applied engineering".

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Buchanan is a remarkable example since his main contributions relate to the field of public choice theory, which was largely developed by applying "the economic method" to a variety of political problems. However, the interest of Buchanan in this approach, "Politics without Romance" as he calls it, was from the start an understanding that politics presented a fundamental constraint in the exercise of good economic policy, and that it made no sense to study only free-market decisions when the public goods are an essential feature of modern economies, where the State plays a significant part and is a major decision maker.

Robbins’s colleague Frederich Hayek, who at the time of this writing was gaining some methodological distance from Robbins (Ebenstein 2001), also wrote (1945, p.1):

“What is the problem we wish to solve when we try to construct a rational economic order? … if we can start out from a given system of preferences, and if we command complete knowledge of available means, the problem which remains is purely one of logic. That is, the answer … is implicit in our assumptions … This, however, is emphatically not the economic problem which society faces”.

In fact the Robbins definition of economics is too general, in the sense that by focusing on the method, its description of economics is also a good description of a large part of contemporary rational-choice political science, psychology and sociology. At the same time it is in a different sense too narrow, since by focusing on optimization, scarcity and choice, as it does, it is a deficient definition of economics itself. The reason is that much of economics is not based on optimization. In fact, beyond the issues Mirowski mentions, there is a lot more to which Robbins’s definition presents a deficient description of the present, even restricting ourselves to mainstream economics: randomized experiments-based economic development and labor, much of modern econometrics, macroeconomics with self-fulfilling expectations (sunspots) and multiple equilibria, non-microfounded mainstream macroeconomics (e.g. search or matching theory as well as money-in-utility), and much of experimental and behavioural economics and finance, Knightian Uncertainty, and all sorts of Bayesian priors. Just like the previous one, this list could go on.

The point is then that even a visionary methodological definition such as Robbins’s has failed to stand up to modern scrutiny. Despite its many successes, the fundamental fact is that Robbins’s definition has become obsolete.

As methods change, a methodological definition will have to change as well. A method-centred definition, even if boiled down to its bare skin is doomed to failure. Furthermore, a pure methodological definition opens way to an easy attack which Robbins warned us against: the "multiplication of activities having little or no connection with the solutions of problems strictly germane to his subject" (Robbins 1935, p.3, our emphasis). Robbins’s warning served to point out that the relevance

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328 Thus for Robbins there was a legitimate subject (object) after all as opposed to a "any object goes" analysis. It contrasts (and predated) the famous definition of page 6, where a somewhat different position seems to exist: "[A]ny kind of human behaviour falls within the scope of economic
of the method cannot be appraised without first considering the pertinence of the subject.

We argue that the optimal subject matter for economics, or political economy for that matter, is the economy. We take for granted the usual, standard definition of the economy that one may easily find in a good dictionary, with some variation: the system of human activities related to the production, distribution, exchange, and consumption of values like goods and services.

Well, you may say, this is not hot news, the horse we are beating cannot be murdered. After all, Gary Becker was accused in the late 1950s that what he was doing was not economics, and he prevailed. It is undeniable that Becker's contributions were important in a variety of senses (including to economics). It is true that good results can be obtained by applying "the economic approach" to a great variety of situations. But is this what we want?

3. Robbins Again: Economics as a Basis for Political Practice

“Gunnar Myrdal has devoted a whole book to the argument that, explicitly or implicitly, all propositions of economic theory, all classifications of happenings having an economic aspect, must involve judgments of value. I do not agree with this position”. (Robbins 1981, p.4)

Robbins considered that economics (or economic science) was concerned with positive science (in John Neville Keynes's 1890 sense), and this was separable from something different, which he labeled political economy, also related to normative propositions, or what Robbins would call ethics. Political economy was the superset of economics, including it as its positive, objective subset, but also embracing its normative dimension.

Robbins admitted that a perfect demarcation between both was possible to establish. He explains this position exceptionally clearly in retrospect, in his Ely Lecture to the American Economic Association, in an essay simply entitled "Economics and Political Economy" (1981, p.1, our emphasis): "At the beginning of my career, in my salad days, I wrote a slender essay entitled [ENSES]... I shall resume my position on the definition of the subject matter of economics … [and] Political Economy". His argument that this distinction was already present in ENSES seems true (though not with that terminology). This distinction is a fundamental one. According to Robbins, it was possible to separate the part of our field which had no judgements of value from our more general inquiry, and it was only this one which deserved the name of science, and in which Robbins was interested in the ENSES.

We have documented how Robbins’s normative program for what he considered to be scientific economics has led him to define economics in the particular way he did. While Robbins had no authority to exclude part of economic research as nonscience,
his more general point that a perfect positive-normative demarcation is possible and to be wished for strikes us as valid.

A constant in the history of economics which still holds today is that from time to time economics has been plagued with the accusation that what they do is not science but propaganda. Economists from all across the political spectrum have complained that their research was discredited by political accusations, from all sides indeed: reporters, the general public, and sometimes even their fellow economists. But economists are indeed sometimes deserving of this accusation, though the "public good" reputation of everyone suffers with the actions of some. In fact Robbins seems to have acquired this aversion to using normative statements disguised as science through his early policy involvements with several policy-making commissions (O'Brien 1988).

It is clear that economics has a normative dimension to which the natural sciences do not have a clear counterpart. Yet there is no intrinsic reason why these cannot be clearly separated. It is therefore the responsibility of the economists, when writing their research, to clarify where they are leaving the field of Economics and entering that of Political Economy.

For example, when imposing a particular functional form for a utility function which will lead to a specific distribution and policy advice for welfare maximization, a specific choice of a welfare metric is almost always normative. However, the pre-functional form model does not need to be normative, especially in regard to any particular question. Let us say, for instance, we are going to analyze the welfare effects of trade. Unlike the claim of many heterodox economists, there is nothing intrinsic in the neoclassical model for or against free-trade. In fact, and for a long time now, the appeal of the canonical neoclassical model lies as much in generating insightful results without compromise to its basic hypothesis as in its flexibility to become non-neoclassical in meaningful ways by strategically compromising some of its self-defining assumptions. In fact, the neoclassical model is the economists' best available benchmark for the building of any non-neoclassical model which is meaningful. Its flexibility has assured its survival. But its main strength is as well its main weakness: somewhere in the hyperspace of assumptions compatible with it the truth lies.

Economists are increasingly aware of this. Model building is not a sufficient condition for theory-making. Perhaps ironically, the backlash came from theoretical developments themselves; both in general equilibrium microeconomics (the Sonnenschein-Mantel-Debreu theorem from the 1970s), game theory (the Folk Theorem also from the 1970s) and in macroeconomics (the Boldrin-Montrucchio theorem from the 1980s) show that, in an important sense, anything goes theoretically. It is possible to reach any set of theoretical results by appropriately choosing the starting assumptions.

Sometimes, perhaps often, it may not be possible to choose one set of objective assumptions. In this respect the methodological approaches of both Robbins and Friedman (1953) lie dead. This does not imply we will have nothing to say in these situations. It means simply that it is up to us to admit ambiguity, even when the public
asks for a number, like with President Johnson's dubbing "ranges are for cattle. Give me a number" or the wish for a "one-handed economist".

In fact much of recent economics research has been in this direction, analyzing the modeler's uncertainty (valid to empirical work too): the establishment of bounds instead of certainties and point-identified models. This has happened mostly through the 1990s and is now in full force: simultaneously in macroeconomics (e.g. Hansen and Sargent's Robust Control 2001a, 2001b), microeconomics and industrial organization (e.g. Sutton's class-of-models approach 2002), as well as econometrics (e.g. Manski 2008). In fact, we would add, we know so little about the economy that we simply cannot afford to discard sets of hypotheses that are consistent with available evidence. This reminds us of J. M. Keynes's attributed dubbing "I'd rather be vaguely right than precisely wrong".

For the benefit of us all as a profession, we need to take preventive defense regarding the attack that so-called "positive results" are an artificial consequence of a normative choice of assumptions. This includes the choice of topics, as well as the method. A post-Keynesian would be quick to argue that the microfoundations project has a normative intention, but the converse argument about ad-hoc behavioural rules is also defensible, as most mainstream macroeconomists will point out. But at the very least recognizing this creates by itself a lower bound on what we know can we say for sure, even if for many issues that is nothing.

It is fine for each of us to have our own political views, and to some extent they can be part of our research, when clearly pointed out. But entering the normative domain implies dropping the word science, which means we have the responsibility to indicate when we believe we are crossing the border.

What does this have to do with Robbins? It does because it suggests the ultimate objective of the scientific part of economics is to provide what Robbins called a basis for political practice. It is the positive starting point from where normative propositions need to start. Being aware of the difficulty of the demarcation put forward by Robbins - a certain epistemological naiveté notwithstanding - we believe this is an important element of ENSES's legacy that is worth retaining today.

4. Conclusion: What are the Questions?

“I wish to avoid attempting to define economic theory - perhaps it's like pornography, in that you know it when you see it”, (Allen 2000, p.1).

Beth Allen is not an isolated example: economics seems to be a sui-generis field which its own practitioners are unwilling or unable to explicitly define.

But why is it relevant that the definition and scope of economics is well defined, if at all? Why should the profession care? If the practices of economists do not implicitly define it, Viner-style, who has the authority to do it? In fact the discussion about what economics should be about is mostly normative (our writings are consistent with our methodology). But what we now wish to stress is that "the divorce" between economics and its natural object, the economy, has serious consequences.
Referring to the Klamer-Colander survey of graduate students in the late 1980s, McCloskey wrote: "Two thirds, 68 percent, said [knowledge about the economy] was unimportant [to have academic success in economics]. What then, was the number who said that knowledge of the economy was 'very important'? 3.4 percent. Student physicists, not to speak of biologists and chemists and historians, would probably give different answers to a similar question: is it important for a student of chemistry to have a thorough knowledge of chemical phenomena? Is it important for a student of history to have a thorough knowledge of history?" (McCloskey 1994, p. 173). We see this problem as a coming not of the bad organization of graduate programs as it is often argued by Klamer-Colander (1990) and others, but as a direct consequence of the partial divorce between economic research and its natural object, the economy. For the record, our criticism has nothing to do with the method of economics. It has to do with the lack of a generally accepted object.

The fact that economists are confused about what their own object is leads them to overemphasize issues where it is easier to arrive at empirically convincing, if relatively unimportant, solutions. Steven Levitt, winner of the de facto second highest honour of the profession, is often quoted as considering that:

[In Levitt's view] economics is a science with excellent tools for gaining answers but a serious shortage of interesting questions. His particular gift is the ability to ask such questions. For instance: If drug dealers make so much money, why do they still live with their mothers? Which is more dangerous, a gun or a swimming pool? ... And how does a homeless man afford $50 headphones? (Dubner 2003).

Are these really the questions? A similar quote appears in the cover of Levitt and Dubner (2005), a book that sold over two million copies and had a considerable influence on the general society (compare with Milton Friedman's popular Capitalism and Freedom, which sold half a million since 1962). Maybe this is true, and there is nothing more to economics than the premise that "incentives matter", and DiNardo (2007, p.975) is unreasonable when he writes that Levitt's questions are "uninteresting because it is impossible to even imagine what a good answer would look like".

Perhaps there is nothing more we can do. Maybe there is no part of economics suitable to serve as a basis for political practice. In fact some of us seem to think we can do very little. Rubinstein (2006) in his infamous recent presidential address to the Econometric Society, writes:

We get to call ourselves economists and the public naively thinks we are improving the economy's performance, increasing the rate of growth or preventing economic catastrophes. Of course, we can justify this image by repeating some of the same fancy sounding slogans we use in our grant proposals, but do we ourselves believe in those slogans? (p.865) ... I believe that as an economic theorist I have very little of relevance to say about the real world and that there are very few models in economic theory that can be used to provide serious advice (p.881).

We are sure Ariel Rubinstein is acquainted with the Lucas critique, so we are not sure as to how much longer he expects society to fall for what he thinks to be our cheap
tricks. It is well known to all of us what McCloskey (1994, p.59) notes: "The National Science Foundation's budget for economics (about $11 million a year) would not pay the light bill for high energy physics". If Rubinstein is right, at some point society will notice.

When Robbins said (1935, p.3) that "There can be little doubt that one of the greatest dangers which beset the modern economist is preoccupation with the irrelevant--the multiplication of activities having little or no connection with the solution of problems strictly germane to his subject" we can only say that this was a sound warning. Can economics not fulfil its promise of providing a basis for political practice? In fact often, not always, the values of the economics department put us into a Panglossian world where some "unintended consequence" will throw us back to where we started: Long-Run Monetary Neutrality, the Modigliani-Miller Theorem and its corollary the "Ricardian" equivalence, the Capture Theorem, or in other words absence of out-of-equilibrium analysis through permanent instantaneous arbitrage.

Comparing Rubinstein's stance with J. M. Keynes's (1936, p. 283-4) "madmen in authority", Frisch's (1933, p.3) very opening address to the founding of Econometrica, or more to the point with Robbins himself (1981 p.2, original emphasis) we find a sharp difference of opinion:

"Mises … [argues that] human action … [is] at all times rational in the sense that given belief in the range of technical knowledge available to individuals or collections of individuals, action must be consistent. I confess that I have never been able to understand this contention: I should have thought that one of the main practical functions of economic science was to enable us to detect inconsistencies in plans."

Robbins conception of inconsistency detection, which is the basis for political practice, is the crucial point (and it has no connection with disbelief in rationality, in which Robbins was a strong believer).

In fact the real economic world is full of interesting and important, crucial indeed, questions to be solved as illustrated by McCloskey (1994, p.59):

"An economic historian ... could attest that [natural] science had little to do with economic growth until the twentieth century, and even now is modest beside the big factors of peace, literacy, shop-floor ingenuity, and sound economic policies ... An historian of public wealth could attest that most of the fall in the death rate since the eighteenth century occurred before medical science could save more people than it killed".

In fact economists hold the potential to provide the fundamental answers behind human welfare. To focus on a much more narrow example, it is today quite uncontroversial that the economists at the Fed were (unintentionally) responsible for the birth as well as the continued persistence of the Great Depression, as the current Chairman has recognized (Bernanke 2002) and is confirmed by recent scholarship (Christiano et al 2003). This is no small historical responsibility for economists as a profession, but as any monetary economist will attest, we are far from having all the answers in how to run a central bank.
The legitimate subject-matter of economics is the economy; it is a difficult, and important enough subject so that all our resources are not sufficient to solve its outstanding puzzles anywhere in the near future. An "anything goes" definition easily provides justification for an "anything goes science". The down-to-earth question we have tried to address is that there is more to economics than the premise that "incentives matter" applied to trivial issues. We need more exciting results than that the expected benefits of some decision exceed those of the expected costs, and that there is some not-so-surprising unintended consequence of some action. This type of research may attract media awareness of economists' work, true, but is the purpose of science media awareness?

It is hard to say how much Robbins's definition contributed to the shaping of modern economics; discussions of method usually have little influence on the practices of the profession, although in this case the definition certainly became well known and seems to have, at the very least, provided some justification for imperialistic advances. But as we argued it is misleading in many ways to define a field by its method: a science of "things generally" is not much of a science. Tout dire c'est dire rien.

In the Introduction we wrote that to obtain what we economists want as individuals we need to settle what we want the object of economics to be. We hope that our two answers were convincing, and that the link between them is clear: our duty as a profession is to concentrate our research on what can serve as a basis for the political practice of economic policy. Furthermore, by separating economics from political economy, science from politics, positive from normative analysis, we can establish a useful frontier which permits us not to throw the baby away with the bathwater. On this point Robbins was right, and his warning was wise. We suggest that this, not the worn-out usual definition, is ENSES's ultimate legacy.
References


Robbins’s *Nature and Significance* and the M²T Seminar: Measurement with Theory and Theory with Measurement

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Abstract

During the late 1950s, a group of young members of the Department of Economics at LSE questioned the attitude towards empirical analysis expressed by Lionel Robbins in his *Essay on the Nature and Significance of Economic Science (N&S)*. Under the influence of Karl Popper’s *Logic of Scientific Discovery*, they founded the Methodology and Measurement (M²T) Seminar. Initially the M²T Seminar was devoted to the discussion of an alternative methodology to that outlined in the *N&S*, but thereafter members of the Seminar produced theoretical critiques of existing economic theories. Later the Seminar witnessed the first programme of applied econometric studies at LSE. This paper explores the importance of the M²T Seminar during this period, its critique of the *N&S* and its longer term implications.

1. Introduction

In the late 1950s, a group of young economists at the London School of Economics got together and established the Methodology, Measurement and Testing Seminar (later abbreviated to the M²T Seminar). The objective of the work carried out in the seminar was to provide an alternative methodology to that put forward by Lionel Robbins in his *Nature & Significance* and, having established the new methodology, to use it to test economic theories. One of the founders, Dick Lipsey, later revealed that he had been unhappy with Robbins’s methodology since his days as an undergraduate at the University of British Columbia in the late 1940s:

Most influential of all the books I read in that course [on the history of economic thought] was Lionel Robbins’s *An essay on the nature and significance of economic science* (1932). Coming to economics as a renegade scientist, I was always interested in methodology: how could anyone really establish natural laws about something so complex as human behaviour? Robbins said many wise things from which I profited greatly, but when I came to his chapter on economic statistics, I balked. There I read for the first time the methodology of the Austrian school, which was, as I later learned from Mark Blaug, also the methodology of many of the classical economists. According to this methodology, which is Euclidean in conception, investigators first make assumptions that are intuitively self-evident, then apply the rules of logic to deduce propositions that may not be self-evident. In economics, the trick was to establish assumptions that really were self-evident, standing the test of introspection. Since the assumptions are obviously correct, the deductions must also be correct, no matter how unobvious they may be. If the facts

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appear to disagree with the deductions of theories, then the facts must be wrong; the deductions cannot be wrong – providing only that they are logically correct deductions – since they are based on assumptions that we know to be correct through introspection. In short, facts are used to illustrate theories but not to test them.

I read and reread the chapter. ‘This cannot be right’, I said to myself, ‘facts based on careful empirical observation must play a more important part in the development of our understanding of the economy than as mere illustrations to be cast aside whenever they disagree with the prevailing theory.’ (Lipsey, 2000, pp. 112-113).

The founding of the M²T Seminar and the methodological debate was analysed extensively in de Marchi (1988) and I do not propose to duplicate that material here. Rather I wish to explore what the ‘chapter on economic statistics’ has to say about Robbins attitude towards empirical studies and then consider the ways in which the M²T Seminar responded to the empirical aspects of the Nature & Significance.


Robbins on problems of estimation:

While there are some criticisms of induction earlier in the text, the main critique of empirical studies in the Nature & Significance comes in Chapter V, Economic Generalisations and Reality.²²³ The discussion here will consider first Robbins views on problems of estimation and then the role of empirical work in ‘testing’ theories.

Robbins raises the question of estimation and asks “Ought we not to wish to be in a position to give numerical values to the scales of valuation, to establish quantitative laws of demand and supply?” (p. 107). His response is negative:

No doubt such knowledge would be useful. But a moment’s reflection should make it plain that we are here entering upon a field of investigation where there is no reason to suppose that uniformities are to be discovered. The “causes” which bring it about that the ultimate valuations prevailing at any moment are what they are, are heterogeneous in nature: there is no ground for supposing that the resultant effects should exhibit significant uniformity over time and space. No doubt there is a sense in which it can be argued that every random sample of the universe is the result of determinate causes. But there is no reason to suppose that the study of a random sample of random samples is likely to yield generalisations of any significance. (p. 107)

A simple illustration should make this quite clear. Let us take the demand for herrings. Suppose we are confronted with an order fixing the price of herrings at a point below the price hitherto ruling in the market. Suppose we were in a position to say, “According to the researches of Blank

²²³ I have worked with the Revised and Extended version of the Nature & Significance, Robbins (1935).
(1907-1908) the elasticity of demand for the common herring (Clupea harengus) is 1.3; the present price-fixing order therefore may be expected to leave an excess of demand over supply of two million barrels”. How pleasant it would be to be able to say things like this! How flattering to our usually somewhat damaged self-esteem vis-à-vis the natural scientists! How impressive to big business! How persuasive to the general public!

But can we hope to attain such an enviable position? Let us assume that in 1907-1908 Blank succeeded in ascertaining that, with a given price change in that year, the elasticity of demand was 1.3. Rough computations of this sort are not really very difficult and may have considerable utility for certain purposes. But what reason is there to suppose that he was unearthing a constant law? No doubt the herring meets certain physiological needs which are capable of fairly accurate description, although it is by no means the only food capable of meeting these needs. The demand for herring, however, is not a simple derivative of needs. It is, as it were, a function of a great many apparently independent variables. It is a function of fashion; and by fashion is meant something more than the ephemeral results of an Eat British Herrings campaign; the demand for herrings might be substantially changed by a change in the theological views of the economic subjects entering the market. It is a function of the availability of other foods. It is a function of the quantity and quality of the population. It is a function of the distribution of income within the community and of changes in the volume of money. Transport changes will alter the area of demand for herrings. Discoveries in the art of cooking may change their relative desirability. Is it possible reasonably to suppose that coefficients derived from the observations of a particular herring market at a particular time and place have any permanent significance - save as Economic History?

Now, of course, by the aid of various devices it is possible to extend the area of observations over periods of time. Instead of observing the market for herrings for a few days, statistics of price changes and changes in supply and demand may be collected over a period of years and by judicious “doctoring” for seasonal movements, population change, and so on, be used to deduce a figure representing average elasticity over the period. And within limits such computations have their uses. They are a convenient way of describing certain forces operative during that period of history. … If we wanted to be helpful about herrings we should never dream of relying on the researches of the wretched Blank who was working in 1907-8. We should work the whole thing out afresh on the basis of more recent data. (pp. 107-109)

I suspect that most readers would agree that if more recent data were available, it would be best to re-estimate the elasticity and not rely on a study of one year from

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330 Robbins does not explain how we are to collect data on changes in demand. Whether he was aware of the Identification Problem or not is unclear, though E.J. Working had published his article in 1927.
over twenty years earlier. Thus the wretched Blank is something of a red herring and might well feel ‘wretched’ as a result of Robbins taking his results out of context.

Lipsey (1963, p. 159) criticises this argument against empirical estimation as follows:

The above argument runs somewhat as follows: ‘I can think of no reasons why the relationship in question (e.g., the relation between demand and price) should be a stable one; I can think of several reasons why it should not be stable; I conclude, therefore, that in the real world the relationship will not be stable, and attempts to observe whether or not it is stable can be ruled out on a priori grounds as a waste of time.’ This argument is rather a curious one, and it appears although it may not have been the author’s intention, that it could have been used to stop at an early stage the investigations which produced observations of practically every stable relation that we know.\(^{331}\)

**Quantitative Economics Bad – the ‘wretched’ Dr Mitchell:**

Having disposed of the estimation of elementary concepts such as demand and supply functions, Robbins goes on to consider empirical studies of more complex phenomena:

If it is true of attempts to provide definite quantitative values for such elementary concepts as demand and supply functions, how much more does it apply to attempts to provide “concrete” laws of the movement of more complex phenomena, price fluctuations, cost dispersions, business cycles and the like. In the last ten years there has been a great multiplication of this sort of thing under the name of Institutionalism, “Quantitative Economics”, “Dynamic Economics”, and what not; yet most of the investigations involved have been doomed to futility from the outset and might just as well never have been undertaken. The theory of probability on which modern mathematical statistics is based affords no justification for averaging where conditions are obviously not such as to warrant the belief that homogeneous causes of different kinds are operating. Yet this is the normal procedure of much of the work of this kind. The correlation of trends subject to influences of the most diverse character is scrutinised for “quantitative laws”. Averages are taken of phenomena occurring under the most heterogeneous circumstances of time and space, and the result is expected to have significance. (p. 112)\(^{332}\)

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\(^{331}\) Lipsey then shows that this method of argument could be used to demonstrate that the laws of gravity, Boyle’s Law and the normal curve of error are all a priori impossible.

\(^{332}\) At this point, Robbins launched into a fierce attack on Wesley Mitchell’s *Business Cycles*. Having praised the book for its “magnificent collection of data”, he criticised Mitchell for attempting to derive an average length for the business cycle by combining data across seventeen countries. He concludes his censure with: “Certainly he has provided the most mordant comment on the methodology of ‘Quantitative Economics’ that any of its critics could possibly wish.” (pp. 112-3). Incidentally, Robbins had already had a go at Mitchell in Robbins (1929c), a review of a book by Josiah Stamp, in which he wrote “We might protest that Sir Josiah gives too much countenance to the pseudo-novelties of Dr. Wesley Mitchell and the institutionalists.” (p.250). Robbins’s reaction to Mitchell’s work will be discussed below.
The discredit of the Historical School in Germany is very largely due to the failure of its members to understand the currency disturbances of the War and the post-War period. It is not improbable that the utter failure of “Quantitative Economics” to understand or predict the great depression may be followed by a similar revulsion. It would certainly be difficult to imagine a more complete or more conspicuous exposure. (p.115, fn 1)

Mitchell’s own views on Quantitative Analysis were spelt out in his Presidential Address to the American Economic Association in December 1924 (Mitchell 1925). Starting from a suggestion by Alfred Marshall that the “higher and more difficult task” of quantitative analysis “must wait upon the slow growth of thorough realistic statistics.”, Mitchell suggests that great progress had been made in (i) the quantity and quality of statistical material being collected (ii) the steady improvement in the technical methods of statistical analysis and (iii) the development of ‘statistical laboratories’, such as the NBER. However, he is pessimistic about much progress being made by applying quantitative analysis to pure economic theory “if the pure theory we have in mind is theory of the type cultivated by Jevons, or by Dr. Marshall himself. … What we must expect is recasting of the old problems into new forms amenable to statistical attack.” (p. 3). Having commented positively on Henry Moore’s empirical studies of demand curves and pointing out that they were not based upon detailed economic theory, he argues that future statistical studies will be based on data derived from real markets and be less linked to existing economic theories. He concludes:

If my forecast is valid, our whole apparatus of reasoning on the basis of utilities and disutilities, or motives, or choices, in the individual economy, will drop out of sight in the work of the quantitative analysts, going the way of the static state. The “psychological” element in the work of these men will consist mainly of objective analysis of the economic behaviour of groups. Motives will not be disregarded, but they will be treated as problems requiring study, instead of being taken for granted as constituting explanations.

The obsolescence of the older type of reasoning in economics will be promoted by the change which is coming over our thinking about human nature. Psychologists are moving rapidly toward an objective conception and a quantitative treatment of their problems. Their emphasis upon stimulus and response sequences, upon conditioned reflexes; their eager efforts to develop performance tests, their attempts to build up a technique of experiment, favour the spread of the conception that all of the social sciences have a common aim—the understanding of human behaviour, a common method—the quantitative analysis of behaviour records; and a common aspiration—to devise ways of experimenting upon behaviour. (pp. 5-6)

This change will have an impact on economists and economic theory and he predicts:

The literature which the quantitative workers are due to produce will be characterized not by general treatises, but by numberless papers and monographs. Knowledge will grow by accretion as it grows in the natural
Mitchell’s view of the future for economic theory, with the introduction of psychological experimentation and the dominance of quantitative analysis is clearly at odds with Robbins’s arguments in the *Nature and Significance* and, while he confines himself to criticising Mitchell’s statistical work here, he must have been aware of Mitchell’s alternative vision of the future of economics.

**Realistic Studies Good:**

Beyond criticising Mitchell and the Historical School of Germany, there are no further examples of “Quantitative Economics” cited before Robbins turns to write more positively about “realistic studies”.

But what, then, are we to say of the more detailed kind of realistic studies? Having ascertained the persistence of the fact of scarcity, the multiplicity of factors of production, ignorance of the future, and the other qualitative postulates of his theory, is the economist then excused from the obligation of maintaining further contact with reality?

The answer is most decidedly in the negative. And the negative answer is implicit in the practice of all those economists who, since Adam Smith and Cantillon, have contributed most to the development of Economic Science. It has never been the case that the exponents of the so-called orthodox tradition have frowned upon realistic studies. (p. 115)

There is no formal definition of what constitutes a “realistic study”, but Robbins considers what may be expected of such studies under three headings:

The first and most obvious is the provision of a check on the applicability to given situations of different types of theoretical constructions. As we have seen already, the *validity* of a particular theory is a matter of its logical derivation from the general assumptions which it makes. But its *applicability* to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation. (pp. 116-7)³³³

³³³ Although Robbins does not provide a formal definition of what he means by “realistic studies”, in a footnote at the end of this section (p. 118, fn 1) he writes: “Professor Jacob Viner’s *Canadian Balance of International Indebtedness* and Professor Tausig’s *International Trade* provide classical examples of this kind of investigation.”. These two studies will be discussed later in this section of the paper.
Secondly, and closely connected with this first function of realistic studies, we may expect the suggestion of those auxiliary postulates whose part in the structure of analysis was discussed in the last chapter. By inspection of different fields of economic activity we may expect to discover types of the configuration of the data suitable for further analytical study. (p. 118)

And, thirdly, we may expect of realistic studies, not merely a knowledge of the application of particular theories, and the assumptions which make them appropriate to particular situations, but also the exposure of areas where pure theory needs to be reformulated and extended. They bring to light new problems. (p. 118)

However, these studies have their limitations and must be held in a proper relationship to theory:

Realistic studies may suggest the problem to be solved. They may test the range of applicability of the answer when it is forthcoming. They may suggest assumptions for further theoretical elaboration. But it is theory and theory alone which is capable of supplying the solution. Any attempt to reverse the relationship must lead inevitably to the nirvana of purposeless observation and record. (p. 120)

Robbins’s choice of Viner (1924) as an example of a good ‘realistic study’ is interesting, as Viner’s methodology was very different to his own and made a strong case for the role of induction in economics.334 In Viner (1917), he argued that “… the abstract economists exaggerate the possibility of obtaining a vast deal of knowledge from a system of deductions derived from an initial set of four or five propositions.” (p. 235). Later in the article he argues that:

Political economy has been too often described as if it were merely a ‘pure’ or a priori psychological theory of value and distribution. Of much greater importance to the economist than any ‘pure’ theory is the knowledge and understanding of the concrete facts of production, distribution, consumption, of the whole economic situation with all its causal processes. (p. 251)

To support the case for induction, he notes that:

Even those economists who were most decided in their contention that the abstract deductive method was the only one available to the economist made considerable use of these inductive methods in their economic researches. In some cases their chief contributions to political economy were predominantly inductive in character. (p. 253)

The sub-title of Viner (1924), which was omitted in Robbins’s footnote, is An Inductive Study in the Theory of International Trade and Viner states at the outset his views on induction and deduction:

334 See Hutchinson (1994) for a detailed analysis of Viner’s views on methodology.
The deductive method in economics, when its general psychological assumptions have not been too much divorced from the true psychology of the market-place, and when the generalizations concerning the environmental data which are used as premised have also been reasonably accurate, has brought valuable results. Deductive conclusions would differ, perhaps, from the results obtained by an inductive investigation of the same problem with a complete record of facts to work from; but they would differ only because they were incomplete. The differences would tend to disappear as the deductive results were supplemented by the results of inductive analysis resting on inference from the facts omitted in the fundamental abstractions of the deductive study. This assumes, of course, that the abstractions of a valid deductive theory are not inconsistent with the facts. They should be abstractions not in the sense that they are untrue, but in the sense that they do not tell the whole truth.

In the field of the theory of international trade, as in all other fields in the social sciences, there are aspects which in practice can be investigated by only one of the methods; and there are other aspects in which both methods can be more or less completely applied, and the results of the one corroborated or discredited by the results of the other. In developing a complete theory both methods must be used; and the utilization of the one method as a means of verifying the other is made possible, not only in that portion of the field to which both can be applied but practically throughout the field, by a study of the success with which one part of the theory obtained by means of one method can be made to fit in with the other portions obtained by the other method or by both together, so as to form a complete and consistent system satisfactory to the reasoning intelligence. (pp. 7-8)

However, in operation, Viner’s approach may be seen as ‘induction in the service of theory verification’. Thus, having outlined the theories of earlier writers in Chapter IX, Changes in Relative Price Levels and the Adjustment of Balances of Indebtedness, he concludes:

The part played by gold movements in the adjustment of the Canadian balance of trade to Canadian borrowings from abroad has already been submitted to an inductive examination, whose results showed that, if allowance is made for the controlling influence exercised on gold movements by the peculiar system of outside reserves of the Canadian banks, gold movements operated in the manner indicated by Thornton, Mill, and their followers. The next chapter is devoted to an inductive analysis of the part played by price changes in the mechanism of adjustment of the Canadian balance of trade. It should provide a further test of the validity of the deductive theory as expounded Thornton and Mill, and it should, moreover, serve to verify the amplification of the theory made by Professor Tausig with reference to the operation of the sectional price levels. (pp. 211-2)
Tausig (1929) is also concerned with ‘induction in the service of the confirmation of economic theory’:

The pure theory of international trade constitutes only the initial stage toward the ascertainment of the things we wish in the end to know. Such indeed is the case with the whole of the pure theory of economics, which can be called “the” theory rather than “a” theory, solely on the ground that no other has been put forward which is generalized, consistent, intellectually satisfactory. After all, what we wish to attain is not a neat logical structure, but an understanding of the actualities. We must inquire whether the facts conform to the elaborated theorems; must make sure that nothing has been forgotten in the premises, nothing has been erroneous in the reasoning. It is incumbent on the economist to follow a procedure similar to that used in the natural sciences. The physicist or biologist who believes that he has hit on a generalization which conforms to the regularities of the external world uses it merely as a working hypothesis. The economist should do the same for his hypotheses. In economics this task is more difficult than in most natural sciences, because the economist is debarred from the method which had proved in them by far the most serviceable, that of experiment. He cannot experiment; he can resort to observation only. Observation, however, he must utilize to the utmost—through history, description, statistics. In so doing he may or may not find confirmation of his hypotheses. Quite probably he will find partial confirmation only; he will have occasion, to a greater or lesser extent, for revision, amendment, restatement. (pp. vi-vii).

In summary, while Viner and Tausig are less deferential towards pure theory than is Robbins, both start from economic theory and use induction to ‘verify’ or ‘confirm’ these theories. Neither could be accused of ‘measurement without theory’, unlike Professor Mitchell.335

**Robbins’s Training and Knowledge of Mathematics and Statistics:**

It is possible that among the reasons why Robbins’s response was *a priori* rather than analytical may include his training and research experience.336 As far as mathematics is concerned, in the first year of the B.Sc.(Econ) degree that Robbins attended at LSE, students had to take either Mathematics or Logic and Scientific Method. Howson (2004, p. 416) notes: “Robbins chose the last and took the course offered year after year (1905 to 1941) by Dr. Abraham Wolf, The course covered both formal logic and inductive logic or scientific method. … In his second year Robbins attended, by choice, Wolf’s lectures on general psychology and on the history of philosophy. His notes of the latter show he stayed the course up to Kant.” However, in his autobiography (Robbins 1971) he writes of his contact with G.H. Hardy at New College in 1927-28 and some study of mathematics:

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335 Robbins criticism of Mitchell’s lack of interest in theory was echoed later in Koopmans criticism of Burns and Mitchell (1946) in (Koopmans 1947) for practising ‘Measurement without Theory’.

336 As I do not have access to the Robbins Archives, my ‘evidence’ is based on published material and not on private papers and other such material, so it is therefore extremely speculative though hopefully of some interest.
He [Hardy] did not think much of the contemporary mathematical economics: I remember that when I showed him Bowley’s *Groundwork*, he was distinctly uncomplimentary, holding the exposition to be deficient in elegance and the results lacking in depth – a quality to which he attached great significance. But he thought well of the possibilities of the subject and took pains to procure me some instruction in calculus which, although it has never led to positive contributions on my part in that section of the field, at least gave me enough understanding of the language not to feel utterly lost amid this sort of construction. He confirmed me, too, in my belief in the importance of theory as the basis of all fruitful science, however dependent on eventual empirical testing. (p. 118)\(^337\)

Turning to his statistical training, I obtained the following information from Susan Howson\(^338\):

As far as statistics is concerned, among the lectures students taking the Final examination for the BSc (Econ) in 1923 had to attend as preparation for the compulsory three papers in economics was Bowley’s course in general statistics. You can find a description of the course in the LSE Calendar for 1922-3 (page 110). As you will see the course was in two parts (Elementary Statistical Methods [mainly descriptive statistics] and More Advanced Statistical Methods: I think LCR only attended some of the first half.

The details of the two courses are as follows:

(a) **Elementary Statistical Methods.** Syllabus: Collection of data, definition and tabulation. Statistical groups; arithmetic average, mode, median, mean and quartile deviation. Statistical series in time; trend and fluctuation. Weighted averages. Index numbers. Simple methods of measuring correlation. Application to statistics of population, production, consumption, commerce, prices, wages, income and capital. The main sources of these statistics, their character and meaning.

(b) **More Advanced Statistical Methods.** Syllabus: Elementary mathematical treatment of variation and error, especially in their application to averages, sampling, description of groups and series and correlation, in relation to economic and social investigations. Methods of interpolation.

This confirms how little statistical theory Robbins would have obtained from this source, even if he had attended both parts of the course. I have been unable to find evidence that he undertook any further studies in statistics.\(^339\)

\(^337\) In her presentation at the Conference, Sue Howson noted that Robbins also had some knowledge of trigonometry from his experience as an artillery officer in World War I.

\(^338\) I am grateful to Sue Howson for the information quoted, which was supplied by an e-mail dated 7 November 2007.

\(^339\) However, Howson (2004, pp. 433-6) reports on correspondence between Robbins and Nathan Isaacs in May 1931 in which Robbins “mentioned, for instance, the recent findings by the French economist Jacques Rueff of a 95 percent correlation between indices of British unemployment and real wages.”, and has further discussion of Robbins interpretation of these correlations. This suggests he had some knowledge of this statistical technique.
In terms of research experience, upon graduation Robbins was exposed to the collection and interpretation of economic statistics, as he spent the year following his graduation in 1923 working as a research assistant to William Beveridge “to help him bring up to date for a second edition the tables and information in his well-known *Unemployment: A Problem of Industry.*” (Robbins 1971, p.96) In addition to updating the statistics, Robbins was able to steep himself in the trade-cycle theories of the period and in reading German newspapers two mornings a week to extract industrial information for the editor of *The Economist.* At the end of 1929 he became a member of the Editorial Committee of the London and Cambridge Economic Service and was involved in the detailed examination of economic data at the regular editorial meetings. While this would have given Robbins experience at looking at economic statistics and interpreting them, his comments on the statistical analysis of the data suggest he had serious misconceptions of what was involved and what could be achieved. He seems to have assumed that the statisticians were claiming to have ‘proved the truth’ with the numerical results they produced, rather than presenting estimates that were subject to sampling errors.

Looking at the articles, books and book reviews he published up to 1935, one is impressed both by his productivity and the wide range of topics he covered, particularly in his book reviews. While a number of the articles involve questions of economic theory (for example, Robbins (1928a, 1930b, 1930c and 1934a)),

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340 Beveridge (1930) provided a generous acknowledgement of Robbins’s contribution to the new edition: “I had … secured the services of Mr. L.C. Robbins (now Professor of Economics in the University of London) to work over the book and the new material available since 1909. He very soon reported to me his conclusion that nothing was to be gained, commensurate with the labour involved in bringing what had been written in 1908 verbally and formally up to date. … He advised me-and convinced me-that I should reprint the original book without change …. ”(pp. ix-x). “My indebtedness to Mr. L.C. Robbins, when he was my research assistant, for suggesting the form of this new edition, has already been acknowledged; in that capacity he did much of the work also of bringing up to date the figures in the Statistical Supplement.” (p. xi). It is a tribute to Robbins maturity that, as a young graduate, he was able to persuade the older and more experienced Beveridge of his views.

341 O’Brien (1988, pp. 170-8) contains a translation from the French of a talk Robbins gave in 1934 in which he describes in some detail his work with the London and Cambridge Economic Service. He returned to the attack on statistical estimation, with the wretched Dr Blank here being replaced by Professor Schultz, and his “employment of subtle statistical methods”. He continues: “If one proves that the elasticity of demand for sugar was, from 1880 to 1925, 0.85, what does that signify?” (p. 174). Professor Schultz might well have responded that he had not *proved* that the elasticity was 0.85, but that 0.85 was the point estimate and that he could provide a confidence interval around that figure with some probability of being correct. Robbins’s view of the value of statistical studies and Schultz’s work seems to have been higher in 1930 when, in his Inaugural Lecture, he wrote “On the one hand recent developments of statistical technique have made much more probable the realisation of the hope, long entertained by theoretical economists, of providing numerical values for the abstract quantities of pure theory. Already a considerable body of important work has been done on the investigation of the elasticity of demand for staple agricultural products in America. And there is little doubt that we are only at the beginning of the movement. No doubt the results of such investigations have profound limitations and must be treated with the greatest caution. Still, when Dr. Schultz assures us that if during a certain period there had been a small increase in the sugar tariff, prices would have risen by 86 per cent of the tariff, we may feel that the precision is dubious, but we must also feel that to have arrived at the roughest approximation to the truth is an important step forward.” (Robbins, 1930a, p. 21).

342 A list of his articles was obtained from AEA (1961), pp 414-5. This was expanded by reference to the Bibliography of Robbins’s publications in O’Brien (1988), pp. 219-21. I have omitted some letters to the *Economist*, Prefaces and Introductions to other writers’ books and some articles in Bank Reviews and other magazines.
had a topical relevance to the economic situation at the time he was writing and these topics are treated in a very theoretical manner, with little or no reference to the relevant statistics. Thus, the discussion of the dynamics of capitalism (Robbins 1926a) and the economic effects of variations of hours of labour (Robbins 1929a) is strictly theoretical. Where there is an appeal to supporting empirical evidence, the effect is minimal: for example, in discussing some arguments for protection (Robbins 1931a) the sole empirical material is provided in a footnote reference to a chart in another author’s work; similarly, in writing about underconsumption theories of the trade cycle (Robbins 1932b) he provided in a footnote reference to a chart showing the effects of the German hyper-inflation; in discussing the probable consequences of a stationary population in Great Britain (Robbins 1929b) he quotes a calculation made by Professor Cassel concerning the rate of increase in the world’s gold supply during the period 1850-1910 required to keep prices stable. Finally, in a discussion of the optimal theory of population (Robbins 1927a), he criticises Alexander Carr-Saunders somewhat simplistic attempts to use changes in real income per head to determine the optimality of population size and refers to “question-begging statistical analysis” (p. 129). Overall, there is a clear choice of theoretical analysis over statistical evidence in this body of work that is consistent with Robbins’s views on ‘empirical economics’ as expressed in the *Nature & Significance*.

However, Robbins analysis of the Great Depression in Robbins (1934c) shows that he could use statistical data when he deemed it necessary. This book has a 36 page Statistical Appendix and the text contains twelve charts and seven tables. The author provided a passionate free market explanation of the causes of the depression and marshals an impressive set of statistics to support the case. While alternative explanations are considered, they are rejected on theoretical or common sense grounds and statistical evidence that might question their validity is not presented. One might therefore argue that Robbins is presenting economic data to support or ‘verify’ his theory, rather than looking at alternative explanations and ‘testing’ his theory against them.

To what extent did Robbins change his view of the relationship of empirical studies to theory? In Robbins (1938) he attempted to clarify a number of methodological issues and wrote:

I do not think that there is a single professional economist living who would dispute that the appropriate method of economics is the

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343 Given Robbins was quoting a calculation based on data for a period that ended nineteen years before his article, one might feel that there is an inconsistency here compared with the treatment allotted to the wretched Dr Blank.

344 Robbins acknowledges his debt “to Mr. Stanley Tucker, Rockefeller Research Assistant in the Department of Economics, without whose loyal and unremitting labours in the preparation of the charts and the statistical appendix publication at this stage would have been impossible.” (p. viii).

345 O’Brien (1988, p. 183, note 141) cites a number of not unfavourable reviews of the book. Robbins later expressed dissatisfaction with the book: in Robbins (1971) he compared *The Great Depression* unfavourably to his *Economic Planning and International Order* writing of the latter “Unlike *The Great Depression*, this is not a work I should now wish not to have written.” Later in his autobiography he wrote “I had long realized that my earlier diagnosis of the causes of the Great Depression had missed the mark in not recognizing sufficiently the paramount role played by the catastrophic contraction in incomes brought about by the deflation due to the volatility of the then existing credit mechanism” (p. 188).
construction and development of hypotheses suggested by the study of reality and the testing of the applicability of the results by reference back to reality. (p. 346)

Although the word ‘testing’ appears here, it should be noted that it is not the ‘theory’ that is to be tested, but its ‘applicability’ and this is confirmed later in the article:

In a subject so wide as economics it is natural that there should develop some division of labour, that some should specialise on the more theoretical developments, some on description and verification. (p. 346)

In a 1959 article Robbins seems to be more positive about the role of empirical studies:

But theory is one thing, practice another. And although the view that the purpose of empirical studies is the testing of theoretical hypotheses is now very widely accepted, it must regretfully be acknowledged that a great deal of realistic study still tends to be wasted for lack of any such clearly conceived task. It is no longer proclaimed in the streets and in university seminars that all facts are born free and equal. But much so-called empirical investigation proceeds as if this were indeed the case.

This is surely a great pity. For, as economic theory becomes more advanced and complicated, the need for testing becomes more and more imperative. I think it is a real reproach to economists in the present age that there is so much untested theorizing. (Robbins, 1959, p. 1360)

However, he then cites Viner (1924) work as an example of good practice, which suggests that he still saw testing as being inductive.

Later, in Robbins (1998), the transcription of his famous lectures on the History of Economic Thought that was made during 1979-80 and 1980-81, he is reported to have expressed a somewhat negative view of Sir William Petty’s statistical work:

Petty obviously attached very great importance to quantitative measurement. Petty subscribed to the Baconian philosophy—or thought he subscribed to the Baconian philosophy—which expressly said that if you collected enough facts they then suggest to you a series, and hence systematic science—which we know since the times of Whewell and Popper and other distinguished writers is standing scientific method on its head. You invent in science hypotheses, you test them for their logic, and then you test them—you try to falsify them or you try to verify them, it is a matter of words—by collecting relevant facts. (Robbins 1998, p. 58)

In this article, a new character appears to replace the wretched Dr Blank: “We should all agree that the mythical Schmoller student, who, after five hundred pages of statistical investigation, decided that the price of pork in the Eastern District of Berlin in the years 1895-1900 was “determined by supply and demand”, had been wasting his time.” (p. 349)
I doubt that Popper would have agreed that whether you try to falsify hypotheses or verify them is merely a matter of words.

These quotations suggest that throughout his career Robbins held to the view that the assumptions of economic theory were self-evident and that the role of statistical studies was to test their ‘applicability’; to verify their relevance in a particular context and not to ‘test’, in the sense of ‘falsify’ the theories themselves.

3. The M^2T Seminar: Methodological and Empirical Studies\(^\text{347}\)

The young economists who were seeking to reject Robbins’s methodology needed an alternative and they found their inspiration in the work of Karl Popper. Important here was the link between Kurt Klappholz in the Economics Department and Joseph Agassi, one of Popper’s younger colleagues in the Philosophy Department.\(^\text{348}\)

Initially, a group of the economists met with Agassi, who expounded Popper’s methodology to them.\(^\text{349}\) Klappholz and Agassi (1959), while ostensibly a review article criticising Schoeffler (1955) and Papandreou (1958) from a Popperian standpoint, began with a detailed critique of Robbins’s methodological position.\(^\text{350}\)

Even though Robbins’s aim was “not to discover how Economics should be pursued” (p. 72), it is clear that methodological prescription was prominent in his Essay. We note, in particular, his suggestion that there should be an \textit{a priori}, water-tight, separation between economics and other sciences. This amounted to the \textit{a priori} decision to view certain variables of economics (e.g. tastes and technology) as essentially “exogenous”, i.e. as not determined within economic models, rather than as not yet explained by any existing economic model. Robbins did not, of course, object to attempts to explain “tastes” psychologically or sociologically, but according to his view no explanation of them should contain (endogenous) economic variables. Secondly, he denied \textit{a priori} the possibility of discovering “quantitative” laws in economics.

\(^{347}\) I was an undergraduate at LSE from 1956 to 1960, studying for the B.Sc.(Econ) and specialising in Computational Methods in the Statistics Department. I took the compulsory introductory courses in Microeconomics and Macroeconomics and a further compulsory course in Applied Economics in Part I of the degree and then specialised in Mathematics and Statistics in my Final Year. Upon graduating in June 1960 with First Class Honours, I applied for and obtained an Assistant Lectureship in Economics at LSE and began teaching in October 1960. I was immediately invited to join the M^2T Seminar and I remember that in the early sessions we discussed Dick Lipsey’s work on the Phillips Curve.

\(^{348}\) Klappholz had a strong interest in philosophy and was involved in teaching in the B.A. Honours in Philosophy and Economics degree, which commenced in 1958-59.

\(^{349}\) By the time I joined the M^2T Seminar in 1960, Chris Archibald was keeping a detailed record of the discussion at the seminar for distribution to the participants. I do not know whether he began this process at the beginning of these meetings, as the seminar archives seem to have completely disappeared over time. For many years I kept my papers from the M^2T Seminar post-1960, but they were discarded during some office move and I confess that I have forgotten most of the detail of the seminars I attended. As a result, I shall concentrate on giving something of the flavour of the seminar, rather than a detailed history. For a fuller discussion, see de Marchi (1988).

\(^{350}\) They also criticised Terrance Hutchinson, who in Hutchinson (1938) had criticised the \textit{Nature & Significance} from a Popperian point of view. This led to an exchange of views, see Hutchinson (1960) and Klappholz and Agassi (1960).
We find these suggestions unacceptable because they are designed to limit the field of argument *a priori*. As a hypothesis the view that tastes are independent of other economic factors could be subject to critical discussion; it could be countered by an alternative hypothesis describing conceivable relations between income, prices, tastes, etc. Robbins apparently did not advance his view that tastes are exogenous as a hypothesis, but rather as a methodological rule designed to delineate the area of economic discussion. The rule, in its turn, was based on more general considerations-on his general view of economics as a science.

Robbins regarded scientific laws as universal statement known with certainty to be true; laws were statements of the necessary aspects of natural phenomena. … How was knowledge of these laws obtained? It is derivable neither from history nor from controlled experiments, but rather obtained by a process of “deduction from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience. … They are so much the stuff of everyday experience that they have only to be stated to be recognised as obvious” (pp. 78, 79).

Everyday experience might perhaps tell us that tastes are exogenous, but it certainly cannot tell us that tastes, or any other factor, must be regarded as exogenous in all future theories. The view, however, that economic laws are certain and must be based on everyday experience, does somehow entail that some factor or other must be exogenous. (pp. 60-1)"

To carry out the programme in Methodology, Measurement and Testing, it was necessary to examine models to derive predictions to be tested and it was necessary for these predictions to be tested. While Popper had a strong influence on the methodology of the M*T Seminar, the interests of the participants differed, as did their responses; some were mainly interested in a methodological investigation of economic theories, while others were more concerned with the process of testing.

**Methodological studies:**

Considering the methodological studies concerned with establishing whether economic models generated testable hypotheses, two are of particular interest.\(^3\)

(1) Archibald versus Chicago:

One of the most ambitious methodological exercises was presented by Christopher Archibald in Archibald (1962) in an analysis of the reaction of Chicago economists to Chamberlin’s theory of Monopolistic Competition (Chamberlin 1933). He pointed out that whereas Friedman (1953) had argued that theories should be judged by their

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\(^3\) Two other studies worth mentioning are Archibald (1960), a critique of the attempt by Stein (1958) to test the Marginal Productivity Theory of Wages, and Klappholz and Mishan (1962), in which the authors argued that it was not possible to derive testable hypotheses from identities, as some macroeconomic modellers seemed to think was the case. This latter point was developed in more detail in Lipsey (1972).
predictions and not their assumptions, Chicago economists had criticised Chamberlin on the basis of his assumptions, rather than his predictions. After a considerable amount of mathematical analysis of the comparative statics of the Chamberlin model he concluded that:

It is obviously extremely difficult to prove that a model is empty, and, indeed, this model is not completely empty: all I can claim is that it yields, so far as I can discover, no qualitative comparative static predictions, and that this is the consequence of a general defect, the incomplete specification of the demand relationship within the group. (p. 19, italics in the original)

He concluded with a criticism of the Chicago approach to Chamberlin:

Perhaps the most serious criticism of Chamberlin’s critics is that they have concentrated upon a priori discussion of his assumptions, instead of on discovering what facts were needed to give the theory content, and endeavouring to obtain them so that they might test it. (p. 21)

The outcome of Archibald’s work here and in his examination of the predictions of Marginal Productivity Theory (Archibald 1960) was that the problem of determining the mathematical signs of the second order partial derivatives involved in comparative static analysis made it difficult to obtain predictions from the models that he had examined. There was also the problem that the ‘verifying evidence’ might be consistent with a number of competing theories and hence not provide evidence to discriminate between them.

(2) Lancaster and Qualitative Predictions:

The second study was carried out by Kevin Lancaster, the mathematician in the group and in Lancaster (1962) he carried out a theoretical analysis of the possibility of predicting the signs of the dependent variables in a system of simultaneous equations from a knowledge of the signs of the coefficients in the system. He was concerned with the basic question of whether comparative static analysis could yield predictions that were testable. He showed that if the signs of the coefficients were arranged into a matrix, it was possible to derive mathematical theorems to generate numerical counting rules that would determine the necessary and sufficient conditions for whether the system does yield predictions. Lancaster’s results were based on only the functional form of the equations in the model and did not depend on algebraic details, such as whether the equations were in log or linear form, so they were completely general. In the context of the aims of the M2T Seminar, this was an important paper, as it provided the theoretical filter for assessing whether the results of comparative static analysis could be tested. That it has not had a long term influence on economic methodology is, I suspect, due to two factors. First, many

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352 The response from Chicago was brief and somewhat dismissive of Archibald’s efforts (see Stigler 1963, Friedman 1963 and Archibald 1963 for his response).
353 He had a BS in Mathematics and Geology from the University of Sydney.
354 One sign of the times of this paper is that the models chosen to illustrate the theoretical results were based on ‘Keynes and the Classics’ debate that predated the ‘Keynesians versus Monetarists’ that was to follow.
economic theorists are not interested in testing the predictions of their theories and so have no need to carry out this analysis. Secondly, the introduction of time lags into econometric models made macroeconomic models dynamic and reduced the interest in comparative static analysis.

**Empirical studies:**

Turning now to the empirical studies, it is necessary to put them into context in terms of the state of statistical teaching and applied research in the late 1950s. In view of its later emergence as the leading centre for econometrics in the United Kingdom, it is important to note that econometrics had not reached the Economics Department at LSE by the time of the M^2T Seminar. It is true that Roy Allen had taught a course entitled ‘Problems in Econometrics’ in the sessions 1946-47 and 1947-48, the course being listed in the LSE Calendar under ‘General Economic Theory’ and was ‘Recommended for postgraduate students’.

However, there were very few postgraduates, compared with the number of undergraduates, and no taught Masters degrees, as both PhD and MSc students were examined by thesis. Allen’s course was not taught after 1948.

For undergraduates, a course ‘Introduction to Econometrics’ appeared in the 1951-52 session, but it was taught in the Statistics Department and offered as a course primarily for students in that department who chose the option of Economic Statistics within the Special Subject of Statistics. It was open as an option for students in other Special subjects, but it is doubtful if many economics students attended. In the first year it was taught by a theoretical statistician, Geoff Penrice, but when he left at the end of the year, it was taken over by Harold Booker, whose interests were in national income accounting and the sources of economic statistics, and George Morton, who taught game theory and linear programming. Although the course was expanded from 10 to 30 lectures and the syllabus remained the same, the econometric content was significantly reduced. In 1953-54, as there was no econometrician at LSE, Wilfred Corlett from UCL was brought in to beef up the econometric content, although economic data sources, game theory and linear programme continued to figure prominently in the course. The course continued in this format into the early 1960s.

For students in the Economics Department, there was no course in statistical theory that would give an exposure to estimation and testing. Instead, there were courses in ‘Applied Economics’, which involved taking economic problems, developing the relevant theory and then illustrating the application of the theory to the problem by

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355 The syllabus was: “An account will be given of the work of Tinbergen and Frisch on econometric business cycle research and of the work of Leontief on import and output relations in the economic system. The emphasis will be as much on the statistical methods used as on the economic implications of the results.” (LSE Calendar 1946-47, p. 170.) This was probably the first formal course in econometrics taught in the UK.


357 His publications are listed in the References.

358 For further discussion of the econometric situation at LSE at this time, see Gilbert (1989).
looking at economic data in the form of tables and charts.\textsuperscript{359} In this context, the idea of testing theories was both novel and being undertaken by economists who had little formal training in what they were doing.

**Lipsey and the Phillips Curve:**

One early study was Lipsey (1960), a further study of the Phillips Curve that Bill Phillips suggested he carry out. Lipsey (1997b, p.ix) recalled. “This I did and spent a year working out regressions (at least my research assistant, June Wickens, did on a mechanical calculator at the rate of about two a day) and trying to understand the curve in terms of microeconomic theory.” (p. ix).\textsuperscript{360} Lipsey’s paper is interesting from several points of view. First, he showed that by running linear regressions on the full data set with non-linear functions of the rate of unemployment, such as \((1/U), (1/U^2)\) and \((1/U^4)\), it was possible to replicate very closely the results that Phillips had obtained through a very idiosyncratic process of averaging the data and fitting the curve by eye. The fact that a Phillips Curve could now be estimated using standard multiple regression analysis opened up the economics profession to the development of a ‘Phillips Curve Industry’, which generated estimates of the Curve for any country where the relevant data existed.

The second interesting feature of the paper was that, whereas the original Phillips paper had not provided any theory to explain the existence of the relationship, Lipsey devoted considerable space to developing a theoretical model and attempted to explain the cyclical ‘loops’ in the observations around the fitted curve. The criticism of ‘measurement without theory’ was a characteristic feature of the M\textsuperscript{2}T Seminar.\textsuperscript{361}

**Lipsey and Steuer testing Kaldor:**

Nicholas Kaldor’s response to Phillips’ original article was to suggest an alternative theory (Kaldor 1959):

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\ldots \text{the rise in money wages depends on the bargaining strength of labour; and bargaining strength, in turn, is closely related to the prosperity of industry, which determines both the eagerness of labour unions to demand higher wages and the willingness and ability of employers to grant them.} \ldots
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\textsuperscript{359} The nature of these courses may be judged from Phelps Brown (1951), which was the recommended textbook for such a course.

\textsuperscript{360} June Wickens, a talented mistress of the Doolittle Technique, was the legendary research assistant in the Economics Department who was responsible for producing most of the regression analysis involved in early econometric work at LSE. Regression analysis in the Economics Department almost ground to a halt when she married a graduate student, A.G. (Bertie) Hines, and moved to the University of Bristol.

\textsuperscript{361} Bernard Corry, another active member of the Seminar, recalled this emphasis on theory: “… So we were into applied work, and then any visitors that came to the school were always invited to the M\textsuperscript{2}T. Quite famous Americans, that the Young Turks tried to, I wouldn’t say tear to bits, but show up methodologically. They were always presenting applied work on, I don’t know, measuring productivity growth; and then people would say, ‘What theory are you testing? All you are doing is empirical work.’” (Corry, 1997, p. 189).
It is when investment is high that profits are high, and it is in periods of rising total production and rising productivity that profits are rising. Such periods in turn are periods of low unemployment, and also periods of falling unemployment. (p. 137 in Lipsey and Steuer)

Kaldor did not supply any evidence to support the suggestion that the correct relationship was between wage changes and profits rather than unemployment. Despite this, Lipsey and Steuer (1961) set out to test Kaldor’s hypothesis against the Phillips Curve as “Kaldor’s counter-explanation gives us a chance to subject the Phillips theory to a serious test in which it has a real chance of being refuted. The test is therefore important from the point of view of our confidence in the Phillips theory.” (p. 139). That is, instead of merely ignoring Kaldor’s hypothesis (or challenging him to produce empirical evidence), within M^2T methodology it was seen as an opportunity to ‘test’ Phillips’s theory.

To do this they first had to formulate Kaldor’s hypothesis in a form that could be tested, as Kaldor had not specified any particular functional form. Having settled this matter, regressions were run using equations that included terms representing both profits and unemployment and it was shown that the effect of unemployment dominated. This was true both using aggregate data and, in general, for disaggregated data covering ten industries. What is of interest here is that although the main theoretical work on testing Nested and Non-Nested Hypotheses did not happen until the 1980s (see Pesaran 1987), Lipsey and Steuer were applying one of the recommended procedures for dealing with rival hypotheses in this paper. It is unfortunate that this example of testing rival theories against one’s own, rather than simply ignoring them, did not receive more attention from other economists carrying out empirical studies.

### A potential teaching development:

There were plans to introduce the approach of the M^2T Seminar to graduate students and in the 1962-63 Session there appeared a Course and Seminar: Case Studies in Measurement and Testing in Economics. After an introductory series of lectures that covered methodological matters, with references to Friedman (1953), Koopmans (1957) and Klappholz and Agassi (1959), there was a one-lecture idiots’ guide to hypothesis testing. A few of the case studies reflected the work of the M^2T Seminar, such as Archibald (1960) and Lipsey (1960), but the rest involved a mixture of qualitative and quantitative testing. The course was repeated in 1963-64 and then dropped. Whether this course might have had a lasting impact on graduate teaching

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362 The course consisted of ten lectures and seven seminars, to be taught by Bernard Corry, Dick Lipsey, Maurice Peston, Max Steuer and Jim Thomas. The syllabus was “Introduction: the place of measurement and testing in the development of economic theory and a survey of the simple statistical tools used in subsequent case studies; testing the theory of the firm; measuring demand; measuring macro-economic relations and testing macro-economic models of income and employment; testing the Cobb-Douglas production function; testing macro-economic models of distribution; testing theories of international trade.” (see the LSE Calendar for 1962-63, p. 302 for details of the Recommended Reading).

363 This was my first ever lecture at LSE and it consisted of an attempt to give a non-technical outline of setting up the Null and Alternative Hypotheses and testing statistical theories. The material was obvious to the US graduates present, all of whom seemed to have taken statistics courses, but new to the British graduates, most of whom were innocent of the subject.
at LSE is hard to assess, as it was not continued beyond the initial offering, for reasons to be outlined below.

**The decline of the M^2T Seminar**:364

The distinctive Popperite flavour of the work of the M^2T Seminar did not last much beyond the mid-1960s as a result of two factors. First, changes in the personnel in the Department of Economics produced a new attitude towards measurement and testing. Secondly, there were major changes in the structure of teaching that put more focus on statistical theory and econometrics.

**Exeunt (fere) Omnes**:365

The process of change was accelerated by the departure of many of the key founder members of the seminar. Dick Lipsey and Chris Archibald moved to the new University of Essex366 and Maurice Peston to a newly formed Department of Economics at Queen Mary College, London, shortly to be followed there by Bernard Corry. Kelvin Lancaster went to Columbia University as a visitor and stayed there. There were also changes in the senior members of the Economics Department, with the semi-retirement of Lionel Robbins and his preoccupation with the work of the Committee on Higher Education (Robbins 1963). Frank Paish and Sir Arnold Plant, neither of whom had been particularly positive with respect to modelling, retired in 1965. The Robbins Seminar ceased to be the main focus for staff and graduates and more specialised seminars appeared, reflecting the division of the subject into narrower areas.367 One important arrival at the LSE in 1963 was Denis Sargan to a Readership in Econometrics in the Statistics Department and his influence will be discussed below.

**Changes in degrees and courses:**

There were dramatic changes in the structure of both undergraduate and graduate degrees in the early 1960s:

**Changes in the BSc(Econ):**

Up until 1963, the structure of the BSc(Econ) (the main undergraduate degree) had been a very general two-year Part I with 8 examination papers and a specialised Part

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364 Dick Lipsey has expressed some reservations about the conclusions drawn by de Marchi (1988) that one of the reasons the M^2T Seminar ended was disillusionment on the part of the participants over the difficulties of applying Popper’s methodology to testing in economics. He has pointed out that the crucial factor was the opportunity to move to the new University of Essex and develop a serious graduate teaching programme that he had tried but failed to convince his colleagues at LSE was important. After the administrative rigours involved in opening a new university and writing his textbook, he returned to working on applied economic studies not unlike those carried out in the M^2T Seminar.

365 (almost)

366 As the University of Essex was set up as a direct result of the recommendations of the Robbins Report (Robbins 1963), it might be argued that Robbins was partly responsible for speeding up the demise of the M^2T Seminar.

367 One of the earliest of these was an informal seminar set up by Roger Alford, Victoria Chick and Jim Thomas and focussing on Monetary Theory. It was commonly known as the ‘Chick Shop’.
II, involving 5 examination papers. In Part II it was possible to take a course in Mathematical Economics, but only as an option along with Public Finance and most students chose the latter. The Corlett econometric course could be taken, but only as an option to the famous three-hour extended Essay that was seen by high-flyers as an opportunity to show First Class quality, so again was not taken by many students. In 1963 a new BSc(Econ) was introduced that reversed the weighting, so that now Part I was a general programme with 5 examinations and Part II and two-year specialist course. There were still some compulsory history and politics courses in Part II, but now students had to take courses in mathematics and statistics. There was more scope for economics students to take outside options, including the Economic Statistics courses that were now taught by Denis Sargan and Bill Phillips for the specialists in the Statistics Department.

The taught MSc:

Denis Sargan switched to a Chair in the Economics Department in 1964 and began to have a significant effect there. When a new taught MSc in Economics was introduced in 1964, it offered two options: the first (Economics), which was taken by most students, involved a compulsory course in basic, non-technical econometrics, while the second Mathematical Economics and Econometrics) offered a programme of more advanced technical courses.

The changes in both undergraduate and graduate teaching programmes meant that there was now an emphasis on nearly all students having some exposure to statistical theory or econometrics and now courses in Applied Economics tended to have more references to applied econometric work, rather than the examination of charts and tables of statistical data.

The final years of the M²T seminar:

After the departures outlined above, the seminar continued for several years with Max Steuer as the Chair, but now renamed as the ‘Wednesday Seminar’. Over time the nature of the seminar changed. The visitors from the United States who were invited to attend were less interested in discussing methodology and often more experienced in carrying out applied econometric studies. While it continued to look for theoretical underpinning for applied analysis, rather than mere ‘empirical work’ it lost its early Popperian fervour. With the disappearance of the Robbins Seminar the Wednesday Seminar became the main general economics seminar and continued as such for some time. However, with the growth in the number of special area seminars being developed in the Economics Department, a general seminar lost some of its appeal and the seminar finally closed.

4. Conclusions

The establishment of the M²T Seminar in the late 1950s was very much an LSE phenomenon that reflected the lasting power of Lionel Robbins’s methodological position as presented in the Nature & Significance. His argument that deductions from self-evident assumptions did not need empirical analysis strongly discouraged an interest in statistical analysis and econometric testing. His further negative attitude to statistical estimation, which seems to have been based on a lack of knowledge of
statistical theory, was extreme. To the extent that he did approve of ‘realistic’ studies, these were to ‘verify’ economic theories and not to test them.

The M^2T Seminar was an LSE phenomenon in the sense that while the methodological studies represented a novel attempt to apply Popper’s methodology to testing economic models and showed how difficult it was to derive any testable predictions, even from simple economic models, the testing was limited by the statistical knowledge of the participants. While some members of the M^2T Group, such as Kevin Lancaster, were well trained in Mathematics, none of them had a serious background in Statistical Theory, with the exception of Dick Lipsey, who had taken the equivalent of five semesters of statistics at the University of British Columbia. The result was a degree of ‘learning by doing’ in the empirical work, which often showed considerable ingenuity, as in Lipsey and Steuer’s testing of Kaldor’s Profits Hypothesis.

We live in a time where there are few problems of data shortage, computing power is virtually unlimited and there is a vast output of applied econometric studies. A large proportion start from an equation (or set of equations) that are not derived from a formal theoretical model, but presented as being ‘plausible’ representations of common sense assumptions about what might affect the phenomenon being considered. Looking at many of these studies, one might feel some nostalgia for the days of the M^2T Seminar and its criticisms of ‘Measurement without Theory’ and insistence on both ‘Theory with Measurement’ and ‘Measurement with Theory’.

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368 As well as by a shortage of time series data and a lack of serious computational power.
References


The Making of Robbins’s Essay

Susan Howson∗

It may seem a little odd to be talking about the origins of Lionel Robbins's Essay on the Nature and Significance of Economic Science (1932, 1935) at the end of a conference devoted to the book and its impact on our discipline. But it may nonetheless be interesting at this stage to go back to what Robbins thought he was trying to do at the time he was trying to do it after our discussions as to whether he succeeded. I have already written on this before (Howson 2004), in an article on the origins of the book, where I used the information I have gathered in writing Robbins’s biography to ascertain the ideas and influences that went into his writing of the first edition of his Essay. His papers (which will eventually be coming to the library here at LSE) include notebooks containing his lecture notes and reading notes from the 1920s and early 1930s (including a set of lecture notes which Robbins himself labelled 'first draft of final form of N & S') as well as correspondence and reviews of the book, all of which throw light on the drafting of the first edition and on the changes Robbins made in preparing the second edition only three years later. Since I focussed on the first edition of Nature and Significance in my article I shall now concentrate on the changes he made in the second edition in response to criticism and reviews of the first edition. I cannot avoid repeating some of what was in the earlier article about the first edition but I shall confine it to summarizing the main conclusions I drew about the origins of the first edition.

The first thing to be said about the making of the first edition is that the information about Robbins’s education and early career confirms his own statement about the origins of the book. He wrote in his autobiography (Robbins 1971, 148):

> 'The beginnings of ... [the Essay] lay some time back in the past. The fundamental textbook on the elements of economics when I was a student at LSE [in 1920-3] was Cannan's Wealth; and the first chapter of this truly excellent work was devoted to elucidations which defined its subject-matter in terms of the causes of material welfare. Shortly after I joined the staff as a teacher [in 1925], I was put to lecture to a special course for Army officers on the Economics of War and readiness for war [having been an artillery officer on the Western front in the First World War]; and I had not been long engaged on my preparation for this task before it was borne in upon me that, although what I was going to say leant heavily on economic analysis as I had been taught it, it yet fell completely outside Cannan's definition - indeed he went specially out of his way to deny that war and its accompaniments fell within its scope. This puzzled me very much; and my perplexities increased when I reflected on the number of activities in which I was especially interested, concerts, theatrical performances ... [etc] which had nothing to do with material welfare but which yet certainly had an economic aspect. What then was the common factor to which our technique was applicable? Gradually it dawned on me that ... the underlying fact which made so many different activities and

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relationships susceptible to economic analysis was the scarcity of the means with which they were concerned and not the materiality of the objectives. There was nothing especially original about this conception. I was deep in the study of the marginal utility theory of value at the time, especially in the works of the Austrians and Philip Wicksteed. Even if they did not say in so many words what I was beginning to say to myself, my formula followed naturally from their explanations. What was it all about if not the behaviour of people disposing of goods and services which in the last analysis were in some way limited in supply rather than freely available - in short, conduct influenced by scarcity?'

The Robbins papers contain his undergraduate notes on Cannan's lectures, his own notes for that course on the Economics of War given in 1926 and also his notes for two sets of lectures on the 'Elements of Economics' (ie introductory economics) - a short course when he was a temporary lecturer in Oxford in 1924-5 and a longer one he gave for first year students here in 1926-7.369

Those notes for his introductory lectures in 1925-7, along with reading notes he made as an undergraduate in 1922-3 show that his longstanding concern was to demarcate economics from other social sciences, especially political science as taught by Harold Laski, which he did not think was scientific at all. (In his second and third years at LSE Robbins specialized in the history of political ideas under Laski's supervision.) He told J.M. Clark thirty years later:

'That I eventually crossed over and made Economics my chief interest was due directly to the fact that I felt it threw light on problems of politics I had been studying from the other side. .... The Nature and Significance was always intended to be a sort of preliminary manifesto designed to forestall the criticism that I did not know where the borderline between the different disciplines really lay.'

His notes for his lectures at Oxford (where he was a fellow of New College 1927-9) include those for a course which he called (following John Stuart Mill) 'Unsettled problems in theoretical economics' and gave in Hilary Term 1928-9 (ie January to March 1929). These notes show that by the end of 1928 he had arrived at his famous definition of economics. He said in those lectures - after criticizing the various definitions of economics given by Alfred Marshall, Edwin Cannan, Henry Clay and A.C. Pigou, and explaining how his own doubts about Cannan's definition in particular had begun - that his own approach was to look for

'not a definition of economic which classifies out a certain set of activities which it labels economic but one which indicates what aspects of human activity in general are significant to the economist.

Now if we think of human activity in general there are two features which seem to have significance from our point of view.

369 There is a mistake in my 2004 article (423): not then realizing that Robbins had given any lectures in Oxford in 1924-5, I assumed the notes for the short course were for a one-term course on the Elements of Economics he gave in Oxford in Michaelmas Term 1927; they were in fact for forestry students (Oxford University Gazette, January 16, 1925).
In the first place the ends are various.

Secondly the means of attaining them are often very limited & are capable of alternative uses.

It is in this aspect of human activity - activity as condition[ed] by the fact of scarcity that I think the economist is interested. He is interested in the way people individuals and societies economize - that is dispose of the things which are scarce & how changes in the scarcity of these things (whether coming from the demand side or the supply side) affect their activities.'

The emphasis on the positive versus the normative aspects of a science comes from this early concern with demarcation. One of his objections to political science was that it kept mixing analysis of what is with pronouncements on what ought to be. He made his first published statement of his view that economics must not include ethics in a review of R.G. Hawtrey's book *The Economic Problem* (1926) in *Economica* in 1927 (and referred his students to it in his Oxford lectures). From this comes, of course, his objection to basing theoretical arguments on interpersonal comparisons of utility since they inevitably involve value judgments.

As far as the *Austrian* influence on the making of Robbins's *Essay* is concerned, the material in Robbins’s papers corroborate Denis O'Brien's (1990,414) conjecture that the 'primary source was undoubtedly Wicksteed's *Common Sense [of Political Economy]*, while he [Robbins] drew from the Austrians precisely those elements which coincided most directly with what he had drawn from Wicksteed.' Although Robbins read Mises's *Die Gemeinwirtschaft*, in German, in 1923 or thereabouts, was very impressed with its arguments against socialism and began to translate that part of that book in 1925, there are far more favourable mentions of Wicksteed in his notebooks, especially in one labelled 'Method etc. Early flounderings 1923 --', than of Mises. Similarly, although Robbins recommended Mises's book *Nation, Staat and Wirtschaft* (1919) in his Economics of War lectures (along with Pigou's *The Political Economy of War* [1921]), there are virtually no references to Mises, but lots to Wicksteed, in his 1929 'Unsettled problems' lectures where *he is developing his arguments leading to his definition of economics*. (As I put it in 2004 [page 426], 'My conclusion to this point is that by the end of 1928 Robbins had found his definition of the subject matter of economics, after brooding about it for some years. It owed most to Wicksteed and there was nothing particularly "Austrian" about it.'). Furthermore, when you look at Robbins’s lecture notes for a course entitled 'The nature of economics and its significance in relation to the kindred social sciences', which he first gave at LSE in the Summer Term of 1930 (by which time he had returned to LSE as professor of economics) and which are the notes he labelled 'first draft of final form of N&S', you find that while there are differences of style and emphasis the structure and argument of the published *Essay* is essentially the same as the lecture notes *but there are almost none of the many footnote references to Austrian
Another conclusion of my article was that Robbins’s views on the methodology of economics developed over time: that it was only after he had solved his demarcation problem to his satisfaction that he began to concern himself with the methodology of economics. Having previously accepted uncritically the conventional views of scientific method he had learned as an undergraduate, in 1929-31 he began to clarify his views on method, a process which can be seen in his notes for his lectures in those years and in correspondence in the summer of 1931 when he was ill with chickenpox.

The 'Unsettled problems' lectures given in Oxford at the beginning of 1929 did not discuss the methodology of economics. The major difference between those lectures and those on 'The nature of economics and its significance' given at LSE in 1930 (and again in 1931 and 1932) is that the latter included two lectures on the subject, the first espousing a deductivist view of economics, the second on the (very limited) uses of induction to suggest and test the assumptions from which economic generalizations followed by deduction. (The notes on 'Definitions' for the first of the later lectures follow those for the earlier lectures very closely: indeed they comprise the same arguments and examples written out in more complete sentences. In the notes for the LSE lectures there follows a section on 'Economics and ethics', where he criticized Hawtrey and J.A. Hobson as he had done before for not keeping positive and normative analyses separate, which became the second chapter of the published Essay, and then a discussion of the meaning of economic quantities which became the third chapter. What became the sixth chapter was entitled 'Economics & political theory': it focussed first on the invalidity of using the law of diminishing marginal utility to justify policies of income redistribution, since it involved interpersonal comparisons of utility, and second on the doctrine of laissez faire, stressing its limitations and the resulting need for public goods.) The arguments of the methodology lectures appear, with considerable elaboration and revision, in the third and fourth chapters of the published Essay on 'The nature of economic generalisations' and 'Economic generalisations and reality'.

At this point I can start to move from the origins of the first edition of the Essay to begin to consider the changes from the first to the second edition, since it was in the methodological chapters that he made significant changes when he revised the book and he made few changes elsewhere.

As Jim Thomas has already mentioned at this conference, Robbins as a first year undergraduate chose to study Logic and Scientific Method instead of mathematics - and hence attended the lectures given here by Abraham Wolf year after year (from 1907 to 1941). The description of scientific method he gave his first year students in 1926-7 is similar to what Wolf taught.371 (Then and later Robbins insisted that economics was a science - even if at that time he still thought of it as 'the science of

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370 Robbins first met Hayek here in January 1931, when Hayek came to give four public lectures on Prices and Production. After the lectures Hayek was invited to come back to the School as a visiting professor for the academic year 1931-2, at the end of which he was offered the vacant Tooke Chair, which he held until 1950.

371 Wolf's Textbook of Logic (1930) consists of two parts, formal logic and inductive logic, which respectively incorporate his earlier textbooks, (1926) and (1925).
material satisfactions' - whose aim was to establish generalizations about economic phenomena, such as - a favourite example - the quantity theory of money.)

'There are two main methods of scientific procedure.

Firstly there is what is known as the inductive method. It consists in the deduction of general statements from the examination of particular instances. You make an exhaustive study of the habits of pigs and you say pigs reach certain proportions at certain parallels of latitude. ...

Secondly there is what is known as the deductive method. Starting from certain proved generalizations or certain hypotheses which you are valid you deduce what must happen if the forces described in these generalizations are combined in isolation. You do this in physics when you deduce the behaviour of projectiles on the assumption which is never true in fact that influences like wind and imperfections in the shell are absent. You do the same thing sometimes in economics when you imagine eg what would be the effect upon wages or interest rates of a certain kind of invention. It is a method which comes in useful when actual experiment with natural forces is out of the question. On these lines the [quantity] theory of money was thought out - in the main - so that when the great modern experiments - if you can dignify [wartime and postwar] inflation by such a name - came economists were able to predict the results with almost quantitative certainty.'

And he emphasized that 'both methods are equally legitimate.'

But when he gave the Nature and Significance lectures in the summer of 1930 he gave deduction definite priority.

In the first of the two lectures, 'Method. Simple Statement of Principles', he stated:

'The business of theory or abstract science is to make generalizations - to lay down propositions which transcend the particular and describe general uniformities. Such generalizations are sometimes described as laws and our business as I conceive it is to enquire into the nature of these generalizations and the logical justification of which they are capable.'

Economic generalizations, like those of other sciences, are both hypothetical and vary in their applicability (the more general being the more widely applicable). To illustrate he took 'a very simple generalization concerning demand[:] If price rises demand diminishes' and pointed out that this is just an implication of the definition of the demand function. 'So long as we assume that the conditions of demand exhibit a negative connection with price the thing is given in our initial assumption.'

He then made a strong claim:

'Now all exact generalizations of Economics are of this nature. They are merely the explanation of the logical consequences of your initial
assumptions. Given the assumptions and assuming a correct logic they are unassailable.'

Hence 'The truth or falsehood of the laws is merely a matter of logical consistency' and 'in a sense, pure economic analysis is simply a matter of exercises in logic, a matter of squeezing the utmost drop of implication out of assumptions which are given.' He referred here explicitly to Keynes, (1917). He then turned to initial assumptions and argued against any dependence on the concept of economic man, using Wicksteed's arguments, and on any ideas drawn from the discipline of psychology.

His second lecture, 'The Place of Induction', began:

'It follows I think from what I was saying last time that the functions of such [empirical] studies are twofold:

(a) Firstly it is in this way that we are enabled to select our assumptions,
(b) Secondly it is in this way that we test the suitability of our theories.'

He went on to claim that when we are considering, for instance, the theory of capital exports or the quantity theory of money, 'We are not testing the truth of the theory - the accuracy of the deductions. We are testing its adequacy to explain certain situations. We are asking whether the assumptions are suitable. We are finding out how to use the theory.' His example was the quantity theory of money.

'We may start e.g. with a very crude formulation of the quantity theory.
If the quantity of money increases the value of money falls. We examine a period during which the quantity of money has been increasing and we find that the value of money has risen. The theory is not wrong. Other things have not been equal. It is not sufficient. We examine other things. We find that the work for money to do has increased. We reformulate. If the quantity of money increases faster than the amount of work which money has to do - the volume of trade [-] then the value of money will fall. We take other cases. We find that velocity of circulation is important. We introduce assumptions taking account of this. Then we find that the term value of money is ambiguous. Which price level do we mean. One mode of measurement gives one result another another. We discover that the whole theory needs recasting to take account of this. And so on.'

He concluded on induction: 'It suggests assumptions. It provides a means of testing & reexamining assumptions when these have been combined in suitable permutations.' He then launched (as he was to in the Essay) into a spirited attack on (American)

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372 He did admit, though, that in selecting assumptions 'We never approach facts with perfect passivity. ... The facts suggest assumptions only to attention that is theoretically active.'
institutionalism, the basis of his attack being the standard philosophical argument about the validity of inductive (including statistical) inference.

As I have already remarked the two lectures on method became the two methodological chapters (4 and 5) of the first edition of the *Essay*. These expounded Robbins’s view of the logical character of economic theory, its lack of dependence upon psychology or 'economic man' and the subsidiary role of empirical studies. As I show in my article his arguments had been sharpened since 1930 by an exchange in the summer of 1931 with an old friend, Nathan Isaacs, whom he had first met in the army in 1916. Isaacs, who was enough of an amateur philosopher to be a member of the Aristotelian Society and publish in its journal (1931), had tried to persuade his friend of the usefulness of induction and the need to test scientific theories. Robbins had responded by pointing to the utility of economic theories derived by deduction, notably the quantity theory of money. In the book he added as examples of fruitful economic theory Hayek's trade cycle theory.

Robbins prepared the second edition of the *Essay on the Nature and Significance of Economic Science* during the academic year 1934-5. He tried to take account particularly of the criticisms of his friends; he did not take much account of the criticisms of most of the critical reviewers. He explained in the preface, which is dated May 1935, that he was not going to change chapter six, which had been the target of most attacks on account of his denial of the scientific legitimacy of interpersonal comparisons of utility, and defended himself against the common and inaccurate charge that he had recommended economists abstain from policy debates.

He had received many complimentary and encouraging letters from colleagues and friends, even those who were critical of the methodological position. Among the colleagues were Hugh Dalton, Evan Durbin and Harold Laski (all men of the left, active members of the Labour Party). (The letters are in the Robbins papers.) Laski told Robbins that his book was 'a brilliant piece of logical and systematic argument', which made him (Laski) proud to have had Robbins as a student and even more as a colleague. His one significant criticism was that he thought chapter two ('Ends and Means') 'slays the slain' since 'the economic historians who matter ... have long deserted Schmoller's camp'. Dalton, who had first taught Robbins as a first year undergraduate, still regarded Robbins as his protegé (and had worked to get Robbins appointed professor over Laski's objections) and had read the manuscript before it was submitted to the publisher in February 1932. He had taken Robbins to task for, for instance, arguing that interpersonal comparisons of utility were always empirically unverifiable and had teased him for the 'usual superlative bouquets to Mises' in his footnotes. But he had approved of much of the methodological argument. He thought chapter two was 'full of good fun & good sense' and he was sympathetic to the criticism of 'economic welfare' as used by either Cannan or Pigou, though 'not yet convinced' it should be given up, since he still believed 'the proposition that "A is better off than B" seems to mean something.' The manuscript Dalton was commenting on does not survive but it is clear from comparison of Dalton's screed and the published version of the book, that some footnotes were altered or omitted. Durbin had been a student of Robbins’s at New College Oxford in 1927-9, reading for PPE after a first degree in zoology. He had like Dalton 'stayed up to excessive hours for the sole purpose of finishing your book - a tribute I rarely pay to anyone.' With his background in zoology he was critical of the methodological
standpoint, where he thought Robbins was guilty of oversimplification and should also have discussed the relation between pure and applied economics. He was very impressed with the last chapter on the significance of economics and the peroration against irrationality.

Jacob Viner, who had been a good friend of Robbins’s since they met in Oxford in 1927, told him that 'There is almost nothing in it with which I would take serious issue, and with most of it I am in violent agreement. It is an excellent piece of work and I am going to make my students read it next year.' Mises said he intended to use it in the discussions in his seminar in Vienna - and he did. He had also written that he 'probably need not tell you that I agree with your arguments throughout' (my translation of the German original). The German liberal economist Wilhelm Röpke offered to translate the book into German; his one criticism of the book was that Robbins, like Max Weber, had gone too far in his attitude towards value judgments. Röpke had to report in March 1933 that he could not find a publisher in Hitler's Germany (which Röpke, as a liberal economist, soon had to leave).

Robbins had met Mises in September 1931 when Mises was in London for the British Association for the Advancement of Science meetings. He had met him again and got to know him better at a 'world conference of economics' in Berlin in May 1932, organized by the Berliner Tageblatt newspaper, on international trade and capital movements (Robbins spoke on the latter) and when Robbins visited Austria in 1933, first in April to give a lecture to the Nationalökonomische Gesellschaft and then in July and August for a long holiday with his wife and children at St Gilgen in the Salzkammergut when they were visited by the Haberlers, the Machlups and Mises. Robbins had met both Gottfried Haberler and Fritz Machlup in London, in 1930 and 1932 respectively. (There was one more long Austrian holiday at Thumersbach near Zell am See in the summer of 1935, when 'a whole succession of our Viennese friends ... visited' the Robbinses including Mises for a week; but by this time Robbins had completed the revisions to the first edition of the Essay.) During these visits he and his friends discussed the Essay: as far as one can tell the younger Austrian economists, whose views were different from Mises', criticized the two methodological chapters. In March 1935 Robbins told Machlup that he was

'quite sure now that certain statements in the fourth chapter of my book were couched in terms which, although I do not think they were wrong, were certainly very liable to give rise to misapprehension and in the second division [sic], which I hope I shall complete this [Easter] vacation, I intend to make quite a number of modifications. I suppose it is natural that the statements which I now think to be least aptly expressed are just those statements which have escaped the notice of those of my critics who attack me so angrily. I owe much more to conversations with you and Haberler on this matter than to anything which has so far been published in any journal. There was, however, what I thought was a very good note in the last number of the 'Review of Economic Studies' which I thought got completely home so far as my use of the term "tautology" was concerned.'

373 The sources of this information are in my biography of Robbins.
374 Robbins to Machlup, 14 January 1934, Machlup Papers 61-1, Hoover. Institution Archives.
The note was by Terence Hutchison who had recently graduated at Cambridge. Hutchison's note (1935) rightly criticized economists (including Robbins on occasion) for criticizing other economists' theories as tautologous, since any deductive theory must be a tautology. As he said, 'A tautology, in the use of modern logicians, is an analytic proposition which cannot conceivably be false because its truth is assured by the, in a certain sense arbitrary, process of assigning definitions.' He pointed out the contradiction in Robbins's remark (page 111) that it was 'the inevitability of economic analysis which gives its very considerable prognostic value. ... given the data in a particular situation, it can draw inevitable conclusions as to their implications' since 'An inevitable implication is a tautology, and can, by its nature, no more prognosticate anything than can the multiplication table.'

Robbins had already told Machlup a year earlier that

'I don’t feel inclined to retract anything so far as the critics are concerned. But the book [the Essay] is out of print & I shall take the opportunity in the second edition of explaining the section we discussed in our drive to Ischl. I think I can meet Haberler & Kaufmann without sacrificing anything fundamental & incidentally this may clear up certain misunderstandings with regard to my own attitude to the empirical element in general.'

The nature of the conversations with Haberler and Machlup (I shall return to Kaufmann shortly) is indicated in subsequent letters from Haberler (in the Robbins papers). In the spring of 1934 Haberler was sure that we shall easily reach an argument in the already overworked controversy about the tautological character of certain marginal utility theories. Except with Mises, with almost everybody I have come to an agreement on this.' A few months later he wrote (from Geneva where he was working for the League of Nations on the business cycle project which produced _Prosperity and Depression_ [1937]):

'I am looking forward with great interest to seeing your new book [Robbins (1934)] and to hear what changes you will make in Nature and Significance. Have you seen the mimeographed papers, which have been discussed in the Mises Seminar? I feel strongly that you should see them, before you write on the question, whether economics is a "a priori science" and which are the aprioristic "Elements" in it. These discussions must have been extremely interesting and I think you should consult someone (Stonier e.g.) who participated in them. I still think that Mises' position is quite untenable and he was, according to what I heard, quite isolated in his group. It is on the other hand very important to clarify the issue. Otherwise one is exposed to such foolish attacks as the one of Souter, who mixes us "Wertfreiheit" and the problem of whether economics is apriorism. ... I think you should take all precautions to

376 It is harder to identify Machlup's position on methodology at this time (though he wrote extensively on methodology later). His 1936 note is concerned to argue that economists should be concerned with methodological issues not with what their position on such issues should be.
Felix Kaufmann was a young Viennese philosopher and a member of the Vienna Circle. The Vienna Circle, which had been meeting since 1924 and was disintegrating as its members were leaving Vienna, had been much concerned with the nature of logic and mathematics and Kaufmann had written on the use of mathematics in economics: Robbins had cited his work in the first edition of the *Essay*. Kaufmann also turned up at the Robbins Seminar at LSE a couple of times in late 1933 and he published an article on economic methodology in *Economica* (1933) and one in the *Review of Economic Studies* (1934). Robbins had referred to an earlier article published in German by Kaufmann (1930) at the end of the paragraph on page 65 of chapter III which read: 'Scientific generalisations, if they are to pretend to the status of laws, must be capable of being stated exactly. That does not mean ... that they must be capable of quantitative exactitude. We do not need to give numerical values to the law of demand to be in a position to use it for deducing important consequences. But we do need to state it in such a way as to make it relate to formal relations which are capable of being conceived exactly.' This statement remained in the second edition (pp 65-6). Alfred Stonier was an Oxford-trained economist (with a first in PPE in 1927) who had gone to Heidelberg for his PhD (1935) and later became a lecturer in political economy at UCL; he had been a student of Roy Harrod's at Christ Church and was a friend of Robbins's former student, Hugh Gaitskell, who was then a lecturer at UCL. Like Gaitskell he spent time in Vienna and attended Mises's seminar.

The Vienna Circle, as is well known, took a hard line on epistemology, and on the demarcation between mathematics and science on the one hand and non-science (or metaphysics) on the other. The propositions of logic and mathematics are necessarily true, true by definition of the terms and hence tautologous, because they are irrefutable: no facts can possibly contradict them. They are *analytic a priori* in Kant's terminology. All other propositions may be true or false and if such propositions are to be scientific they must be capable of confirmation or refutation by empirical facts. Such propositions are *synthetic a posteriori* statements. From this you get the verification principle, that scientific propositions that are not logical or mathematical must be verifiable if they are to be counted as science. If they are not verifiable then they are not science, ie metaphysical. The hard line implication is that there can be no *synthetic a priori* statements in a science, because such statements are neither analytic nor verifiable. Hence one of the things that Kaufmann, Stonier, Haberler and Hutchison spotted was that Mises's conception of economics ran into the problem that in so far as it was purely analytical and *a priori* true it could not also be an empirical science.

Kaufmann (1933) makes no mention of Robbins (or Mises for that matter: he mentions Stonier as a friend who helped him with the article.) He was concerned with a more general problem, that much of the methodological controversy in economics was muddled because of a failure to distinguish the separate questions of scope

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377 It was this principle that Karl Popper challenged on the valid ground that no synthetic a posteriori proposition can be verified: if n facts are consistent with the hypothesis there is always the possibility that an (n+1)th will not be. This is of course just a way of stating the problem of induction. Popper was, however, just as concerned as the Vienna Circle with the problem of demarcation, proposing falsifiability as the test instead of verification.
(subject matter) and method. As he said, if economists had found a truly fruitful method there would be no controversies about the subject matter of the discipline, which would like physics be defined by its method. He ended this article by summarizing his views on mathematics in economics, on which he had written in the (1930) article cited by Robbins.

'... no mathematical proposition as such contains any statement regarding reality; it is, therefore, not possible to reach apodictical conclusions about the course of economic events with the help of the exact mathematical method. Einstein's dictum that "in so far as the propositions of mathematics refer to reality they are refutable, and in so far as they are irrefutable they do not apply to reality," is no longer open to doubt and it is just as applicable to economics as to physics. On the other hand, there is no sphere of reality which cannot in principle be investigated with the help of the mathematical method. It is not necessary that the phenomena to be investigated shall be themselves measurable, since as we have already argued, the chief part of the investigation can be conducted at a level of abstraction far removed from them. The mathematical method can neither be shown "on philosophical grounds" to be the only scientific method in economics, nor can it be rejected on other philosophical grounds as in principle inadequate. One can only try by careful analysis to get evidence as to the extent of its usefulness.'

Kaufmann also referred to his forthcoming book (1936).378

Kaufmann (1934) pointed out that misconceptions as to the nature of mathematical propositions and concepts had played a very large part in methodological discussion in the social sciences, especially in economics.

'The propositions of mathematics with their precision and their apodictic validity, were regarded as providing a model for scientific laws of all kinds, for it was not realised that apodictical validity was incompatible with the nature of statements about facts. Until the influence of such misconceptions has been eradicated, the problems connected with the laws of social science cannot be stated clearly. ...

‘No proposition in logic or in pure mathematics tells us anything about reality; one can never learn from it whether a particular event is occurring, has occurred, or will occur at a definite time and place. The service of Logic and Mathematics is to translate implicit assumptions into explicit form. Logical and mathematical propositions are therefore analytical.

But when it came to general statements or laws which purported to describe reality, there was the problem of induction. 'It is not permissible to contrast deduction with induction, as is often done, on the ground that the former is an inference from the universal to the particular, while the

378 Kaufmann's English book, Methodology of the Social Sciences (Oxford University Press, 1944), is a rather different book as he had changed his views by then. Kaufmann published another paper in Economica in 1937, a reply to a review of his 1936 book.
latter is an inference from the particular to the universal, since an inference from the particular to the universal is impossible. In so far as we can speak at all of inductive inferences, they are also inferences from the universal to the particular, but in this case the major premises are, as a rule, only partly conscious.

Thus a law, even when formulated, remains a hypothesis, and the distinction between established theory and unproved hypothesis represents only a difference of degree, like the distinction between "rigid" laws and mere rules or tendencies.'

Kaufmann, like many others, found a way round the problem of induction by regarding empirical laws as conventions: though the 'laws' were really only hypotheses and could be falsified, 'Confidence in the validity of a law may sometimes be so great that any observation which does not agree with it is regarded as fallacious, or at least incomplete. In this case the possibility of refuting the law is suspended.' But he emphasized such conventionalism easily led to misunderstandings: it was all too easy to slip from accepting theories made irrefutable only by convention as scientific 'laws' into thinking that there exist synthetic a priori true statements.

His example was the principle of marginal utility, which he had discussed at length in his 1933 article. As in his Economica article he concluded this article by referring readers to his forthcoming book.

Kaufmann's book was reviewed in Economica along with Stonier's published dissertation (1935) by Harro Bernadelli (1936), criticized Kaufmann for 'fluctuat[ing] in a somewhat staggering way between the conventionalist thesis which sees in acts of mere arbitrariness the ultimate source of philosophical and mathematical principles, and the thesis of rationalism which tries to brand all such principles as analytical. In his book the assertion that a "necessary connection" between subject and predicate can be found only if the predicate by definition is determined as a property of the subject, "in which case the proposition in question is an analytical one" ... can be found side by side with the statement that geometrical axioms ... or laws of nature such as the principle of energy ... are only "conventions based on the test of experience," and it is left to the reader to find his way through these inconsistencies. It will, I hope, be obvious to the reader that the rationalist way of justifying principles cannot succeed because these principles are by nature synthetic, and the menace is apparent to which a science is laid open if sheer arbitrariness of conventions is declared its supreme authority.' Stonier's book received less attention from Bernadelli, though it too was criticized for lack of clarity, since Stonier was, as Bernadelli described it, 'only entangled in a fierce polemic against those who wish to claim a general phenomenological intuition (Wesensschau) as a source from which philosophical principles flow. The fact that such an intuition obviously does not exist is sufficient reason for him to conclude that therefore these principles must be of analytical nature' - 'a hasty conclusion' in Bernadelli's opinion.

What is relevant here is that Stonier's targets included Mises. According to Bernadelli (1936, 448-96), 'his [Stonier's] main point is to show Mises is mistaken in asserting pure economics (catalactics) to be a science a priori. This he tries to do by pointing out the improbability of a phenomenological intuition of economic activities. Now, as far as I am aware, Mises has never claimed to be in possession of so sublime a source
of knowledge; all that he means, if I understand him rightly, is that economics is based on certain principles which cannot possibly be derived from experience. The question whether this is so or not can be decided independently of the problem of the origin of such principles. Stating that phenomenological intuition is not likely to be the source misses the point, especially if one keeps in mind that such an intuition need not be the only possible source from which these principles flow.'

Returning to Robbins, while he was aware of these issues and all the more so given conversations with Haberler and Machlup, he did not want to get embroiled in the controversy. He explicitly stated this later, in his 'Live and dead issues in the methodology of economics' (1938), where he wrote of one of the live issues, 'the exact logical status of certain of the more general assumptions on which pure economics is based':

'One school of thought regards them as essentially rational principles which are given \textit{a priori} and which, while they show themselves in experience, yet require no appeal to experience to demonstrate their ultimate validity. Another, while not disputing the wide generality of their applicability, regards them as being derived from experience and having the same provisional status as the more obviously empirical assumptions. The former view has been very powerfully urged by Professor Mises and Dr Bernadelli, the latter by Dr Kaufmann and Mr Hutchison among others ...

'There are very fundamental epistemological questions involved here; and he would be a bold man who would regard the problems of epistemology as settled. I myself would confess to real doubt on the issue; and in the work alluded to above \cite{Robbins} I have tried - in the first edition unsuccessfully, in the second, I hope, with greater, if not complete success - to use a terminology which steers clear of the ultimate questions involved.'

It is also clear from an appendix which he drafted but did not use in the second edition of the \textit{Essay}. (The draft is in the Robbins papers.) This began: 'In the body of the book it will be noticed that I have made little or no allusion to recent controversial discussions of the ultimate status of economic generalizations. Indeed, the careful reader, prying behind the actual structure of my sentences, may even detect a deliberate avoidance of terms which commit me to one view or the other. Such an inference would be perfectly legitimate.' One reason for this was his philosophical incompetence; another was that he had come to believe that economists are capable of agreeing on what the core of their discipline is. (As I shall indicate below, he was helped in this by actual recent developments in economic theory.) I quote selectively (I admit I don't find his second argument very convincing):

'The question at dispute is the ultimate nature of economic laws. Are they given \textit{a priori} or are they in some sense the generalization of experience? Is economics in this respect different from the physical sciences of which we have knowledge?
'Now, on the question whether it is legitimate to describe the main generalisations of economics as given a priori it seems to me that we must abide by the verdict of philosophers. If, as we are assured by some, the term a priori applies only to purely formal propositions such as a white horse is a white horse, then no doubt it is desirable when describing propositions which are supposed to have reference to reality to abstain from using this term. It is clear that whatever has been meant by those who have used it in connexion with economic laws that they have not meant this. Indeed so far from implying that their generalisations related to no reality (which is the case with purely formal propositions) they were anxious to find a way of suggesting that their propositions related to all reality. If therefore the use of the term a priori implies a necessary absence of reference to reality I do not see that there should be any concession in retreating from this position. But are philosophers really agreed about this?

'But this brings me to the second question. It is clear that there is room for difference of opinion on the way in which our knowledge of certain of these truths arises. ... But on the question whether this knowledge, whether it be given "a priori" or be a limiting abstraction from experience, is similar to character to the generalisations of the world of physical science which point to reality, [t]here seems to me to be no real room for difference of opinion. It surely will not be denied that our knowledge of the existence of scales or relative valuation is different from our knowledge of the entities which are the subject of the proposition[s] of the physical sciences. We have an immediate inner acquaintance with the ultimate foundations of our generalisations in the social sciences which is not and cannot be the case with the generalisations of the physical sciences.

'It is this truth which as I understand it is insisted on by Professor Mises. Stated in this way there can surely be no room for disagreement among competent practitioners of the social scientists.'

However, 'On the other hand it may well be questioned to what extent it is possible to build a scheme of generalisations which are very helpful upon notions of the degree of generality. This seems to me a substantial point and in the text of the essay I have again and again insisted on the necessity at almost every stage of invoking subsidiary postulates of less complete generality than those which have hitherto been cited. It is clear I think that nearly every application involves less general propositions as well as the fundamental concepts.'

In a draft preface to the second edition which he also did not use Robbins stated that he was not going to change chapter six which had been the object of so much criticism. But he had revised the first part of chapter four: 'the revision will not make it any more acceptable to [most of] the critics ... for the net effect is to make the aspect of Economics there treated more abstract & formalistic than ever. But I hope it will do something to meet the suggestions of my friends D' Gottfried Haberler & M' A.W. Stonier with whom I have had many instructive conversations on these matters.'
In the actual preface, referring to the changes he had made in chapter four and parts of chapter five he mentioned Hayek, Paul Rosenstein-Rodan (who was then at UCL) and Stonier, 'whose advice and criticisms on these difficult matters have taught me much.' In these chapters he referred to his conversations with Machlup in the section (2) of chapter V on the nature of economic laws. In the new parts of chapter IV he referred also to Joan Robinson's Economics of Imperfect Competition, (1933) which he hoped 'will have done much to convince many hitherto sceptics of the utility and significance of the kind of abstract reasoning from very simple postulates', and to her pamphlet, Economics is a Serious Subject, (1932), adding a section on the rationality assumption to meet her point that he had not done so in the first edition.\(^\text{379}\)

The first and most obvious change Robbins made to chapter IV of the Essay is the use of the Hicks-Allen innovations in demand analysis to illustrate the nature of the propositions of economic theory. These had, as is well known, been developed in discussions at LSE especially in the Robbins Seminar over which Robbins and Hayek jointly presided in 1931-6 (Hicks, 1939). By following Pareto in using the technique of indifference curves rather than marginal utility theory to explain consumer behaviour, Hicks and Allen eliminated the need to rely on the unmeasurable concept of utility. This was very convenient for Robbins’s methodological standpoint since the marginal utility theory had been a major issue in the methodological controversies. The Hicks-Allen (1934) approach was also very fruitful in resolving a host of knotty problems about competing and complementary goods. So Robbins could begin his discussion of the 'nature of economic generalizations' boldly (1935, 75).

'It does not require much knowledge of modern economic analysis to realise that the foundation of the theory of value is the assumption that the different things that the individual wants to do have a different importance to him, and can be arranged therefore in a certain order. This notion can be expressed in various ways and with varying degrees of precision, from the simple want systems of Menger and the early Austrians to the more refined scales of relative valuations of Wicksteed and Schönfeld and the indifference systems of Pareto and Messrs Hicks and Allen. But in the last analysis it reduces to this, that we can judge whether different possible experiences are of equivalent or greater or lesser importance to us. From this elementary fact of experience we can derive the idea of the substitutability of different goods, of the demand for one good in terms of another, of an equilibrium distribution of goods between different uses, of equilibrium of exchange and of the formation of prices.

In the theory of production, since 'the Law of Diminishing Returns is simply one way of putting the obvious fact that different factors of production are imperfect substitutes for one another', this law followed from the assumption that there is more than one class of scarce factors of production (pp 76-7).

Furthermore, he went on to state strongly (pp 78-9):

\(^{379}\) Rosenstein's (1934) article is referred to on page 102.
'The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious.'

'No one,' he claimed (page 81), 'will really question the universal applicability of such assumption as the existence of scales of relative valuation, or of different factors of production, or of different degrees of uncertainty regarding the future, even though there may be room for dispute as to the best mode of describing their exact logical status.'

'In the light of all that has been said the nature of economic analysis should now be plain. It consists of deductions from a series of postulates, the chief of which are almost universal facts of experience present whenever human activity has an economic aspect, the rest being assumptions of a more limited nature based upon the general features of particular situations or types of situations which the theory is to be used to explain.' (pp 99-100)

So far so Misesian. But he also wrote, still in chapter four (page 94),

'The purpose of these assumptions [of rationality in one sense or another] is not to foster the belief that the world of reality corresponds to the constructions in which they figure, but rather to enable us to study, in isolation, tendencies which, in the world of reality, operate only in conjunction with many others, and then, by contrast as much as by comparison, to turn back to apply the knowledge thus gained to the explanations of more complicated situations. In this respect, at least, the procedure of pure economics has its counterpart in the procedure of all physical sciences which have gone beyond the stage of collection and classification.' And he began chapter five with the categorical statement (page 104): 'It is a characteristic of scientific generalisations that they refer to reality. Whether they are cast in hypothetical or categorical form, they are distinguished from the propositions of pure logic and mathematics by the fact that in some sense their reference is to that which exists, or that which may exist, rather than to purely formal relations. 'In this respect ... the propositions of Economics are on all fours with the proposition[s] of all other sciences.'

It seems to me that Robbins was inclined like Kaufmann to waver between a priorism and conventionalism, also that he was fudging the issue (as he came close to admitting in the unpublished appendix).
When Haberler read the second edition of the Essay he was quite critical of the revised methodological chapters. He wrote to Robbins in January 1936:

'I have studied the book very carefully and like it more and more the more I read it. I am still unconvinced by what you say about the logical nature of economic generalisations, but it seems to me that the point of disagreement is pushed back so far as to be of practically no importance in the practice of theory. Your emphasis of the logical difference of economic laws which are apodictic and absolutely safe on the one hand, and sociological laws which are always a little vague and uncertain, seems to me overdone. I still believe that there is only a difference of degree not only between sociological and economic laws but, between various economic laws. Instead of trying to show that economic laws are not of the apodictic nature, I want to draw your attention to the fact that it is easy to make the sociological laws just as exact - and (in my opinion) meaningless - as you want to make economic laws. ...

You quote somewhere as a striking example for the usefulness of the "deductive" method the derivation of the law of diminishing returns from the fact that land is not a free good. ... Here again it can be shown that the deduction becomes absolutely exact only at the expense of becoming meaningless. The deduction holds only, if you assume rational behaviour. ... I think it can be shown that you have to define rationality in such a manner that it contains the law, which you later derive from it.'

Mises was critical too, though for the opposite reason. He was apparently more critical of (the methodological chapters of) the second edition of the Essay than he had been of the first. After making several detailed criticisms of Robbins’s terminology, especially in relation to 'inner experience', he went on (my translation of the German original):

'Actually our arguments are not different and I count you ... among those who accept a priori experiential knowledge. I believe that you oppose to the open acknowledgment of this position only an infinitely small remainder of a former metaphysical prejudice, which you resist energetically and with excellent reasoning on every point - for example, in the comments about the standard of value, about the comparability of judgments of different subjects, and in those about the standpoint of historicism and the representation of statistical laws. For it is nothing other than metaphysics, if like for example, Simiand, Schumpeter, recently Felix Kaufmann, the science of human exchange will look like a kind of physics. Because this metaphysics befogs so many minds I hold it dangerous to employ a means of expression that does not allow the essence of character to come out strongly and can easily be misunderstood.

'That, what one today calls modern logic, the works of Russell, Whitehead, Schlick, Carnap etc. is still biased to, like the work of the old logicians, only to physics and at best adjusted to biology. Of history and above all economics they know nothing. If only it would not be that! But
they are full of disdain for all that the economists practise, and as socialists full of resentment against the findings of economic science. One could sometimes believe that the overcompensation of some inferiority complex is involved. It is a pity that talented men like Kaufmann and Rosenstein let themselves be influenced by this.'

But this attempt to detach Robbins from the younger Austrian economists, influenced by the logical positivism of the Vienna Circle, serves, in my view, to show the difference between Robbins and Mises. Robbins may have wanted to follow Mises's lead in adopting an *a priorist* interpretation of economics, but he was sensitive to the logical positivists because, unlike Mises and like his friends Haberler and Machlup, he wanted economics to be an empirical science.
References


Kaufmann, Felix (1936), Methodenlehre der Socialwissenschaften, Vienna: Verlag Julius Springer.


Kaufmann, Felix (1944), Methodology of the Social Sciences, London: Oxford University Press.


Robinson, Joan (1932), *Economics is a Serious Subject*, Cambridge: W.Heffer & Sons Ltd.

Robinson, Joan (1933), *Economics of Imperfect Competition*, London: Macmillan.


Value-Free Economics in Management and Engineering Education

Kevin Christ∗

Abstract

In his influential Essay on the Nature and Significance of Economic Science Lionel Robbins argued that “economic analysis is wertfrei (value free)” and logically distinct from ethics. In the decades that followed, economics became, in the words of Amartya Sen, “self-consciously non-ethical.” This paper reviews the origins and nature of the value-free and ethically blind approach that came to dominate economic analysis in the years following publication of the Essay. The paper also considers some consequences of this approach, with particular emphasis on current curricular discussions in management and engineering education.

JEL Classification: A20, Economics Education and Teaching of Economics
B41, Economic Methodology

Key Words: Robbins, Wertfreiheit, Value-Neutrality, Ethics

1. Introduction

In the decades following the publication of Lionel Robbins’s An Essay on the Nature and Significance of Economic Science (hereafter ENSES), economics became, in the words of Amartya Sen, “self-consciously non-ethical” (Sen, 1987, 2). Although movement in this direction had been going on since the early 19th century, there can be little doubt that Robbins’s Essay, with its focus on the German concept of Wertfreiheit – the separation of value judgments from analysis, or the conscious pursuit of ethical neutrality – was influential on the subsequent development of economics. Many years after the publication of the Essay, Kenneth Boulding, in his presidential address to the American Economic Association, lamented the path economics had taken as it evolved into a social science distinct from moral science, consciously incorporating the Wertfreiheit terminology with its historical baggage:

“We are strongly imbued today with the view that science should be wertfrei and we believe that science has achieved its triumph precisely because it has escaped the swaddling clothes of moral judgment and has only been able to take off into the vast universe of the ‘is’ by escaping from the treacherous launching pad of the ‘ought’” (Boulding, 1969, 1).

Even though many professional economists today have probably never read Robbins’s Essay, they would recognize in it an outline for an institutionally acceptable approach to economic research. In general, the methodologies of economics still rely heavily on deductive analysis, mimic the disinterestedness that is

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part of the ethos of science in general, and enforce a ban on ethical considerations. Although a debate over the relationship of economics to ethics continues in some corners of the discipline, in general mainstream professional economists assiduously avoid any trappings of value-influenced analysis. For example, in a recent op-ed piece on fairness debates involving the U.S. tax code, Greg Mankiw writes “Fairness is not an economic concept. If you want to talk fairness, you have to leave the department of economics and head over to philosophy” (Mankiw, 2007). Comments such as Mankiw’s are testament to Robbins’s polemical success and to the continuing influence of the value-free approach he espoused in the Essay. They may, however, reflect an overzealous application of the Wertfreiheit doctrine. There is a difference between ethical neutrality in pursuit of scientific credibility and being “self-consciously non-ethical.” As Sen suggests, economists have in general adopted the second of these approaches in their work, but this overly-strict interpretation of Wertfreiheit may have had some unexpected consequences.

In the years following publication of the Essay, Robbins’s influence on the ethos of economics, specifically his strident advocacy of value-free analysis and a strict separation of ethics and economics, contributed to the development of an ethically blind approach in economics that has ultimately become associated with negative consequences in some parts of the discipline. This paper focuses on two areas outside of economics – management and engineering education – where an ethically blind approach to economics has exerted or has the potential to exert important influence. In each of these areas, an institutional adherence by economists to ethical blindness – a stance that might be called a “vertfrei ideology” – has had unintended and indirect consequences. In the first case, the development of economic theories and models under the guise of Wertfreiheit has contributed to an ethically blind management science that has become the subject of harsh criticisms. In the second case, an interpretation of Wertfreiheit as a ban on ethical evaluations runs counter to current pedagogical initiatives and threatens to contribute to a low level of economic literacy among professional engineers. In both cases a strident advocacy of value neutrality has led to adoption of methods and models that may increasingly cause economics to be associated with ethically questionable outcomes.

Given the theoretical hegemony that economics exerts on other fields and the degree to which its conclusions and models are pervasive outside of economics, the consequences of taking a value free approach within economics would seem to be an important consideration as we evaluate the continuing influence of Robbins’s Essay. With this broad goal in mind, this paper first considers the nature of the Wertfreiheit doctrine by reviewing the genealogy of the ideas that informed Robbins’s position and by examining the subsequent development of the ethically blind approach that came to dominate economic analysis in the years following publication of the Essay. After this review, the paper goes on to consider some of the consequences of this approach in two areas outside of economics where its methods and models have been aggressively applied – management and engineering education.

2. The Origins and Nature of Ethical Blindness in Economics

Almost from its birth, modern economics, in pursuit of status as a “true” science, seems to have been preoccupied with separating itself from its roots in moral philosophy. Albert Hirschman has noted that “modern social science arose to a
considerable extent in the process of *emancipating* itself from traditional moral teachings*” (Hirschman, 1981, 294). In economics, this process of emancipation is usually thought to have begun with David Ricardo and Nassau Senior, who actively facilitated the separation of economics from ethics and narrowed the focus of the developing discipline (Flubacher, 1950). Regarding issues related to distribution, for example, Senior was unequivocal:

“… all these are questions of great interest and difficulty, but no more form part of the Science of Political Economy, in the sense in which we use that term, than Navigation forms part of the Science of Astronomy.”

(Senior, 1938, 2)

Later in the 19th century the first and second generation marginalists seemed consciously aware of the implications of the growing distance between economics and ethics. Almost uniformly, they opened their treatises with extended discussions about the relationship of economics to philosophy and science, often expressing a desire to establish economics on an epistemological par with the physical or natural sciences.

Despite this long history, economics, at least as it was practiced in English-speaking academic circles in the early 20th century, had not yet fully divested itself of its holdings in moral philosophy. Although Keynes identified Marshall with the process of “building up the subject as a separate science” (Keynes, 1925), Marshall, who epitomized the British economic tradition at the turn of the century, does not seem to have contemplated a complete severance of economics from ethics (Coats, 1990). At his inaugural lecture at Cambridge University in 1885, Marshall, while discussing the need for technically proficient economists, seemed equally interested in attending to their ethical sensitivities:

“It will be my most cherished ambition, my highest endeavor, to do what with my poor ability and my limited strength I may, to increase the numbers of those, whom Cambridge, the great mother of strong men, sends out into the world with *cool heads but warm hearts* …”

(Marshall, 1925, 174, *italics* added)

Keynes himself, while lauding the evolution of economics as a distinct discipline, nevertheless always remained a complicated mixture of deductive scientist and moral philosopher. Thus, by the early 1930s, the dominant strand in British economic thought still could be characterized as one that accepted economics as a complex mixture of pure science and moral reasoning.

Into this established tradition, Robbins introduced an Austrian-influenced view of social science with a Weberian-inspired *wertfrei* leitmotif. In the *Essay*, Robbins was unyielding in his view that ethics and economics were logically distinct, and that if economic knowledge was to be held in the same esteem as other scientific knowledge, economists may adopt different methods but must eschew value judgments:

“… it does not seem logically possible to associate the two studies in any form but mere juxtaposition. Economics deals with ascertainable facts;
ethics with valuations and obligations. The two fields of enquiry are not on the same plane of discourse.” (ENSES, 148)

Robbins’s methodological axiom that economic analysis must remain free of value judgments had its origins in the German Methodenstreit of the 19th century and the more recent Werturteilsstreit, or value judgment dispute between Max Weber and Gustav Schmoller, which had occurred in the early years of the 20th century (Vickers, 1997, 62 – 64; Ciaffa, 1998, 13 – 18). Schmoller and others had argued that social sciences should develop judgments concerning the desirability of institutions and policies. In opposition, Weber argued for an “intrinsically simple demand” that investigators should maintain an unconditional separation between the establishment of facts and the evaluation of those facts as satisfactory or unsatisfactory (Weber, 1949, 11). Robbins was clearly influenced by Weber’s arguments, and constructed his own arguments around a careful and clear demarcation of the proper subject matter of economics, which also seems to have been influenced by Weber. His means-ends definition of economic science (ENSES, 16) mirrored another of Weber’s hypotheses:

“All serious reflection about the ultimate elements of meaningful human conduct is oriented primarily in terms of the categories ‘end’ and ‘means’.” (Weber, 1949, 52).

The influence of Weber seems to have been both direct and indirect through the Austrians whom Robbins had come to know personally, especially von Mises (Caldwell, 1994; Robbins, 1971, 105 – 107).

Questions concerning choice among ends deal with ethical matters of subjective value, and Robbins took these to be exogenous facts, or at the very least decisions beyond the scope of scientific economic inquiry. Economists were to take ends as data points, ascertainable facts of the landscape. As Robbins conceived of economics, it was “incapable of deciding as between the desirability of different ends” (ENSES, 152). This demarcation of appropriate subjects for study allowed Robbins to authoritatively claim that “economic analysis is wertfrei in the Weber sense” (ENSES, 91, italics in the original).

Looking back from an era in which it is common for economists to delegate normative questions to other disciplines, one may mistakenly see in the Essay the swagger of a discipline that had finally and decisively moved from the realm of moral philosophy to true science, and which was comfortable in leaving questions with moral dimensions to other fields. But this interpretation would be a mistake. In declaring this clean break with ethics, Robbins was “taking a position that was quite unfashionable then, though extremely fashionable now” (Sen, 1987, 2). At the time, Keynes for one had serious misgivings on this issue, commenting in a letter to Roy Harrod that “as against Robbins, economics is essentially a moral science and not a natural science. That is to say, it employs introspection and judgments of value” (Keynes, 1938).

In the wake of Robbins’s Essay and in the name of good social science economists seemed to adopt what might be called a strong form of value neutrality, which requires them to uniformly refrain from making value statements as economists. This
strict interpretation of the Wertfreiheit doctrine might be distinguished from a weak form of value neutrality, which seeks only to “contain” value judgments to a restricted domain (Mongin, 2006). This stance nudged the Wertfreiheit doctrine beyond ethical neutrality towards ethical blindness. Over the years, economists came to view this separation of economics from ethics as complete, and it became institutionalized in economics textbooks (Davis, 2005). In this version of the Wertfreiheit doctrine, however, Robbins’s own suggestion that economists speculate “long and widely” on ethical questions (ENSES, 150) seemed to be forgotten. It is useful to remember that neither Weber nor Robbins, in arguing for “ethically neutral” or “value free” social science, required that social scientists take a vow of abstinence from making policy pronouncements. As Blaug (1992, 116 – 117) has pointed out, Wertfreiheit requires only that economists be clear about the basis of their pronouncements and not attempt to give them “scientific” validation. Weber himself had put it this way:

“…it can never be the task of an empirical science to provide binding norms and ideals from which directives for immediate practical activity can be derived … What is the implication of this proposition? It is certainly not that value-judgments are to be withdrawn from scientific discussion in general simply because in the last analysis they rest on certain ideals and are therefore ‘subjective’ in origin.” (Weber 1949, 52)

Similarly, in distinguishing between pure scientific analysis and postulates about “different judgments of value”, Robbins concludes:

“Our methodological axioms involve no prohibition of outside interests! All that is contended is that there is no logical connection between the two types of generalization, and that there is nothing to be gained by invoking the sanctions of one to reinforce the conclusions of the other.” (ENSES, 150)

It is also worthwhile to remember that, despite his strident advocacy of a value-free approach, Robbins certainly could never have been accused of pursuing a disinterested approach to policy.

It may be that Robbins merely meant for his strict ban on ethical evaluations to apply only to that part of economics that he thought of as economic science, and not that part of economics that he called political economy (Colander, 2007). He made his position on the proper place of value judgments in economics very clear throughout his career, most notably perhaps in his Ely lecture to the American Economic Association in 1981. Perhaps sensing that the planes of discourse for economics and ethics might not be so distinct after all, he argued there for a revival of the term Political Economy to cover “that part of our sphere of interest which essentially involves judgments of value” (Robbins, 1981, 8). Figure 1 summarizes his views.

Nevertheless, a broad acceptance of an extreme value neutrality approach in the years following publication makes it easy to look back at the Essay as “a rather successful attempt to eliminate from economic analysis the last vestiges of … any reference to normative ideas” (Pribram, 1983, 420). In that other famous methodological essay of the 20th century, Milton Friedman argued that “positive economics is in principle
independent of any particular ethical position” (Friedman, 1953, 181), and Ronald Meek, in discussing the evolution of economics in the late 1950s and early 1960s into an engineering-like discipline, would proclaim that “the days of the intrusion of value-judgments into ‘positive’ economics are numbered” (Meek, 1964, 95). Such methodological pronouncements are one thing, but the theoretical developments of the 1950s and 1960s, pursued with elegant mathematical formality, illustrate even more clearly the evolution of economics as not just a value-free science, but as an amoral science. Historian William Letwin’s characterization of the contributions of seventeenth century political philosophers to the eventual development of scientific economics seems equally appropriate as a description of much of the most notable economic theorizing of the mid-twentieth century:

“…economic theory owes its present development to the fact that some men, in thinking of economic phenomena, forcefully suspended all judgments of theology, morality, and justice, [and] were willing to consider the economy as nothing more than an intricate mechanism, refraining for the while from asking whether the mechanism worked for good or evil” (Letwin, 1964, 158 – 159).

Ethical neutrality became ethical blindness.

There are important criticisms of the value neutrality approach, which apply to both its weak and strong forms. The first is the “value basis” argument (Ciaffa, 1998), which is usually associated with Gunner Myrdal’s agnosticism concerning the “existence of a body of scientific knowledge acquired independently of all valuations” (Myrdal, 1953, vii; 1970). In general, this line of reasoning argues that value-free analysis is not possible in the social sciences because the researcher and model builder always approaches his or her project with valuations, hidden or not. This criticism comes in many forms from many quarters, including philosophers:

“While economists like to present themselves as disinterested scientists, they function today more typically as ideologists for our political and economic ‘elites’ – much like most theologians did in an earlier age.” (Pogge, 2005, 30)

For some, this issue is unavoidable, because humans are unavoidably driven by ideology, and the pretence of disinterest is just that – a pretence. As one historian of economic thought put it, “… the very decision to be ethically neutral may be related to one’s ideology” (Maloney, 1985, 203). It seems that in some quarters, adherence to a value-free approach exposes economists to charges of masking their ideologies.

The best one can hope for, according to this view, is that social scientists are meticulously explicit in setting forth their biases. Moral Philosophers often initially sketch out the “ethical perspective” being taken before proceeding to their actual arguments. This seems to be akin to the laying out of assumptions that economists often use as a preamble to their arguments. As an out-of-the-mainstream economist commenting on the possibility of ethical neutrality in economics 30 years before Robbins’s essay simply said “open bias is better than veiled bias” (Devas, 1892).
A second and more recent critique of the extreme value neutrality view is that in
proscribing prescriptive statements it disregards the possibility of justifiable
evaluative statements in both positive and normative analysis (Mongin, 2006).
Employing tools of analytic philosophy, the argument is that value judgments, to the
extent that they are evaluative and not prescriptive, are amenable to rational
discussion, and therefore have a rightful place within economics. If this position is
accepted, both strong and weak neutrality positions become logically weak and
unnecessarily limiting.

Finally, we may consider a third criticism of the value neutral approach that focuses
not on the possibility of ethical neutrality, but on the consequences that flow from it.
Theories and models that are developed under the guise of ethical neutrality (or
ethical blindness) get applied in environments that are anything but value free. The
conclusions that economists reach within their rarified scientific domain often
become the basis for normative recommendations despite the protestations of
economists that their work has no normative implications. To say that economics is
“incapable of deciding as between the desirability of different ends” (ENSES, 152)
overlooks the influence of economic analysis on individual and social choices
concerning ends. It is not necessarily true that questions concerning selection of
means for the attainment of a pre-ordained end have no ethical dimensions. This
becomes apparent when we look at how economic “knowledge” gets used in various
realms of human activity.

Of course one could easily respond to this criticism by arguing that economists
cannot be held responsible for the uses to which their positive tools of analysis are
put. We might call this the “models don’t kill people, people kill people”
argument. Even the physical sciences, however, in dealing with issues such as
weaponry, genetic engineering, or nanotechnology, offer examples where it is
difficult if not impossible to disentangle scientific research from its ethical
implications, whether they are anticipated or not. While economists may never
convene an Asilomar conference, there are nevertheless valid arguments to be
made that economists should include ethical considerations in the feedback loop of
their scientific method (Figure 2). To argue differently is to argue that the
consequences that follow from the acceptance of a theory are irrelevant to the
evaluation of the theory. Once theories become the basis for action, such a stance
seems problematic. Furthermore, there are good reasons to suspect that social science
theories are fundamentally different from physical science theories in that they tend to
change those environments in which they are applied. (Merton, 1948; Ferraro, et al,
2005). Theories about molecule interaction do not change the way molecules
interact as those theories become widely accepted. Theories about how people
behave in markets, on the other hand, might change the way people behave in markets
if those theories are widely accepted. To the degree that the adoption of economic

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380 In the U.S., the catchphrase “guns don’t kill people, people kill people” is sometimes used by
opponents of gun control to argue against restrictions on gun ownership. Its sentiment appeals to
personal responsibility, and absolves producers and sellers of guns of any responsibility for the uses to
which their products might be put.

381 This refers to a 1975 conference organized by scientists engaged in recombinant DNA research,
who had become concerned that current research in the field could generate potentially dangerous and
ethically troubling outcomes.
ways of thinking have consequences – intended or not – those consequences may be useful inputs in the evaluation of the theories and of the scientific ethos under which those theories are developed.

This consequentialist critique of value neutrality is relevant in any venue where economic methods and models are applied. The next two sections of this paper argue that the application of economic methods and models in management and engineering education illustrates different aspects of unintended consequences at work. In the first case, the influence of economics on curriculum development has been explicit and, of late, the object of stinging criticisms. In the second case, curriculum development is moving toward more ethical content, not less, and this threatens to diminish further an already meagre role for economics in the education of engineers. With these thoughts in mind, let us consider more fully the influence of value-free economics on management and engineering education.

3. Value Neutrality and Ethical Blindness in Management Education

In recent years, U.S. business school academics have engaged in serious bouts of introspection about how well they are doing their job (Mintzberg and Gosling, 2002; Donaldson, 2002; Pfeffer and Fong, 2002), or even if management education as currently practiced is detrimental to the “art” of management in general (Augier and March, 2007; Bennis and O’Toole, 2005; Ghoshal, 2005). Many of these criticisms, coming as they do during and after a period of notable ethical lapses in American business, have focused on an apparent lack of ethical sensitivity among business school graduates. For present purposes, Ghoshal’s critique is the most prescient, focusing as it does on the role that economics played (as Ghoshal saw it) in fostering an “ethically blind” approach to management. In castigating the influences of academic research on management pedagogy, Ghoshal laments:

“…the incorporation, within the worldview of managers, of a set of ideas and assumptions that have come to dominate much of management research. More specifically, I suggest that by propagating ideologically inspired amoral theories, business schools have actively freed their students from any sense of moral responsibility” (Ghoshal, 2005, 76).

Arguably the three sets of ideas from economics that have exerted the greatest influence on management education in the past generation are agency theory, transaction cost economics, and game theory. Ghoshal cites all three in what might be called a summary statement of his indictment:

“Combine agency theory with transaction costs economics, add in a standard versions of game theory and negotiation analysis, and the picture of the manager that emerges is one that is now very familiar in practice: the ruthlessly hard-driving, strictly top-down, command-and-control focused, shareholder-value-obsessed, win-at-any-cost business leader of which Scott Paper’s ‘Chainsaw’ Al Dunlap and Tyco’s Dennis Kozlowski are only the most extreme examples” (Ghoshal, 2005, 85).

During the 1970s and 1980s, all these economic theories became new tools in the economist’s toolbox for explaining the inner workings of business firms, and
economists were not shy about proclaiming their contribution to the development of management science:

“The last decade has been marked by a growing interest in organizations within the economics profession … The science of organizations is still in its infancy, but the foundation for a powerful theory of organizations is being put in place.” (Jensen, 1983, 324)

Collectively, agency theory, transactions cost economics, and game theory provided the theoretical hardware for the new sub discipline of organizational economics:

“Until recently … economists had little to offer on what might be called “sub-micro” level issues, e.g., internal organization, management systems, and contracting arrangements. The growth of transaction cost economics, principal-agent analysis, and related extensions of game theory promises to change that. The economics of organization is emerging as an important new field of study.” (Dees, 1992, 25)

In business schools, where a transformation had been going on from collecting and transmitting best business practices to developing and disseminating theories about phenomena connected with management of complex organizations, strategy specialists greeted the new tools with open arms (Rumelt, et al, 1991). Here were models, forged in the respected realm of economics, which went inside the black box of production and ultimately offered managers practical guides to behaviour.

The integration of agency theory, transaction cost economics, and game theory into the curriculum of management education is a great illustration of economic hegemony. In each case the theories came to business schools with behavioural assumptions that claimed to be workable, if not realistic, and with policy prescriptions justified on efficiency grounds. Over time, the models became ubiquitous in modern MBA texts (Milgrom and Roberts, 1992; Brickley et al, 1997), and were used to promote practical rules for managers. Often these rules ended up having normative implications despite the fact that the underlying theories were developed under the auspices of value free positive science.

In his critique of management education, however, Ghoshal argues that some of agency theory’s key assumptions – in particular the existence of alternative markets for the agents’ services and the value of the principals’ human capital input relative to that of the agents – undermine the normative prescriptions for which the model is often used as justification. A second and more general aspect of Ghoshal’s withering critique is that the business schools’ amoral theories (principally inherited from economics) were “ideologically inspired.” Specifically he identifies classical liberalism as the ideology that underlies theoretical developments in economics, and maintains that it embodies an overly pessimistic vision of human nature.

Ghoshal’s criticisms are not the only charges that have been leveled against agency theory. Ethicists have argued that principal-agent analysis is vulnerable to abuse and inappropriate application (Dees, 1992). Further, the possibility that an agent serves multiple principles introduces complicating factors that undermine normative prescriptions that practitioners often take from agency theory (Bowie and Freeman,
Finally, it also can be argued that through its singular focus on incentives as an inducement to action, agency theory implicitly introduces an ethical dimension into the discussion of organizational efficiency (Grant, 2002).

In a similar vein, transaction cost economics (TCE) has been criticized for promoting obvious normative conclusions (Pfeffer, 1994; Ghoshal and Moran, 1996). In opening their criticism of TCE, Ghoshal and Moran argue:

“All positive theories of social science are also normative theories, whether intended or not. The normative implications of TCE, in particular, are inescapable” (Ghoshal and Moran, 1996, 14).

In making this argument, Ghoshal and Moran refer in particular to the use of TCE to prescribe specific directives for internal organization and management practices within firms, and quote a prominent proponent of TCE analysis as saying “transaction-cost economics aspires to influence as well as understand behaviour” (Masten, 1993, 120).

These criticisms arise in spite of the contention that TCE was originally conceived as a positive theory to explain the efficient boundaries of a firm (Coase, 1937; Williamson, 1979, 1985, 1991). Using concepts of asset specificity, opportunism, and incomplete contracting, TCE has been promoted as a tool for delineating between transactions that will most efficiently occur in markets and those that will most efficiently occur within the hierarchical structure of firms. Opportunism – defined by Williamson as “self-interest with guile” – is a stronger form of the self-interest assumption usually employed in economic analysis. The concept often seems to imply extreme self-interest unencumbered by ethical considerations. In their popular MBA textbook, Milgrom and Roberts (1992) continually describe opportunism as an endemic problem that necessitates managerial and contractual responses. In the TCE framework, organizations supplant markets because hierarchical controls within organizations are more effective in curbing opportunism.

This story of the infiltration of economic theories into management education, and of the unintended effects that became associated with it, describe a much different path than that which Alfred Marshall had envisioned for the influence economics would have on business. Speaking in his inaugural lecture at Cambridge in 1885 about the need to professionalize the field of economics and to attract the best minds to the cause, Marshall had expressed a hope that through the study of economics “just and noble sentiments might be introduced into counting-house and factory and workshop” (Marshall, 1925, 173). Instead, many management theorists today argue that there have been both positive and negative consequences to the influence of economics on their discipline. In evaluating the ethos of the economics discipline, it may at least be prudent to consider the possibility that theories developed with a greater sensitivity to ethical concerns may have emerged differently from the foundry of economics. If they had been delivered out of that foundry with product warning labels, economic theories might have been used differently in the construction of management science, and the consequences of their use might have been much different.

But given the direction in which the science of economics was moving in the post-war years, it’s difficult to see how such an alternative path could have emerged. For
those unacquainted with the strict methodological prescriptions then evolving in
economics, the neglect of ethical concerns seemed puzzling:

“A strange characteristic of social thinking in recent times has been the
lack of contact between ethics and economics. It is strange because in the
same period we have in fact been typically and dominantly concerned
with business, so that one would hardly have expected moralists to ignore
the problems of conduct arising there. And, especially recently, we have
become so sensitive as to the morality of our business behaviour that it is
equally odd to find ‘scientific’ economists ignoring the ethics of business”
(Macfie, 1953, 57).

4. Value Neutrality and Ethical Blindness in Engineering Education

In engineering education a different problem is developing. While the role of
economics in engineering curricula has been minimal – often limited to the provision
of frameworks for cost-benefit and risk analyses – current trends in engineering
education offer opportunities for other fields to influence the ethical training of
engineers. Engineering educators today, especially in the United States and to a
lesser degree in the U.K., are increasingly concerned with the ethical dimensions of
their trade. The aim of recent curriculum initiatives in engineering, spurred by
accreditation agencies, is not only to instill a sense of ethical responsibility via
traditional code-based approaches, but also to make ethical reasoning a pervasive
component of engineering pedagogy (Lincourt and Johnson, 2004; Bird, 2003; Uff,
approach is sometimes referred to as “ethics across the curriculum” (Herkert, 2007;
Lincourt and Johnson, 2004), and in engineering schools non-engineering disciplines
are being asked to consider the issues involved in teaching engineering ethics
(Rudnicka, 2005; Kline, 2001). In part, this increased emphasis on ethics among
American and British engineering educators may stem from a realization that
technical proficiency is a minimum expectation in the engineering job market, and
that if American and British engineering schools are to maintain a competitive
advantage in the engineering labor market their graduates must be prepared to deal
broadly with complex issues relating to technology and society.

In general, however, engineers are problem solvers who exhibit little interest in
theory. Historically, engineers have accepted value-free economics without
suspicion, seeing it as a credible source of pragmatic tools of analysis that can be used
in the application of technology and design principles to their projects. Courses in
“engineering economics” – which for some engineers are the only economics they
learn – reflect this pragmatism, focusing mainly on cost behaviour and capital
budgeting techniques. To the extent that the social science aspect of economics can
be said to have exerted any broader influence on the training of engineers, it has been
through the provision of an understanding of the capitalist system in which engineers

382 For additional insight into the pervasive role of ethical considerations in engineering education,
accreditation agencies also provide extensive resources. See, for example, the Accreditation Board for
Engineering and Technology (ABET) accreditation criteria, http://www.abet.org/, or The Royal
Academy of Engineering’s engineering ethics curriculum map,
usually end up finding employment. One overarching theme is that of an historic tension between the demands of economic efficiency and the demands of technical professionalism (Layton, 1969).

This tension can become more apparent when the ethical sensitivities of the engineering discipline bump into the ethical blindness of economics. Engineers may sometimes feel as if they have to choose between their ethical responsibilities and the dictates of respectable economic analysis. Consider this explanation of a “degree of harm” criterion in a popular engineering ethics textbook:

> “When pollutants pose a clear and pressing threat to human health, they must be reduced below any reasonable threshold of harm. *Cost should not be considered a significant factor.* Insofar as substances pose an uncertain (but possible) risk to health or when the threshold of danger cannot be determined, economic factors may be considered. If a harm is irreversible, it should be given higher priority” (Harris, et al, 2000, 219, italics added).

This criterion circumscribes traditional cost-benefit analysis based on economic considerations. As engineering educators debate and formulate such criteria, an amoral science of resource allocation is unable to participate in the discussion.

The notion of a public interest also has the potential to create tension between engineering and economics. The dominant economic views concerning corporate governance generally dismiss the notion of a public interest as irrelevant to private decision making (Friedman, 1982, chapter VIII). Engineers, however, have never abandoned the notion of a public interest or their obligation to serve it. Thus, in the area of corporate governance there exists potential for the concept of public interest to drive a wedge between engineers and economists. When economists use the term public interest, it is usually within the context of the need to ameliorate market failures to improve economic efficiency. Engineers on the other hand seem to view the concept as a sort of collective preference ordering over a broader set of objectives. In an earlier era, engineers and economists shared this latter view. For example, six years before Robbins offered his sharp demarcation of economics from ethics, Keynes expressed this older view of a public interest when he said “our problem is to work out a social organization which shall be as efficient as possible without offending our notions of a satisfactory way of life” (Keynes, 1926). Of course, one cannot enter into discussions about the meaning of “satisfactory” without allowing for some relaxation of the strict value neutrality view.

As it stands, what ethical lessons, if any, could engineers take away from economics? One lesson, primarily learned in case studies of what is derisively called “disaster ethics”, is that amoral cost-benefit calculations sometime lead to disastrous or morally repugnant outcomes. In studying the Ford Pinto case 383, for example, engineers are

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383 The Ford Pinto was a subcompact car manufactured by the Ford Motor Company for the North American market from 1971 to 1980. It became the focus of a scandal when it was alleged that the car’s design allowed its fuel tank to be easily damaged and perhaps explode in the event of a rear-end collision. Ford was aware of this design flaw but allegedly refused to incur the expense of a redesign. Instead, it was argued, Ford decided it would be cheaper to pay off possible lawsuits for resulting deaths. (Wikipedia, “Ford Pinto”)
often left with an unsatisfactory view of the sterile way in which economics balances benefits and costs. Another case study in engineering ethics deals with the implications of dumping unsafe electrical products in developing markets. The ethical issues in this case mirror those associated with the well-documented Lawrence Summers World Bank Memo which has been used to argue that economists cannot avoid moral issues in their analyses (Hausman and McPherson, 1996). In this case, the application of economic logic to the topic of optimal asset ownership is straightforward – ownership of assets should migrate to agents who value them most highly. Unfortunately, and as both cases demonstrate, the implications of “straightforward” economic logic sometimes can be ethically troubling.

Another lesson that engineers might pick up from their study of economics is that discounting future benefits and costs at a constant and positive rate can support ethically troubling recommendations. The evaluation of large-scale public projects unavoidably involves discounting monetary values over very long time horizons, with significant issues surrounding assumptions about the preferences of future generations. In contemporary economic discussions of discount rates for publicly financed large-scale projects with long time horizons it is often suggested that the appropriate rate of discount is a weighted average of before-tax and after-tax interest rates where the weightings are the proportions of funds coming from displaced private investment and displaced private consumption. Proponents of social rates of discount argue, however, that opportunity costs revealed in market rates of return may not provide adequate representations of intergenerational tradeoffs, and that the appropriate rate of discount is somewhere below this market-determined rate, perhaps even close to zero. Such rates significantly increase the importance of benefits and costs in the distant future. As long ago as 1928, Frank Ramsey argued more broadly that the use of a social rate of discount greater than zero was “ethically indefensible and arises merely from the weakness of the imagination” (Ramsey, 1928, 543). These arguments usually revolve around issues of market imperfections or missing markets – claims that distortions in current credit markets make interest rates misleading indicators or that future generations are inadequately represented in today’s markets. In any event, the economist’s claim to have a rule for determination of an “appropriate” rate cannot entirely insulate the analysis from ethical issues. Not addressing such issues at the introductory level threatens to leave the student of economics inadequately prepared to deal with real-world issues.

In areas such as these economics actually has a rich history of debate over methods and moral implications. Adherence to a strong value neutrality position, however, pushes these issues to the background and contributes to missed opportunities to use economic analysis to sharpen ethical sensitivities and moral reasoning skills. While more philosophical debates within economics may arise in graduate seminars and sometimes in well-taught upper-level undergraduate courses, most engineering students never find themselves in such venues and thus are likely to emerge from the economics courses they manage to take with a relatively unsophisticated level of economic literacy.

In prognosticating on the future of microeconomic theory, Beth Allen saw “vast potential for economists to learn from engineers” (Allen, 2000, 149). Her primary
interest was the possibility of interdisciplinary knowledge transfer – she speculated that economists could enhance their theories of production by studying the ways in which engineers deal with problems of technology selection and design. It may also be fruitful to consider if economists can learn anything from the ways in which engineers wrestle with ethical issues in an attempt to pursue a unified approach to their design problems. More than this, a broader conception of economics as a moral science could conceivably allow economists to contribute in meaningful ways to the discussions of complex and vexing issues that often arise in engineering. In this way, both fields could be enriched.

5. Summary

The foregoing discussion offers a new perspective for considering the continuing influence of Robbins’s Essay and for understanding how an overly strict adherence to the value-free approach Robbins espoused may have led to unintended consequences. The argument here is that under the guise of value neutrality, economics actually developed an ethos of ethical blindness, and that management and engineering education offer good venues for studying the consequences that follow from such an approach. In the course of the 20th century, the science of economics supplied management and engineering educators with important tools, which for different reasons educators in those fields eagerly put to use. In both instances, however, the influence of economics yields a cautionary tale.

In the first instance, that of management education, adherence to a strong form of value neutrality unnecessarily exposes economics to criticisms having to do with the ultimate consequences of its research. There is no more straightforward illustration of this than the debate over chief executive compensation in the U.S. During the 1970s and 1980s, some economists began to argue that American companies were being poorly run because their top managers were not always making decisions with the owners’ best interests in mind. Viewed through the lens of principal-agent analysis, this implied that, as agents hired by stockholder principals, CEOs really didn’t have the right incentives. Two economists closely associated with this view, Michael Jensen and Kevin Murphy, concluded in a 1990 Harvard Business Review article that many American CEOs were paid like bureaucrats. By this they meant that CEO pay was not contingent enough on the fortunes of the companies they ran. In response to research such as this, boards began revising the way they paid their CEOs, placing a lot less emphasis on annual salary, and a lot more on stock options. As this trend developed momentum, advocates of stock options began to make broad claims about their widespread use in CEO pay. Some argued that stock options could actually help explain why the U.S. economy seemed more efficient than, say, European economies, where the practice was not as widespread. In early 1998, The Wall Street Journal proclaimed that high CEO pay “helps the U.S. economy thrive” (Kay, 1998). Ultimately, however, this agency-inspired trend generated a backlash against incentive pay, raising important questions about how we are to respond when application of economic analysis is associated with outcomes that offend broader moral sensibilities.

In the second instance, that of engineering education, adherence to a strong form of value neutrality satisfied engineering educators’ requirements for credible tools of analysis in much the same way as mathematics provides tools of analysis to the
science of mechanics. But a tension exists between economic prescriptions based on a value-free approach and the ethical requirements of the engineering professions, and these tensions are likely to become more obvious as engineering education moves in a more ethically sensitive direction.

In both management and engineering education, extreme value neutrality limits the ability of economics to contribute meaningfully to discussions surrounding curricular content. While economics maintains a pretense of ethical neutrality, important environments within which economics is taught are moving in the opposite direction. In the wake of corporate scandals of the last decade, many business schools have been busily redesigning their curriculum, or introducing new elements such as a first-year course at Harvard that focuses on leadership, ethics and values. In engineering schools an increasing awareness of the need to foster ethical sensitivities has led to an emphasis on “ethics across the curriculum.” In the face of such discussions, it is reasonable to ask how an “ethically neutral” discipline may contribute to these developments?

It is of course also reasonable to argue that the separation of economics from moral philosophy has yielded great benefits. After all, academic specialization is a form of division of labour, and Marshall had laid out the case for a separate path for economics in his inaugural lecture at Cambridge. Economics enjoys a pre-eminent position among the social sciences today, and some would argue that this is due to its methodological rigor and its refusal to make ethical pronouncements, which strengthen its claims of scientific authority. Nevertheless, the cost-benefit analysis associated with this methodological evolution seems to have been decidedly one-sided, with little discussion of the costs. Sen (1987), who argued that both economics and ethics had been “impoverished” by the growing distance between the two fields, is the most notable exception. Hausman and McPherson (1996), Vickers (1997), and Dees (1992) echo Sen, each suggesting that both fields could learn a lot from each other. In homage to Sen, the continued exile of ethical considerations from economics threatens to impoverish not just economics and ethics, but disciplines such as management and engineering where economics and ethics have important roles to play. The weighing of benefits and costs in such a matter can be daunting. Would our understanding of markets have progressed as far as they have if economics had not been consciously steered down the road of value-free analysis? Perhaps not. At this stage in its development, however, it may be time to ask whether economics can rediscover its moral philosophic roots without sacrificing too much of its scientific advantage. And whether a loosening of the ethical blinders that we often wear might improve our contributions to society.

In 1981, the same year in which Robbins called for a revival of the term “political economy” to clarify the disciplinary division between an economics with ethics and an economics without ethics, Albert Hirschman, in a discussion of the moral dimensions of the social sciences in general, envisioned a different transformation:

“Down the road, it is then possible to visualize a kind of social science that would be very different from the one most of us have been practicing: a moral social science where moral considerations are not repressed or kept apart, but are systematically commingled with analytic argument, without guilt feelings over any lack of integration; where the transition
from preaching to proving and back again is performed frequently and with ease; and where moral considerations need no longer be smuggled in surreptitiously, nor expressed unconsciously, but are displayed openly and disarmingly” (Hirschman, 1981, 305 – 306).

It remains to be seen whether a strict value-free approach will continue to make a division between economic science and political economy necessary, or whether a reincorporation of ethical considerations might be part of a reunification. As it stands, the ethos of economic science, influenced by Robbins’s Essay and strongly imbued with a sense of ethical neutrality, continues to exercise broad influence. Careful attention to the consequences of that influence in areas such as management and engineering education might offer useful insights as we ponder the future evolution of economic methodology.
References


Attractive Polarities, Narrow Boundaries

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Abstract

Lionel Robbins’s seminal “Essay on the Nature and Significance of Economic Science” is about half right and half wrong. It is right in its insistence that economists question their assumptions, and its modest suggestion that “by itself Economics affords no solution to any of the important problems in life.” It is wrong in its attempt to divide neatly positive versus normative inquiry neatly—and in doing so, confine economics to the former. As Hume reminded us nearly 200 years before Robbins, the demarcation between “is” and “ought” is not so clear. Indeed, a central problem in social science has been our inability to develop a method to go neatly from descriptive science to prescriptive policy. As a consequence, we inevitably muddle the two. Still today, as Coase warns, “problems of welfare economics must ultimately dissolve into a study of aesthetics and morals.”

To the extent that Robbins’s great ambition is to define an “Economic Science” that spews out objective truth, his elegant definition is left wanting. Discussions of the differences between “material welfare” and “scarcity” are arguably simplistic. Treatment of objective rankings of welfare and interpersonal comparisons of utility could have been further developed. Early repudiation of what later emerged as behavioural economics, not to mention well a broader scepticism toward empirical analysis, might not survive the passage of time.

To the extent that any of these criticisms of Robbins’s monumental work are convincing, they are unlikely to matter. Whether Robbins’s arguments are right or wrong, or his definitions are convincing or not, is of precious little importance. The book’s enduring appeal lies in the issues it so presciently raised. Visionaries don’t always need to be right or come up with robust definitions. They need to make us think and question. Judged by this standard, “Essay” is a resounding success.

1. Introduction

It is difficult to overestimate the importance of Lionel Robbins’s Essay on the Nature and Significance of Economic Science (Essay). This exceptionally well-written text on economic methodology has been variously described as a “classic tract” (Caldwell, 1982, p. 99), “the locus classicus from which all discussion [of economic methodology] begins” (Blaug, 1978, p. 725), and “the ‘official’ statement of the general ontology and epistemology of the discipline.” (Ross, 2005, p. 87). One commentator even notes that Essay, “together with the Pareto-Hicks-Samuelson approach-has most greatly influenced the epistemology of microeconomics in the twentieth century.” (Bruni, 2005, p. 225). Put simply, Robbins “was expounding the foundations of the neoclassical microeconomics that would dominate for the rest of the century.” (Kirzner 2000, p. 19). But how did Robbins make his seminal

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arguments? And, notwithstanding Essay’s extraordinary success, are these arguments sound?

This article attempts to address these questions in three parts. Part 2 attempts to understand Robbins’s rhetorical appeal. It argues that the persuasiveness and attraction of his arguments derive largely from an extraordinarily subtle, yet masterful, exposition of a series of beautifully-crafted polarities, each building from the next: material welfare versus scarcity, society versus individual, empiricism versus apriorism, normativity versus positivism, interpersonal comparisons versus ordinal rankings, political economy versus economic science. In each case, Robbins exposes a seeming divergence, then takes sides. He then uses this position as an anchor to make taking a position on the next polarity and seemingly natural and effortless exercise. Put simply, his scarcity-based definition of economics leads him to espouse an individualistic, aprioristic and positivist perspective on economics. This stance then naturally leads to a rejection of interpersonal comparisons of utility. Without the ability to engage in such comparisons, the scope for economic science diminishes considerably. Convenient to the status quo, welfare and social institutions are no longer within the science’s purview.

Part 3 addresses the potential objection that part 2 is too crude a depiction of Robbins’s position. It argues that, notwithstanding some nuances notably related to Robbins’s possible ambivalence around empirical studies, the sequence is essentially accurate.

Finally, part 4 proceeds to show that the attractive polarities Robbins has created are flimsy. Moreover, Robbins’s too often facile arguments have the ultimate effect of unnecessarily narrowing the boundaries of economics, leading unfortunately to a diminished role for the discipline in public policy. Notwithstanding these criticisms, though, Essay’s enduring appeal lies in the issues it so presciently raised.

2. Attractive Polarities

Essay, while unusually lucid, does not follow a linear path from polarity to polarity. As a consequence, deconstructing its arguments is often complex. The exercise is nonetheless rewarding since it reveals the essence of Robbins’s extraordinary rhetorical skill.

The first polarity, material welfare versus scarcity, begins with Robbins’s striking definition of economics. In a bold departure, he rejects the definition of economics espoused by such luminaries as Cannan and Marshall; namely, “the study of the causes of material welfare.” (Robbins, 1935, p. 4; see also Howson, 2004, p. 416). Simply contending that “[w]hatever Economics is concerned with, it is not concerned with the causes of material welfare as such” (Robbins, 1935, p. 9; see also Alkire, 2002, p. 114), Robbins declares that economics “is the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses.” (Robbins, 1935, p. 16; see also Ross, 2005, p. 157). Robbins’s source for such a conceptualization appears to lie with his belief in the inevitability of limited time and resources in life (Robbins, 1935, pp. 13-15), as well as with the traditions of the Austrian School-notably, he cites Menger, Mises, Strigl, and Mayer to support his
definition (Robbins, 1935, p. 16 n.1). Notwithstanding these respectable continental roots (Seligman, 1962, p. 521), Robbins introduced a radically new definition to the Anglo-Saxon world. He thus sets up a definitional polarity, and declares himself squarely on one side of it. To this day, “Robbins’s book is perhaps best remembered for his articulation of the ‘scarcity’ over the ‘materialist’ definition of the subject area of economics.” (Caldwell, 2004, p. 187).

If scarcity is the coin of the realm, then this conjures up a rather Darwinistic image of individuals competing for life and lucre—rather than a more complex and nuanced analysis of social institutions and history that a study of welfare might encompass. This is precisely what Robbins seems to intend. *Essay* is largely concerned with the canonical Robinson Crusoe problem, “the behaviour of an isolated man disposing of a single scarce commodity.” (Robbins, 1935, p. 34). Indeed, Robbins contends that “the phenomena of the exchange economy itself can only be explained by going behind such relationships and invoking the operation of those laws of choice which are best seen when contemplating the behaviour of the isolated individual.” (Robbins, 1935, p. 20). Perhaps most importantly for our purposes here, the focus on individual rather than society flows effortlessly from Robbins’s definition of his field:

> “it is only when contemplating the conditions of isolated man that the importance of the condition that the scarce means must have alternative uses if there is to be economic activity, which was emphasized above, leaps clearly to the eye. In a social economy of any kind, the mere multiplicity of economic subjects leads one to overlook any possibility of the existence of scarce goods with no alternative uses.” (Robbins, 1935, p. 20 n. 2).

The first polarity thus neatly feeds the second.

By contrast, there is less concern for social or institutional behaviour (Robbins, 1935, p. 17), as the Institutionalists are squarely criticized. (Robbins, 1935, p. 124). It is thus unsurprising that James Buchanan would put Robbins in a group of economists who “would say that ‘society,’ as such, must always be conceived of as its individual members.” (Buchanan, 1964, p. 215; see also Bruni, 2005, p. 225). Talcott Parsons is more critical, noting that Robbins does not “have any clear conception of the relation of institutions to economic activities, nor any systematic place for a theory of institutions in their scheme of the social sciences.” (Parsons, 1934, p. 533).

Relatedly, Robbins is not too interested in history (Robbins, 1935, p. 39), arguing for example that “historical induction, unaided by the analytical judgment, is the worst possible basis of prophecy.” (Robbins, 1935, p. 74; see also Hodgson, 2001, pp. xvi, 195). Much like he slights the Institutionalisiticians, Robbins too diminishes the Historical School (Robbins, 1935, p. 80; see also Catlin, 1933, p. 463), even noting somewhat amusingly that “[i]f it is historico-relative, then a term is needed to describe what we know as historico-relative studies” (Robbins, 1935, p. 82) or that “[t]he only difference between Institutionalism and Historismus is that Historismus is much more interesting.” (Robbins, 1935, p. 83). With both institutions and history tossed aside, we are left with an asocial, ahistorical individual as the unit of economic analysis.
Ingeniously, Robbins uses this second polarity to set up the third: as institutional and historical analysis is deemphasized, so too the importance placed on empirical studies. Robbins contends that “[i]t is really not possible to understand the concepts of choice, of the relationship of means and ends, the central concepts of our science, in terms of observation of external data.” (Robbins, 1935, pp. 89-90; see also Robbins, 1935, pp. 74-75. The fascinating link here is his belief that empirical studies are of limited usefulness because they only capture historical reality and are therefore of little predictive use—“there is no presumption that they must continue to describe the future.” (Robbins, 1935, p. 109). As one commentator has noted, Robbins “is not interested in a historicist approach, which would look for past trends in the allocation of scarce means. For, he believes, past trends (or empirical generalizations expressing what are alleged to be trends) are not to be relied on.” (Hollis, 1975, p. 58; see also Caldwell, 2004, p. 189). Robbins even seems to deride the Historical School and the Institutionalists as users of flawed “inductive methods” redolent with empiricism. (Robbins, 1935, p. 114). To him, “it is theory and theory alone which is capable of supplying the solution.” (Robbins, 1935, p. 120).

Having critiqued empirical work, Robbins then proceeds to declare boldly that “[t]he propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates.” (Robbins, 1935, p. 78; see also Robbins, 1935, pp. 99-100). To support his theory of postulates, Robbins declares that “[n]o one will really question the universal applicability of such assumptions as the existence of scales of relative valuation, or of different factors of production, or of different degrees of uncertainty regarding the future, even though there may be room for dispute as to the best mode of describing their exact logical status.” (Robbins, 1935, p. 81). In a manner reminiscent of theorists such as Condillac and Destutt de Tracy (Klein, 1985, p. 69), Robbins envisions “economics as the a priori exploration of deductions from the axioms of rational choice.” (Hodgson, 2001, p. 209; see also Blaug, 1978, p. 698). Put simply, “economics cooked to Robbins’s recipe is a self-contained deductive structure resting on an introspective foundation that is taken to be maximally epistemologically modest.” (Ross, 2005, p. 90; see also Catlin, 1933, p. 463; Klein, 1985, p. 68). Deductive introspection, not inductive empiricism, becomes the order of the day, as the third polarity is decided.

To this point, Robbins has emphasized scarcity over material welfare, the individual over social institutions and history, and apriorism over empiricism. The reader is seemingly told that what matters is an ahistorical, asocial Robinson Crusoe going about life in response to scarcity. And economists should analyze this behaviour via scientific deduction.

One might be forgiven at this point for assuming that Robbins is likely to frown upon normativity. After all, if he claims that economics consists of deductions from a series of universal postulates, and universality implies a confidence in self-evident neutrality, then one might expect precious little room for consideration of ethics and judgment. True to form, Robbins cleverly sets up another polarity—positivism versus normativity—and declares economics to occupy squarely the former camp. The way he builds to this conclusion, however, is characteristically elegant and does not even directly depend on his apriorism. Casually noting that “[t]he criterion of economy which follows from our original definition is the securing of given ends with least means,” (Robbins, 1935, p. 145), he then proceeds to observe that “[t]here is nothing
in the corpus of economic analysis which in itself affords any justification for regarding these ends as good or bad. Economic analysis can simply point out the implications as regards the disposal of means of production of the various patterns of ends which may be chosen.” (Robbins, 1935, p. 147). He thus magically takes the question of determining ends away from economics, concluding matter-of-factly that “[e]conomics is neutral as between ends. Economics cannot pronounce on the validity of ultimate judgments of value.” (Robbins, 1935, p. 147; see also Robbins, 1935, p. 24). As Talcott Parsons has noted, in Robbins’s “anxiety to make economics a ‘positive’ science free from ‘metaphysics,’ he is continually being pressed into a radically positivistic position which really eliminates ends altogether.” (Parsons, 1934, p. 514). By eliminating a discussion of ends, Robbins eschews normativity.

Robbins’s claim seems to be that economics cannot logically encompass an ethical component. According to him, “[t]he two fields of enquiry are not on the same plane of discourse. Between the generalisations of positive and normative studies there is a logical gulf fixed which no ingenuity can disguise and no juxtaposition in space or time bridge over.” (Robbins, 1935, p. 148; see also Robbins, 1935, p. 32). Simply put, “[p]ropositions involving ‘ought’ are on an entirely different plane from propositions involving ‘is.’” (Robbins, 1935, p. 142-43). As scholars have observed, Robbins is “strictly confining economic science to the Weberian ideal of wertfrei endeavour [one free from value judgments].” (Groenewegen, 1996, p. 1; see also Vickers, 1997, p. 7). The power of Robbins’s declaration derives significantly on how the Wettfreihet doctrine emerges quite innocuously from his definition of economics (Kirzner, 1994, p. 315). As one commentator has noted, “the word welfare was suspect as having ethical implications.” (Howson, 2004, p. 424; cf. Screpanti and Zamagni, 1993, p. 270). Robbins’s immensely influential focus on scarcity appears seemingly more neutral, somehow more scientific. (Cf. Screpanti and Zamagni, 1993, p. 172, Vickers, 1997, p. 39). He has seemingly effortlessly sown the seeds of positivism beginning with his definition.

Essay’s fifth polarity is a specific application that derives largely from privileging positivism over normativity. If economics is merely descriptive science, then it becomes problematic for it to assess relative utility or welfare or happiness among individuals. Is it possible, for example, to establish objectively that an incremental $100 is worth more to a family living in poverty than to a billionaire? In contrast to distinguished economists such as Pigou (Pigou, 1948, p. 83) and Viner (Viner, 1925, p. 369), Robbins criticizes the notion of marginal utility as one whose assumptions “can never be verified by observation or introspection.” (Robbins, 1935, p. 137). The implications of this stance are dramatic:

“Hence the extension of the Law of Diminishing Marginal Utility, postulated in the propositions we are examining, is illegitimate. . . . Recognition of this no doubt involves a substantial curtailment of the claims of much of what now assumes the status of scientific generalization in current discussions of applied Economics. . . . It does not tell us that a graduated income tax is less injurious to the social dividend than a non-graduated poll tax. Indeed, all that part of the theory of public finance which deals with “Social Utility” must assume a different significance. . . .” (Robbins, 1935, p. 141)
Rejecting the notion of marginal utility “begs the great metaphysical question of the scientific comparability of different individual experiences.” (Robbins, 1935, p. 137). Indeed, Robbins’s criticism of utility culminates in the statement that “[t]here is no means of testing the magnitude of A’s satisfaction with B’s. . . . There is no way of comparing the satisfactions of different people.” (Robbins, 1935, pp. 139-40; see also Sen, 1987, p. 30; Persky, 2001, p. 201). He suggests that only ordinal preference rankings, without a measure of intensity, are possible (Robbins, 1935, p. 78-79; see also Ross, 2005, p. 87).

It is striking how elegantly the previous polarities build to this point. After all, having individuals simply order their preferences is ethically far less challenging than making comparisons among individuals. Robbins’s rejection of interpersonal comparisons thus becomes the canonical application of Essay’s desire to separate economics from ethics. (Cf. Davis, 2005, p. 599; Vickers, 1997, p. 109). To boot, even trying to compare utilities among individuals suggests a need for empirical analysis such as behavioural studies. Yet such a notion obviously does not sit well within a framework that privileges aprioristic deduction. (Cf. Seligman, 1962, p. 520; Screpanti and Zamagni, 1993, p. 270). Not to mention, of course, that self-centred ordinal rankings fit well within a radically individualistic, scarcity-based, conception of economics.

Finally, what do the prior five polarities suggest about the scope of economics? As Robbins himself indirectly acknowledges, a move away from interpersonal comparisons of utilities toward ordinal rankings essentially shunts distributional questions out of economics by delegitimizing social changes that might be justified by the concept of marginal utility. (Robbins, 1935, p. 136). As the scope and stance of economics are narrowed, so is the discipline’s ability to contribute to positive social change. As one commentator points out, Essay’s argument against interpersonal comparisons of utility “placed redistribution outside of the bounds of the analyzable in neoclassical discourse and put a scientific imprimatur on the status quo. The scientific approach to economic analysis had tremendous political consequences, providing a scientific argument for the existing distribution of wealth.” (Hackney, 1997, p. 29). This stance fuels the sixth and ultimate polarity: the separation of economic science from political economy. Put simply, equity should not belong within economic science—let politicians and philosophers worry about things like fairness and redistribution. In Robbins’s words, “it is worth while delimiting the neutral area of science from the more disputable area of moral and political philosophy.” (Robbins, 1935, 151). We are back to Robbins’s assertion that economists should not worry about ends (Seligman, 1962, p. 521).

As a corollary to its disregard of distributional issues, Essay has been instrumental in launching a sea change privileging the concept of equilibrium—presumably to occupy economists’ time now that the scope of their work has been dramatically reduced. As Robbins articulates it, “[i]nstead of dividing our central body of analysis into a theory of production and a theory of distribution, we have a theory of equilibrium, a theory of comparative statics and a theory of dynamic change.” (Robbins, 1935, p. 68). Crucially, “it is of the essence of the conception of equilibrium that, given his initial resources, each individual secures a range of free choice, bounded only by the limitations of the material environment and the exercise of a similar freedom on the part of the other economic subjects.” (Robbins, 1935, p. 143). True to form, Robbins
ingeniously relates ideas—he integrates the new concept he introduces, equilibrium, to choice and Darwinistic individualism.

Several implications emerge from such a view of equilibrium. First and most importantly, a focus on initial distribution of resources without contemplating redistribution necessarily privileges the status quo. Second and more technically, emphasizing equilibrium successfully sets the stage for greater emphasis on formal mathematical modelling in economics—a phenomenon that has indeed emerged since Essay’s publication. (Vickers, 1997, p. 7). This push has been to the detriment of encouraging economics to integrate insights of other social sciences such as cognitive psychology—an approach which Robbins disdainfully characterizes as “the happy hunting-ground of minds avers to the effort of exact thought.” (Robbins, 1935, p. 83). After all, if one can rely on the seemingly neutral formalism of mathematical models, why bother understanding the messy reality of human psychology or even group sociology? (Cf. Ross, 2005, p. 167; Seligman, 1962, p. 522).

Where does all of this leave us? By emphasizing scarcity over welfare, individuals over society, apriorism over empiricism, positivism over normativity, ordinal rankings over interpersonal comparisons, and economic science over political philosophy, Robbins effectively hastened the decline of welfare economics as a meaningful discipline. As one scholar notes, such a project is not only symbolized by Robbins’s rejection of interpersonal comparisons of utility, but also has significant political implications:

“Such explicit rejection of utility had useful and practical by-products in the 1930s for a devoted economic liberal like Robbins. Removal of the imprimatur of scientific inference from the case for a steeply progressive income tax via the ‘law’ of diminishing marginal utility of income, was one such desirable by-product. Eliminating the ‘scientific’ foundations for an interventionist welfare economics as developed by Pigou (in the footsteps of Marshall) was another.” (Groenewegen, 1996, p. 2)

In a nutshell, Robbins’s polarities ultimately culminate in the claim that “[w]elfare economics is ethics and not economics” (Peck, 1936, p. 496; see also Rima, 1967, p. 318). To the degree that welfare economics might represent the progressive branch of economics, then it becomes a second-class citizen.

3. An Unfair Reading?

Before analyzing the boundaries that emerge from Essay’s arguments, it is perhaps worth pausing to ask whether part 2 has been unfair to Robbins. Is his argument more nuanced than made out above? I discuss the six polarities, in rough order of interpretative ambiguity. Two are straightforward. First, there does not seem to be debate over emphasis of the scarcity over the welfare definition of economics. Second, except for a very small number of curious passing bows to acknowledge that societal institutions actually matter (Robbins, 1935, pp. 44, 143, 155), it seems readily apparent that Robbins emphasizes individuals as the unit of analysis.
Where *Essay* stands on the four other polarities is arguably more controversial. To begin with, one might conceivably argue that Robbins is not pointing to interpersonal comparisons of utility as impossible or useless, but merely observing that they rest on subjective judgments. There is some support for this position. For example, in the Preface to the Second Edition, in response to his critics, Robbins claims that he merely “contended that the aggregation or comparison of the different satisfactions of different individuals involves judgments of value rather than judgments of fact, and that such judgments are beyond the scope of positive science.” (Robbins, 1935, p. vii). Robbins makes a similar point in a later article focused on interpersonal comparisons. (Robbins, 1938, pp. 637, 640). Indeed, as Paul Samuelson lamented years later, “those who took their Robbins too literally grasped at the straw of a ‘new welfare economics’ which was to be independent of interpersonal ethical elements. But freed from the obscurities of geometry and Paretian French, the new welfare economics stands revealed as being merely a set of *incomplete necessary* conditions whose whole *raison d’être* disappears if the additional ethical conditions are not adjoined.” (Samuelson, 1949, pp. 371-72).

To the extent that the text of *Essay* itself might be ambiguous, Robbins’s other writing suggests that while he might ostensibly claim the real issue is objectivity, he privileges this response as a convenient way to mask his displeasure with interpersonal comparisons. Decades later, for instance, he still defies readers to “demonstrate to me how objectively to judge between Maine’s Brahmin who thought he was many times more capable of satisfaction than an Untouchable, and a radical of the Benthamite tradition who assumed equal capacity all round.” (Robbins, 1971, p. 148). Or he notes that “in every day life, we do make comparisons between the satisfactions of different people. . . . [W]henever we discuss distributional questions, we make our own estimate of the happiness afforded or the misery endured by different persons or groups of persons. But these are *our* estimates. There is no objective measurement conceivable.” (Robbins, 1981, p. 5). While rhetorically attractive, such a putatively clean separation seems to obfuscate more than clarify. If common sense and empirical research are not “objective” enough, then what is? As Amartya Sen wryly observes, “we might find it absurd . . . [to argue] that there was indeed a net gain in the utility-sum from the burning of Rome while Nero played the fiddle.” (Sen, 1982, p. 22). To argue that interpersonal comparisons are somehow not “objective” conveniently becomes code for disparaging the distributional problem.

Similarly, to the extent that Robbins creates a polarity between positivism and normativity, might he merely be arguing that normative judgments are different, neither better nor worse? As he points out in the Preface to the Second Edition, “[a]ll that I contend is that there is much to be said for separating out the different kinds of propositions involved by the different disciplines which are germane to social action, in order that we may know at each step exactly on what grounds we are deciding.” (Robbins, 1935, p. ix). Indeed, he continues in the body of *Essay*:

“Nor is it in the least implied that economists should not deliver themselves on ethical questions, any more than an argument that botany is not aesthetics is to say that botanists should not have views of their own on the lay-out of gardens. On the contrary, it is greatly to be desired that economics should have speculated long and widely on these matters,
As he further notes in his autobiography, “[a]ll that I had intended to do was to make it clear that statements about the way in which an economic system worked or could work did not in themselves carry any presumption that that was the way in which it should work.” (Robbins, 1971, pp. 147-48; see also Peston and Corry, 1972, p. vii). To his credit, Robbins displays a significant dose of modesty in this regard. He reminds us that economics “does not, and it cannot, enable us to evade the necessity of choosing between alternatives” (Robbins, 1935, p. 156) and that “by itself Economics affords no solution to any of the important problems of life.” (Robbins, 1935, p. ix).

While perhaps encouraging in its humility, such an approach might once again ultimately gloss over Robbins’s real agenda. Much like with the narrower question of interpersonal comparisons, trying to create a neat diving line between “positive” science and “normative” judgment merely skirts the problem by turning it into a definitional question. This problem is present in Robbins’s later work as well. For example, he argues that “[t]o recognize the claims of science in fields where scientific method was applicable was one thing; to attempt to claim scientific sanction for judgments of questions not capable of scientific proof was another” (Robbins, 1938, pp. 638-39), or that “[t]here can be events or institutions having an economic aspect which we ourselves regard as ethically acceptable or unacceptable. But in so far as the explanations or their causes or consequences are scientific, they are neutral in this respect.” (Robbins, 1981, p. 4). Such distinctions, while rhetorically masterful, are ultimately empty: it is easy to throw around terms like “scientific” and “neutral” without defining either. A fair reading thus suggests that he has little patience for normativity and is simply trying to toss it outside by defining it to be outside the limits of economics.

In fact, the supposed positive versus normative divide is directly analogous to the economic science versus political economy one. Again, the argument might proceed that perhaps Robbins is not privileging one over the other, but merely pointing out their separateness. As a scholar with broad intellectual interests spanning well beyond economics, Robbins may have simply been trying to set a clear boundary around economics (Howson, 2004, p. 417)-after all, Essay was apparently based on a course Robbins taught in 1929, tellingly entitled “The Nature of Economics and Its Significance in Relation to the Kindred Social Sciences.” (Howson, 2004, p. 427). As Robbins describes it in his autobiography, political economy is simply different from economic science:

“There was another level, however, on which economic analysis was conjoined with assumptions about the ultimately desirable ends of society which, because of my past training and my continuing spiritual interests, had no less a hold on my attention. In accordance with the methodological views explained above, I adopted the habit of designating such interests by the old-fashioned term Political Economy, to make clear
their dependence on judgments of value and to distinguish them from pure science“. (Robbins, 1971, p. 150; see also Robbins, 1981, pp. 7-8).

Similarly, commentators have argued that Essay is “misunderstood as an ‘attack’ on economists indulging in political economy. Far from this being the case...it is a clarification of the conditions under which economic science relates to political economy.” (Peston and Corry, 1972, p. vii; see also Alkire, 2002, p. 114). Or in Robbins’s typically elegant phrasing in a later article, all he had really wanted to do in Essay was for economists “to better realize the exact connection between the normative and the positive, and that their practice as political philosophers might be made thereby more self-conscious” (Robbins, 1938, p. 640). But here again, the problem repeats itself: where and how to draw the line between economic science and political economy?

It is fair to argue that Robbins is privileging the former. And his practically lifelong urging “that the claims of Welfare Economics to be scientific are highly dubious” (Robbins, 1981, p. 9) places welfare economics within the murky latter. Since economics should be scientific, and welfare economics is not, then all of a sudden welfare economics is relegated to outside the mainstream. Once welfare economics is out, an economic liberal like Robbins can conveniently disassociate the remainder of the discipline, the high-minded scientific part no less, from the approaches adopted by more progressive economists such as Pigou (Robbins, 1938, p. 636). To argue that Robbins is somehow not passing judgment on the superiority of the portion of economics he considers “scientific” is a bit fanciful.

Last and most nuanced is the empiricism versus apriorism divide. In the Preface to the First Edition, published in 1932, Robbins thanks Ludwig von Mises (Robbins, 1935, p. xvi), likely indicating that in its first published incarnation, Essay was written under the influence of Miesian deductivism (Cf. Aslanbeigui, 1987, p. 328). By the Second Edition in 1935 Robbins had softened his stance, noting decades later that he was by 1935 trying “to make it clear that the ultimate assumptions were elementary facts of experience whose appropriateness was always subject to testing by reference back to reality.” (Robbins, 1971, p. 149; see also Caldwell, 2004, pp. 193-94; Hollis and Nell, 1975, p. 196). Yet despite this shift, he candidly admits in the Preface to the Second Edition, he has “never been satisfied with the chapter on the nature of economic generalisations.” (Robbins, 1935, p. x). Notwithstanding this admission, where does he stand?

Robbins’s perspective on the relationship between empiricism and apriorism is a moving target. On rare occasions, he appears to suggest that if economics is a “science,” and gathering data to test hypotheses is the essence of the scientific method, then empirical experiment rather than deductive introspection is the essence of economics. (Robbins, 1935, p. 87; see also Robbins, 1938, p. 639). In one instance, he even writes—quite stunningly, given his apparent affection for deduction—that “even if we restrict the object of Economics to the explanation of such observable things as prices, we shall find that in fact it is impossible to explain them unless we invoke elements of a subjective or psychological nature.” (Robbins, 1935, p. 88). Given these assertions, it is “difficult to ignore the empirical elements that Robbins
saw as the basis for the deductive chains of economic reasoning.” (Kirzner, 2000, p. 20). Yet this emphasis on observation and testing is only a small part of the story.

In other passages, Robbins’s point is likely more modest: empirical work should merely play a supporting role to grand theory. For instance, he notes that the point is “not to unearth ‘empirical’ laws in an area where such laws are not to be expected, but to provide from moment to moment some knowledge of the varying data on which, in the given situation, prediction can be based.” (Robbins, 1935, p. 122). Another, even narrower, possibility for where Robbins stands might be the notion that economists merely need to make explicit the assumptions behind their empirical work, as Robbins seems to contend in the Preface to the Second Edition. (Robbins, 1935, p. ix). Unfortunately, clarity and consistency about what value Robbins ultimately ascribes to empirical work are elusive. One scholar has even devoted a paper to grappling with this question, concluding that there are “two inconsistent themes in Lionel Robbins’s methodology. The first is the anti-empirical emphasis on introspection due to the different nature of social sciences as opposed to the natural sciences. The second is his implicit and explicit emphasis on observation and empirical verification.” (Aslanbeigui, 1987, p. 333). Notwithstanding this important observation, on balance it is relatively safe to posit that even in the Second Edition, Robbins wishes to emphasize deductive introspection over inductive empiricism. He admits as much in his autobiography, where he characterizes his stance as “a reaction-doubtless overdone-against the ridiculous claims of the institutionalists and the cruder econometricians and an attempt to persuade Beveridge and his like that their simplistic belief in ‘letting the facts speak for themselves’ was all wrong.” (Robbins, 1971, p. 149; cf. Ross, 2005, p. 88). Overdone or not, Essay privileges introspection over hypothesis-testing.

To be sure, Essay is overall more complex than it first appears. As one writer has observed, to “note that Robbins’s position contains elements of subjectivism, methodological individualism, and the belief that the basic postulates of economics are self-evident may be less concise than affixing a label to his views, but is also more accurate descriptively.” (Caldwell, 1982, p. 106; cf. Blaug, 1978, pp. 697-98). One must also bear in mind that Essay reads largely like a series of lectures, and thus fulfills a pedagogical role to present challenges, questions, and ambiguities-rather than to read as a straightforward and unambiguous exposition. In addition, the work was written over several years (Robbins, 1935, p. xiii; Howson, 2004, p. 440), and the author’s views may have simply evolved across pages and chapters.

All of these observations may be fine as far as they go, but the argument that Robbins was simply and innocuously urging readers to make their subjective value statements explicit seems incomplete at best. To be sure, there is some ambiguity over where Robbins stands precisely, notably with respect to the empiricism versus apriorism divide. But careful reading suggests he remained firmly convinced of the distinction between normativity versus positivism, interpersonal comparisons versus ordinal rankings, and political economy versus economic science-and in each case, even decades later, he continued to privilege the latter over the former. In sum, Essay seems to espouse a formalistic belief that there is a privileged realm consisting of “economic science”—a space limited to scarcity, individuals, ordinal rankings, and positivism. Messy but essential concepts like welfare, social institutions,
interpersonal comparisons, normativity, political economy, and perhaps even empiricism are conveniently defined out of bounds. Ostensibly, he claims the latter do not belong because they are not scientific or objective. But careful reading of Essay, in conjunction with Robbins’s other writings on methodology, suggests he really cares little for them—at least in his capacity as an academic economist.

4. Narrow Boundaries

Robbins’s polarities do not withstand critical inquiry. Unfortunately, however, these attractive yet flimsy arguments, taken together, have unnecessarily narrowed the boundaries of economics’ involvement in public policy. Perhaps this predicament should come as no surprise, since in a putative effort to make economics scientific, Robbins has simply defined away many of its most interesting and enduring questions.

Problems start immediately with the definition. A simple philosophical critique suggests that Robbins relies on scarcity, or choice, as his underlying paradigm “without much consideration of what ‘choice’ actually meant in philosophical terms.” (Hodgson, 2001, p. 197). More pragmatically, Robbins’s definition “fails to exclude coercive power as a means of acquisition of scarce means. It fails also to distinguish between the ‘intermediate’ and ‘ultimate’ sectors of the means-end chain. It concerns not merely acquisition and allocation of means, but also the processes of ‘want-satisfaction’ themselves, and any other manner in which human ends may be related to scarce means.” (Parsons, 1934, p. 528 n.7). Choice, at least taken alone, is too broad a concept to be analytically useful. As James Buchanan has noted, “in his attempt to remain wholly neutral as to ends, Robbins left economics ‘open-ended,’ so to speak.” (Buchanan, 1964, p. 214; see also Marciano 2007). Or in Coase’s plain words, making “economics a study of human choice... is clearly too wide if regarded as a description of what economists do.” (Coase, 1978, p. 207). Such an abstract definition, while rhetorically elegant, is left wanting.

Robbins’s definitional problem, however runs deeper than an academic debate. Ironically, Essay’s great success has sown dissonance between mainstream academic conceptions and real-world applications. As Geoffrey Hodgson summarizes:

“In global academic circles, it is Robbins who has had his way. Economics is now narrowly conceived, within most university departments of economics, as ‘the science of choice’. Regrettably, the alternative and much broader definitions, by prominent figures such as Marshall and Coase, have not prevailed in academia. But outside the college walls, among the lay public and in the bustle of business, there is a commonsense view that economics is, or should be, concerned with the workings of a money and market economy. The tension between these two conceptions of the subject has led to all sorts of havoc and misunderstanding. The definition of economics is under dispute. On the one hand are the academic proponents of ‘science of choice’. On the other are the more worldly practitioners, who would define economics in
Perhaps this dissonance should not be altogether surprising. After all, economic analysis must take into account context to be meaningful in addressing real-world problems—yet, “[i]n a masterly stroke, he [Robbins] simply redefined economics in terms that would exclude institutionalism and historicism from within its disciplinary boundaries.” (Hodgson, 2001, p. 207). By slighting these elements, Robbins’s definition becomes of precious little practical utility.

It is precisely the slighting of institutions and history of context that breeds trouble. The attempt to separate elements of economic analysis “from the social setting which determines them (above all income distribution) may be for some purposes heuristically useful, but as a picture of reality and as a basis for recommendations it is misleading because it ignores the social determination of wants and hence of real income.” (Streeten, 1954, p. 215). For example, as Buchanan observes, a “market is not competitive by assumption or by construction. A market becomes competitive, and competitive rules come to be established as institutions emerge to place limits on individual behaviour patterns.” (Buchanan, 1964, p. 218). By contrast, Robbins’s focus on the asocial, ahistorical individual “necessarily prevented the economist from talking about important problems. Choice became an abstract act of behaviour; genuine judgment was impossible because no comment was allowed on how choice was conditioned.” (Seligman, 1962, p. 522; see also Hodgson, 2001, p. 29; Lawson, 2003, pp. 155-56). Individualistic Robbinsian neoclassicism provides an uncomfortably selective analytical framework. (Cf. Peck, 1936, p. 496).

A similar type of reductionism regretfully prevails in the privileging of apriorism over empiricism. As commentators have observed, “Robbins’s postulates are disputable, in that there is no pressing reason to accept them. This does not show them false but it does show that Robbins has no right to claim to know they are true.” (Hollis and Nell, 1975, p. 204). Crucially, “[i]ntrospection is a fallible way of arriving at generalities and exactly Robbins’s claim has been made for now suspect postulates like ‘We are all tainted with original sin’ or ‘Man is born free’.” (Hollis and Nell p. 202; see also Hollis, 1975, p. 60). Paul Samuelson has even called Robbins’s reliance on aprioristic deduction the “bad element of Robbins’s book” (Suzumura, 2005, p. 332). One does not even need to go so far as to argue that “introspectionism in general is a hopeless thesis” (Ross, 2005, p. 226) to recognize “that even the more deductive or hypothetical method of advance should be fortified by statistical verification.” (Harrod, 1938, p. 407; see also Parsons, 1934, p. 537). Apriorism may sound seductive in the abstract, but until Robbins is willing to articulate with some specificity what his postulates are and why they are scientific, it is of little use, except maybe as an exercise in abstract thought.

In a crucial sense, then, Robbins has simply assumed. Assumed that economics is defined by scarcity not welfare, that economists should analyze individuals not institutions, and that deductive methods are superior to inductive ones. A similar assumption carries over into the fourth polarity, normativity versus positivism. Robbins’s separation of economics from ethics, “derived largely from his redefinition
of economics as a science of choice in which ends (exogenously given) and scarce, substitutable means were rigidly separated. This removed ends from the legitimate province of economists qua economists, by assumption rather than argument.” (Groenewegen, 1996, p. 1; see also Vickers, 1997 p. 45). In short, Robbins apparently suggests that somehow “economics is ‘value free’ when it comes to ends.” (Caldwell, 2004, p. 187). “By stripping ends from economics, he is likely at one level motivated by his desire to make economics look like a hard science.” (Parsons, 1934, p. 521). Robbins ostensibly believes that since only positivism is scientific, economics should eschew normativity.

But this assumed separation of ends from means is deeply flawed. One is reminded of A Treatise of Human Nature where Hume discusses the unclear boundaries of “is” and “ought” (Hume, 1740, p. 469). It is overly formalistic simply to posit that description is scientific and prescription is not. As Amartya Sen observes, “[b]oundaries are not always clear-cut between prescription and description. . . Description isn’t just observing and reporting; it involves the exercise-possibly difficult-of selection.” (Sen, 1980, p. 353). Note, for example, how seemingly neutral concepts such as “rational man” or “efficiency” have embedded normative judgments supporting them. “Facts and values blend, and economics can hardly claim value-neutrality” (Mongin, 2006). The idea that one can separate out normative judgment from positive description, while seductive, is simply a fallacy. As Roy Harrod notes, “it is possibly rather ridiculous for an economist to take such a bright line.” (Harrod, 1938, p. 396). “To insist otherwise, as Robbins does, is to elevate form over function.” (Gruchy, 1949, p. 254).

A new, more functional, post-Robbinsian economics must once again appreciate “the heavy moral import of the fact that it is a human science, whose objects of investigation are thinking, sentient, acting and reacting beings” (Vickers, 1997, p. 73-74; see also Vickers, 1997, p. 36-37). A human science should inevitably be concerned with both ends and means. One need not go so far as Coase, who suggests that “problems of welfare economics must ultimately dissolve into a study of aesthetics and morals” (Coase, 1960, p. 43; cf. Pigou, 1932, p. 10) to appreciate Frank Knight’s insight that “[c]oncrete and positive answers to questions in the field of economic science or policy depend in the first place on judgments of value and as to procedure on a broad, general education in the cultural sense, and on ‘insight’ into human nature and social values, rather than on the findings of any possible positive science.” (Knight, 1999, p. 393; see also Knight, 1936, p. 425). Put simply, “there is no ‘economics’ that is not political economics. The only question is whether politics is part of the vision, or is denied, and therefore is ideology in the pejorative sense.” (Forstater, 2004, p. 415). To pretend otherwise is either naïve or disingenuous. Most ominously, pretending to strip out ethical judgments from economics suggests that economic science should not be concerned about a supervening notion of “the good,” (cf. Suzumura, 2005, p. 333) while simultaneously endorsing the status quo through its inaction.

Take the denial of interpersonal comparisons of utility as an application of this phenomenon. It is infected with the same facile reasoning that privileges positivism over normativity. Robbins’s tirade against such comparisons is vulnerable along several dimensions. First, and most simply, it defies common sense. As Cannan asked in an early review of Essay, is it necessary for economists to claim “that they
do not know what ‘better off’ means?” (Cannan, 1932, p. 426). To use just one example, psychology might provide important insights into how to make interpersonal comparisons, but Robbins has little patience for it. He dismisses the discipline as one prone to the vagaries of academic fashion (Robbins, 1935, pp. 83-84) and at one point even labels it a “queer cult.” (Robbins, 1935, p.87). But as Amartya Sen has observed, “[f]ocussing only on predicting behaviour, the richness of human psychology has been substantially ignored, refusing to see anything in utility or happiness other than choice.” (Sen, 1980, p. 362; see also Seligman, 1962, p. 524). Robbins’s reasoning is also logically inconsistent in assuming that interpersonal comparisons are subjective and therefore unscientific, whereas individual preferences somehow magically do not suffer either of these limitations. (Ross, 2005, p. 227; cf. Seligman, 1962, p. 523). Perhaps recognizing this tension, even Essay itself uncharacteristically seems to hedge on this point (Robbins, 1935, p. 86 n.1).

Second, relying on ordinal preference rankings can lead to very troubling conclusions. Take simply Pareto optimality based on ordinal rankings, which survives the Robbinsion critique of interpersonal comparisons of utility. As Amitai Etzioni notes, relying on such a construct can engender deeply problematic results:

“In their discussion of general welfare neoclassical economists draw on the principle of Pareto optimality. On the face of it, it seems nobody could fault such an objective criterion as adjusting allocations only to benefit some if they do not harm others. In effect, the principle raises many moral questions, such as whether or not all preferences are to be judged of equal standing (drug addiction?), and whether the same status is to be accorded to choices by all individuals (including criminals?).” (Etzioni, 1988, p. 247)

To use Etzioni’s examples, one would therefore have to claim that economic science should treat the preferences of drug addicts and criminals on par with those of the rest of the society. To do otherwise would, under Robbins’s logic, smack of a value judgment.

Third, and perhaps more profoundly, reliance on ordinal rankings implicitly privileges one philosophical approach over others. Defining “well-being or welfare exclusively in terms of subjective preference satisfaction, requires making a significant (and questionable) value-judgment, since well-being or welfare is also commonly defined in terms of a host of additional normative matters that the theory suppresses, such as justice, fairness, rights, liberty, and dignity.” (Davis, 2005, p. 590). In addition, as Screpanti and Zamagni observe, it is epistemologically inconsistent to assume congruence between an individual’s expressed preferences and the choices she can realistically make. In other words, we shouldn’t forget that “[p]references can be made operational [only] by means of a definition in terms of choice: the assertion ‘the state x will be chosen by a subject if only x and y are available.’ The doubt did not even cross the minds of Robbins and the other authors who followed this orientation that the definiens, as a conditional proposition, can perform its function only after the concept of preference has been defined. I may well
prefer health to illness, but I certainly cannot choose to be well or ill.” (Screpanti & Zamagni, 1993, p. 271). The claim that preference orderings are somehow more scientific than interpersonal comparisons of utility, then, is left wanting for a variety of reasons.

The practical implication of denying interpersonal comparisons is dramatic. Disturbingly, “[i]f the incompatibility of utility to different individuals is strictly pressed, not only are the prescriptions of the welfare school ruled out, but all prescriptions whatever. The economist as an adviser is completely stultified, and unless his speculations be regarded as of paramount aesthetic value, he had better be suppressed completely.” (Harrod, 1938, p. 397). One might plausibly argue that by narrowing the realm of economics, Robbins sought to give it scientific respectability-after all, one might “draw the conclusion that the scientific status of economics today has been achieved by lowering the status of ethics and by economics distancing itself from ethics.” (Davis, 2005, p. 600). To the extent, however, that the formal models of mainstream economics can even be considered scientific, this purportedly exalted status has come squarely at the expense of welfare economics.

The path from denial of interpersonal comparisons to the fall of welfare economics is eerily straightforward. Robbins’s critique has been so influential that it heralded “the beginnings of the collapse of cardinal utility theory with its richer normative framework, and ushered in ordinalist methods and Pareto efficiency judgments” (Davis, 2005, p. 592). The embrace of ordinalism, in turn, was the essential prerequisite in the move away from Pigovian marginalism toward Kaldor-Hicks efficiency. (Vickers, 1997, p. 56). Indeed, both Hicks (Hicks, 1939, p. 697) and Kaldor (Kaldor, 1939, p. 549) graciously acknowledge their debt to Robbins. And we should not forget that Arrow’s impossibility theorem is based on ordinal rankings-it only holds “[i]f we exclude the possibility of interpersonal comparisons of utility.” (Arrow, 1950, p. 342). If such comparisons are allowed, however, Arrow’s theorem is overcome, and the scope for welfare economics expands dramatically. Tibor Scitovszky well articulates the importance of interpersonal comparisons to any social function when he observes that “a change in institutions or policy almost always redistributes welfare sufficiently not to have a uniform effect on everybody but to favour some people and prejudice others. It follows from this that economic welfare propositions cannot as a rule be made independently of interpersonal comparisons of utility.” (Scitovszky, 1941, p. 79). Despite the deep problems and troubling implications of relying on ordinal preference rankings, however, there is unfortunately little interest in mainstream economics to reconsider this sacred canon. (Davis, 2005, p. 591). Robbins’s immense influence is largely to blame.

These flimsy polarities culminate in Essay’s putative separation between economic science and political economy. The central problem with such a distinction is that by relegating an essential question-distribution-to politicians and philosophers, the scope of welfare economics is narrowed and the status quo is implicitly defended. To understand this final point, it might be useful to recap briefly the recent evolution of welfare economics. “So-called Pareto optimality survives Robbins’s critique because it is operational even without interpersonal comparisons” (Waterman, 2002, p. 24). “However, the Pareto standard is useless in practice because its condition that no one be made worse off by the transaction is too stringent.” (Pigou, 1932, 647-55).
This limitation, of course, led to the embrace of so-called “modern,” or “new,” welfare economics, with Kaldor-Hicks optimality as its standard. Kaldor-Hicks conveniently posits that a transaction is welfare-enhancing if third-parties could potentially be compensated, whether or not they actually are—the famous hypothetical compensation standard (Kaldor, 1939; Hicks, 1939). Most crucially for our purposes, however, neither Kaldor nor Hicks seems to care about distribution. Hicks suggests that “whether or not compensation should be given in any particular case is a question of distribution, upon which there cannot be identity of interest, and so there cannot be any generally accepted principle.” (Hicks, 1939, p. 711). For his part, Kaldor warns that “concerning distribution, the economist should not be concerned with ‘prescriptions’ at all, but with the relative advantages of different ways of carrying out certain political ends.” (Kaldor, 1939, p 511). Kaldor-Hicks, after Robbins, was the next act in setting the stage for the separation of efficiency and equity in economics. (Persky, 2001, p. 202).

Yet the Kaldor-Hicks standard, while seemingly impressive, is logically empty. As Amartya Sen points out with a touch of humour:

“One way of extending Paretian welfare economics without introducing any interpersonal comparisons is to use a ‘compensation test’ . . . . The losers could include the worst off and the most miserable in society, and it is little consolation to be told that it is possible to compensate them fully, but (‘good God!’) no actual plan to do so. If, on the other hand, the losers are in fact compensated, then the overall outcome-after compensation-is a Pareto improvement, and then there is no need for the compensation test as a supplement to the Pareto principle. So the compensation criteria are either unconvincing or redundant.” (Sen, 1987, p. 33 n. 4)

Put bluntly, the separation of efficiency and equity is false: one cannot simply put a box around efficiency and ignore the distribution of economic power in society (Seligman, 1962, p. 522).

Perhaps, though, given the serious problems inherent in the polarities discussed above, this logical inconsistency inspired by Essay should not come as a surprise:

“Robbins may have felt that the market accurately did reflect values, but his theory could in no way comment on valuations which stemmed from economic coercion because it assumed a scarcity of small buyers and sellers. Resource allocation based on power did not enter into the individualist analysis. One could not help but wonder whether the proper questions were being asked in the first place.” (Seligman, 1962, p. 521; see also Streeten, 1954, p. 215; Hodgson, 2001, p. 33)

But if we are asking the wrong questions and the separation of efficiency cannot “be convincingly sustained in practice” (Veljanovski, 1980, p. 178), then what explains its staying power? Perhaps precisely because Robbins’s world view has meshed well with a fashionable laissez-faire agenda prevalent over the past few decades. Such a world view, among other facile assumptions, takes the initial distribution of entitlements as given:
“Modern welfare economics has an unfortunate conservative bias. If situation A prevails, the welfare economist cannot recommend the adoption of situation B if the economic reorganization involved would leave some people worse off than before. But if situation B prevails, the economist cannot urge the adoption of situation A. To the extent that economic changes would affect some individuals adversely, welfare economics provides a justification for maintaining the status quo.” (Allen, 1952, p. 34; see also Persky, 2004, p. 936)

By contrast, and as Robbins himself readily admits, economics once cared both about efficiency and equity, production and distribution (Robbins, 1935, p. 64). Regrettably, Essay’s greatest impact has been to pluck the former out of economics. As one scholar laments, “the central point is that Robbins left economics without the ability to value gains in equality. Equality had been made an incommensurable and noneconomic objective.” (Persky, 2004, p. 934).

Robbins’s influential work thus engendered a precipitous decline in the legitimacy of a welfare economics formerly concerned with issues such as distribution and equality. As Amartya Sen has noted, works such as Essay “kept prescriptive studies somewhat immersed in a pool of apology from the mid-thirties until relatively recently.” (Sen. 1980, p. 363). Sen continues:

“Welfare economics was for a long time the “untouchable” in the community of economics and when economists spoke “qua economist” to use that lovely expression brought into circulation by positivism-they tried to speak in a value-free “scientific” language, with “expletives” deleted. Welfare economics was seen as the subject, if not of expletives, at least of emotive utterances, which the cool positivist scientists found “meaningless” in terms of the their narrow theory of meaning.” (Sen, 1980, pp. 363-64)

Robbins’s polarities had successfully argued welfare economics into impotence.

Instead, mainstream economics the descendant of Robbins’s economic science has found comfort in increasingly sophisticated mathematical models that somehow give the illusion of certainty, no matter how divorced these numerical abstractions might be from the pressing problems that beset society. As James Buchanan presciently warned in 1964, largely in response to Robbins’s reliance on equilibria, “[i]f there is really nothing more to economics than this, we had as well turn it all over to the applied mathematicians.” (Buchanan, 1964, p. 216; cf Coase, 1998, p. 72). Sadly, Buchanan’s worry has been borne out. The importance of welfare economics has gotten lost in a sea of beguiling mathematical models, each promising to offer the answer. As one scholar laments:

“The damage that Robbins created by redefining economics as the science of choice will not be easily undone. The minority of dispossessed economists, concerned above all with the study of the provisioning institutions of a market economy, are not contented that their territory and opportunities have been confiscated by the mathematical formalists of
abstract choice. Against the hegemonic majority they claim legitimacy for their territorial claim.” (Hodgson, 2001, p. 348)

While mathematical modelling seems neutral and scientific, it too often skirts the critical questions by hiding them in often unstated assumptions.

In the end, Robbins dealt a devastating blow to the legitimacy of welfare economics. Somewhat oversimplified, the essence of his argument seems to be that science is objective, but interpersonal comparisons of utility are not; since it thus becomes impossible to measure objectively the impact of policy decisions, there is precious little space for welfare economics within economic science. As one commentator laments, “[i]t began to seem as if Professor Robbins had written an essay on the nature and insignificance of economic science.” (Allen, 1952, p. 29). Fortunately for those of us who continue to believe in the significance of economic science, Robbins’s arguments toward insignificance simply do not withstand careful scrutiny. While rhetorically masterful, “[t]he battle against historicism and institutionalism had been won—but more by act of definition than by force of theoretical argument or achievement.” (Hodgson, 2001, p. 209; cf. Veljanovski, 1980, p. 192). Stated succinctly, Robbins was “wrong...in various crucial assumptions.” (Ross, 2005, p. 123; see also Parsons, 1934, p. 534 n. 4). By using the concept of polarities to question these very assumptions, this article has sought to join the debate.

5. Conclusion

Perhaps part of Essay’s extraordinary success is its allure of simplicity: the notion that economic methodology has definitions and boundaries that can be neatly systematized. (Cf. Groenewegen, 1996, p. 2; Hodgson, 2001, p. 213; Suzumura, 2005, p. 331). But it is time to move beyond Robbins’s narrow conception of welfare economics toward a conception more robust and useful. The discipline must, after the decades-long hiatus created largely by Essay’s success, begin to think about how economics relates to ethics (Groenewegen, 1996, p. 1). Similarly, economics should move closer, not further away, from other social sciences. (Hodgson, 2001, p. 197). While Robbins may be correct that “there is no agreement yet on the ultimate desiderata of the good society,” (Robbins, 1981, p. 8) economists need to be part of the debate.

In other work, I have attempted to help make welfare economics relevant again by offering a social welfare function that takes both consequential and deontological perspectives into account. I argue that, like it or not, social choice theory must come to grips with two functions: paternalism and redistribution. The former transforms a set of individual preferences into a set of modified individual preferences; the latter aggregates these modified individual preferences into social welfare (Dibadj, 2006). The approach is offered as a starting point, albeit imperfect, to reframe the discussion and make welfare economics relevant again. For instance, it avoids the intractable question of defining utility as well-being or relying on individualistic preference rankings. Instead utility is redefined with respect to how closely a set of individual preferences maps to a set of welfare-enhancing preferences.
Notwithstanding my critique and alternative approach, I readily acknowledge that *Essay* is a masterpiece. Perhaps most profoundly, its extraordinary scope provides a rich roadmap for future discussion and research. As Robbins observes in his autobiography, the goal of his book is “to investigate the nature of economic generalizations, the status of empirical studies and finally the significance of all this for the conduct of policy.” (Robbins, 1971, p. 145; see also Robbins, 1935, p. xiv; Caldwell, 2004, p. 186). This constitutes a tall order, especially if placed as the paradigmatic contribution to Robbins’s larger unifying search in economic theory “for a coherent apparatus of analytical thought.” (Robbins, 1971, p. 145) in economic theory. While this article has argued that Robbins has taken welfare economics down the path of irrelevance, he must nonetheless be applauded for having the courage to broach the immensely complex topics he does.

Constructive disagreement with regard to a work of such vast ambition is not only inevitable, but healthy. After all, “the matters upon which he [Robbins] has dwelt in his long career are questions of fundamentals; these are the areas where intellectual battle must take place, and there is no place in them for the faint-hearted or uncommitted.” (Peston & Corry, 1972, p. vii). In the end, perhaps it is less important whether one agrees or disagrees with Robbins’s approach and conclusions. The real value of *Essay* may lie in the forum it provides to debate the multitude of basic issues it so presciently raised.
References


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