

ROBBINS'S *ESSAY* AND THE AXIOMATIZATION OF ECONOMICS*

BY

ROGER E. BACKHOUSE AND STEVEN G. MEDEMA

I. INTRODUCTION

Lionel Robbins's *Essay on the Nature and Significance of Economic Science* (1932, 2nd edition 1935) was not arguing for an axiomatic economics as this term is commonly understood.¹ Though he was a prominent economic theorist, and though he encouraged younger colleagues who were using mathematical methods—John Hicks being the most notable example—Robbins was not a mathematical economist. In his preface, he acknowledged his “especial indebtedness” to the work of Ludwig von Mises, who stood in the Austrian tradition of opposition to the use of mathematical methods in the social sciences. According to this tradition, there was a fundamental distinction between *Naturwissenschaft* and *Geisteswissenschaft*, with the result that mathematical methods that were successful in the former were inappropriate for the latter. Whilst Robbins did not go this far, regarding the use of mathematics or words as “solely a matter of convenience” (1932, p. 81, n. 1), and though he considerably softened his Misesian language in his second edition, his sympathies with the Austrian approach represented by Mises were clear.²

The move towards axiomatization, as the term was understood in modern economics, on the other hand, came later, and grew out of a different intellectual culture from the one he inhabited—that of the United States during and after the Second World War. However, a vital input into this culture came with the arrival of many emigrés from continental Europe as a result of the threat and subsequent reality of Nazism. Amongst these were John von Neumann and Abraham Wald who, in the 1930s, had presented mathematical proofs of the existence of general

*Backhouse: Department of Economics, University of Birmingham and Erasmus Institute for Philosophy and Economics, Erasmus University Rotterdam (r.e.backhouse@bham.ac.uk). Medema: Department of Economics, University of Colorado Denver (steven.medema@ucdenver.edu). We are grateful to two anonymous referees and to Robert Leonard for invaluable comments on an earlier version of this paper, previously presented at the European Conference on the History of Economics held at Siena in October 2007.

¹Though we focus on the use of formal axiomatic methods, exemplified by Debreu (see below), these shade into the attempts to use rigorous mathematical models that do not meet the formal requirements of an axiomatic system. Drawing a precise boundary is not necessary for the argument that follows, for such work shares the key feature of being driven by the requirements of mathematical rigor (on which see Weintraub 1998).

²Terence Hutchison (2009) argues that the changed language did not change the substance of Robbins's original claims about economic theory.

equilibrium to Karl Menger's mathematical seminar in Vienna.³ Karl Menger, along with Oskar Morgenstern, another participant in the seminar and later to be von Neumann's collaborator on *The Theory of Games and Economic Behavior* (1944/1947), was amongst those who challenged the traditional Austrian view, held by Mises, of the *Naturwissenschaft–Geisteswissenschaft* distinction, arguing strongly for the use of mathematical methods in economics. As Morgenstern put it, "the potential use of mathematics in the social sciences means nothing else but that their problems can be formulated and treated in an exact manner" (Morgenstern 1936/1976, p. 390), and he felt that the development of scientific theories in the social sciences, such as economics, required the application of the axiomatic method.⁴

It was these Austrian emigrés who paved the way for economists to move beyond the use of differential and integral calculus to embrace a much broader range of mathematical tools, including set theory and real analysis.⁵ What was more important was that it involved reformulating the theory "within a framework of hypotheses perfectly delimited and rigorously expressed in mathematical language in such a way as to allow the freest and most complete mathematical developments," even if it was not possible for the theory to remain as realistic as might be desired (Ingrao and Israel 1990, p. 176).⁶ This approach is often said to have reached its perfection in Gerard Debreu's *Theory of Value*, which itself stimulated a great deal of work in axiomatic general equilibrium theory even though it described an economy that could not possibly exist.

In the standard account of the rise of mathematical economics, discussed in Section 3 below, the Robbins definition appears inconsequential: it appears to do no more than sum up developments that would have happened anyway. Such an account might explain why, aside from economists associated with the Cowles Commission—arguably the main center of mathematical economics in the United States—economists who discussed the Robbins definition in the journals were, at least until the 1960s, generally critical,⁷ whereas from the 1960s, *without there having been any serious arguments in its favor*, at least in economics journals, economists began to write as though the Robbins definition was generally accepted.⁸

Our argument is that the Robbins definition may have been much more important than this in that, despite the fundamental differences between Mises and

³Ingrao and Israel (1990, p. 255) go so far as to suggest that modern general equilibrium theory, and with it the use of the axiomatic method in economics, "provides perhaps the best example of the effects of Nazism in impoverishing scientific culture in Europe and developing it to the point of unchallenged supremacy in the United States."

⁴While von Neumann is often the focus of the histories, Morgenstern's role should not be minimized, not least because of his influence on the undergraduate Gary Becker.

⁵Weintraub (2002, 2008); Debreu (2008).

⁶Note that this is a weaker statement than Ingrao and Israel's claim that these developments proceeded "with no worries as to how realistic the model was."

⁷This literature is comprehensively reviewed in Backhouse and Medema (2009) and no attempt will be made to discuss it systematically here. Support came from Cowles Commission members Lange (1945) and Tintner (1953), and from Murray Rothbard (1957, p. 314), an opponent of mathematical economics, who recognized in Robbins, so he thought, a fellow praxeologist.

⁸Thus Harry Johnson (1960, p. 552) wrote that most economists "would probably accept it," and Hicks (1960, p. 707) could simply assume that it was an appropriate definition of the subject. Even an institutionalist critic, Kapp (1968, p. 2), admitted that it characterized "very well the prevailing preoccupation of many economists."

Robbins on the one hand, and Morgenstern and postwar mathematical economists on the other, there are important connections between the rise of axiomatic methods in economics and acceptance of the Robbins definition of economics. In the 1930s and 1940s, many economists believed that significant parts of economics could not be accommodated within the Robbins definition, and they rejected it or argued that it needed to be qualified in significant ways. These objections related to the scope of economics were inextricably linked to objections that were being raised to the increased use of mathematical methods in economics. If economics was to be dominated by rigorous economic theory based on consumers' utility maximization (or some analog such as revealed preference) and cost minimization—with axiomatic methods at the heart of the modeling process—it was necessary that economics be defined according to the Robbins definition, thereby excluding those problems for which this type of economic theory could not provide a solution. It is, of course, possible that economics *could*, in principle, have been axiomatized on foundations that did not fit so well with the Robbins definition, but this did not happen. Our claim is that in this period the spread of mathematical methods and axiomatization was closely linked to the narrowing of economics implied by acceptance of the Robbins definition, a narrowing that applied not merely to what came to be known as “positive” economics but also to welfare economics, which was pared down to what could be said rigorously, focusing on the concept of Pareto efficiency.

The problem will be addressed in three stages. The first is to present, in Section 2, relevant facets of Robbins's discussion. In Section 3, we present what we describe as the insiders' view. Gerard Debreu and Tjalling Koopmans are used to show that axiomatization of economics as understood or practiced by the Cowles Commission was associated with a vision of economics that fitted squarely within the Robbins definition of economics. Section 4 then presents evidence on how this view of economics was challenged, showing that if axiomatic methods were to be accepted, either it had to be recognized that they dealt with only a part of economics, or the scope of economics had to be narrowed so that it could be encompassed by the Robbins definition. The paper ends by concluding that the Robbins definition played an important role and summarizing how that role should be understood.

II. ROBBINS'S VIEW OF ECONOMICS

Robbins defined economics as “the science which studies human behavior as a relationship between ends and scarce means which have alternative uses” (1935, p. 16). He went on to say that that “Economics is essentially a series of relationships—relationships between ends conceived as the possible objectives of conduct, on the one hand, and the technical and social environment on the other” (1935, p. 38).⁹ This not only ruled out ends from the subject matter of economics—with significant implications for welfare economics—but also ruled out “the technical and social environment,” on the grounds that “It is the *relationships* between these things and not the things in themselves which are important for the economist” (1935, p. 38, emphasis added).

⁹In the first edition, Robbins uses the expressions “a relationship” and “tendencies to conduct” (1932, p. 37).

Referring to this statement, Terence Hutchison criticized Robbins for excluding “all facts” from economic analysis, in that the technological, psychological, and social facts that Robbins pushed to the side “comprise the entire possible factual material for the social scientist (1938, p. 54). Rather than studying things economic, Hutchison argued, Robbins would make the “task of the economist” the “pure deduction from selected postulates of what we have called “propositions of pure theory,” that is, propositions devoid of all empirical factual content and concerned solely with terminology” (1938, p. 54). Of course, as Nicola Giocoli (2003, p. 86) has pointed out, Robbins’s approach also freed economists from having to ground their theory in any specific psychological doctrine.

Robbins argued, in a manner reminiscent of Mises, that “The propositions of economic theory, like all scientific theory, are obviously deductions from a series of postulates” (1935, p. 78). In the first edition, he linked this specifically to his definition of economics:

In the last analysis, therefore, our proposition [that price ceilings below the market equilibrium price lead to excesses of demand over supply] rests upon deductions which are implicit in our initial definition of the subject-matter of Economic Science as a whole. Economics is concerned with the disposal of scarce goods with alternative uses. This is our fundamental conception. And from this conception we are able to derive the whole complicated structure of modern Price Theory (1932, pp. 75–76).

This view reflected his belief that the postulates on which economics rested were true. He continued by insisting “That goods are scarce and have alternative uses is a fact. Economic analysis consists in elucidating the manifold implications thereof.” In the second edition, he elaborated on this, insisting that the postulates underlying economic analysis *do* in some way reflect “simple and indisputable facts of experience” relating to manifestations of the scarcity principle in reality (1935, p. 78). The three basic postulates on which economic theory rests are simple: individuals have ordered preferences, production involves more than one factor, and agents are uncertain about the future. Robbins believed that these are at once trivially obvious and yet can be (and are) both used for and necessary for the construction of “the complicated theorems of advanced analysis” (1935, p. 79). Their “realism” matters because the existence of the conditions reflected in these postulates is what gives the derived results a measure of “general applicability” (1935, p. 79).

However, there were complications. Two significant and regularly postulated assumptions did not correspond with reality. The assumptions of rationality and perfect foresight were not meant to reflect reality but 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” (1935, p. 94). The rationality and perfect foresight hypotheses are neither of the self-evident type nor reflective of real world conditions. They have no empirical content, but rather are like propositions in logic or mathematics.¹⁰ This

¹⁰See Giocoli (2003, p. 89). Robbins says that “if it were generally realized that Economic Man is only an expository device—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 a justification of their procedure—it is improbable that he would be such a universal bogey” (1935, p. 97).

meant that there was potentially scope for empirical evidence to determine the applicability of economic theory, to suggest extensions that needed to be made to theory, and to suggest auxiliary postulates (Robbins 1932, pp. 106–7). He thus argued that economists could not ignore empirical work. This fit with an apparent softening of his attitude towards theory in the second edition, where he removed his claim that the propositions of economic theory were formal in character (Robbins 1932, pp. 98, 109). However, this did not undermine his belief in the primacy of economic theory derived from premises that were known to be true.¹¹

Robbins's view that "On the analytical side Economics proves to be a series of deductions from the fundamental concept of scarcity of time and materials" (1932, p. 77) is reminiscent of an axiomatic approach. Robbins considered the postulational method grounded in the scarcity-based definition of economics the key to unifying the disparate areas of economic analysis. He saw in the marginal approach "the basis for a completely unitary Economic Theory" (1932, p. 77), with both "the general Theory of Economic Equilibrium" and "the pure Theory of Money" capable of being "deduced from the fundamental conception of goods which are scarce in relation to the possible uses which may be made of them" (1932, p. 83). This is the direction in which the rise of mathematical, axiomatic approaches to economic theory was to take economics.

However, two points need to be mentioned. The first is that Robbins did not have any commitment to the use of mathematical methods. As has been mentioned already, where Karl Menger and Oskar Morgenstern challenged the view of Carl Menger and Ludwig von Mises that the methods of the natural sciences did not apply to the social sciences, Robbins took the intermediate position that the question was a practical one. He, himself, did not use mathematics, but he encouraged John Hicks, who did. It is natural to infer that the reason his *Essay* paid no significant attention to the role of mathematics in economics was that he did not believe it raised any significant issues. This differentiated him sharply from those engaged in the project of placing economic theory on an axiomatic foundation in the ensuing decades. The second is that Robbins did not see any need for what are now termed econometric techniques: the notion, characteristic of the Cowles Commission and pervasive in the Econometric Society, that rigorous theory needed to be developed alongside more powerful empirical techniques did not occur to Robbins (see Backhouse and Durlauf 2009).

III. THE INSIDERS' STORY: THE INEXORABLE PROGRESS OF AXIOMATIC METHODS

The outstanding example of axiomatic economics is, with little doubt, Gerard Debreu's *The Theory of Value: An Axiomatic Analysis of Economic Equilibrium* (1959). In his Preface, Debreu (1959, p. x) makes it clear that he is not making any concessions:

The theory of value is here treated with the standards of rigor of the contemporary formalist school of mathematics. . . . Allegiance to rigor dictates the axiomatic form

¹¹This argument is covered in much more detail in Backhouse and Durlauf (2009).

of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations.¹²

His view was that there was no rigorous analysis of the problem of equilibrium until the work of Abraham Wald in 1935–6, but that the problems being tackled had a much longer history, going back to the late nineteenth century, to Leon Walras and Vilfredo Pareto. Tools were developed by John von Neumann, S. Kakutani, John Nash, and Tjalling Koopmans, all contemporaries, and by Augustin Cournot. When assessing the history of mathematical economics, Debreu contrasted the “sweeping movement” that took place after 1944 with the preceding century during which progress had been due only to “several major scientific accidents” (Debreu 1987, pp. 401, 399). He thus painted a picture of an “inevitable phase in the evolution of mathematical economics,” and “a powerful, irresistible current of thought” released by these techniques, set off by *The Theory of Games and Economic Behavior* (von Neumann and Morgenstern 1944/1947), “which freed mathematical economics from its traditions of differential calculus and compromises with logic” (Debreu 1959, p. x). Though a modern development, it was a natural outgrowth of earlier ideas, for the use of mathematics had been “invited” by deductive reasoning, a long-established characteristic of economics.

Debreu’s expressed view on the relationship between economic theory and its application contrasted with the position taken by other proponents of axiomatic methods in economics. Von Neumann (1947, p. 196) wrote eloquently of the dangers of a mathematical discipline becoming detached from reality: “at a great distance from its empirical source, or after much ‘abstract’ inbreeding, a mathematical subject is in danger of degenerating.”¹³ Similarly, Morgenstern (1963) was concerned not simply with economics as an axiomatic discipline, but also with the accuracy of economic observations. They both attached great importance to the fact that their theories referenced reality, even if the axioms abstracted significantly from it. Thus they located the origins of *The Theory of Games and Economic Behavior* (1944/1947) in “the attempts to find an exact description of the endeavor of the individual to obtain a maximum of utility, or, in the case of the entrepreneur, a maximum of profit” (1944/1947, p. 1). Doing so meant accounting for the strategic element, which in turn brought in the theory of games, with its axiomatizable structures of rules. However, this different emphasis did not cause them to compromise on the mathematical rigor with which their arguments were developed.

Debreu did not reflect on the relation of axiomatic work to economics more generally. An economist who did was Koopmans, in his *Three Essays on the State of Economic Science* (1957). In many ways, Koopmans held a view of economics that appears to be very different from Debreu’s: Koopmans, like Robbins, emphasized that progress came through interaction between observation and reasoning and even admitted that the subject matter of economics might necessitate a certain informality (Koopmans 1957, p. 131). Yet his approach was fundamentally in line with Debreu’s, for theory was presumed to have a “postulational structure,” starting with a set of

¹²It is natural to assume that the first sentence refers to the Bourbaki group, with whom Debreu was associated in France in the 1940s. See Mirowski and Weintraub (1994).

¹³This is quoted at greater length in Backhouse (1998).

postulates and then applying the rules of logic. These postulates would need interpretation, but interpretation was logically separate from the reasoning itself (1957, p. 133).

Koopmans (1957, p. 135) explicitly linked the postulational, or axiomatic, method with the Robbins definition of economics, noting that “The postulates of economics are concerned with human ends and choices of means, and with technological and physiological possibilities for production and consumption.”¹⁴ Koopmans (1957, pp. 135–6) then turned to Robbins (1935, pp. 78–80) for an account of what “the postulates of economics” were. Here he was being explicit in making a claim about what economics is. Unlike Debreu, he does not say that he is talking about the theory of value, which might be a part of economics, but about economics itself. In his ensuing discussion of methodology Koopmans focused on the status of the postulates—whether they were obviously true (Robbins) or whether their truth was irrelevant to the usefulness of the resulting economic theories (Friedman), which he resolved by arguing that economics comprised a sequence of models, each of which was more realistic than the previous one (Koopmans 1957, p. 142–4).

Koopmans’s justification for an explicit postulational approach rested on the grounds of clarity concerning the basis on which economic theory rested, economy of effort in its development and improved communications between the sciences (1957, p. 145).¹⁵ Formalizing “the essential ideas” permitted the emergence of a division of labor both within economics (between theorists and applied workers) and across disciplines. The use of mathematical methods could ensure that assumptions were not contradictory, a step towards axiomatization (1957, p. 172–3).

Thus Koopmans (1957, p. 4) could praise Paul Samuelson for having, in his *Foundations of Economic Analysis* (1947), brought out “the common logical structure of these problems [in diverse parts of economic theory],” this being “maximization under constraints” imposed (one can infer from his many illustrations) by scarcity. He saw his own task as being to take the process a step further, applying new tools (notably the linear analysis presented in his first essay). The sequence of models he envisaged was thus driven by new tools that permitted economic theory to be expressed more simply and with greater clarity.

Given the problems that critics raised concerning the inadequacy of mathematical tools, it is important to note the specific problems Koopmans picked out. These were that constraints were inequalities rather than equations; that non-negativity constraints were overlooked; and that calculus analyzed only local optima. None of these raised any fundamental conceptual economic issues. They raised problems because they meant that the conclusions reached lacked either necessity or sufficiency. There is no suggestion that this process raised questions about the nature of the theory in question.

Thus when Koopmans discussed Alfred Marshall’s verbal analysis, he wrote as though Marshall’s text was doing no more than providing an account of the mathematical analysis that lay beneath it, in the footnotes and appendices: “the technical aspects of his reasoning are somewhat between the lines, or relegated to

¹⁴See also Koopmans (1957, pp. 133, 169).

¹⁵Koopmans’s view was entirely consistent with the prevailing philosophy of science, which focused on the logical structure of economic theories.

appendices” (Koopmans 1957, p. 131). He could do this because of “the fortunate circumstance that the most important insights achieved by economic analysis” can be “strongly suggested” by a non-technical form of discourse (ibid.) Verbal expositions such as Marshall’s were what Koopmans chose to call a “Diplomatic style” (contrasted to his favored “Postulational style”). While it had the advantage of appealing to those without relevant technical training, for those who did, it was made learning the subject harder:

I can testify from personal experience about the obstacles encountered by one trained in another field who embarks on the study of economics and seeks to absorb its substance from Wicksell’s *Lectures* or Marshall’s *Principles*—perhaps the best textbooks handed down to us from the period in which the basic ideas of current economic theory were conceived (Koopmans 1957, p. 145).

Koopmans’s third essay (1957, pp. 169–220) dealt with the interaction of tools and problems, implying that he may have a broader view than we are claiming. He wrote of tools having lives of their own, and he cited George Stigler and Maurice Allais on the dangers of over-emphasizing formal reasoning (1957, p. 173) in a manner reminiscent of the remark by von Neumann quoted above. However, in developing this, what he said was that the economic problem under consideration and the postulates deemed appropriate for the study determine the mathematical tools necessary for the analysis. That is, the “postulational structure” of the mathematical tools parallels that of the theory being constructed (1957, p. 177). Yet though he wrote of “mathematical” and “literary” economics coming together, he saw the latter as expounding the former. This is a very limited interaction between tools and problems, containing no suggestion that the interaction of tools and problems might involve tools dictating problems in the manner implied by von Neumann—in a way that might call into question his underlying conception of economics.

From Koopmans’s perspective, the adoption of Robbins’s definition of economics was uncontroversial. For him, economics *was* about issues such as constrained optimization and the efficiency of the price mechanism in enabling consumers to achieve preferred outcomes, all of which are encompassed by seeing the subject as allocating scarce resources that have alternative uses to achieve given ends. However, the historical problem is that economists did not see things this way in the 1930s and 1940s.

IV. AXIOMATIC METHODS AND THE SCOPE OF ECONOMIC THEORY

The significance of the Robbins definition of economics depends on how it is interpreted. A “loose” interpretation simply postulates scarcity but leaves open the conclusions drawn. John R. Commons (1931), for example, accepted the basic fact of scarcity but, apart from deducing the need for institutions that would enable conflicts to be resolved without violence, drew few conclusions from it. An inductive approach, analyzing the historical evolution of capitalism’s legal foundations, was required to understand how problems of resource allocation were in practice solved. In contrast, Robbins’s *Essay* proposed a “tight” interpretation, according to which economic theory was “the study of the *formal implications* of this relationship

between ends and means” (Robbins 1932, p. 37, emphasis added),¹⁶ implying that economics is thus about starting with the definition and working out what it implies. This greatly narrows the scope of economic theory, which comprises only deductions that necessarily follow from the relationship between ends and means (or are believed to follow from it).

This narrowing was crucial to the spread of axiomatic methods because axiomatization, as Koopmans’s discussion made clear, was based on the fact of choice, assumed to be a universal phenomenon, and could therefore provide a completely general economic theory. Theorizing based on the Robbins definition ruled out the construction of theories based on assumptions that were not completely general. Contemporaries saw this as elevating the position of a certain type of theory. Thus Dobb (1933, p. 590, quoting Robbins 1932, p. 75) wrote

Professor Robbins . . . emphasises the purely formal character of economic theory, without, however, seizing the full implications of this statement. . . . 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.”

Because theory was, for Robbins, known with certainty, empirical work played no essential role.¹⁷

An economist who spelled out in great detail the narrowing and anti-empirical implications of this was Rutledge Vining (1949). He criticized the axiomatic methods of the Cowles Commission, represented by Koopmans (1947), precisely on these grounds. If one knew the correct theory, and if that theory was based on the behavior of individuals with fixed motives or preferences, the methods of the Cowles Commission made sense. However, he denied that this was the case: not only was it yet to be established that individual motives were fixed, but it was not necessarily the case that the behavior of aggregates could be derived from the behavior of individuals. He drew on physical analogies to argue that theories might be based on relationships between aggregates that bore no known relationship to the behavior of individuals. One implication was that empirical work was about the *discovery* of economic relationships, not simply about testing, meaning that measurement was as creative an activity as deduction or testing. Another implication, clearly implied though admittedly not drawn explicitly by Vining, was that axiomatic methods were of limited use. If the properties on which theories were to be based could be found only through empirical work, they would not be completely general. The dream of finding a completely general set of axioms on which economic theory could be based might be one that could never be realized.

Axiomatic methods ruled out, at least to Vining’s mind, economics as conceived by Wesley Mitchell and Arthur Burns, and much of the National Bureau of Economic Research (NBER). Insofar as it was based on observed regularities rather than being a formal (necessary) consequence of the fact of scarcity, Keynesian economics was also excluded from the status of economic theory. From the perspective of a tight interpretation of the Robbins definition, it became part of economic theory only when

¹⁶In the 2nd edition (1935, p. 38) Robbins added “on various assumptions concerning the ultimate data.”

¹⁷See Backhouse and Durlauf (2009). Backhouse and Medema (2009) discuss criticisms made along these lines at the time.

it was grounded in individual optimizing behavior; if the empirical evidence for Keynesian phenomena was accepted, success of the project of axiomatizing economics *required* that such microfoundations be provided. However, whilst the end result might be a theory consistent with the tight interpretation of the Robbins definition, the process whereby it was reached vindicated Vining's view that economics developed through discovering relationships between aggregates on which theories could be constructed.

It was not simply institutionalists or economists at the NBER who had a vision of economics that was incompatible with the idea of an axiomatic economics based on Robbins's definition. Roy Harrod (1938, p. 387) is interesting because he went so far as to accept, citing Robbins on the point, that the static laws of value and distribution could be derived from a single principle. He even outlined something close to an axiomatic method, attributing it to David Ricardo (Harrod 1938, p. 398). Furthermore, his arguments about the need to quantify those laws so that predictions could be made sound very much like the approach of the econometricians at the Cowles Commission. However, Harrod stopped short of endorsing this program, favoring a broader approach to economics than could be provided by an axiomatic approach. He wrote,

There is no reason why the quest for causal laws should be limited to those propositions which may be derived from the law of demand. We may expect future progress to lie outside that ambit (Harrod 1938, p. 402).

His main example was dynamic theory, which depended on principles unrelated to the static theory of value. He also argued that due to the "fog of uncertainty with which the entrepreneur is ... shrouded" it was necessary to develop inductive methods, including the questionnaire methods of the Oxford Economics Research Group that were later highly controversial, to establish how firms behaved.

Even more significant, Harrod offered a vision of how economics developed that was closer to the one later outlined by Vining than to an axiomatic approach. The "radically empirical method" of Mitchell, Harrod (1938, p. 408) argued, had not been as barren as had sometimes been suggested. He cited two examples, Gresham's Law and the tendency of interest rates to rise in times of economic expansion, to make the point that these were empirical discoveries that were afterwards given *ex post* rationalizations by theorists. In other words, Harrod was adopting a loose interpretation of the Robbins definition: economics may be consistent with it but it does not provide an exclusive research agenda. Economics is, at least in practice, broader, resting on propositions derived from empirical inquiries that have no axiomatic foundation. Economics has to go beyond the axiomatic method.

This view echoes the position of Alfred Marshall, whose work lay behind so many debates over economic theory in the 1930s. He would never have accepted Koopmans's account of the relationship between mathematical and verbal analysis. Pure theory, which included mathematical theory, was essential but comprised only a small part of economics. His theory of partial equilibrium was not a confused simplification of the general equilibrium theory described in his mathematical appendix: it was an exploration of factors that were incapable of being encompassed within a simple deductive model. This meant that the movement away from Marshall in the 1920s and 1930s was not simply about sorting out Marshall's logical

errors—though some, such as Piero Sraffa (1926) and Robbins (1928), might present it that way—but involved simplifying economic theory to the point where it became equilibrium analysis that was amenable to being placed on an axiomatic foundation. Theories of actual markets, informed by reasoning about ideal markets, were replaced by theories of ideal markets. Describing the contrast between Marshallian economics, represented by her Cambridge colleagues Dennis Robertson and Gerald Shove, and equilibrium theory, Joan Robinson (1933a, p. 124) wrote

Indeed, it is obvious that his realistic method of analysis and my highly formalised method do not operate in the same *terrain*, and any argument which turns upon the *results* obtained from such different sets of assumptions must in the nature of the case be idle.

This difference also marks the gulf between Robinson's *Economics of Imperfect Competition* (1933b) and Edward Chamberlin's *Theory of Monopolistic Competition* (1933). The latter was in the Marshallian tradition, paying great attention to dimensions of competitive behavior that could not be formalized within the confines of an equilibrium theory. The point of this is not only that there were still, in the 1920s and 1930s, many economists who rejected the Robbins definition, but also that influential and important work was being carried out that did not fit within the definition. For the axiomatic methods that were being developed from the 1930s to the 1950s to be seen as central to economics, the Robbins definition had to be accepted.

So far, the discussion has been about the “positive” economics but a similar case can be argued in relation to “normative” economics. The difference was that in welfare economics the crucial element was ruling out all but completely general, and seemingly uncontroversial, ethical judgments. The changes that accompanied acceptance of the Robbins definition are discussed in Backhouse (2009)¹⁸ and need not be rehearsed in any detail here: in brief, their effect was not simply to challenge Pigovian welfare economics, the utilitarian foundations of which could, at least in principle, be placed on an axiomatic foundation, but to undermine any welfare economics based on what might be called context-specific ethical judgments: ethical judgments that were taken as generally accepted within a given culture, even if that culture was believed to be mankind as a whole and the ethical judgments were believed to be rooted in human nature. Such value judgments could not, at least given the knowledge available in the 1930s, offer any basis for a completely general axiomatic theory.

V. CONCLUSIONS

In the period under consideration, there were many economists—probably a clear majority of those publishing explicit discussions of methodology—who saw economics as an empirical discipline for which formal theory was of limited use. Thus if economists were to claim that the axiomatic (or postulational) method was the right one, it was necessary to provide a definition of the boundaries of economics that had logical force. It is, in principle, possible that this could have been done in other ways, but in this period the Robbins definition (or very similar ones that had the same implications) played this role. If economics was defined only by describing its subject

¹⁸See also Backhouse and Nishizawa (2010).

matter—as a list of human activities—there was no reason why deductive logic alone would be plausible or even feasible as a research strategy, let alone optimal. By offering an analytical definition of economics that defined the subject in terms of *relationships*, playing down the importance of the empirical element, Robbins made it possible for axiomatic methods to be seen as central to the subject. This explains why the earliest explicit statements of support for the Robbins definition of economics found in academic journals were made by members of the Cowles Commission, notably Lange and Koopmans, for whom allocation problems were central to the discipline. There is a certain irony in this: a definition proposed by someone whose work drew on the Austrian tradition—many of whose members held strongly to the *Naturwissenschaft/Geisteswissenschaft* distinction and strongly opposed the use of mathematics in economics, and who could be recognized by Murray Rothbard (1957) as a “fellow praxeologist”—turned out to play an important role in legitimating the rise of mathematical economics.

This is not to say that attitudes towards the Robbins definition and the methods underlying more formal ways of doing economics were homogeneous. At least as important as the approach coming out of the Cowles Commission was Paul Samuelson’s portrayal of economics in *Foundations of Economic Analysis* (1947). He did not use the Robbins definition and he specifically denied that useful economic propositions could be “deduced from thin air or from *a priori* propositions of universal truth and vacuous applicability” (1947, p. 5). Yet he argued that “operationally meaningful theorems” derived from just two hypotheses: that propositions about equilibrium are equivalent to maximization or minimization problems, and that the system is in stable equilibrium or motion. Thus despite implicitly distancing himself from Robbins’s methods, he defined economics in such a way as to make it very close to that delineated by the Robbins definition. The Robbins definition and ones similar to it defined a space within which, if the definition were accepted, a case could be made for axiomatic methods. It held out, as no alternative definition could do, the possibility of a set of assumptions on which economic theory could be based that was so general that they could be raised to the status of axioms; indeed, a “tight” interpretation of the Robbins definition implied that economics involved nothing other than working out the implications of choice under conditions of scarcity. Those who questioned deductive theorizing explored all of the questions raised above: whether a single deductive system were possible, whether it was necessarily better than a less rigorous system, and whether or not the deductive method was the best research strategy to follow.

There was a parallel movement in welfare economics. Welfare economics came to be seen as concerned simply with demonstrating what could be proved using a very narrow and completely general set of ethical judgments. The ethical judgments underlying the Pareto criterion were deemed to be acceptable—they were taken for granted to an extent not true before 1932. However, in welfare economics, Robbins’s argument that economics was concerned only with how to achieve given ends was taken as implying that such arguments were not part of economic science which, given the prestige associated with being “scientific” in the postwar era, meant that they were in practice not considered part of economics.¹⁹

¹⁹This should not be taken as implying that Robbins himself would have agreed with this.

This has important implications for the way the rise of mathematical economics and, with it, axiomatic methods in economics, should be viewed. The story of inexorable progress in which economists gradually perceived the economic problem and analyzed it with ever-greater rigor, culminating in the use of formal axiomatic methods by Kenneth Arrow and Gerard Debreu, distorts the history. This narrative could be constructed only by ignoring the way that these developments required that economics be narrowed and demarcated according to an analytical definition that made it possible to believe that there might be completely general assumptions that merited the status of axioms.

Contemporary protests about what was happening demonstrate that this is more than a rational reconstruction. The Robbins definition was challenged so widely that it is hard to find statements of support for it in the journals; the narrowness of what came to be called the new welfare economics was challenged; economists from Vining to Harrod questioned whether inductive methods might be needed to establish foundations on which theorizing could be based; and, as is well known, Keynes (1936) questioned the axioms on which orthodox theory rested, using an approach that clearly did not fit within the Robbinsian approach. What this highly heterogeneous group of critics had in common was that they opted for realism rather than the simplifications needed to prioritize logical rigor.

The point here is not to establish who was right and who was wrong. Neither is it to establish that the Robbins definition caused changes to come about. The choices that took place in economics no doubt reflected economists' experiences, the Great Depression and later the Second World War being particularly important, ideology and many cultural factors affecting beliefs about whether economics should be scientific and what that implied. What we argue is simply that the Robbins definition played a significant role in the transformation of economics that took place, in that it was inseparable from the reconceptualization of economics that made it possible for economists to believe that axiomatic methods could be relevant to economics. This came about even though Robbins himself was not a proponent of mathematical economics, or of the development of axiomatic methods. As regards economic theory, the Robbins specification provided an analytical definition of the subject that made it possible to think of economic theory as dependent on a narrow set of basic assumptions; through offering apparently secure foundations for theory, it opened up the prospect that deductive, axiomatic methods might be not only a viable but also a desirable research strategy.²⁰ In welfare economics, it was seen to cut away the need even to think about the value judgments needed to draw conclusions about welfare.

The fact that this was a new strategy, and that it marked a departure from the methods pursued by a very large number of economists during the 1930s and 1940s, explains why, at the time when economics was being transformed into a discipline that was widely perceived as being centered on axiomatic foundations, the Robbins definition was widely and vigorously questioned. It was, paradoxically, only in the 1960s and 1970s when the project of axiomatization around the framework of general competitive equilibrium theory began to break down (see Ingrao and Israel 1990) that

²⁰The developments here were part of a much more pervasive narrowing of economics to what could be handled within extant formal models. On microeconomics, see Mandler (1999) and Backhouse (2003); on macroeconomics, see Backhouse and Laidler (2004).

the Robbins definition had come to be spoken of as a generally accepted definition of economics.

By way of a coda, it is worth noting that Gary Becker's application of rational choice theory to social problems took place at just the time when the Robbins definition came to be widely accepted. This period—the 1960s and 1970s—marked a transition in the highly influential Chicago school, from the more overtly Marshallian price theory of Jacob Viner and Milton Friedman to the more rigorous rational choice analysis found in Becker's work. The result of this was a significant, albeit not complete, convergence between methods followed at Chicago and those being followed at MIT, Stanford, and elsewhere. Becker was not an advocate of axiomatic methods, but when compared with Friedman's approach Becker's work marked a step in that direction. A possible interpretation of his approach is that he was reconciling the axiomatic methods of one of his teachers, Morgenstern, with Chicago economics. His statement, "The combined assumptions of maximizing behavior, market equilibrium, and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach" (1976, p. 5), illustrates perfectly the symbiotic relationship of the Robbins definition and the axiomatization of economics.

REFERENCES

- Backhouse, R. E. 1998. "If Mathematics Is Informal, Perhaps We Should Accept that Economics Must Be Informal Too." *Economic Journal* 108 (451): 1848–58.
- Backhouse. 2003. "The Stabilization of Price Theory, 1920–1955." In W. J. Samuels, J. E. Biddle, and J. B. Davis, eds., *A Companion to the History of Economic Thought*. Oxford: Blackwell, pp. 208–24.
- Backhouse. 2009. "Robbins and Welfare Economics: A Reappraisal," *Journal of the History of Economic Thought* 31 (December): 474–484.
- Backhouse, R. E. and S. Durlauf. 2009. "Robbins on Economic Generalizations and Reality in the Light of Modern Econometrics." *Economica* 76 (October): 873–90.
- Backhouse, R. E. and D. Laidler. 2004. "What Was Lost with IS-LM." In M. De Vroey and K. D. Hoover, eds., *The IS-LM Model: Its Rise, Fall, and Strange Persistence. Annual Supplement to History of Political Economy* 36. Durham, NC: Duke University Press, pp. 25–56.
- Backhouse, R. E. and S. G. Medema. 2009. "Defining Economics: The Long Road to Acceptance of the Robbins Definition." *Economica* 76 (October): 805–20.
- Backhouse, R. E. and T. Nishizawa, eds. 2010. *No Wealth But Life: Welfare Economics and the Welfare State in Britain, 1880–1945*. Cambridge: Cambridge University Press.
- Chamberlin, E. 1933. *The Theory of Monopolistic Competition*. Cambridge, MA: Harvard University Press.
- Commons, J. R. 1931. "Institutional Economics." *American Economic Review* 21 (December): 648–57.
- Debreu, G. 1959. *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. Cowles Foundation Monograph 17. New Haven: Yale University Press, 1976.
- Debreu. 2008. "Mathematical Economics." In L. Blume and S. Durlauf, eds., *The New Palgrave Dictionary of Economics*, 2nd edition, volume 5, pp. 454–60.
- Dobb, M. 1933. "Economic Theory and the Problems of a Socialist Economy." *Economic Journal* 43, 588–98.
- Friedman, M. 1953. "The Methodology of Positive Economics." In M. Friedman, *Essays in Positive Economics*. Chicago, IL: Chicago University Press.
- Giocoli, N. 2003. *Modeling Rational Agents*. Cheltenham: Edward Elgar.
- Harrod, R. F. 1938. "Scope and Method of Economics." *Economic Journal* 48 (September): 383–412.
- Hicks, J. R. 1960. "Linear Theory." *Economic Journal* 70 (December): 671–709.

- Hoover, K. D. 2002. *The Methodology of Empirical Macroeconomics*. Cambridge: Cambridge University Press.
- Hutchison, T. W. 1938. *The Significance and Basic Postulates of Economic Theory*. New York: Augustus M. Kelly, 1960.
- Hutchison. 2009. "A Formative Decade: Methodological Controversy in the 1930s." *Journal of Economic Methodology* 16 (3): 297–314.
- Ingrao, B. and G. Israel 1990. *The Invisible Hand: Economic Equilibrium in the History of Science*, translated by Ian McGilvray. Cambridge, MA: MIT Press.
- Johnson, H. G. 1960. "The Political Economy of Opulence." *Canadian Journal of Economics and Political Science* 26 (4): 552–64.
- Kapp, K. W. 1968. "In Defense of Institutional Economics." *Swedish Journal of Economics* 70 (1): 1–18.
- Keynes, J. M. 1936. *The General Theory of Employment, Interest and Money*. London: Macmillan.
- Kinkaid, H. 1997. *Individualism and the Unity of Science: Essays on Reduction, Explanation and the Special Sciences*. Lanham, MD: Rowman and Littlefield.
- Koopmans, T. C. 1957. *Three Essays on the State of Economic Science*. New York: McGraw-Hill.
- Lakatos, I. 1976. *Proofs and Refutations*. Cambridge: Cambridge University Press.
- Lange, O. 1945. "The Scope and Method of Economics." *Review of Economic Studies* 13 (1): 19–32.
- Mandler, M. 1999. *Dilemmas in Economic Theory: Persisting Foundational Problems of Microeconomics*. New York: Oxford University Press.
- Mirowski, P. and E. R. Weintraub. 1994. "The Pure and the Applied: Bourbakism Comes to Mathematical Economics." *Science in Context* 7 (2): 245–272.
- Morgenstern, O. 1936. "Logistics and the Social Sciences." Reprinted in Andrew Schotter, ed., *Selected Economic Writings of Oskar Morgenstern*. New York: New York University Press, 1976, pp. 389–404.
- Morgenstern. 1963. *On the Accuracy of Economic Observations*. Princeton, NJ: Princeton University Press.
- Robbins, L. C. 1928. "The Representative Firm." *Economic Journal* 38 (September): 387–404.
- Robbins. 1932. *An Essay on the Nature and Significance of Economic Science*. London: Macmillan.
- Robbins. 1935. *An Essay on the Nature and Significance of Economic Science*, 2nd edition. London: Macmillan.
- Robbins. 1938. "Live and Dead Issues in the Methodology of Economics." *Economica n.s.* 5 (August): 342–52.
- Robinson, J. 1933a. Response appended to G. F. Shove, "The Imperfection of the Market." *Economic Journal* 43 (March): 113–25.
- Robinson. 1933b. *The Economics of Imperfect Competition*. London: Macmillan.
- Rothbard, M. N. 1957. "In Defense of 'Extreme Apriorism'." *Southern Economic Journal* 23 (3): 314–20.
- Rutherford, M. 2007. "American Institutionalism and Its British Connections." *European Journal of the History of Economic Thought* 14 (June): 291–323.
- Samuelson, P. A. 1947. *Foundations of Economic Analysis*. Cambridge, MA: Harvard University Press.
- Sraffa, P. 1926. "The Laws of Returns under Competitive Conditions." *Economic Journal* 36 (December): 335–50.
- Tintner, G. 1953. "The Definition of Econometrics." *Econometrica* 22 (January): 77–100.
- Von Neumann, J. 1947. "The Mathematician." In R. B. Heywood, ed., *The Works of the Mind*. Chicago: University of Chicago Press, pp. 180–96.
- Von Neumann, J. and O. Morgenstern. 1944/1947. *The Theory of Games and Economic Behavior*, 2nd edition. Princeton: Princeton University Press.
- Weintraub, E. R. 1998. "Axiomatisches Missverständnis." *Economic Journal* 108 (November): 1837–47.
- Weintraub. 2002. *How Economics Became a Mathematical Science*. Durham, NC: Duke University Press.
- Weintraub. 2008. "Mathematics and Economics." In L. Blume and S. Durlauf, eds., *The New Palgrave Dictionary of Economics*, 2nd edition, volume 5, pp. 462–5.