ON THE ROLE OF VALUES IN ECONOMIC SCIENCE: ROBBINS AND HIS CRITICS

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I. INTRODUCTION

Lionel Robbins's (1932) *An Essay on the Nature and Significance of Economic Science* (henceforth, *Essay*) is one of the two most important works on economic methodology of the twentieth century, together with Milton Friedman's classic "The Methodology of Positive Economics" (1953).[1] The fundamental legacy of the *Essay* is having forcefully championed a conception of economic science as axiomatic, value-free, and primarily concerned with scarcity—a conception that is today widely, even though not universally, accepted.

Robbins (1932) presented his position as if it captured what was at the time a broadly shared understanding of economic science. As it turns out, Robbins's *Essay* was met by largely negative reactions in academic journals.[2] The main purpose of this paper is to describe and assess the nature of the negative reactions to Robbins's work in the 1930s, primarily with respect to the value-freeness of economics.

Some interpreters have argued that the hostility to Robbins's work was mostly based on a misunderstanding of his position (e.g. Peston and Corry 1972; Masini Forthcoming). Robbins himself supported this interpretation, arguing that, with respect to his work on methodology, "I have never succeeded in making my views immune from misunderstanding" (Robbins 1963, Preface to his *Politics and Economics,* p. vii).

Even though some reactions to the *Essay* did originate from failure to appreciate the subtleties of Robbins's views, I want to argue that a great many were based on a substantive disagreement over the very possibility and desirability of keeping values outside of economic science. Recent developments in the debate on the role of values in science show that Robbins's critics, although clumsily, pointed to genuine weaknesses of the thesis that economic science should be value-free.

I will reconstruct and evaluate what I take to be the most insightful lines of criticisms leveled against Robbins from 1932, when the *Essay* was published, to

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[1]Cf. O'Brien (1988b), p. 109; Peston (1981), p. 184; Baumol (1981), p. 14.
[2]According to Backhouse and Medema (2009), Robbins's axiomatic conception of economics did not become fully mainstream until the 1960s.

1939, when the New Welfare Economics was born.[3] The choice of this particular time period is motivated by the fact that the emergence of the New Welfare Economics, which I will also briefly discuss, marks the time when the economic profession begins moving in the direction recommended by Robbins. One of the central innovations of the New Welfare Economics was in fact the elimination of interpersonal comparisons of utility, singled out by Robbins as a major threat to the scientific status of economics.

Yet the reactions to the *Essay* prior to 1939 show that a significant and vocal part of the economic profession did not endorse Robbins's strict requirements, hinting at a different understanding of what economic science should be all about. Even though Robbins's position ultimately won the day, we can benefit from understanding what motivated the early opposition to the understanding of economic science proposed in the *Essay*—an opposition a number of contemporary economists have come to endorse (e.g. Sen 1980, Mongin 2006). I will ultimately argue that the jury is still out on who was right between Robbins and his critics on the desirability of a value-free economic science.

To put the 1932–1939 methodological debate into historical perspective, our first order of business is to get clear on the contents of the *Essay.* We will then be in a position to consider the reception of the *Essay* in the years immediately following its publication.

II. THE CONTENTS OF ROBBINS'S *ESSAY*

Economics as the Science of Scarcity

When the *Essay* was published in February 1932, Robbins was Chair of the Economics Department at the London School of Economics.[4] He had been appointed to the Chair three years before, at the strikingly young age of 31, "on the basis of promise rather than proven performance" (O'Brien 1988b, p. 107). Under the controversial circumstances of his appointment, an important role was played by the fact that Robbins himself had graduated at the LSE in 1923 and had kept in professional contact with several LSE professors, most notably Dalton whose help was pivotal to secure him the appointment (O'Brien 1988b, p. 106). The "promise" on the basis of which Robbins became Chair was a handful of reviews and well-crafted articles written "on particular points of analysis"[5] in the years between 1923 and 1929, while Robbins was holding jobs as a lecturer at the New College in Oxford (1924–25), at the LSE (1925–27), and again at the New College (1927–1929).[6] When he arrived at the LSE in his new, prestigious role, he "decided on a more ambitious enterprise, a short treatise on the subject-matter of economics and the implications thereof" (Robbins 1971, p. 146). The result of this enterprise secured Robbins a place among the most eminent methodologists of economics of the century and it "sold far better than anything else I have written and it goes on selling even at the present day" (Robbins 1971, p. 147).[7]

[3]For a history of the New Welfare Economics, see for example Mishan (1960); Chipman and Moore (1978); Dobb (1969).
[4]Cf. O'Brien (1988a), p. 10.
[5]Robbins (1971), p. 145.
[6]Cf. O'Brien (1988a), pp. 9–10.
[7]After its first edition in 1932, the *Essay* underwent a second, slightly modified edition in 1935. A third edition appeared in 1984, after Robbins's death.

In the preface to the first edition, Robbins claimed that the purpose of the *Essay* was ''twofold'':

> In the first place, it seeks to arrive at precise notions concerning the subject matter of economic science and the nature of the 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 1932 [1984], p. xii).

Importantly, Robbins's intention was to present in an organized manner a set of ideas about methodology which he believed were tacitly shared by most economists:

> For the views I have advanced, I make no claim whatever to originality. I venture to hope that in one or two instances I have succeeded in giving expository force to certain principles not always clearly stated. But, in the main, my object has been to state, as simply as I could, propositions which are the common property of most modern economists (ibid., p. xiii).

This is what motivates some interpreters of Robbins's work to characterize his definition of economics as ''descriptive,'' rather than ''prescriptive'' (see Colander Forthcoming). In other words, Robbins aimed to reconstruct what economists were doing, not to tell economists what they ought to be doing. The *Essay* comprises six chapters that deal with the following three questions of methodology:

(a) The subject matter of economics (chapters 1–3);
(b) The nature of the generalizations of economics (chapters 4–5);
(c) The bearing of economic science on practice (chapter 6).

I will briefly discuss (a) and (b), and then analyze (c) more thoroughly, because this is where the discussion on value judgments I am most interested in is located. At Robbins's time, the most influential account of the subject matter of economics was represented by Cannan's definition, according to which economics is the study of the causes of material welfare (Cannan 1914, p. 4). In the *Essay*, Robbins stated that this was ''the definition of Economics which would probably command most adherents, at any rate in Anglo-Saxon countries'' (ibid., p. 4).[8]

The new definition Robbins proposed is that economics is the ''the science which studies human behavior as a relationship between ends and scarce means which have

[8]In Robbins (1971, p. 146), Robbins described the intellectual process that brought him to search for a new definition of economics: ''Shortly after I joined the staff as a teacher at the LSE [in 1925], I was put to lecture on a special course for Army officers on the Economics of War and readiness for war; and I had not been long engaged in 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. . . . 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, not to mention the design of decorative, as distinct from utility, architecture and the like, 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 of thought was applicable? Gradually it dawned to me that the idea of material welfare was the ignis fatuus in this connection; that the underlying fact which made so many different activities and relationships susceptible to economic analysis was the scarcity of the means with which they were concerned and not the materiality of the objectives.''

alternative uses'' (ibid., p. 16).[9] Whereas the old ''materialist'' definition classified as part of the subject matter of economics only behaviors directed at procuring material welfare, the new ''scarcity'' definition included in the domain of the discipline any human behavior that has to do with scarcity.

This shift in definition, as Robbins remarked, significantly broadened the domain of inquiry for the economist: ''in so far as it presents this aspect, every kind of human behavior falls within the scope of economic generalizations'' (ibid., p. 17). This expansion ultimately led to what Boulding (1969, p. 8) eventually labeled ''economics imperialism,'' the notion that all human behaviors, from choosing a toothbrush to choosing a life partner, are suitable to economic modeling insofar as they involve scarcity.

Concerning the nature of ''economic generalizations,'' the second of the three major topics of the *Essay*, Robbins stated they were ''deductions from simple assumptions reflecting very elementary facts of elementary experience'' (ibid., p. 104).[10] Robbins's ''a prioristic deductivism'' stood in opposition to ''American Institutionalism,'' which in the 1920s was ''threatening to become the dominant stream in American economic thought'' (Blaug 1992, p. 76) by reviving the inductivist approach of the German Historical School.[11]

In the *Essay*, Robbins argued that inductivism had proven sterile, because ''not one single law deserving of that name, not one quantitative generalization of permanent validity has emerged from their efforts'' (ibid., p. 114). In later years, Robbins came to dislike the treatment of economic generalization proposed in the *Essay*. In Robbins (1971), he recognized that it ''was written before the star of Karl Popper had risen above our horizon,'' adding that ''if I had know then of his pathbreaking exhibition of scientific method . . . this part of the book would have been phrased very differently'' (Robbins 1971, pp. 149–150).

We now come to the third and arguably most influential part of the *Essay* regarding the ''bearing of Economic Science on practice,'' a theme that led Robbins to set forth his controversial views on role of value judgments in economic science.

The Ideal of a Value-Free Economic Science

In chapter 6 of the Essay, Robbins criticized the view that ''certain developments in modern Economic Theory furnish *by themselves* a set of norms capable of providing

[9]The definition proposed by Robbins, as acknowledged in Robbins (1971, p. 146), was not original but simply a reformulation of ''the works of the Austrians and Philip Wicksteed,'' who proposed definitions of economizing behavior based on the concept of scarcity. Wiseman (1985, p. 151) remarked that Robbins's definition is a repetition, almost verbatim, of a definition presented in Mayer (1922).

[10]Robbins took into consideration two alternatives to ''a prioristic deductivism,'' namely ''historical induction'' and ''controlled experiment.'' He rejected the former because there is ''no sufficient reason for supposing that 'history would repeat itself''' and the latter because ''it would be very superficial to suppose that the results of . . . 'experiments' can be held to justify [propositions] of such wide applicability'' (ibid., p. 74).

[11]Robbins's views on economic generalizations were a restatement in modern language of what he called the '''orthodox conception' of the [economic] science since the time of Senior and Cairnes'' (ibid., p. 82), which was eminently a prioristic and deductivistic. On the relationship between Robbins's position and earlier formulations of deductivism in economics, see for example Blaug (1992, pp. 51–79). For a description of the Historical School, see for example Roll (1973), pp. 303–311; Schumpeter (1954), pp. 807–824; for a description of the Institutionalist School, see for example Blaug (1996), pp. 700–703.

a basis for political practice'' (ibid., p. 136; emphasis his). In order to move from a statement of fact to a prescription, Robbins claimed, judgments are necessary of a kind that cannot be submitted to scientific test.[12] As Robbins (1934, p. 97) put it, ''[s]cientific method demands that we should leave out of account anything which is incapable of direct observation. Valuation is a subjective process and falls outside this region.''

Robbins distinguished between two types of non-scientific valuations used in economic prescriptions: ''interpersonal comparisons of utility'' and ''judgements of value.'' Interpersonal comparisons of utility are involved whenever economists calculate the total utility produced by a policy. Judgments of value are involved whenever economists state that a given policy ought to achieve a certain end or that a certain end is desirable.

The reason why the comparison of utilities of different individuals falls outside the scope of science is that it includes ''an element of conventional valuation'' and is therefore ''essentially normative [and with] no place in pure science'' (ibid., p. 139).[13] For instance, if we take any two persons A and B, Robbins argued, ''*there is no means of testing the magnitude of A's satisfaction as compared with B's*'' (ibid., pp. 139–140, emphasis his). It is of course true, he added, that ''in daily life we do continually assume that the comparison can be made.'' But ''the very diversity of the assumptions actually made at different times and in different places is evidence of their conventional nature'' (ibid., p. 140).

Similarly, when two people disagree with respect to value judgments, namely with respect to what ought to be done or about which ends are desirable, there is no scientific test which can settle their disagreement, or, in Robbins's own words, ''it is a case of thy blood or mine—or live and let live, according to the importance of the difference, or the relative strength of our opponents'' (ibid., p. 150). On the contrary, when people disagree about means, ''scientific analysis can often help . . . to resolve [their] differences'' (ibid. p. 150). ''Economics,'' argued Robbins, ''is neutral as between ends [and] it cannot pronounce on the validity of ultimate judgements of value'' (ibid., p. 147).[14]

A key consequence followed from Robbins's views: The entire field of Welfare Economics, which dealt with policy advice, no longer belonged to economic science proper.[15] Welfare Economics began to officially exist as an independent branch of

[12]Robbins considered introspection and observation to be the only two forms of scientific, i.e. intersubjectively controllable, testing (Robbins 1932, p. 139).

[13]As we read in Robbins (1934, note 3, p. xvii) and Robbins (1938a, p. 637), this view on the lack of scientific foundation of interpersonal comparisons of utility had been anticipated by Jevons (1871) and by Wicksteed (1910). In Robbins (1963, p. 15), Robbins also claimed to have found evidence that Bentham himself had recognized the conventional nature of interpersonal comparisons of utility.

[14]The positive–normative distinction was of course not new with Robbins. It had been clearly stated several times in the nineteenth and early twentieth centuries, starting with Mill (1844) and Senior (1836). For a history of the positive–normative distinction in economics, see for example Hutchison (1964), pp. 23–50; Blaug (1992), pp. 112–134; Machlup (1978), pp. 425–450 (the latter is a philosophical analysis of the problem rather than an historical reconstruction).

[15]Benham (1930), written before the publication of the *Essay*, can be seen as an anticipation of the Robbinsonian attack to Welfare Economics for ''lack of scientific foundations.'' But Benham did not publish any other contribution on the topic after the publication of the *Essay.*

Economics in 1920, when Arthur Cecil Pigou wrote *The Economics of Welfare* (Pigou 1920) (Hicks 1981, p. 218). As clarified by Pigou, the goal of Welfare Economics was ''to make easy practical measures to promote [economic] welfare,'' which is defined as ''that part of the social welfare which can be brought, directly or indirectly, into relation with the measuring-rod of money'' (Pigou 1920 [1932], p. 11). Or, as Pigou further explained, ''the economic welfare of a community consists in the balance of satisfactions from the use of the national dividend . . . over the dissatisfactions involved in the making of it'' (Pigou 1920 [1932], p. 85).

Robbins thought that Welfare Economics involved both interpersonal comparisons of utility and value judgments. The first type of judgment was implicit in the very concept of economic welfare, which required for its calculation the assumption of interpersonal comparability. The second type of judgment was required to move from the premise that a given policy increases the total welfare to the conclusion that the policy is ''economically justified,'' namely the judgment that economic policies ought to increase the total welfare (ibid., p. 141).[16]

''Certain economists,'' Robbins concluded, ''realizing 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'' (ibid., p. 147). Hawtrey (1926), for instance, had asserted that ''economics cannot be dissociated from ethics.'' Robbins thought this was not acceptable, because ''economics deals with ascertainable facts,'' whereas ethics deals with ''valuations and obligations'' and therefore ''the two fields of enquire are not on the same plane of discourse'' (ibid., p. 148).[17]

''Between the generalizations of positive and normative studies,'' Robbins concluded, ''there is a logical gulf fixed which no ingenuity can disguise and no juxtaposition in space or time bridge over'' (ibid., p. 148). Robbins was clear that his position did not entail 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'' (ibid., p. 150). On the contrary, he emphasized that ''it is greatly to be desired that economists should have speculated long and widely on these matters, since only in this way they will be in a position to appreciate the implications as regards *given* ends of problems which are put to them for solution'' (ibid., p. 150). Simply put, economists cannot deliver themselves to normative questions *qua* scientific economists. The significance of economic science consisted for Robbins in clarifying the ''implications of the different end we may choose'' rather than the ''desirability of different ends'' (ibid., p. 152).

[16]Robbins illustrated the lack of scientificity of Welfare Economics by considering a popular argument in favor of the redistribution of income. The argument presupposes that the marginal utility of a rich man's income is lower than the marginal utility of a poor man's income. If this is the case, transfers of money from the rich to the poor, provided they ''do not appreciably affect production,'' will increase the total utility. Robbins pointed out that, at least in principle, a rich person could get much more satisfaction from money than a poor does. In practice, there is no scientific way to compare the amounts of satisfaction of different individuals, so the argument for redistribution relies on a fallible and scientifically unverifiable assumption.

[17]Robbins had already dealt with Hawtrey's (1926) views on the relationship between ethics and economics in Robbins (1927), in which most of the content of chapter six of the *Essay* is anticipated.

III. THE RECEPTION OF ROBBINS'S *ESSAY*

The reception of the *Essay* was mixed. On one hand, the economic profession soon began moving in the direction recommended by Robbins. The emergence of the New Welfare Economics in 1939 marks the beginning of a tectonic shift in economics whose long-term impact can hardly be overestimated. As Nobel Prize winner Amartya Sen put it, the conception of economic science that was to emerge in the wake of the *Essay* "kept prescriptive studies somewhat immersed in a pool of apology from the mid-thirties until relatively recently" (Sen 1980, p. 363). On the other hand, the works on methodology published in the period 1932–1939 reveal strong hostility towards Robbins's "scientificity requirements." After a brief reconstruction of how the *Essay* led to the emergence of the New Welfare Economics, I turn my attention to the nature of the resistance to Robbins's methodological prescriptions.

The Rise and Fall of the New Welfare Economics

The birth of the New Welfare Economics in 1939, propelled by Kaldor (1939) and Hicks (1939), is the first tangible sign of the impact of Robbins's *Essay* on the actual practice of economics. Both Kaldor and Hicks had been under the influence of Robbins's methodological views at an early stage of their careers.[18] When Robbins arrived at the LSE in 1929, Hicks was a young assistant professor and Kaldor still an undergraduate. Having taught Kaldor the rudiments of economics, Robbins was impressed by the qualities of his student and in 1932 helped him secure a position in the economics department.[19] To be part of the economics department in the early thirties meant to be part of *Robbins Circle*, a group of economists who worked in close contact with one another.[20] The main venue of intellectual exchange was a weekly seminar, organized and chaired by Robbins, in which professors and senior students gathered to discuss subjects of mutual interest.[21]

[18]In 1939, Hicks was not anymore a professor at the LSE (Presley 1981, p. 97) and Kaldor's relationship with Robbins had started to become colder (Thirlwall 1987, p. 26). But they both had been active members of the Robbins Circle from 1929 to 1935, when Hicks left the school to go to Cambridge (1935–1938) and later to Manchester (1938–1946) and Kaldor started to lean towards Keynesian economics. In Robbins (1971, p. 129), Robbins recalled that Hicks, who was an extraordinarily talented theoretical economist, had to leave the LSE to get "the advancement that was his due" because of "Beveridge's insensate hostility to pure theory." Every appointment in the School had to be approved by Beveridge, the Director of the LSE from 1919 to 1937. The relationship between Robbins and Beveridge, as we read in Robbins (1971, pp. 135–144), was also at times conflictual. This was due in part to Beveridge's difficult character and in part to radically different views on the nature and scope of economics. For a description of the LSE experience from Beveridge's viewpoint, see Beveridge (1960).

[19]Cf. Thirlwall (1987), p. 1.

[20]Besides Hicks and Kaldor, the circle included in the mid 1930s some of the most gifted economists of the century. In 1935, for a short period because Hicks left in that year, three future Nobel Prize winners for economics were working in the department: Hicks, who won it in 1972, Hayek, who won it in 1972, and Coase, who won it in 1991.

[21]Papers and chapters of works-in-progress were presented and discussed in the seminar. Robbins (1971, pp. 131–132) reported a sample of the type of works which were discussed in the 1930s: "Chapters from Hayek's *Pure Theory of Capital*, Hicks and Allen's *Reconsideration of the Theory of Value*, Plant's *Economics of Patents*, Lerner's *Factor Prices in International Trade*, Kaldor's *Classificatory Note on Conceptions of Economic Equilibrium*, Victor Edelberg's *Ricardian Theory of Profits*."

Kaldor and Hicks shared Robbins's methodological views on the lack of scientific foundation of interpersonal comparisons of utility, but they wanted to resist the implication that Welfare Economics is not part of economic science proper. Their strategy was to develop a theoretical tool that could be used to compare policies without engaging in interpersonal comparisons of utility. Kaldor's (1939, p. 549) central proposal was that if a policy is such that "it is *possible* to make everybody better off than before, or at any rate to make some people better off without making anybody worse off" (Kaldor 1939, p. 550), the policy can be prescribed without the need of comparing utilities.

Given that policies ordinarily make some people better off and some people worse off, the job of the scientific economist as a policy advisor is to show that "even if all those who suffer as a result [of the policy] are fully compensated for their loss, the rest of the community will still be better off than before" (Kaldor 1939, p. 550). This was the so-called Compensation Principle, which can be reformulated as follows: A policy change is advisable whenever those who gain from the change can potentially compensate those who lose from it.[22] In modern terms, this would be called a Potential Pareto Improvement. The question regarding the actual payment of the compensation, on the other hand, lies outside the domain of economic science, because it is "a political question on which the economist, qua economist, could hardly pronounce an opinion" (Kaldor 1939, p. 550).

Hicks realized the potential of the Compensation Principle to deal with some of Robbins's objections. He argued that the New Welfare Economics was meant to be "a theory of economic policy which is immune from the objections brought against previously existing theories" (Hicks 1939, p. 60), the standard representative being "Professor Pigou's economics of welfare." Among the most serious defects of Pigou's Welfare Economics, remarked Hicks, was precisely that it involved comparisons of utilities of different individuals.[23]

By adopting Kaldor's Compensation Principle, Hicks hoped to have demonstrated "the right of Welfare Economics . . . to be considered as an integral part of economic theory, capable of the same logical precision and the same significant elaboration as its twin brother, Positive Economics" (Hicks 1939, p. 76). The New Welfare Economics, however, soon revealed two major flaws.

Firstly, Scitovsky (1941) showed that the Compensation Principle was capable of giving contradictory results. It is possible that a shift from state X to state Y and its reversal are both such that the losers could be compensated by the gainers. Which policy should the scientific economist prescribe in such a case? The Principle was amended to take into consideration Scitovsky's critique, but other limitations of applicability were discovered throughout the years until Samuelson (1950) conclusively demonstrated that only in some special cases it is possible to rank alternative policies on the basis of the Compensation Principle.[24]

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[22]For a rigorous analysis of the Compensation Principle, see for example Nath (1969), pp. 94–124.

[23]This, according to Hicks (1939, p. 61), was one of the three theoretical weaknesses of Pigou's Welfare Economics. The other two were (a) the assumed "direct correlation between economic welfare and social welfare" and (b) the replacement, for purposes of measurability, of the "sum of the consumers' surpluses derived from the various commodities in the social dividend" with the "real value of the dividend."

[24]Cf. Mishan (1960), p. 218.

Secondly, Little (1950) was the first to argue that the New Welfare Economics was not value-free, because to prescribe policies that pass the Compensation Test amounts to committing oneself to an implicit value judgment, i.e. that a Potential Pareto Improvement is a desirable end for policy making.[25] Little (1950) showed that, contra Kaldor (1939) and Hicks (1939), value judgments cannot be confined to the discussion of whether the compensation has to be paid, because they are intrinsic to the activity of policy making. As Chipman and Moore (1978, p. 548) remarked in their survey of the New Welfare Economics, ''judged in relation to its basic objective of enabling economists to make welfare prescriptions without having to make value judgements . . . the New Welfare Economics must be considered a failure.''[26]

The rise of the New Welfare Economics and its ultimate fall on account of its value-ladenness are both signs that Robbins's conception of economic science was making inroads. What is less appreciated is the force of the early methodological opposition to the *Essay*, to which I now turn.

The Methodological Critique of the Essay, 1932–1939

A significant portion of the economic profession reacted to the *Essay* very differently. Instead of trying to shape economic science so as to satisfy Robbins's requirements, many flatly rejected such requirements as ill conceived.[27] As we read in Robbins (1971), the most intense criticisms in the years following the publication of the *Essay*

[25]On the lack of ''neutrality'' of the New Welfare Economics, see for example Streeten (1953); Nath (1969); Sen (1979); Hausman and McPherson (1993).

[26]This was also Robbins's opinion, as expressed for example in Robbins (1981), where he claimed that the New Welfare Economics had ''broken down in the strictly scientific sense,'' in that it could not avoid to finally acknowledge that ''all recommendations of policy involve judgements of value'' (Robbins 1932 [1984], p. xxiv). After the 1930s, Robbins's wrote very rarely on methodology, devoting his efforts mainly to the history of economic thought, macroeconomics, and international economics (for a description of his contributions in these fields, see for example O'Brien (1988a), pp. 41–50 and 106–162). The sparse comments he made on the New Welfare Economics came several years after Kaldor (1939) and Hicks (1939), namely in Robbins (1959) and in the above mentioned Robbins (1981). In Robbins (1959), he stated that the ''attempt to reconstruct'' Welfare Economics ''in such a way as to be wholly wertfrei'' could not be considered a success. ''As one contemplates the mountainous profusion of the literature'' on the Compensation Principle, he remarked, ''the final impression is one of effort which is largely wasted'' (Robbins 1959, pp. 44–45).

[27]I have based my research of works written in reaction to Robbins's ''scientificity requirements'' on four bibliographical sources: the *Index of Economic Journals* (American Economic Association 1961), the electronic journal browser *JSTOR* (http://www.jstor.org), the *History of Economic Analysis: A Guide to Information Sources* (Hutchinson 1976), and the secondary literature about the *Essay* (see references). The *Index* ''lists by author and subject English language articles in major professional economic journals published during the period 1886–1959" (American Economic Association 1961, p. vii) and it is considered the most reliable reference for journal articles in economics concerning the period under consideration. It includes, besides articles, ''comments, replies, rejoinders and correction of articles,'' whereas it does not comprise ''book reviews and notes about them appearing in a book review section'' (ibid., p. viii). The articles are listed in classes depending on their content. The class of ''Scope and Method of Economics,'' the one I have consulted, ''includes general discussions of the scope and method of economics and of economic research'' and it reports the critics' reactions to the *Essay* as well as Robbins's replies. In order to avoid neglecting the reviews of the *Essay*, I have searched the electronic journal browser *JSTOR*, which enables a specific research for book reviews. References to books written in reaction to the *Essay* have been found in Hutchinson (1976) and in the secondary literature about Robbins.

regarded the "denial of normative status to economic generalizations and [the] emphasis on the conventional nature of the assumptions introduced into discussions of welfare economics" (Robbins 1971, p. 147).[28]

I distinguish three main objections to Robbins's views, labeled for simplicity the *Pragmatic Objection*, the *Interpersonal Comparability Objection,* and the *No Sharp Positive–Normative Distinction Objection*. I consider them in turn, including in my discussion Robbins's counter objections.

The most common objection to Robbins's *Essay* was that it had misrepresented the goal of economics. Whereas Robbins had portrayed such goal as the acquisition of pure knowledge, some critics argued, economics has a fundamental pragmatic dimension. If we eliminate interpersonal comparisons of utility and value judgments from it, economists are going to be unable to fulfill their goal of giving policy advice. Call this the *Pragmatic Objection*.

The first to voice the *Pragmatic Objection* was Cannan (1932), who stated that if economics were really to be "neutral between ends," economists would not be able to give advice, because they would have no way of deciding what is better or worse between alternative courses of policy. But the belief in the capacity of economists to give useful advice in practical matters is the very reason why "benefactors endow Chairs of Economics, audiences listen to economic lectures, purchasers buy economic books" (Cannan 1932, p. 426). "Is it really necessary for professors of economics," concluded Cannan rhetorically, "to destroy this demand of economic teaching by alleging that they do not know what better off means?" (Cannan 1932, p. 426).

Fraser (1932) pushed Cannan's criticism one step further, emphasizing that the concern for practical matters is a constitutive rather than accidental feature of the discipline of economics. The theoretical work of the economist "is important not so much because it yields truth as because it provides a technique whereby practical and social problems may be solved." Fraser (1932) added that "knowledge in economics is valuable as being 'fruit-bearing' rather than 'light-bringing' . . . if by a science is meant the building up of a system of knowledge for its own sake, then economics is not a pure science. Its investigations are bound up with practice, and it is by its practical usefulness that it must be judged" (p. 565).

If economists abstained from giving advice "in their professional capacity," Fraser concluded worriedly, an unfortunate consequence would follow: The discussion of practical problems would be left to "demagogues and fanatics, with no interest in logical reasoning and no knowledge of economic technique" (Fraser 1932, p. 567).

A second prominent objection was that interpersonal comparisons of utility are not necessarily unscientific: Robbins thought otherwise because his standards on what counted as scientific were unreasonably high. Call this the *Interpersonal Comparability Objection*. For instance, Fraser (1932) argued that, even if they cannot be established "as a matter of ascertainable fact," interpersonal comparisons should not be eliminated from economic science. Judgments about interpersonal comparisons will always have some degree of uncertainty, but this aspect should not be overemphasized, in that "all social relations are shot through with ultimate uncertainties of this kind" and economics,

[28]The new definition of economics and Robbins's "a prioristic" views on economic generalizations were also, to some extent, the object of controversy. A description of these controversies, however, lies outside the scope of this paper.

being a social study, ''cannot really expect to keep itself forever within the realm of deductive necessity'' (Fraser 1932, p. 562). As Fraser put it, ''it is not in the least a necessary part of the concept of science that it should yield a priori certain results . . . why not also acknowledge an a posteriori economics?'' (p. 560).

Souter (1933) also addressed Robbins's claim that interpersonal comparisons of utility are not scientific because they are based on conventional assumptions, and rejected it by saying ''all sciences, including pure mathematics itself, rest in the last resort on initial 'conventions''' (Souter 1933, p. 405).

A third important objection was that there is no logical gulf between the positive and the normative, contra Robbins (1932). Call this the *No Sharp Positive–Normative Distinction Objection*. In regards to the distinction drawn by Robbins between statements concerning ''what is'' and statements concerning ''what ought to be,'' Fraser (1932) commented that it ''scarcely requires refutation,'' in that ''discussions of what is and what ought to be are often linked together in the closest and most intricate manners—as Professor Robbins has admirably demonstrated with his own book'' (Fraser 1932, p. 563).[29]

Souter (1933) further argued that normative statements about ''what ought to be'' can also be scientific. Formal logic, for instance, is an example of a science which ''tells us how we 'ought' to reason'' (Souter 1933, p. 402). But if formal logic is scientific, normative economics can be scientific as well. According to Souter, it ''can be conceived, provisionally, as taking over from philosophical ethics (as 'given') an already developed abstract conception of the ethical end (let us say 'maximum net social satisfaction'), and confining itself to the problem of ascertaining the 'scientific' economic conditions of its realization'' (Souter 1933, p. 402). In this sense, Souter continued, ''Pigou is quite right in implying that his Economics of Welfare is positive'' (Souter 1933, p. 403).[30] Souter's conclusion was that there is no logical gulf ''between a simple something called 'the positive' and a simple something called 'the normative,' but . . . a hierarchy of levels of discourse, each intermediate link in which is normative to the one below and positive to the one above'' (Souter 1933, p. 403).[31]

Souter (1933) focused on a further implication of the assumption that there is a sharp divide between economic science and normative studies, namely that cross fertilization with other disciplines may be blocked. An '' 'economics' which is not a mere formal mechanics,'' Souter (1933, p. 389) wrote, ''is . . . inextricably bound up and 'dependent' upon both ethics and psychology.''

[29]Fraser further articulated his views on economic methodology in Fraser (1937).

[30]Robbins's likely reply would have been that adopting 'maximum net satisfaction' as an end is precisely what demands a value judgment.

[31]Some critics also accused Robbins of confusing the ethical with the normative. Souter (1933) wrote for instance that ''the terms 'normative' and 'ethical' are not synonymous'' (402), and gave the example of logic as a discipline that is normative but not ethical. Parsons (1934) reiterated Souter's points, emphasizing a further risk for economics if the Robbinsonian idea that ''a science dealing with 'facts' cannot also deal with 'norms''' were to be accepted, namely that of becoming mere ''behaviorism.'' He then stated, as Souter previously, that the fact that ''economics is necessarily in one sense a normative science does not imply that it rests on ethical postulates'' (Parsons 1934, p. 521). These criticisms strike me as unfair. Even though Robbins used primarily ethics as an example of a normative discipline, it is quite clear that his worries concerned normative studies writ large.

Finally, Spengler (1934) pointed out a further aspect of the lack of a sharp divide between the positive and the normative by arguing that it is difficult to keep values outside the practice of science even if one tries. As he put it, ''it is virtually impossible for anyone, scientist or otherwise, to divest himself completely of a scale of values. . . . To postulate that an economist, to be a scientific economist, must avoid any expression of opinion which is not an indicative and verified proposition, is to define an inanimate meat grinder into which non-economists toss facts and theories'' to grind (p. 330).

These severe criticisms took Robbins by surprise, as we gather from Robbins (1934) and from the preface to the second edition of the *Essay* (1935), where his first reactions to the reception of his ideas appeared. Robbins's line of defense, which was to remain unchanged until his death, failed to substantially alter the nature of the criticisms aimed at his work throughout the 1930s.[32] Robbins (1934) addressed the *No Sharp Positive–Normative Distinction Objection* by stating one more time that ''economic analysis is 'Wertfree' in the Weber sense.'' This proposition, he acknowledged, ''has been much questioned in recent years by certain English-speaking economists,'' but added that ''they have not yet shown that propositions involving 'ought' are on the same logical footing as propositions involving 'is'" (Robbins 1934, p. 100n1).

Robbins also addressed the *Pragmatic Objection* head on, pointing out a potential misunderstanding on the part of some of his critics. He argued that what motivated the *Pragmatic Objection* was the unwarranted fear that economists ''may be precluded from discussing problems of social improvement from a normative point of view'' (Robbins 1934, p. 100n1). But their position is ''groundless,'' in that ''nobody wishes to limit their freedom of action. Most of us think that it is very desirable that they should discuss such matters'' (Robbins 1934, p. 100n1). All that is wished, concluded Robbins, is a clear acknowledgment on the part of economists that there is a logical distinction between the two types of propositions and that ''propositions involving ought'' cannot be scientifically supported.

It is useful at this juncture to emphasize that for Robbins ''economic science'' is a narrower category than ''economics.''[33] For Robbins, ''economics'' divides into ''economic science,'' which tries to be as positive and value-free as possible, and ''political economy,'' which is explicitly and unabashedly normative. Some critics wrongly interpreted Robbins's position as implying that economists cannot give policy advice *qua* economists, namely in their professional capacity (e.g. Cannan 1932). Robbins's claim was actually different, namely that economists cannot give policy advice *qua* practitioners of economic science. Nothing prevented them from giving policy advice *qua* practitioners of political economy, another facet of their professional capacity.

This being said, Robbins was adamant that the advice given by economists wearing a political economist's hat is not scientifically grounded. As I mentioned

[32]Robbins's last reflection on methodology is Robbins (1981), the T. Ely Lecture given at a meeting of the American Economic Association. In that work, he confirmed his early views on interpersonal comparisons and value judgments, both of which are not ''warranted by anything which is assumed by scientific economics.''

[33]I want to thank David Colander for emphasizing this point to me.

earlier, he compared the opinions an economist has on matters of economic policy to the opinions a botanist has on the layout of gardens (Robbins 1934, p. 150). In 1927 he was even bolder, arguing that ''there is no reason why economists alone among their fellows should deny themselves the liberty of dogmatizing'' (Robbins 1927, pp. 177–178).

Finally, Robbins addressed the *Interpersonal Comparability Objection* by reiterating his position with more vigor: ''I am afraid that, without the least disposition to be intransigent, here or elsewhere, I am still quite unconvinced [by the criticisms]. I contended that the aggregation or comparison of different satisfactions of different individuals involves judgements of value rather than judgements of facts and that such judgements are beyond the scope of positive science'' (Robbins 1932 [1984], p. xxxiv).[34]

Notice here Robbins's blurring of the distinction between the concept of ''conventional judgement'' and the concept of ''value judgement.'' Whereas in the *Essay* the assumption of interpersonal comparability was characterized as being based on a ''convention,'' in the preface to the second edition Robbins claimed that it was based on a ''judgement of value.'' This led New Welfare Economists such as Kaldor and Hicks to wrongly assume that an economic policy that didn't require interpersonal comparisons of utility would be value-free (see the section entitled *The Rise and Fall of the New Welfare Economics*).[35] It is instead clear that Robbins considered interpersonal comparisons to be just one of the value judgments involved in Welfare Economics (Robbins 1932 [1984], pp. 139–147). From here on, I will include in the class of value judgments also interpersonal comparisons of utility, consistently with Robbins terminology in the preface to the second edition of the *Essay.*

In 1938, the criticisms started again, with renewed intensity. Hutchison (1938), the first methodologist to apply Popper's criterion of falsifiability to economics,[36] reproposed the *Interpersonal Comparability Objection* in slightly different form. He stated that it was ''perfectly possible and consistent with the usual use of language to define the concept of the comparison of the utilities of different individuals in a scientifically legitimate way'' (Hutchison 1938, pp. 150–51). A test could in principle be devised to verify how people react to stimuli and their reactions would be intersubjectively observable. Against Robbins, he argued that ''[t]here is, then, a logically sound peg on which to hang 'welfare' analysis, which can thus be just as 'positive' as the other parts of Economics'' (Hutchison 1938, p. 153).[37]

[34]Sutton (1937) supported Robbins's reply to the *Interpersonal Comparability Objection*. In the article, Sutton stated that ''Professor Robbins's contention that the concept of welfare has no place in economic science remains both unanswered and unanswerable'' (Sutton 1937, p. 45). The refusal to accept Robbins's claims, argued Sutton, is due to the fact that ''the acceptance . . . must involve the abandonment of large sections of traditional doctrine, especially in the field of public finance'' (Sutton 1937, p. 45).

[35]Cf. Blaug (1992), p. 112; Chipman and Moore (1978), p. 578.

[36]Cf. Blaug (1992), p. 83.

[37]Hutchinson also criticized Robbins's use of ''introspection'' as a legitimate scientific test, in that he believed that introspection, differently from interpersonal comparison, cannot be submitted to an intersubjectively controllable test. Statements about introspective states, he concluded, have always to be supported by statements about behaviors in order to provide scientific evidence (Hutchison 1938, p. 141).

Wootton, in a 1938 polemic attack on the economic profession writ large, offered a sharpened version of the *Pragmatic Objection*. The motivation for the book was that, even though ''the preoccupation of the world at large with what are commonly regarded as economic problems . . . has perhaps never been so great as it is today,'' there ''can seldom have been an age in which the public at large placed a lower estimate on the usefulness of professional students of economics'' (Wootton 1938, pp. 13–14). Among the fundamental accusations ''which are today laid at the door of the economists,'' Wootton reported a remoteness from reality and a lack of concern for the practical uses of theoretical research. These features of the discipline were ascribed to the profession in part because of the influence of Robbins's views.

This remark prompted Fraser's (1938) reaction, who claimed that Wootton ''writes as though she were completely unaware of the protests which the publication of the *Essay* called forth from Professor Robbins's professional colleagues'' (Fraser 1938, p. 200). In the same paper, Fraser set the record straight, and acknowledged that Robbins had not aimed to prevent economists from giving policy advice. As he rightly emphasized, ''[i]t is surely obvious that the author of Economic Planning and International Order is the last person to accuse to confining himself to pure and abstract analysis'' (Fraser 1938, p. 200).

At the same time, what bothered Fraser and many other economists was the idea that the policy advice of an economist is utterly unscientific. Economists wanted to be able to give policy advice qua scientific economists. Fraser expressed the point as follows: ''Do we not ask for trouble by the way we use the word ''science'' in connection with economics? Professor Robbins . . . in effect confine[s] the word to value theory, as distinct from the other, by implication unscientific, branches of economic study.''

Later and more sophisticated formulations of the *Pragmatic Objection* emphasized that the issue of contention between Robbins and his critics concerned whether the policy advice offered by an economist could qualify as scientific. For instance, Harrod (1938) warned, more forcefully than any other economist before him, about the dismal practical consequences that would follow from the acceptance of Robbins's ''scientificity requirements.'' If they were accepted, he believed, ''the economist as an adviser [would be] completely stultified, and unless his speculations be regarded as of paramount *aesthetic value*, he had better be suppressed completely'' (Harrod 1938, p. 397; emphasis added). Harrod's view was that if the advice of an economist on economic matters could *not* be vindicated as being scientifically grounded, it would lose much of its political relevance. To go back to Robbins's earlier metaphor, economists such as Harrod wanted to be considered *botanists* rather than *aestheticians* when it came to the policy advice they gave in their professional capacity.

Harrod offered another take on the *No Sharp Positive–Normative Distinction Objection*. Value judgments, he argued, are not only inevitable when economists give policy advice, they are also inevitable when economists engage in positive means–ends analysis. This is because there are innumerable means to achieve any given end, and the economist cannot help deciding between alternative means according to his own ''criterion of individual preference.'' ''If it were true that there is a latent ethical or political bias when [the economist] gives advice simpliciter,'' concluded Harrod, ''it would be equally true when he advises on the means to achieve an end laid down by moralists or politicians'' (Harrod 1938, p. 392).

Finally, Harrod reiterated the *Interpersonal Comparability Objection* by commenting that Robbins's concerns ''would be very weighty if economics itself were a mature and exact science. Yet in fact its achievements outside a limited field are so beset on every side by matters which only admit of conjecture that it is possibly rather ridiculous for an economist to take such as high line'' (Harrod 1938, p. 396). Interpersonal comparisons are certainly ''vague,'' but this just means that economists have to be cautious and maintain their judgments within the confines of ''common sense.''

In Robbins (1938a), Robbins replied to Harrod (1938) by sharing with his readers the intellectual process that brought him to reject Welfare Economics. He recounted that he began his economic career as a utilitarian, believing in the maximization of social utility as the basic instrument of economic policy. Slowly, he developed the conviction that the comparison between utilities was outside the realm of science. At first, he said, ''I found the implications very hard to swallow. For it meant, as Mr Harrod has rightly insisted, that economics as a science could say nothing by way of prescription'' (Robbins 1938a, p. 637). But further thought convinced him of the irrationality of his discomfort. ''All that I was doing,'' he remarked, ''was only to recognize that, in a field of generalizations hitherto thought to involve no normative elements, there were in fact such elements concealed'' (Robbins 1938a, p. 637).

Robbins (1938b), finally, represents Robbins's attempt to sum up the debate centered around the *Essay,*[38] trying to distinguish between live and dead issues in the methodology of economics. The live issues were those ''about which intelligent men may well differ even when they become fully aware of each others' premises'' and the dead issues those ''upon which there is no real dispute once the question is clearly stated'' (Robbins 1938b, p. 342). In the category of dead issues, Robbins put the proposition that ''the object of economics is to understand reality'' and the question of the definition of economics, in that the ''difference between [the scarcity definition] and other definitions now current is not a very serious matter'' (Robbins 1938b, p. 344).

Among the live issues, Robbins placed the ''question whether economics, as such, can be said to include judgements of what is good or bad in the world of relative scarcities, as distinct from judgements of what exists or what might be conceived to exist'' (Robbins 1938b, p. 345). One more time, Robbins attributed the criticisms to his position largely to a misunderstanding, namely that he intended to prevent economists from making ''excursions into social philosophy.'' The idea that if ''such excursions are not dignified by the title [of] economic science'' they will not be performed, stated Robbins, is objectionable. A ''man of spirit'' can at the same time recognize the distinction between judgments of fact and judgments of value and be interested in both (Robbins 1938b, p. 345).

[38]In Robbins (1938b), Robbins did not cite the name of any of his critics, explaining the lack of references as follows: ''it would be easy enough to indicate the authorship of the misconceptions which are mentioned; and in certain cases the temptation has been strong. But I cannot help thinking that the purpose of the note is better served by deliberately refraining from assigning responsibility for what seems to be obvious error. The issues are intellectual, not personal'' (Robbins 1938b, p. 342).

IV. THE DEBATE ON ECONOMIC SCIENCE AND VALUES UPDATED

It appears clear that by the end of the 1930s the debate on the ''scientificity requirements'' incited by Robbins's *Essay* had reached a deadlock. On one side, Robbins's critics kept reformulating pretty much the same criticisms, the most widely voiced being that the acceptance of Robbins's position would have entailed a serious drawback for the advisory role of the economist, ultimately confined to a fate of political irrelevance. On the other side, Robbins seemed incapable of developing his starting position into something more appealing to the profession.

His characteristic rejoinder to the accusation of depriving the economist of an advisory role—that economists could give all the advice they wanted, provided they did not give it *qua* scientific economists, was unsatisfying to his critics. Economists knew (or should have known) all along that nobody prevented them from giving advice, but this was not the point. The profession wanted to be reassured about the possibility of transferring the authority of science on to policy advice. And this was precisely what Robbins denied to be possible: ''economic science'' and ''political economy,'' Robbins concluded, are non-overlapping sub-domains of ''economics.''

The core issue of disagreement between Robbins and his critics boiled down to whether empirically nontestable judgments of value (or, more generally, values) have any legitimate role to play in economic science. If they do, then arguably economists can give policy advice qua scientific economists, interpersonal comparisons of utility can belong to economic science, and there is no sharp divide between the study of what is and the study of what ought to be. If they do not, Robbins's conclusion that economics as a science can say nothing by way of prescription appears hard to resist.

The last portion of my paper aims to offer an update on the debate between the methodological positions represented by Robbins and his critics. My objective is not to arbitrate the debate, a task that far exceeds the scope of this paper. Rather, it is to distinguish live from dead issues *within* what Robbins himself had considered to be a live issue, i.e. the issue of what role values should play in economic science. I will do so by relying on recent developments in the philosophy of science, where the value-freeness of science has been widely discussed since the 1930s.

Three Notions of Value-Free Economic Science

I propose we distinguish three views on the role of values (and value judgments) in economic science.[39] I call them the 'naïve positivist view,' the 'separatist view,' and the 'non-separatist view.' Understanding how they differ will allow us to place

[39]The concepts of ''value'' and ''value judgment'' are not easily defined. Here, I borrow a working definition from McMullin (1983). According to McMullin, ''a property or set of properties may count as a value in an entity of a particular kind because it is desirable for an entity of that kind'' (p. 5). On this view, a ''value judgment'' is a judgment to the effect that a certain property or set of properties is desirable with respect to an entity of a certain kind (e.g. a policy, a scientific theory, etc.). This working definition does not cover all shades of meaning of the concept as it is used in the common language, or as it is discussed in the philosophical literature. For an introduction to the philosophical debate on the concepts of ''value'' and ''value judgment,'' see for example Rescher (1969).

Robbins and his critics in the context of the contemporary debate on science and values.

According to the 'naïve positivist view,' values should not play any role in the activities of scientific economists. If they influence such activities, they do so illegitimately. I call this the 'naïve positivist view' because it was echoed in some of the positions expressed by logical positivists between 1930 and 1960, but it is at this point an untenable position.[40] It disregards the fact that science itself is governed by *epistemic values* widely acknowledged to be essential to the very pursuit of scientific knowledge.[41] The following list of epistemic values provided by Thomas Kuhn (1977) can be taken as representative: accuracy, consistency, scope, simplicity, and fruitfulness.

It is quite clear that Robbins was not a ''naïve positivist.'' By value judgments, he meant judgments concerning, say, the social desirability of a particular economic policy such as the redistribution of wealth. Robbins would have had no qualms with the claim that an economist can judge a theory to be preferable on scientific grounds because it is more accurate, or more consistent with the existing body of economic knowledge, or endowed with broader scope, or simpler or more fruitful than an alternative theory. To avoid confusing types of values, let us call *non-epistemic values* all those values whose pursuit is not instrumental to the achievement of scientific knowledge.[42] The list contains all sorts of personal, ethical, political, and socio-cultural values. These, and only these, are the sorts of values Robbins aimed to keep outside of economic science.

To forestall another misunderstanding, let us also distinguish ''bordering activities'' from ''internal activities'' of scientific economists. What I call ''bordering activities'' concern two main areas: the selection of the economic problem to investigate, and the use to make of economic knowledge once acquired.[43] These are activities that occur ''at the border,'' as it were, of economic science itself. Myrdal (1970), for example, pointed out that economists make non-epistemic value judgments whenever they decide to study one issue rather than another. For instance, the study of the economics of underdeveloped countries and the study of the economics of trading basketball players in the NBA may lead two economists to asking very different questions. The selection of the problem to investigate is a domain in which non-epistemic value judgments are clearly crucial.

They are also crucial with respect to the use economists decide to make of a certain piece of economic knowledge once they have acquired it. Consider an example of a factual judgment in economics: ''in terms of the Gini coefficient, the distribution of income of most countries is less unequal than the distribution of wealth'' (Mongin 2006, p. 257).[44] What economists do with this piece of factual

[40]For a careful analysis of how best to interpret the ideal of value-free science in logical positivism, see for instance Roberts (2007).

[41]Epistemic values have also been called ''constitutive values'' (Longino 1990) or ''cognitive values'' (Laudan 1984)).

[42]Non-epistemic values have also been called ''contextual values'' (Longino 1990) or ''non-cognitive'' values (Laudan 1984).

[43]For a more general discussion of the multiple roles values play in science, see for instance Longino (1990).

[44]Mongin (2006, p. 257) has contrasted this paradigmatic factual judgment with the following paradigmatic value judgment: ''The more equal the income distribution, the more just it is.''

knowledge depends on whether they consider the higher inequality of wealth to be a social problem worth addressing, which involves a non-epistemic value judgment.

Now, it is very unlikely that Robbins would have denied that non-epistemic value judgments have a legitimate role to play in the ''bordering activities'' of scientific economists. Firstly, it is quite obvious that scientific economists will have to decide on which topic they want to work at least in part in light of personal, ethical, political, and socio-cultural value judgments. Secondly, Robbins was clear that a *scientific economist* could also be a *political economist*, who gives advice in light of non-epistemic value judgments on the basis of discoveries acquired in the domain of economic science proper.

So I count two dead issues in the contemporary debate on economic science and values: the issue of whether economic science involves epistemic values (it does), and the issue of whether scientific economists engage in non-epistemic value judgments in their ''bordering activities'' (they do).

What Robbins wanted to resist is the idea that non-epistemic value judgments have a legitimate role to play with respect to the ''internal activities'' of scientific economists. ''Internal activities'' include, first and foremost, the formulation and testing of economic hypotheses (what philosophers of science call the ''context of justification,'' as opposed to the ''context of discovery''). In the framework of the contemporary debate, Robbins's position is best understood as ''separatist'' (rather than ''naïve positivist'').

The 'separatist view' holds that non-epistemic values should play no role in the ''internal activities'' of scientific economists.[45] Importantly, being a separatist is compatible with regarding ''bordering activities'' of scientific economists as (non-epistemically) value-laden, but either prior or posterior to the pursuit of economic knowledge, and therefore unable to affect its value-freeness. It is also compatible with the realization that values of an epistemic sort cannot be kept outside the ''internal activities'' of economic scientists. When compared with the 'naïve positivist view,' the 'separatist view' represents a move from an ideal of science as *free from values*, at this point a bankrupt ideal, to an ideal of science as *free from non-epistemic values*.

Many of Robbins's critics are instead best understood as 'non-separatists.' The 'non-separatist view' holds that both epistemic and non-epistemic values have a legitimate role to play in the ''internal activities'' of scientific economists.[46] On a non-separatist view, scientific economists can make non-epistemic value judgments *qua* scientific economists. In the next section, I will explain how the non-separatist position represented by Robbins's critics has been developed in recent philosophy of science, partially with arguments that echo those of Robbins's (1932) critics in the 1930s. Whether non-epistemic values have a role to play in economic science, I will conclude, is still very much up for grabs.

[45]For a general defense of this position, see Scriven (1980) and McMullin (1983).
[46]For a general defense of this position, see Longino (1990) and Douglas (2007).

The No Sharp Positive-Normative Distinction Objection Updated

A number of studies have argued that the boundary between the positive and the normative is very porous in areas of science that directly impinge on public policy.[47] Since economic science often directly impinges on public policy, we should pay close attention to this literature. The studies I will mention offer an updated version of the *No Sharp Positive–Normative Distinction Objection* voiced, with somewhat different arguments, by Fraser (1932), Souter (1933), Spengler (1934), Harrod (1938), and others.

The area where the influence of non-epistemic values has been studied most closely is that of risk management policy. Multiple papers have shown that the distinction between *risk assessment*, the supposedly detached collection of scientific facts about risks, and *risk management*, the socio-political decision process through which risks are managed according to policy values, is not as sharp as the 'separatist' would like it to be (Shrader-Freschette 1985; Lynn 1986; Douglas 2000).

Douglas (2000), for example, has focused on the impact of non-epistemic values in the way pathologists study the effects of dioxin exposure in rodents. She explored the effects of different non-epistemic value judgments about regulation of chemicals in the environment on how the testing methods are chosen, the data gathered, and the evidence about rodent cancers interpreted. In particular, she documented that the very same data on cancers of the liver gathered in the seminal study that spurred environmental regulation of dioxin levels (cf. Kociba et al. 1978) have been interpreted differently by different sets of pathologists in 1978, 1980, and 1990. This is to say that the ''judgment of fact'' about whether a certain rat has developed cancer is not insulated from the (changing) non-epistemic ''judgments of value'' pathologists hold about environmental regulation of chemicals. Douglas (2000), a non-separatist, offers a variety of reasons why this influence is legitimate and to be welcomed.

Scholars interested in arbitrating the (updated) debate between Robbins and his critics should investigate the extent to which the non-epistemic values governing political economy influence the way hypotheses are formulated and tested in economic science, as well as the way economic experiments are structured and run, economic data are gathered and interpreted, and conclusions about the acceptability of economic hypotheses are drawn. This will allow a well-informed assessment of the sharpness of Robbins's divide between ''economic science,'' the supposedly value-free study of human behavior as a relationship between ends and scarce means which have alternative uses, and ''political economy,'' the branch of economics primarily concerned with value-laden policy.

I conclude with an example of the sort of influence non-epistemic values may play in the ''internal activities'' of scientific economists—as they did on the internal activities of pathologists in Douglas' (2000) study—by focusing on the acceptability

[47]A related worry is that the very distinction between epistemic and non-epistemic values is problematic and cannot be drawn in any principled way (e.g. Rooney 1992 and Machamer and Douglas 1999). If this is the case, the positions of Robbins and his critics will be hard to keep neatly separate. I will bracket this issue, and assume for the sake of argument that we can draw the epistemic values vs. non-epistemic values distinction.

of economic hypotheses. It is fairly uncontroversial that economic hypotheses are rarely if ever conclusively confirmed by the empirical evidence. What the empirical evidence does it to provide economic hypotheses with varying degrees of inductive support. This being the case, the acceptance of an economic hypothesis H, say that there is a wage difference between equally qualified men and women performing relevantly similar jobs, carries the inductive risk of accepting a false hypothesis. By the same token, the rejection of H carries the inductive risk of rejecting a true hypothesis. Various philosophers of science have argued that non-epistemic values have a legitimate role to play in the management of inductive risk.[48]

I will briefly summarize philosopher of science Carl Hempel's (1965) views on this matter, as they apply to economic science. The starting point is the idea that scientific economists have to decide not only what kind of evidence can confirm the wage differential hypothesis H, but also how strong the evidential support for H has to be in order for H to be accepted as part of economic knowledge, and consequently used as a basis for advice in political economy. Deciding how strong the evidence has to be for hypothesis acceptance demands comparing the desirability of false negatives and false positives. This comparative evaluation is a place where non-epistemic values arguably have a legitimate role to play in the ''internal activities'' of scientific economists.

As Hempel put it, ''the problem of formulating adequate rules of acceptance or rejection [of some scientific hypothesis H] has no clear meaning unless standards of adequacy have been provided by assigning definite values or disvalues to those different possible 'outcomes' of acceptance or rejection'' (Hempel 1965, p. 92). A contemporary non-separatist may argue that the evaluation of the consequences of accepting a false economic hypothesis and rejecting a true one should be made in light of non-epistemic policy values. ''In the cases where the hypothesis under test, if accepted, is to be made the basis of a specific course of action,'' Hempel emphasized, both false negatives and false positives will bear practical consequences (pp. 92–93).

The degree of confirmation required for an economic hypothesis to be accepted will depend on how the scientific economist evaluates these practical consequences in light of non-epistemic value judgments, namely judgments ''to the effect that a certain state of affairs . . . is good, or that it is better than some specified alternative'' (p. 85). For instance, if a scientific economist A is convinced that it is better to accept the hypothesis that there is a gender gap when there isn't one (false positive) than to reject the hypothesis that there is a gender gap when one is present (false negative), he or she will demand a lower degree of confirmation prior to acceptance than a scientific economist B who considers false positives to be less desirable than false negatives.

When ''no practical applications are contemplated,'' Hempel concluded, the evaluation of inductive risk will be ''more problematic.'' Scientific hypotheses will have to be evaluated only in the light of epistemic values, namely those values that serve the objective of ''the attainment of an increasingly reliable, extensive, and theoretically systematized body of information about the world'' (p. 93). A significant portion of economic science is indeed fairly detached from practical applications. But

[48]See for instance Churchman (1956), Rudner (1953), and Douglas (2000).

another significant portion is essentially tied to them, and it may reveal the influence of non-epistemic value judgments in determining rules of acceptance for economic hypotheses.

Of course, a separatist may reject this conclusion, and argue that if non-epistemic values influence rules of acceptance in economic science, they do so illegitimately.[49] For example, a contemporary proponent of Robbins's separatist position may propose that accepting an economic hypothesis is not equivalent to choosing to act on the basis of it. If one does not act, the argument goes, there are no practical consequences to be judged in light of non-epistemic values. And even if one does act, different actions may lead to different practical consequences, so what is evaluated is at best a combination of an economic hypothesis and a specific course of action based upon it. Alternatively, a separatist may argue that the job of a scientific economist is not to accept or reject economic hypotheses, but simply to establish their degree of confirmation (Jeffrey 1956). A non-separatist may retort that this is not what scientists take themselves to be doing when they test economic hypotheses (Rudner 1953), or perhaps that they *ought* to evaluate false positives and false negatives in light of non-epistemic values, because they are the only ones in a position to fully evaluate the consequences, and likelihoods of occurrence, of their mistakes (Douglas 2007).

I will not take position on the complex debate between separatists and non-separatists.[50] My point is just that the non-separatist position cannot be dismissed out of hand. Robbins thought that a stubborn disregard for the obvious motivated his critics: statements about what is and statements about what ought to be clearly belong to two different logical realms. Several historians have endorsed this interpretation, and suggested that the methodological opposition to Robbins in the 1930s was primarily motivated by failure to understand what Robbins was up to (e.g. Peston and Corry 1972; Masini 2009). Granted, some of Robbins's critics misunderstood his methodological views, especially with respect to whether or not economists could give advice in their professional capacity.

This being said, I have argued that at the core of the methodological debate did not lie a misunderstanding, but rather a substantive clash between two equally legitimate, but fiercely divergent, views on the role of (non-epistemic) values in economic science. On the substance, it is far from obvious that Robbins, rather than his critics, got it right. The jury on whether there is a fundamental gulf between the positive and the normative is still out.

V. CONCLUSION

In the sixth chapter of the *Essay*, Robbins stated that neither ''interpersonal comparisons of utility'' nor ''value judgements'' could be submitted to a scientific test, and were consequently not part of economic science proper. Since Welfare Economics required both types of judgments, Robbins considered it unscientific.

[49]See for instance Jeffrey (1956) and Levi (1960).
[50]See Kincaid et al. (2007) for further discussion.

Robbins's methodological views had a mixed reception in the community of Anglo-American economists.

On one hand, the rise and subsequent fall of the New Welfare Economics demonstrated that Robbins's methodological recommendations were drawing converts. The cornerstone of the New Welfare Economics was in fact the Compensation Principle, which stated that a policy can be prescribed if those who gain from it can potentially compensate those who lose from it. As critics eventually pointed out, the Compensation Principle could not get around Robbins's ban on value judgments, because it involved the judgment that Potential Pareto Improvements are a desirable end for policy.

On the other hand, the published reactions to the ''scientificity requirements'' set forth in the *Essay* were uniformly hostile.[51] I have distinguished three main sorts of objections. The *Pragmatic Objection*, which highlighted some negative consequences of Robbins's views on the viability and authority of economic advice; the *Interpersonal Comparability Objection*, which defended the scientificity of interpersonal comparisons of utility; and the *No Sharp Positive–Normative Distinction Objection*, which called into question the sharpness of the divide between statements about what is and statements about what ought to be.

Robbins quickly dismissed such objections, arguing in effect that the distinction between the positive and the normative is unassailable. I have argued that recent philosophy of science has called into question the sharpness of the positive–normative divide, giving new force to the positions expressed by Robbins's critics. It remains to be decided who is right and who is wrong on the role of (non-epistemic) values in economic science. Until the issue is settled, we should consider Robbins and his critics to represent two theoretically viable methodological positions.

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[51]Aslanbeigui's (1990, p. 622) claim that Robbins's requirements of scientific economics, besides being criticized, also received ''a great deal of support'' in the course of the debate has to be considered unsupported by historical evidence. There certainly were some economists who sympathized with Robbins's ''scientificity requirements,'' especially at the LSE, but to the best of my knowledge they did not publish any works in explicit defense of the *Essay*, with the exception of Sutton (1937), between 1932 and 1939.

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