

## **THE MAKING OF ROBBINS'S ESSAY**

It may seem a little odd to be talking about the origins of Lionel Robbins's Essay on the Nature and Significance of Economic Science at the end of a conference devoted to the book and its impact on our discipline. But it may nonetheless be interesting at this stage to go back to what Robbins thought he was trying to do at the time he was trying to do it after our discussions as to whether he succeeded. I have already written - and published - on this before, in an article on the origins of the book, where I used the information I have gathered in writing Robbins's biography to ascertain the ideas and influences that went into his writing of the first edition of his Essay.<sup>1</sup> His papers (which will eventually be coming to the library here at LSE) include notebooks containing his lecture notes and reading notes from the 1920s and early 1930s (including a set of lecture notes which Robbins himself labelled 'first draft of final form of N & S') as well as correspondence and reviews of the book, all of which throw light on the drafting of the first edition and on the changes Robbins made in preparing the second edition only three years later. Since I focussed on the first edition of Nature and Significance in my article I shall now concentrate on the changes he made in the second edition in response to criticism and reviews of the first edition. I cannot avoid repeating some of what was in the earlier article about the first edition but I shall confine it to summarizing the main conclusions I drew about the origins of the first edition.

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1. The first thing to be said about the making of the first edition is that the information about Robbins's education and early career confirms his own statement about the origins of the book. He wrote in his autobiography:

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1. 'The origins of Lionel Robbins's Essay on the Nature and Significance of Economic Science', History of Political Economy 36:3 (Fall 2004) 413-43.

The beginnings of ... [the Essay] lay some time back in the past. The fundamental textbook on the elements of economics when I was a student at LSE [in 1920-3] was Cannan's Wealth; and the first chapter of this truly excellent work was devoted to elucidations which defined its subject-matter in terms of the causes of material welfare. Shortly after I joined the staff as a teacher [in 1925], I was put to lecture to a special course for Army officers on the Economics of War and readiness for war [having been an artillery officer on the Western front in the First World War]; and I had not been long engaged on my preparation for this task before it was borne in upon me that, although what I was going to say leant heavily on economic analysis as I had been taught it, it yet fell completely outside Cannan's definition - indeed he went specially out of his way to deny that war and its accompaniments fell within its scope. This puzzled me very much; and my perplexities increased when I reflected on the number of activities in which I was especially interested, concerts, theatrical performances ... [etc] which had nothing to do with material welfare but which yet certainly had an economic aspect. What then was the common factor to which our technique was applicable? Gradually it dawned on me that ... the underlying fact which made so many different activities and relationships susceptible to economic analysis was the scarcity of the means with which they were concerned and not the materiality of the objectives. There was nothing especially original about this conception. I was deep in the study of the marginal utility theory of value at the time, especially in the works of the Austrians and Philip Wicksteed. Even if they did not say in so many words what I was beginning to say to myself, my formula followed naturally from their explanations. What was it all about if not the behaviour of people disposing of goods and services which in the last analysis were in some way limited in supply rather than freely available - in short, conduct influenced by scarcity?<sup>12</sup>

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2. Autobiography of an Economist (London: Macmillan, 1971) page 148.

The Robbins papers contain his undergraduate notes on Cannan's lectures, his own notes for that course on the Economics of War given in 1926 and also his notes for two sets of lectures on the 'Elements of Economics' (ie introductory economics) - a short course when he was a temporary lecturer in Oxford in 1924-5 and a longer one he gave for first year students here in 1926-7.<sup>3</sup>

2. Those notes for his introductory lectures in 1925-7, along with reading notes he made as an undergraduate in 1922-3 show that his longstanding concern was to demarcate economics from other social sciences, especially political science as taught by Harold Laski, which he did not think was scientific at all. (In his second and third years at LSE Robbins specialized in the history of political ideas under Laski's supervision.) He told J.M. Clark thirty years later:

'That I eventually crossed over and made Economics my chief interest was due directly to the fact that I felt it threw light on problems of politics I had been studying from the other side. .... The Nature and Significance was always intended to be a sort of preliminary manifesto designed to forestall the criticism that I did not know where the borderline between the different disciplines really lay.'

3. His notes for his lectures at Oxford (where he was a fellow of New College 1927-9) include those for a course which he called (following John Stuart Mill) 'Unsettled problems in theoretical economics' and gave in Hilary Term 1928-9 (ie January to March 1929). These notes show that by the end of 1928 he had arrived at his famous definition of economics. He said in those lectures - after criticizing the various definitions of economics given by Alfred Marshall, Edwin Cannan, Henry Clay and A.C. Pigou, and explaining how his own doubts about Cannan's definition in particular had begun - that his own approach was to look for

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3. There is a mistake in my 2004 article: not then realizing that Robbins had given any lectures in Oxford in 1924-5, I assumed the notes for the short course were for a one-term course on the Elements of Economics he gave in Oxford in Michaelmas Term 1927.

'not a definition of economic which classifies out a certain set of activities which it labels economic but one which indicates what aspects of human activity in general are significant to the economist.

Now if we think of human activity in general there are two features which seem to have significance from our point of view.

In the first place the ends are various.

Secondly the means of attaining them are often very limited & are capable of alternative uses.

It is in this aspect of human activity - activity as condition[ed] by the fact of scarcity that I think the economist is interested. He is interested in the way people individuals and societies economize - that is dispose of the things which are scarce & how changes in the scarcity of these things (whether coming from the demand side or the supply side) affect their activities.'

4. The emphasis on the positive versus the normative aspects of a science comes from this early concern with demarcation. One of his objections to political science was that it kept mixing analysis of what is with pronouncements on what ought to be. He made his first published statement of his view that economics must not include ethics in a review of R.G. Hawtrey's book The Economic Problem (1926) in Economica in 1927 (and referred his students to it in his Oxford lectures).<sup>4</sup> From this comes, of course, his objection to basing theoretical arguments on interpersonal comparisons of utility since they inevitably involve value judgments.

5. As far as the Austrian influence on the making of Robbins's Essay is concerned, the material in Robbins's papers corroborate Denis O'Brien's conjecture that the 'primary source was undoubtedly Wicksteed's Common Sense [of Political Economy], while he [Robbins] drew from the Austrians precisely those elements which coincided most directly with what he had drawn from Wicksteed.'<sup>5</sup> Although Robbins read Mises's Die Gemeinwirtschaft, in

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4. 'Mr Hawtrey on the scope of economics' Economica 7 (June 1927) 172-8.

5. 'Lionel Robbins and the Austrian connection' in Carl Menger and his legacy in economics ed Bruce J Caldwell, History of Political Economy 22 (supplement 1990) 155-84.

German, in 1923 or thereabouts, was very impressed with its arguments against socialism and began to translate that part of that book in 1925, there are far more favourable mentions of Wicksteed in his notebooks, especially in one labelled 'Method etc. Early floundering 1923 ---', than of Mises. Similarly, although Robbins recommended Mises's book Nation, Staat and Wirtschaft (1919) in his Economics of War lectures (along with Pigou's The Political Economy of War [1921]), there are virtually no references to Mises, but lots to Wicksteed, in his 1929 'Unsettled problems' lectures where he is developing his arguments leading to his definition of economics. (As I put it in my article [page 426], 'My conclusion to this point is that by the end of 1928 Robbins had found his definition of the subject matter of economics, after brooding about it for some years. It owed most to Wicksteed and there was nothing particularly "Austrian" about it.') Furthermore, when you look at Robbins's lecture notes for a course entitled 'The nature of economics and its significance in relation to the kindred social sciences', which he first gave at LSE in the Summer Term of 1930 (by which time he had returned to LSE as professor of economics) and which are the notes he labelled 'first draft of final form of N&S', you find that while there are differences of style and emphasis the structure and argument of the published Essay is essentially the same as the lecture notes but there are almost none of the many footnote references to Austrian economists that adorn the first edition of the Essay. These were added in the winter of 1931-2 when Friedrich Hayek had become Robbins's colleague at LSE.<sup>6</sup>

6. Another conclusion of my article was that Robbins's views on the methodology of economics developed over time: that it was only after he had solved his demarcation problem to his satisfaction that he began to concern himself with the methodology of economics. Having previously accepted uncritically the conventional views of scientific method he had learned as an undergraduate, in 1929-31 he began to clarify his views on method, a process which can be seen in his notes for his lectures in those years and in correspondence in the summer of 1931 when he was ill with chickenpox.

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6. Robbins first met Hayek here in January 1931, when Hayek came to give four public lectures on Prices and Production. After the lectures Hayek was invited to come back to the School as a visiting professor for the academic year 1931-2, at the end of which he was offered the vacant Tooke Chair, which he held until 1950.

The 'Unsettled problems' lectures given in Oxford at the beginning of 1929 did not discuss the methodology of economics. The major difference between those lectures and those on 'The nature of economics and its significance' given at LSE in 1930 (and again in 1931 and 1932) is that the latter included two lectures on the subject, the first espousing a deductivist view of economics, the second on the (very limited) uses of induction to suggest and test the assumptions from which economic generalizations followed by deduction. (The notes on 'Definitions' for the first of the later lectures follow those for the earlier lectures very closely: indeed they comprise the same arguments and examples written out in more complete sentences. In the notes for the LSE lectures there follows a section on 'Economics and ethics', where he criticized Hawtrey and J.A. Hobson as he had done before for not keeping positive and normative analyses separate, which became the second chapter of the published Essay, and then a discussion of the meaning of economic quantities which became the third chapter. What became the sixth chapter was entitled 'Economics & political theory': it focussed first on the invalidity of using the law of diminishing marginal utility to justify policies of income redistribution, since it involved interpersonal comparisons of utility, and second on the doctrine of laissez faire, stressing its limitations and the resulting need for public goods.) The arguments of the methodology lectures appear, with considerable elaboration and revision, in the third and fourth chapters of the published Essay on 'The nature of economic generalisations' and 'Economic generalisations and reality'.

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At this point I can start to move from the origins of the first edition of the Essay to begin to consider the changes from the first to the second edition, since it was in the methodological chapters that he made significant changes when he revised the book and he made few changes elsewhere.

As Jim Thomas has already mentioned at this conference, Robbins as a first year undergraduate chose to study Logic and Scientific Method instead of mathematics - and hence attended the lectures given here by Abraham Wolf

year after year (from 1907 to 1941). The description of scientific method he gave his first year students in 1926-7 is similar to what Wolf taught.<sup>7</sup>

(Then and later Robbins insisted that economics was a *science* - even if at that time he still thought of it as 'the science of material satisfactions' - whose aim was to establish generalizations about economic phenomena, such as - a favourite example - the quantity theory of money.)

There are two main methods of scientific procedure.

1) Firstly there is what is known as the inductive method. It consists in the deduction of general statements from the examination of particular instances. You make an exhaustive study of the habits of pigs and you say pigs reach certain proportions at certain parallels of latitude. ...

2) Secondly there is what is known as the deductive method. Starting from certain proved generalizations or certain hypotheses which you are valid you deduce what must happen if the forces described in these generalizations are combined in isolation. You do this in physics when you deduce the behaviour of projectiles on the assumption which is never true in fact that influences like wind and imperfections in the shell are absent. You do the same thing sometimes in economics when you imagine eg what would be the effect upon wages or interest rates of a certain kind of invention. It is a method which comes in useful when actual experiment with natural forces is out of the question. On these lines the [quantity] theory of money was thought out - in the main - so that when the great modern experiments - if you can dignify [wartime and postwar] inflation by such a name - came economists were able to predict the results with almost quantitative certainty.'

And he emphasized that 'both methods are equally legitimate.'

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7. Wolf's Textbook of Logic (London: George Allen & Unwin, 1930) consists of two parts, formal logic and inductive logic, which respectively incorporate his earlier textbooks, Essentials of Logic (London: George Allen & Unwin, 1926) and Essentials of Scientific Method (London: George Allen & Unwin, 1925).

But when he gave the Nature and significance lectures in the summer of 1930 he gave deduction definite priority.

In the first of the two lectures, 'Method. Simple Statement of Principles', he stated:

'The business of theory or abstract science is to make generalizations - to lay down propositions which transcend the particular and describe general uniformities. Such generalizations are sometimes described as laws and our business as I conceive it is to enquire into the nature of these generalizations and the logical justification of which they are capable.'

Economic generalizations, like those of other sciences, are both hypothetical and vary in their applicability (the more general being the more widely applicable). To illustrate he took 'a very simple generalization concerning demand[:] If price rises demand diminishes' and pointed out that this is just an implication of the definition of the demand function. 'So long as we assume that the conditions of demand exhibit a negative connection with price the thing is given in our initial assumption.'

He then made a strong claim:

'Now all exact generalizations of Economics are of this nature. They are merely the explanation of the logical consequences of your initial assumptions. Given the assumptions and assuming a correct logic they are unassailable.'

Hence 'The truth or falsehood of the laws is merely a matter of logical consistency' and 'in a sense, pure economic analysis is simply a matter of exercises in logic, a matter of squeezing the utmost drop of implication out of assumptions which are given.' He referred here explicitly to John Neville Keynes, The Scope and Method of Political Economy (4th edition, 1917). He then turned to initial assumptions and argued against any dependence on the concept of economic man, using Wicksteed's arguments, and on any ideas drawn from the discipline of psychology.

His second lecture, 'The Place of Induction', began:

'It follows I think from what I was saying last time that the functions of such [empirical] studies are twofold:

(a) Firstly it is in this way that we are enabled to select our assumptions.

(b) Secondly it is in this way that we test the suitability of our theories.<sup>8</sup>

He went on to claim that when we are considering, for instance, the theory of capital exports or the quantity theory of money, 'We are not testing the truth of the theory - the accuracy of the deductions. We are testing its adequacy to explain certain situations. We are asking whether the assumptions are suitable. We are finding out how to use the theory.' His example was the quantity theory of money.

'We may start e.g. with a very crude formulation of the quantity theory.

If the quantity of money increases the value of money falls.

We examine a period during which the quantity of money has been increasing and we find that the value of money has risen.

The theory is not wrong. Other things have not been equal. It is not sufficient.

We examine other things. We find that the work for money to do has increased.

We reformulate. If the quantity of money increases faster than the amount of work which money has to do - the volume of trade [-] then the value of money will fall.

We take other cases. We find that velocity of circulation is important. We introduce assumptions taking account of this.

Then we find that the term value of money is ambiguous. Which price level do we mean. One mode of measurement gives one result another another.

We discover that the whole theory needs recasting to take account of this.

And so on.'

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8. He did admit, though, that in selecting assumptions 'We never approach facts with perfect passivity. ... The facts suggest assumptions only to attention that is theoretically active.'

He concluded on induction: 'It suggests assumptions. It provides a means of testing & reexamining assumptions when these have been combined in suitable permutations.' He then launched (as he was to in the Essay) into a spirited attack on (American) institutionalism, the basis of his attack being the standard philosophical argument about the validity of inductive (including statistical) inference.

As I have already remarked the two lectures on method became the two methodological chapters (4 and 5) of the first edition of the Essay. These expounded Robbins's view of the logical character of economic theory, its lack of dependence upon psychology or 'economic man' and the subsidiary role of empirical studies. As I show in my article his arguments had been sharpened since 1930 by an exchange with an old friend, Nathan Isaacs, whom he had first met in the army in 1916. Isaacs, who was enough of an amateur philosopher to be a member of the Aristotelian Society and publish in its journal,<sup>9</sup> had tried to persuade his friend of the usefulness of induction and the need to test scientific theories. Robbins had responded by pointing to the utility of economic theories derived by deduction, notably the quantity theory of money. In the book he added as examples of fruitful economic theory Hayek's trade cycle theory.<sup>10</sup>

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9. Including an article, 'Psycho-logic', Proceedings of the Aristotelian Society 31 (1931) 225-62, which Robbins read in bed with chickenpox and then wrote to Isaacs about it.

10. Robbins's view of economic methodology helps to explain why he Robbins was so impressed with Hayek's *Prices and Production* lectures.

On the first of them, which reviewed the existing literature on the quantity theory, Robbins thought 'our guest seemed to have the rare gift of gazing at bodies of apparently well known theories and then speaking of them in a way which cast them in an entirely new light.'

As for the subsequent lectures on Hayek's own theory, Robbins wrote in his preface to the published version: 'I can only say for profound theoretical insight and power to open up totally new horizons, I know only one work ... published in English since the war with which they can be compared - Mr Dennis Robertson's Banking Policy and the Price Level [1926].' As for the practical implications, it seemed 'to fit certain facts of the American slump better than any other explanation I know.' He did not think it was 'altogether an accident that the Austrian Institut für Konjunkturforschung ... was one of the very few bodies of its kind which, in the spring of 1929, predicted a setback in America with injurious repercussions on European conditions.'

Robbins prepared the second edition of the Essay on the Nature and Significance of Economic Science during the academic year 1934-5. He tried to take account particularly of the criticisms of his friends; he did not take much account of the criticisms of most of the critical reviewers. He explained in the preface, which is dated May 1935, that he was not going to change chapter six, which had been the target of most attacks on account of his denial of the scientific legitimacy of interpersonal comparisons of utility, and defended himself against the common and inaccurate charge that he had recommended economists abstain from policy debates.

He had received many complimentary and encouraging letters from colleagues and friends, even those who were critical of the methodological position. Among the colleagues were Hugh Dalton, Evan Durbin and Harold Laski (all men of the left, active members of the Labour Party). Laski told Robbins that his book was 'a brilliant piece of logical and systematic argument', which made him (Laski) proud to have had Robbins as a student and even more as a colleague. His one significant criticism was that he thought chapter two ('Ends and Means') 'slays the slain' since 'the economic historians who matter ... have long deserted Schmoller's camp'. Dalton, who had first taught Robbins as a first year undergraduate, still regarded Robbins as his protégé (and had worked to get Robbins appointed professor over Laski's objections) and had read the manuscript before it was submitted to the publisher in February 1932. He had taken Robbins to task for, for instance, arguing that interpersonal comparisons of utility were always empirically unverifiable and had teased him for the 'usual superlative bouquets to Mises' in his footnotes. But he had approved of much of the methodological argument. He thought chapter two was 'full of good fun & good sense' and he was sympathetic to the criticism of 'economic welfare' as used by either Cannan or Pigou, though 'not yet convinced' it should be given up, since he still believed 'the proposition that "A is better off than B" seems to mean something.' The manuscript Dalton was commenting on does not survive but it is clear from comparison of Dalton's screed and the published version of the book, that some footnotes were altered or omitted. Durbin had been a student of Robbins's at New College Oxford in 1927-9, reading for PPE after a first degree in zoology. He had like Dalton 'stayed up to excessive hours for the sole purpose of finishing your book - a tribute I rarely pay to anyone.'

With his background in zoology he was critical of the methodological standpoint, where he thought Robbins was guilty of oversimplification and should also have discussed the relation between pure and applied economics. He was very impressed with the last chapter on the significance of economics and the peroration against irrationality.<sup>11</sup>

Jacob Viner, who had been a good friend of Robbins's since they met in Oxford in 1927, told him that 'There is almost nothing in it with which I would take serious issue, and with most of it I am in violent agreement. It is an excellent piece of work and I am going to make my students read it next year.' Mises said he intended to use it in the discussions in his seminar in Vienna - and he did. He had also written that he 'probably need not tell you that I agree with your arguments throughout'. The German liberal economist Wilhelm Röpke offered to translate the book into German; his one criticism of the book was that Robbins, like Max Weber, had gone too far in his attitude towards value judgments. Röpke had to report in March 1933 that he could not find a publisher in Hitler's Germany (which Röpke, as a liberal economist, soon had to leave).<sup>12</sup>

Robbins had first met Mises in September 1931 when Mises was in London for the British Association for the Advancement of Science meetings. He had met him again and got to know him better at a 'world conference of economics' in Berlin in May 1932, organized by the Berliner Tageblatt newspaper, on international trade and capital movements (Robbins spoke on the latter) and when Robbins visited Austria in 1933, first in April to give a lecture to the Nationalökonomische Gesellschaft and then in July and August for a long holiday with his wife and children at St Gilgen in the Salzkammergut when they were visited by the Haberlers, the Machlups and Mises. Robbins had met both Gottfried Haberler and Fritz Machlup in London, in 1930 and 1932 respectively. (There was one more long Austrian holiday at Thumersbach near Zell am See in the summer of 1935, when 'a whole succession of our

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11. Laski to Robbins, 23.iv.32, Dalton to Robbins, '2 a.m. 5/2/32', and Durbin to Robbins, 21 May 1932, N & S Letters file, N & S Box, Robbins Papers.

12. Viner to Robbins, 26 August 1932, Mises to Robbins, 18 June 1932 [my translation from German], Röpke to Robbins, 13 July 1932 [my translation] and 1 March 1933, N & S Letters file, N & S Box, Robbins Papers; programme for Mises seminar 1933-4 in Gaitskell Papers B3, UCL.

Viennese friends ... visited' the Robbinses including Mises for a week; but by this time Robbins had completed the revisions to the first edition of the Essay.)<sup>13</sup> During these visits he and his friends discussed the Essay: as far as one can tell the younger Austrian economists, whose views were different from Mises', criticized the two methodological chapters. In March 1935 Robbins told Machlup that he was

'quite sure now that certain statements in the fourth chapter of my book were couched in terms which, although I do not think they were wrong, were certainly very liable to give rise to misapprehension and in the second division [sic], which I hope I shall complete this [Easter] vacation, I intend to make quite a number of modifications. I suppose it is natural that the statements which I now think to be least aptly expressed are just those statements which have escaped the notice of those of my critics who attack me so angrily. I owe much more to conversations with you and Haberler on this matter than to anything which has so far been published in any journal. There was, however, what I thought was a very good note in the last number of the 'Review of Economic Studies' which I thought got completely home so far as my use of the term "tautology" was concerned.'<sup>14</sup>

The note was by Terence Hutchison who was then a research student at Cambridge. Hutchison's note rightly criticized economists (including Robbins on occasion) for criticizing other economists' theories as tautologous, since any deductive theory must be a tautology. As he said, 'A tautology, in the use of modern logicians, is an analytic proposition which cannot conceivably be false because its truth is assured by the, in a certain sense arbitrary, process of assigning definitions.' He pointed out the contradiction in Robbins's remark (Essay, page 111) that it was 'the inevitability of economic analysis which gives its very considerable prognostic value. ... given the data in a particular situation, it can draw inevitable conclusions as to their implications' since 'An inevitable implication is a tautology, and can, by its nature, no more prognosticate anything than can the multiplication table.'<sup>15</sup>

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13. The sources of this information are in my biography of Robbins.

14. Robbins to Machlup, 14 January 1934, Machlup Papers 61-1.

15. T.W. Hutchison, 'A note on tautologies and the nature of economic theory', Review of Economic Studies vol 2 no 2 (February 1935), pp 159-61.

Robbins had already told Machlup a year earlier that

I dont feel inclined to retract anything so far as the critics are concerned. But the book [the Essay] is out of print & I shall take the opportunity in the second edition of explaining the section we discussed in our drive to Ischl. I think I can meet Haberler & Kaufmann without sacrificing anything fundamental & incidentally this may clear up certain misunderstandings with regard to my own attitude to the empirical element in general.<sup>16</sup>

The nature of the conversations with Haberler and Machlup (I shall return to Kaufmann shortly) is indicated in subsequent letters from Haberler.<sup>17</sup> In the spring of 1934 Haberler was 'sure that we shall easily reach an argument in the already overworked controversy about the tautological character of certain marginal utility theories. Except with Mises, with almost everybody I have come to an agreement on this.' A few months later he wrote (from Geneva where he was working for the League of Nations on the business cycle project which produced Prosperity and Depression):

‘I am looking forward with great interest to seeing your new book [The Great Depression (1934)] and to hear what changes you will make in Nature and Significance. Have you seen the mimeographed papers, which have been discussed in the Mises Seminar? I feel strongly that you should see them, before you write on the question, whether economics is a "a priori science" and which are the aprioristic "Elements" in it. These discussions must have been extremely interesting and I think you should consult someone (Stonier e.g.) who participated in them. I still think that Mises' position is quite untenable and he was, according to what I heard, quite isolated in his group. It is on the other hand very important to clarify the issue. Otherwise one is exposed to such foolish attacks as the one of Souter, who mixes us "Wertfreiheit"

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16. Robbins to Machlup, 25 March 1935, Machlup Papers 61-1.

17. It is harder to identify Machlup's position on methodology at this time (though he wrote extensively on methodology later). His note, 'Why bother with methodology?' (Economica new series vol 3 no 9 (February 1936) pp 39-45), is, as its title suggests, concerned to argue that economists should be concerned with methodological issues not with what their position on such issues should be.

and the problem of whether economics is apriorism. This curious mistake I find rather frequently with Anglosaxon economists e.g. with Opie. I think you should take all precautions to make it clear to these people that there are two problems, which have nothing to do with each other.<sup>18</sup>

Felix Kaufmann was a young Viennese philosopher and a member of the Vienna Circle. The Vienna Circle, which had been meeting since 1924 and was disintegrating as its members were leaving Vienna, had been much concerned with the nature of logic and mathematics and Kaufmann had written on the use of mathematics in economics:

Robbins had cited his work in the first edition of the Essay. Kaufmann also turned up at the Robbins Seminar at LSE a couple of times in late 1933 and he published an article on economic methodology in Economica in

November 1933 and one in the Review of Economic Studies in February 1934. Robbins had referred to an earlier article published in German by Kaufmann at the end of the paragraph on page 65 of chapter III which read:

'Scientific generalisations, if they are to pretend to the status of laws, must be capable of being stated exactly. That does not mean ... that they must be capable of quantitative exactitude. We do not need to give numerical values to the law of demand to be in a position to use it for deducing important consequences. But we do need to state it in such a way as to make it relate to formal relations which are capable of being conceived exactly.' This statement remained in the second edition (pp 65-6).<sup>19</sup> Alfred Stonier was an Oxford-trained economist (with a first in PPE in 1927) who had gone to Heidelberg for his PhD (1934) and later became a lecturer in political economy at UCL; he had been a student of Roy Harrod's at Christ Church and was a friend of Robbins's former student, Hugh Gaitskell,

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18. Haberler to LCR, undated but March or early April 1934, and undated but June or early July 1934, Letters from Economists file, Corresp with Economists Box, Robbins Papers; Gottfried Haberler, Prosperity and Depression: A Theoretical Analysis of Cyclical Movements (Geneva: League of Nations, 1937).

19. Georg Tugendhat to Haberler, 2 November 1933, Haberler Papers Box 66, Hoover Institution Archives; Felix Kaufmann, 'On the subject-matter and method of economic science', Economica vol 13 (November 1933), pp 381-401, and 'The concept of law in economic science', Review of Economic Studies vol 1 no 2 (February 1934), pp 102-9, and 'Was kann die mathematische Methode in der Nationalökonomie leisten?', Zeitschrift für Nationalökonomie vol 2 (1930), pp 754-79.

who was then a lecturer at UCL. Like Gaitskell he spent time in Vienna and attended Mises's seminar. He also reviewed the Essay for the Zeitschrift für Nationalökonomie.<sup>20</sup>

The Vienna Circle, as is wellknown, took a hard line on epistemology, and on the demarcation between mathematics and science on the one hand and non-science (or metaphysics) on the other. The propositions of logic and mathematics are necessarily true, true by definition of the terms and hence tautologous, because they are irrefutable: no facts can possibly contradict them. They are *analytic a priori* in Kant's terminology. All other propositions may be true or false and if such propositions are to be scientific they must be capable of confirmation or refutation by empirical facts. Such propositions are *synthetic a posteriori* statements. From this you get the verification principle, that scientific propositions that are not logical or mathematical must be verifiable if they are to be counted as science. If they are not verifiable then they are not science, ie metaphysical.<sup>21</sup> The hard line implication is that there can be no *synthetic a priori* statements in a science, because such statements are neither analytic nor verifiable. Hence one of the things that Kaufmann, Stonier, Haberler and Hutchison spotted was that Mises's conception of economics ran into the problem that in so far as it was purely analytical and a priori true it could not also be an empirical science.

Kaufmann's Economica article, 'On the subject-matter and method of economic science', makes no mention of Robbins (or Mises for that matter: he mentions Stonier as a friend who helped him with the article.) He was concerned with a more general problem, that much of the methodological controversy in economics was muddled because of a failure to distinguish the separate questions of scope (subject matter) and method. As he said, if

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20. Zeitschrift für Nationalökonomie volume 5 no 3 (1933), pp 417-24.

Stonier's doctoral dissertation was published as Der logische Charakter der Wirtschaftswissenschaft, Beiträge zur Philosophie no 29 (Heidelberg: Carl Winters Universitätsbuchhandlung, 1935).

21. It was this principle that Karl Popper challenged on the valid ground that no synthetic a posteriori proposition can be verified: if n facts are consistent with the hypothesis there is always the possibility that an (n+1)th will not be. This is of course just a way of stating the problem of induction. Popper was, however, just as concerned as the Vienna Circle with the problem of demarcation, proposing falsifiability as the test instead of verification.

economists had found a truly fruitful method there would be no controversies about the subject matter of the discipline, which would like physics be defined by its method. He ended this article by summarizing his views on mathematics in economics, on which he had written in the Zeitschrift article cited by Robbins.

'... no mathematical proposition as such contains any statement regarding reality; it is, therefore, not possible to reach apodictical conclusions about the course of economic events with the help of the exact mathematical method. Einstein's dictum that "in so far as the propositions of mathematics refer to reality they are refutable, and in so far as they are irrefutable they do not apply to reality," is no longer open to doubt and it is just as applicable to economics as to physics. On the other hand, there is no sphere of reality which cannot in principle be investigated with the help of the mathematical method. It is not necessary that the phenomena to be investigated shall be themselves measurable, since as we have already argued, the chief part of the investigation can be conducted at a level of abstraction far removed from them. The mathematical method can neither be shown "on philosophical grounds" to be the only scientific method in economics, nor can it be rejected on other philosophical grounds as in principle inadequate. One can only try by careful analysis to get evidence as to the extent of its usefulness.'

Kaufmann also referred to his forthcoming book, which was published in 1936.<sup>22</sup>

Kaufmann's Review of Economic Studies article pointed out that misconceptions as to the nature of mathematical propositions and concepts had played a very large part in methodological discussion in the social sciences, especially in economics.

The propositions of mathematics with their precision and their apodictic validity, were regarded as providing a model for scientific laws of all kinds, for it was not realised that apodictical validity was incompatible with the

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22. 'On the subject-matter and method of economic science', Economica vol 13 (November 1933), page 401; Methodenlehre der Socialwissenschaften (Vienna: Verlag Julius Springer, 1936). (Kaufmann's English book, Methodology of the Social Sciences (Oxford University Press, 1944), is a rather different book as he had changed his views by then.)

Kaufmann published another paper in Economica in 1937, a reply to a review of his 1936 book: 'Do synthetic propositions a priori exist in economics: a reply to Dr Bernardelli', Economica new series vol 4 no 15 (August 1937) pp 337-42.

nature of statements about facts. Until the influence of such misconceptions has been eradicated, the problems connected with the laws of social science cannot be stated clearly. ...

'1. No proposition in logic or in pure mathematics tells us anything about reality; one can never learn from it whether a particular event is occurring, has occurred, or will occur at a definite time and place. The service of Logic and Mathematics is to translate implicit assumptions into explicit form. Logical and mathematical propositions are therefore analytical.'

But when it came to general statements or laws which purported to describe reality, there was the problem of induction. 'It is not permissible to contrast deduction with induction, as is often done, on the ground that the former is an inference from the universal to the particular, while the latter is an inference from the particular to the universal, since an inference from the particular to the universal is impossible. In so far as we can speak at all of inductive inferences, they are also inferences from the universal to the particular, but in this case the major premises are, as a rule, only partly conscious.'

'Thus a law, even when formulated, remains a hypothesis, and the distinction between established theory and unproved hypothesis represents only a difference of degree, like the distinction between "rigid" laws and mere rules or tendencies.'

Kaufmann, like many others, found a way round the problem of induction by regarding empirical laws as conventions: though the 'laws' were really only hypotheses and could be falsified, 'Confidence in the validity of a law may sometimes be so great that any observation which does not agree with it is regarded as fallacious, or at least incomplete. In this case the possibility of refuting the law is suspended.' But he emphasized such conventionalism easily led to misunderstandings: it was all too easy to slip from accepting theories made irrefutable only by convention as scientific 'laws' into thinking that there exist synthetic a priori true statements.

His example was the principle of marginal utility, which he had discussed at length in his Economica article. As in his Economica article he concluded this article by referring readers to his forthcoming book.<sup>23</sup>

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23. Kaufmann, 'The concept of law in economic science', Review of Economic Studies vol 1 no 2 (February 1934) pp 102-9.

Kaufmann's book was reviewed in Economica along with Stonier's published dissertation: the reviewer (Harro Bernadelli, a recent refugee from Hitler's Germany) criticized Kaufmann for 'fluctuat[ing] in a somewhat staggering way between the *conventionalist* thesis which sees in acts of mere arbitrariness the ultimate source of philosophical and mathematical principles, and the thesis of *rationalism* which tries to brand all such principles as analytical. In his book the assertion that a "necessary connection" between subject and predicate can be found *only* if the predicate by definition is determined as a property of the subject, "in which case the proposition in question is an analytical one" ... can be found side by side with the statement that geometrical axioms ... or laws of nature such as the principle of energy ... are only "conventions based on the test of experience," and it is left to the reader to find his way through these inconsistencies. It will, I hope, be obvious to the reader that the rationalist way of justifying principles cannot succeed because these principles are by nature synthetic, and the menace is apparent to which a science is laid open if sheer arbitrariness of conventions is declared its supreme authority.<sup>24</sup> Stonier's book received less attention from Bernadelli, though it too was criticized for lack of clarity, since Stonier was, as Bernadelli described it, 'only entangled in a fierce polemic against those who wish to claim a general phenomenological intuition (*Wesensschau*) as a source from which philosophical principles flow. The fact that such an intuition obviously does not exist is sufficient reason for him to conclude that therefore these principles must be of analytical nature' - 'a hasty conclusion' in Bernadelli's opinion. What is relevant here is that Stonier's targets included Mises. According to Bernadelli, 'his [Stonier's] main point is to show Mises is mistaken in asserting pure economics (catalactics) to be a science *a priori*. This he tries to do by pointing out the improbability of a phenomenological intuition of economic activities. Now, as far as I am aware, Mises has never claimed to be in possession of so sublime a source of knowledge; all that he means, if I understand him rightly, is that economics is based on certain principles which cannot possibly be derived from experience. The question whether this is so or not can be decided independently of the problem of the origin of such principles. Stating that phenomenological intuition is not likely

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24. Bernadelli, 'What has philosophy to contribute to the social sciences, and to economics in particular?', Economica new series vol 3 no 12 (November 1936) pp 337-54 (quotation from pp 447-8).

to be the source misses the point, especially if one keeps in mind that such an intuition need not be the only possible source from which these principles flow.<sup>25</sup>

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Returning to Robbins, while he was aware of these issues and all the more so given conversations with Haberler and Machlup, he did not want to get embroiled in the controversy. He explicitly stated this later, in his 'Live and dead issues ...' paper, where he wrote of one of the live issues, 'the exact logical status of certain of the more general assumptions on which pure economics is based':

'One school of thought regards them as essentially rational principles which are given *a priori* and which, while they show themselves in experience, yet require no appeal to experience to demonstrate their ultimate validity. Another, while not disputing the wide generality of their applicability, regards them as being derived from experience and having the same provisional status as the more obviously empirical assumptions. The former view has been very powerfully urged by Professor Mises and Dr Bernadelli, the latter by Dr Kaufmann and Mr Hutchison among others ...

'There are very fundamental epistemological questions involved here; and he would be a bold man who would regard the problems of epistemology as settled. I myself would confess to real doubt on the issue; and in the work alluded to above [the Essay] I have tried - in the first edition unsuccessfully, in the second, I hope, with greater, if not complete success - to use a terminology which steers clear of the ultimate questions involved.'<sup>26</sup>

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25. 'What has philosophy to contribute to the social sciences, and to economics in particular?', Economica new series vol 3 no 12 (November 1936) pp 448-9.

Bernadelli had obtained a DPhil in Frankfurt in 1931 with a dissertation on the foundations of economics (Über der Grundlagen der Okonomische Theorie [Tubingen: J.C.B. Mohr, 1933]); he arrived in London at the beginning of 1934 and found a job at Rangoon in 1935 (information from file SPSL 228/6, Society for the Preservation of Science and Learning Archives, Bodleian Library, Oxford).

26. 'Live and dead issues in the methodology of economics', Economica new series vol 5 no ... (August 1938), reproduced in Susan Howson, Economic Science and Political Economy (London: Macmillan, 1997), page 194.

It is also clear from an appendix which he drafted but did not use in the second edition of the Essay. This began: 'In the body of the book it will be noticed that I have made little or no allusion to recent controversial discussions of the ultimate status of economic generalizations. Indeed, the careful reader, prying behind the actual structure of my sentences, may even detect a deliberate avoidance of terms which commit me to one view or the other. Such an inference would be perfectly legitimate.' One reason for this was his philosophical incompetence; another was that he had come to believe that economists are capable of agreeing on what the core of their discipline is. (As I shall indicate below, he was helped in this by actual recent developments in economic theory.) I quote selectively (I admit I don't find his second argument very convincing):

'The question at dispute is the ultimate nature of economic laws. Are they given a priori or are they in some sense the generalization of experience? Is economics in this respect different from the physical sciences of which we have knowledge?

'Now, on the question whether it is legitimate to describe the main generalisations of economics as given a priori it seems to me that we must abide by the verdict of philosophers. If, as we are assured by some, the term a priori applies only to purely formal propositions such as a white horse is a white horse, then no doubt it is desirable when describing propositions which are supposed to have reference to reality to abstain from using this term. It is clear that whatever has been meant by those who have used it in connexion with economic laws that they have not meant this. Indeed so far from implying that their generalisations related to no reality (which is the case with purely formal propositions) they were anxious to find a way of suggesting that their propositions related to all reality. If therefore the use of the term a priori implies a necessary absence of reference to reality I do not see that there should be any concession in retreating from this position. But are philosophers really agreed about this?

'But this brings me to the second question. It is clear that there is room for difference of opinion on the way in which our knowledge of certain of these truths arises. ... But on the question whether this knowledge, whether it be given "a priori" or be a limiting abstraction from experience, is similar to character to the generalisations of the world of physical science which point to reality, [t]here seems to me to be no real room for difference of opinion. It surely will not be denied that our knowledge of the existence of scales or relative valuation is different from our

knowledge of the entities which are the subject of the proposition[s] of the physical sciences. We have an immediate inner acquaintance with the ultimate foundations of our generalisations in the social sciences which is not and cannot be the case with the generalisations of the physical sciences.

...

'It is this truth which as I understand it is insisted on by Professor Mises. Stated in this way there can surely be no room for disagreement among competent practitioners of the social scientists.'

However, 'On the other hand it may well be questioned to what extent it is possible to build a scheme of generalisations which are very helpful upon notions of the degree of generality. This seems to me a substantial point and in the text of the essay I have again and again insisted on the necessity at almost every stage of invoking subsidiary postulates of less complete generality than those which have hitherto been cited. It is clear I think that nearly every application involves less general propositions as well as the fundamental concepts.'<sup>27</sup>

In a draft preface to the second edition which he also did not use Robbins stated that he was not going to change chapter six which had been the object of so much criticism. But he had revised the first part of chapter four: 'the revision will not make it any more acceptable to [most of] the critics ... for the net effect is to make the aspect of Economics there treated more abstract & formalistic than ever. But I hope it will do something to meet the suggestions of my friends D<sup>r</sup> Gottfried Haberler & M<sup>r</sup> A.W. Stonier with whom I have had many instructive conversations on these matters.' In the actual preface, referring to the changes he had made in chapter four and parts of chapter five he mentioned Hayek, Paul Rosenstein-Rodan (who was then at UCL) and Stonier, 'whose advice and criticisms on these difficult matters have taught me much.' In these chapters he referred to his conversations with Machlup in the section (2) of chapter V on the nature of economic laws. In the new parts of chapter IV he referred also to Joan Robinson's Economics of Imperfect Competition, which he hoped 'will have done much to convince many hitherto sceptics of the utility and significance of the kind of abstract reasoning from very simple postulates',

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27. Ts APPENDIX. On certain recent discussions of The Ultimate Status of Economic Laws, N & S Box, Robbins Papers.

and to her pamphlet, Economics is a Serious Subject. He had corresponded with her on the latter, largely to point out how much they agreed on the methods of economic science.<sup>28</sup>

The first and most obvious change Robbins made to chapter IV of the Essay is the use of the Hicks-Allen innovations in demand analysis to illustrate the nature of the propositions of economic theory. These had, as is wellknown, been developed in discussions at LSE especially in the Robbins Seminar over which Robbins and Hayek jointly presided in 1931-6. By following Pareto in using the technique of indifference curves rather than marginal utility theory to explain consumer behaviour, Hicks and Allen eliminated the need to rely on the unmeasurable concept of utility. This was very convenient for Robbins's methodological standpoint since the marginal utility theory had been a major issue in the methodological controversies. The Hicks-Allen approach was also very fruitful in resolving a host of knotty problems about competing and complementary goods.<sup>29</sup> So Robbins could begin his discussion of the 'nature of economic generalizations' boldly.

'It does not require much knowledge of modern economic analysis to realise that the foundation of the theory of value is the assumption that the different things that the individual wants to do have a different importance to him, and can be arranged therefore in a certain order. This notion can be expressed in various ways and with varying degrees of precision, from the simple want systems of Menger and the early Austrians to the more refined scales of relative valuations of Wicksteed and Schönfeld and the indifference systems of Pareto and Messrs Hicks and Allen. But in the last analysis it reduces to this, that we can judge whether different possible experiences are of equivalent or greater or lesser importance to us. From this

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28. An Essay on the Nature and Significance of Economic Science 2nd edition pp 112n, 77n and 91n; ms draft letter to Robinson, not dated, N & S Letters file, N & S Box, Robbins Papers. Rosenstein's article, 'The role of time in economic theory', Economica new series vol 1 no 1 (February 1934), pp 77-97, is referred to on page 102.

29. J.R. Hicks and G.D. Allen, 'A reconsideration of the theory of value', Economica new series vol 1 no 1 (February 1934) 52-76 and vol 1 no 2 (May 1934) 196-219; J.R. Hicks, Preface to 1st edition of Value and Capital (Oxford: Clarendon Press, 1939); Susan Howson, The Robbins Seminar, HES meetings, Tacoma WA, June 2005.

elementary fact of experience we can derive the idea of the substitutability of different goods, of the demand for one good in terms of another, of an equilibrium distribution of goods between different uses, of equilibrium of exchange and of the formation of prices.'

(Essay, 2nd edition, page 75)

In the theory of production, since 'the Law of Diminishing Returns is simply one way of putting the obvious fact that different factors of production are imperfect substitutes for one another', this law followed from the assumption that there is more than one class of scarce factors of production (pp 76-7).

Furthermore, he went on to state strongly (pp 78-9):

'The main postulate of the theory of value is the fact that individuals can arrange their preferences in an order, and in fact do so. The main postulate of the theory of production is the fact that there are more than one factor of production. The main postulate of the theory of dynamics is the fact that we are not certain regarding future scarcities. These are not postulates the existence of whose counterpart in reality admits of extensive dispute once their nature is fully realised. We do not need controlled experiments to establish their validity: they are so much the stuff of our everyday experience that they have only to be stated to be recognised as obvious.'

'No one,' he claimed (page 81), 'will really question the universal applicability of such assumption as the existence of scales of relative valuation, or of different factors of production, or of different degrees of uncertainty regarding the future, even though there may be room for dispute as to the best mode of describing their exact logical status.'

'In the light of all that has been said the nature of economic analysis should now be plain. It consists of deductions from a series of postulates, the chief of which are almost universal facts of experience present whenever human activity has an economic aspect, the rest being assumptions of a more limited nature based upon the general features of particular situations or types of situations which the theory is to be used to explain.'(pp 99-100)

So far so Misesian. But he also wrote, still in chapter four (page 94), 'The purpose of these assumptions [of rationality in one sense or another] is not to foster the belief that the world of reality corresponds to the constructions in which they figure, but rather to enable us to study, in isolation, tendencies which, in the world of reality, operate only in conjunction with many others, and then, by contrast as much as by comparison, to turn back to apply the knowledge thus gained to the explanations of more complicated situations. In this respect, at least, the procedure of pure economics has its counterpart in the procedure of all physical sciences which have gone beyond the stage of collection and classification.' And he began chapter five with the categorical statement (page 104): 'It is a characteristic of scientific generalisations that they refer to reality. Whether they are cast in hypothetical or categorical form, they are distinguished from the propositions of pure logic and mathematics by the fact that in some sense their reference is to that which exists, or that which may exist, rather than to purely formal relations. 'In this respect ... the propositions of Economics are on all fours with the proposition[s] of all other sciences.'

It seems to me that Robbins was inclined like Kaufmann to waver between a priorism and conventionalism, also that he was fudging the issue (as he came close to admitting in the unpublished appendix).

When Haberler read the second edition of the Essay he was quite critical of the revised methodological chapters. He wrote Robbins:

I have studied the book very carefully and like it more and more the more I read it. I am still unconvinced by what you say about the logical nature of economic generalisations, but it seems to me that the point of disagreement is pushed back so far as to be of practically no importance in the practice of theory. Your emphasis of the logical difference of economic laws which are apodictic and absolutely safe on the one hand, and sociological laws which are always a little vague and uncertain, seems to me overdone. I still believe that there is only a difference of degree not only between sociological and economic laws but, between various economic laws. Instead of trying to show that economic laws are not of the apodictic nature, I want to draw your attention to the fact that it is easy to make

the sociological laws just as exact - and (in my opinion) meaningless - as you want to make economic laws.'

(Haberler gave an example which I skip over.)

'You quote somewhere as a striking example for the usefulness of the "deductive" method the derivation of the law of diminishing returns from the fact that land is not a free good. ... Here again it can be shown that the deduction becomes absolutely exact only at the expense of becoming meaningless. The deduction holds only, if you assume rational behaviour. ... I think it can be shown that you have to define rationality in such a manner that it contains the law, which you later derive from it.'

He ended by referring Robbins to two Vienna Circle pamphlets, one by Rudolf Carnap the other by Hans Hahn.<sup>30</sup>

Mises was critical too, though for the opposite reason. He was apparently more critical of (the methodological chapters of) the second edition of the Essay than he had been of the first. After making several detailed criticisms of Robbins's terminology, especially in relation to 'inner experience', he went on:

'Actually our arguments are not different and I count you ... among those who accept a priori experiential knowledge. I believe that you oppose to the open acknowledgment of this position only an infinitely small remainder of a former metaphysical prejudice, which you resist energetically and with excellent reasoning on every point - for example, in the comments about the standard of value, about the comparability of judgments of different subjects, and in those about the standpoint of historicism and the representation of statistical laws. For it is nothing other than metaphysics, if like for example, Simiand, Schumpeter, recently Felix Kaufmann, the science of human exchange will look like a kind of physics. Because this metaphysics befogs so many minds I hold it dangerous to employ a means of expression that does not allow the essence of character to come out strongly and can easily be misunderstood.

'That, what one today calls modern logic, the works of Russell, Whitehead, Schlick, Carnap etc. is still biased to, like the work of the old logicians, only to physics and at best adjusted to biology. Of history and above all

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30. Haberler to Robbins, undated but January 1936, Correspondence file, Robbins Papers.

economics they know nothing. If only it would not be that! But they are full of disdain for all that the economists practise, and as socialists full of resentment against the findings of economic science. One could sometimes believe that the overcompensation of some inferiority complex is involved. It is a pity that talented men like Kaufmann and Rosenstein let themselves be influenced by this.<sup>31</sup>

But this attempt to detach Robbins from the younger Austrian economists, influenced by the logical positivism of the Vienna Circle, serves, in my view, to show the difference between Robbins and Mises. Robbins may have wanted to follow Mises's lead in adopting an *a priorist* interpretation of economics, but he was sensitive to the logical positivists because, unlike Mises and like his friends Haberler and Machlup, he wanted economics to be an empirical science.

(Which brings me back to the beginning: to Robbins's initial concerns about demarcation, about differentiating between economics which was scientific and political science which was not.)

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31. Mises to Robbins, 10 December 1935 (my translation), N & S Letters file, Robbins Papers.

