

**Robbins' *Nature and Significance*
and
the M²T Seminar**

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1. Introduction

In the late 1950s, a group of young economists at the London School of Economics got together and established the Methodology, Measurement and Testing Seminar (later abbreviated to the M²T Seminar). The objective of the work carried out in the seminar was to provide an alternative methodology to that put forward by Lionel Robbins in the *Nature & Significance* and, having established the new methodology, to use it to test economic theories. One of the founders, Dick Lipsey, later revealed that he had been unhappy with Robbins' methodology since his days as an undergraduate at the University of British Columbia in the late 1940s:

Most influential of all the books I read in that course [on the history of economic thought] was Lionel Robbins' *An essay on the nature and significance of economic science* (1932). Coming to economics as a renegade scientist, I was always interested in methodology: how could anyone really establish natural laws about something so complex as human behaviour? Robbins said many wise things from which I profited greatly, but when I came to his chapter on economic statistics, I balked. There I read for the first time the methodology of the Austrian school, which was, as I later learned from Mark Blaug, also the methodology of many of the classical economists. According to this methodology, which is Euclidean in conception, investigators first make assumptions that are intuitively self-evident, then apply the rules of logic to deduce propositions that may not be self-evident. In economics, the trick was to establish assumptions that really were self-evident, standing the test of introspection. Since the assumptions are obviously correct, the deductions must also be correct, no matter how unobvious they may be. If the facts appear to disagree with the deductions of theories, then the facts must be wrong; the deductions cannot be wrong – providing only that they are logically correct deductions – since they are based on assumptions that we know to be correct through introspection. In short, facts are used to illustrate theories but not to test them.

I read and reread the chapter. 'This cannot be right', I said to myself, 'facts based on careful empirical observation must play a more important part in the development of our understanding of the economy than as mere illustrations to be cast aside whenever they disagree with the prevailing theory.' (Lipsey, 2000, pp. 112-1130)

The founding of the M²T Seminar and the methodological debate was analysed extensively in de Marchi (1988) and I do not propose to duplicate that material here. Rather I wish to explore what the 'chapter on economic statistics' has to say about Robbins attitude towards empirical studies and then consider the ways in which the M²T Seminar responded to the empirical aspects of the *Nature & Significance*.

2. Robbins and Applied Economics: 'Quantitative Economics' versus 'Realistic Studies'

Robbins on Problems of Estimation:

While there are some criticisms of induction earlier in the text, the main critique of empirical studies in the *Nature & Significance* comes in Chapter V, Economic Generalisations and Reality.¹ The discussion here will consider first Robbins views on problems of estimation and then the role of empirical work in ‘testing’ theories.

Robbins raises the question of estimation and asks “Ought we not to wish to be in a position to give numerical values to the scales of valuation, to establish quantitative laws of demand and supply?” (p. 107). His response is negative:

No doubt such knowledge would be useful. But a moment’s reflection should make it plain that we are here entering upon a field of investigation *where there is no reason to suppose that uniformities are to be discovered*. The “causes” which bring it about that the ultimate valuations prevailing at any moment are what they are, are heterogeneous in nature: there is no ground for supposing that the resultant effects should exhibit significant uniformity over time and space. No doubt there is a sense in which it can be argued that every random sample of the universe is the result of determinate causes. But there is no reason to suppose that the study of a random sample of random samples is likely to yield generalisations of any significance. (p. 107)

A simple illustration should make this quite clear. Let us take the demand for herrings. Suppose we are confronted with an order fixing the price of herrings at a point below the price hitherto ruling in the market. Suppose we were in a position to say, “According to the researches of Blank (1907-1908) the elasticity of demand for the common herring (*Clupea harengus*) is 1.3; the present price-fixing order therefore may be expected to leave an excess of demand over supply of two million barrels”. How pleasant it would be to be able to say things like this! How flattering to our usually somewhat damaged self-esteem *vis-à-vis* the natural scientists! How impressive to big business! How persuasive to the general public!

But can we hope to attain such an enviable position? Let us assume that in 1907-1908 Blank succeeded in ascertaining that, with a given price change in that year, the elasticity of demand was 1.3. Rough computations of this sort are not really very difficult and may have considerable utility for certain purposes. But what reason is there to suppose that he was unearthing a constant law? No doubt the herring meets certain physiological needs which are capable of fairly accurate description, although it is by no means the only food capable of meeting these needs. The demand for herring, however, is not a simple derivative of needs. It is, as it were, a function of a great many apparently independent variables. It is a function of fashion; and by fashion is meant something more than the ephemeral results of an Eat British Herrings campaign; the demand for herrings might be substantially changed by a change in the theological views of the economic subjects entering the market. It is a function of the availability of other foods. It is a function of the quantity and quality of

¹ I have worked with Robbins (1935), the Revised and Extended version of the *Nature & Significance*.

the population. It is a function of the distribution of income within the community and of changes in the volume of money. Transport changes will alter the area of demand for herrings. Discoveries in the art of cooking may change their relative desirability. Is it possible reasonably to suppose that coefficients derived from the observations of a particular herring market at a particular time and place have any *permanent* significance—save as Economic History?

Now, of course, by the aid of various devices it is possible to extend the area of observations over periods of time. Instead of observing the market for herrings for a few days, statistics of price changes and changes in supply and demand² may be collected over a period of years and by judicious “doctoring” for seasonal movements, population change, and so on, be used to deduce a figure representing average elasticity over the period. And within limits such computations have their uses. They are a convenient way of describing certain forces operative during that period of history. ... If we wanted to be helpful about herrings we should never dream of relying on the researches of the wretched Blank who was working in 1907-8. We should work the whole thing out afresh on the basis of more recent data. (pp. 107-109)

I suspect that most readers would agree that if more recent data were available, it would be best to re-estimate the elasticity and not rely on a study of one year from over twenty years earlier. Thus the wretched Blank is something of a red herring and might well feel ‘wretched’ as a result of Robbins taking his results out of context.

Lipsey (1963, p. 159) criticises this argument against empirical estimation as follows:

The above argument runs somewhat as follows: ‘I can think of no reasons why the relationship in question (e.g., the relation between demand and price) should be a stable one; I can think of several reasons why it should not be stable; I conclude, therefore, that in the real world the relationship will not be stable, and attempts to *observe* whether or not it is stable can be ruled out on *a priori* grounds as a waste of time.’ This argument is rather a curious one, and it appears although it may not have been the author’s intention, that it could have been used to stop at an early stage the investigations which produced *observations* of practically every stable relation that we know.³

Quantitative Economics Bad:

Having disposed of the estimation of elementary concepts as demand and supply functions, Robbins goes on to consider empirical studies of more complex phenomena:

If it is true of attempts to provide definite quantitative values for such elementary concepts as demand and supply functions, how much more does it

² Robbins does not explain how we are to collect data on changes in demand. Whether he was aware of the Identification Problem or not is unclear, though E.J. Working had published his article in 1927.

³ Lipsey then shows that this method of argument could be used to demonstrate that the laws of gravity, Boyle’s Law and the normal curve of error are all *a priori* impossible.

apply to attempts to provide “concrete” laws of the movement of more complex phenomena, price fluctuations, cost dispersions, business cycles and the like. In the last ten years there has been a great multiplication of this sort of thing under the name of Institutionalism, “Quantitative Economics”, “Dynamic Economics”, and what not; yet most of the investigations involved have been doomed to futility from the outset and might just as well never have been undertaken. The theory of probability on which modern mathematical statistics is based affords no justification for averaging where conditions are obviously not such as to warrant the belief that homogeneous causes of different kinds are operating. Yet this is the normal procedure of much of the work of this kind. The correlation of trends subject to influences of the most diverse character is scrutinised for “quantitative laws”. Averages are taken of phenomena occurring under the most heterogeneous circumstances of time and space, and the result is expected to have significance. (p. 112)⁴

The discredit of the Historical School in Germany is very largely due to the failure of its members to understand the currency disturbances of the War and the post-War period. It is not improbable that the utter failure of “Quantitative Economics” to understand or predict the great depression may be followed by a similar revulsion. It would certainly be difficult to imagine a more complete or more conspicuous exposure. (p.115, fn 1)

Realistic Studies Good:

Beyond citing Mitchell and the Historical School of Germany, there are no further examples of “Quantitative Economics” cited before Robbins turns to write more positively about “realistic studies”.

But what, then, are we to say of the more detailed kind of realistic studies? Having ascertained the persistence of the fact of scarcity, the multiplicity of factors of production, ignorance of the future, and the other qualitative postulates of his theory, is the economist then excused from the obligation of maintaining further contact with reality?

The answer is most decidedly in the negative. And the negative answer is implicit in the practice of all those economists who, since Adam Smith and Cantillon, have contributed most to the development of Economic Science. It has never been the case that the exponents of the so-called orthodox tradition have frowned upon realistic studies. (p. 115)

⁴ At this point, Robbins launched into a fierce attack on Wesley Mitchell’s *Business Cycles*. Having praised the book for its “magnificent collection of data”, he criticised Mitchell for attempting to derive an average length for the business cycle by combining data across seventeen countries. He concludes his censure with: “Certainly he has provided the most mordent comment on the methodology of “Quantitative Economics” that any of its critics could possibly wish.” (pp. 112-3). In view of Koopmans criticism of Geoffrey Moore, Mitchell’s successor at the NBER, (Koopmans 1947), Robbins might be seen as an early critic of ‘Measurement without Theory’. Incidentally, Robbins had already had a go at Mitchell in Robbins (1929c), a review of a book by Josiah Stamp, in which he wrote “We might protest that Sir Josiah gives too much countenance to the pseudo-novelties of Dr. Wesley Mitchell and the institutionalists.” (p.250).

There is no formal definition of what constitutes a “realistic study”, but Robbins considers what may be expected of such studies under three headings:

The first and most obvious is the provision of a check on the applicability to given situations of different types of theoretical constructions. As we have seen already, the *validity* of a particular theory is a matter of its logical derivation from the general assumptions which it makes. But its *applicability* to a given situation depends upon the extent to which its concepts actually reflect the forces operating in that situation. (pp. 116-7)⁵

Secondly, and closely connected with this first function of realistic studies, we may expect the suggestion of those auxiliary postulates whose part in the structure of analysis was discussed in the last chapter. By inspection of different fields of economic activity we may expect to discover types of the configuration of the data suitable for further analytical study. (p. 118)

And, thirdly, we may expect of realistic studies, not merely a knowledge of the application of particular theories, and the assumptions which make them appropriate to particular situations, but also the exposure of areas where pure theory needs to be reformulated and extended. They bring to light new problems. (p. 118)

However, these studies have their limitations and must be held in a proper relationship to theory:

Realistic studies may suggest the problem to be solved. They may test the range of applicability of the answer when it is forthcoming. They may suggest assumptions for further theoretical elaboration. But it is theory and theory alone which is capable of supplying the solution. Any attempt to reverse the relationship must lead inevitably to the nirvana of purposeless observation and record. (p. 120)

This supports the view of Robbins as an early critic of ‘Measurement without Theory’.

Robbins’ choice of Viner (1924) as an example of a good ‘realistic study’ is interesting, as Viner’s methodology was very different to his own and made a strong case for the role of induction in economics.⁶ In Viner (1917), he argued that “... the abstract economists exaggerate the possibility of obtaining a vast deal of knowledge

⁵ Although Robbins does not provide a formal definition of what he means by “realistic studies”, in a footnote at the end of this section (p. 118, fn 1) he writes: “Professor Jacob Viner’s *Canadian Balance of International Indebtedness* and Professor Tausig’s *International Trade* provide classical examples of this kind of investigation.” These two studies will be discussed later in this section of the paper.

⁶ See Hutchinson (1994) for a detailed analysis of Viner’s views on methodology.

from a system of deductions derived from an initial set of four or five propositions.”

(p. 235). Later in the article he argues that:

Political economy has been too often described as if it were merely a ‘pure’ or a priori psychological theory of value and distribution. Of much greater importance to the economist than any ‘pure’ theory is the knowledge and understanding of the concrete facts of production, distribution, consumption, of the whole economic situation with all its causal processes. (p. 251)

To support the case for induction, he notes that:

Even those economists who were most decided in their contention that the abstract deductive method was the only one available to the economist made considerable use of these inductive methods in their economic researches. In some cases their chief contributions to political economy were predominantly inductive in character. (p. 253)

The sub-title of Viner (1924), which was omitted in Robbins’ footnote, is *An Inductive Study in the Theory of International Trade* and Viner states at the outset his views on induction and deduction:

The deductive method in economics, when its general psychological assumptions have not been too much divorced from the true psychology of the market-place, and when the generalizations concerning the environmental data which are used as premises have also been reasonably accurate, has brought valuable results. Deductive conclusions would differ, perhaps, from the results obtained by an inductive investigation of the same problem with a complete record of facts to work from; but they would differ only because they were incomplete. The differences would tend to disappear as the deductive results were supplemented by the results of inductive analysis resting on inference from the facts omitted in the fundamental abstractions of the deductive study. This assumes, of course, that the abstractions of a valid deductive theory are not inconsistent with the facts. They should be abstractions not in the sense that they are untrue, but in the sense that they do not tell the whole truth.

In the field of the theory of international trade, as in all other fields in the social sciences, there are aspects which in practice can be investigated by only one of the methods; and there are other aspects in which both methods can be more or less completely applied, and the results of the one corroborated or discredited by the results of the other. In developing a complete theory both methods must be used; and the utilization of the one method as a means of verifying the other is made possible, not only in that portion of the field to which both can be applied but practically throughout the field, by a study of the success with which one part of the theory obtained by means of one method can be made to fit in with the other portions obtained by the other method or by both together, so as to form a complete and consistent system satisfactory to the reasoning intelligence. (pp. 7-8)

However, in operation, Viner's approach may be seen as 'induction in the service of theory verification'. Thus, having outlined the theories of earlier writers in Chapter IX, Changes in Relative Price Levels and the Adjustment of Balances of Indebtedness, he concludes:

The part played by gold movements in the adjustment of the Canadian balance of trade to Canadian borrowings from abroad has already been submitted to an inductive examination, whose results showed that, if allowance is made for the controlling influence exercised on gold movements by the peculiar system of outside reserves of the Canadian banks, gold movements operated in the manner indicated by Thornton, Mill, and their followers. The next chapter is devoted to an inductive analysis of the part played by price changes in the mechanism of adjustment of the Canadian balance of trade. It should provide a further test of the validity of the deductive theory as expounded Thornton and Mill, and it should, moreover, serve to verify the amplification of the theory made by Professor Tausig with reference to the operation of the sectional price levels. (pp. 211-2)

Tausig (1929) is also concerned with 'induction in the service of the confirmation of economic theory':

The pure theory of international trade constitutes only the initial stage toward the ascertainment of the things we wish in the end to know. Such indeed is the case with the whole of the pure theory of economics, which can be called "the" theory rather than "a" theory, solely on the ground that no other has been put forward which is generalized, consistent, intellectually satisfactory. After all, what we wish to attain is not a neat logical structure, but an understanding of the actualities. We must inquire whether the facts conform to the elaborated theorems; must make sure that nothing has been forgotten in the premises, nothing has been erroneous in the reasoning. It is incumbent on the economist to follow a procedure similar to that used in the natural sciences. The physicist or biologist who believes that he has hit on a generalization which conforms to the regularities of the external world uses it merely as a working hypothesis. The economist should do the same for his hypotheses. In economics this task is more difficult than in most natural sciences, because the economist is debarred from the method which had proved in them by far the most serviceable, that of experiment. He cannot experiment; he can resort to observation only. Observation, however, he must utilize to the utmost—thru history, description, statistics. In so doing he may or may not find confirmation of his hypotheses. Quite probably he will find partial confirmation only; he will have occasion, to a greater or lesser extent, for revision, amendment, restatement. (pp. vi-vii).

In summary, while Viner and Tausig are less deferential towards pure theory than is Robbins, both start from economic theory and use induction to 'verify' or 'confirm'

these theories. Neither could be accused of ‘measurement without theory’, unlike Professor Mitchell.

Robbins Training and Knowledge of Mathematics and Statistics:

It is possible that among the reasons why Robbins’ response was *a priori* rather than analytical one may include his training and research experience.⁷ As far as mathematics is concerned, in the first year of the B.Sc.(Econ) degree that Robbins attended at LSE, students had to take either Mathematics or Logic and Scientific Method. Howson (2004, p. 416) notes: “Robbins chose the last and took the course offered year after year (1905 to 1941) by Dr. Abraham Wolf, The course covered both formal logic and inductive logic or scientific method. ... In his second year Robbins attended, by choice, Wolf’s lectures on general psychology and on the history of philosophy. His notes of the latter show he stayed the course up to Kant.”. However, in his autobiography (Robbins 1971) he writes of his contact with G.H. Hardy at New College in 1927-28 and some study of mathematics:

He [Hardy] did not think much of the contemporary mathematical economics: I remember that when I showed him Bowley’s *Groundwork*, he was distinctly uncomplimentary, holding the exposition to be deficient in elegance and the results lacking in depth – a quality to which he attached great significance. But he thought well of the possibilities of the subject and took pains to procure me some instruction in calculus which, although it has never led to positive contributions on my part in that section of the field, at least gave me enough understanding of the language not to feel utterly lost amid this sort of construction. He confirmed me, too, in my belief in the importance of theory as the basis of all fruitful science, however dependent on eventual empirical testing. (p. 118)

Turning to his statistical training, I obtained the following information from Susan Howson⁸:

As far as statistics is concerned, among the lectures students taking the Final examination for the BSc(Econ) in 1923 had to attend as preparation for the compulsory three papers in economics was Bowley’s course in general statistics. You can find a description of the course in the LSE Calendar for 1922-3 (page 110). As you will see the course was in two parts (Elementary Statistical

⁷ As I do not have access to the Robbins Archives, my ‘evidence’ is based on published material and not on private papers and other such material, so it is therefore extremely speculative though hopefully of some interest.

⁸ I am grateful to Sue Howson for the information quoted, which was supplied by an e-mail dated 7 November 2007.

Methods [mainly descriptive statistics] and More Advanced Statistical Methods: I think LCR only attended some of the first half.

The details of the two courses are as follows:

(a) Elementary Statistical Methods. Syllabus: Collection of data, definition and tabulation. Statistical groups; arithmetic average, mode, median, mean and quartile deviation. Statistical series in time; trend and fluctuation. Weighted averages. Index numbers. Simple methods of measuring correlation. Application to statistics of population, production, consumption, commerce, prices, wages, income and capital. The main sources of these statistics, their character and meaning.

(b) More Advanced Statistical Methods. Syllabus: Elementary mathematical treatment of variation and error, especially in their application to averages, sampling, description of groups and series and correlation, in relation to economic and social investigations. Methods of interpolation.

This confirms how little statistical theory Robbins would have obtained from this source, even if he had attended both parts of the course. I have been unable to find evidence that he undertook any further studies in statistics.⁹

In terms of research experience, upon graduation Robbins was exposed to the collection and interpretation of economic statistics, as he spent the year following his graduation in 1923 working as a research assistant to William Beveridge “to help him bring up to date for a second edition the tables and information in his well-known *Unemployment: A Problem of Industry*.” (Robbins 1971, p.96)¹⁰ In addition to updating the statistics, Robbins was able to steep himself in the trade-cycle theories of the period and in reading German newspapers two mornings a week to extract industrial information for the editor of *The Economist*. At the end of 1929 he became

⁹ However, Howson (2004, pp. 433-6) reports on correspondence between Robbins and Nathan Isaacs in May 1931 in which Robbins “mentioned, for instance, the recent findings by the French economist Jacques Rueff of a 95 percent correlation between indices of British unemployment and real wages.”, and has further discussion of Robbins interpretation of these correlations. This suggests he had some knowledge of this statistical technique.

¹⁰ Beveridge (1930) provided a generous acknowledgement of Robbins’ contribution to the new edition: “I had ... secured the services of Mr. L.C. Robbins (now Professor of Economics in the University of London) to work over the book and the new material available since 1909. He very soon reported to me his conclusion that nothing was to be gained, commensurate with the labour involved in bringing what had been written in 1908 verbally and formally up to date. ... He advised me—and convinced me—that I should reprint the original book without change”(pp. ix-x). “My indebtedness to Mr. L.C. Robbins, when he was my research assistant, for suggesting the form of this new edition, has already been acknowledged; in that capacity he did much of the work also of bringing up to date the figures in the Statistical Supplement.” (p. xi). It is a tribute to Robbins maturity that, as a young graduate, he was able to persuade the older and more experienced Beveridge of his views.

a member of the Editorial Committee of the London and Cambridge Economic Service and was involved in the detailed examination of economic data at the regular editorial meetings.¹¹ While this would have given Robbins experience at looking at economic statistics and interpreting them, his comments on the statistical analysis of the data suggest he had serious misconceptions of what was involved and what could be achieved. He seems to have assumed that the statisticians were claiming to have ‘proved the truth’ with the numerical results they produced, rather than presenting estimates that were subject to sampling errors.

Looking at the articles, books and book reviews he published up to 1935¹², one is impressed both by his productivity and the wide range of topics he covered, particularly in his book reviews. While a number of the articles involve questions of economic theory (for example, Robbins (1928a, 1930b, 1930c and 1934a)), others had a topical relevance to the economic situation at the time he was writing and these topics are treated in a very theoretical manner, with little or no reference to the relevant statistics. Thus, the discussion of the dynamics of capitalism (Robbins 1926) and the economic effects of variations of hours of labour (Robbins 1929a) is strictly theoretical. Where there is an appeal to supporting empirical evidence, the effect is minimal: for example, in discussing some arguments for protection (Robbins 1931) the sole empirical material is provided a footnote reference to a chart in another author’s work; similarly, in writing about underconsumption theories of the trade cycle (Robbins 1932b) he provided a footnote reference to a chart showing the effects of the German hyper-inflation; in discussing the probable consequences of a stationary population in Great Britain (Robbins 1929b) he quotes a calculation made by Professor Cassel concerning the rate of increase in the world’s gold supply during

¹¹ O’Brien (1988, pp. 170-8) contains a translation from the French of a talk Robbins gave in 1934 in which he describes in some detail his work with the London and Cambridge Economic Service. He returned to the attack on statistical estimation, with the wretched Dr Blank here being replaced by Professor Schultz, and his “employment of subtle statistical methods”. He continues: “If one proves that the elasticity of demand for sugar was, from 1880 to 1925, 0.85, what does that signify?” (p. 174). Professor Schultz might well have responded that he had not *proved* that the elasticity was 0.85, but that 0.85 was the point estimate and that he could provide a confidence interval around that figure with some probability of being correct.

¹² A list of his articles was obtained from AEA (1961), pp 414-5. This was expanded by reference to the Bibliography of Robbins’ publications in O’Brien (1988), pp. 219-21. I have omitted some letters to the *Economist*, Prefaces and Introductions to other writers’ books and some articles in Bank Reviews and other magazines.

the period 1850-1910 required to keep prices stable.¹³ Finally, in a discussion of the optimal theory of population (Robbins 1927a), he criticises Alexander Carr-Saunders somewhat simplistic attempts to use changes in real income per head to determine the optimality of population size and refers to “question-begging statistical analysis” (p. 129). Overall, there is a clear choice of theoretical analysis over statistical evidence in this body of work that is consistent with Robbins’ views on ‘empirical economics’ as expressed in the *Nature & Significance*.

However, Robbins analysis of the Great Depression in Robbins (1934c) shows that he could use statistical data when he deemed it necessary. This book has a 36 page Statistical Appendix and the text contains twelve charts and seven tables.¹⁴ The author provided a passionate free market explanation of the causes of the depression and marshals an impressive set of statistics to support the case. While alternative explanations are considered, they are rejected on theoretical or common sense grounds and statistical evidence that might question their validity is not presented. One might therefore argue that Robbins is presenting economic data to support or ‘verify’ his theory, rather than looking at alternative explanations and ‘testing’ his theory against them.¹⁵

To what extent did Robbins change his view of the relationship of empirical studies to theory? In Robbins (1938) he attempted to clarify a number of methodological issues and wrote:

I do not think that there is a single professional economist living who would dispute that the appropriate method of economics is the construction and development of hypotheses suggested by the study of reality and the testing of the applicability of the results by reference back to reality. (p. 346)

¹³ Given Robbins was quoting a calculation based on data for a period that ended nineteen years before his article, one might feel that there is an inconsistency here compared with the treatment allotted to the wretched Dr Blank.

¹⁴ Robbins acknowledges his debt “to Mr. Stanley Tucker, Rockefeller Research Assistant in the Department of Economics, without whose loyal and unremitting labours in the preparation of the charts and the statistical appendix publication at this stage would have been impossible.” (p. viii).

¹⁵ O’Brien (1988, p. 183, note 141) cites a number of not unfavourable reviews of the book. Robbins later expressed dissatisfaction with the book: in Robbins (1971) he compared *The Great Depression* unfavourably to his *Economic Planning and International Order* writing of the latter “Unlike *The Great Depression*, this is not a work I should now wish not to have written.”. Later in his autobiography he wrote “I had long realized that my earlier diagnosis of the causes of the Great Depression had missed the mark in not recognizing sufficiently the paramount role played by the catastrophic contraction in incomes brought about by the deflation due to the volatility of the then existing credit mechanism” (p. 188).

Although the word ‘testing’ appears here, it should be noted that it is not the ‘theory’ that is to be tested, but its ‘applicability’ and this is confirmed later in the article:

In a subject so wide as economics it is natural that there should develop some division of labour, that some should specialise on the more theoretical developments, some on description and verification. (p. 346)¹⁶

Later, in Robbins (1998), the transcription of his famous lectures on the History of Economic Thought that was made during 1979-80 and 1980-81, he is reported to have expressed a somewhat negative view of Sir William Petty’s statistical work:

Petty obviously attached very great importance to quantitative measurement. Petty subscribed to the Baconian philosophy—or thought he subscribed to the Baconian philosophy—which expressly said that if you collected enough facts they then suggest to you a series, and hence systematic science—which we know since the times of Whewell and Popper and other distinguished writers is standing scientific method on its head. You invent in science hypotheses, you test them for their logic, and then you test them—you try to falsify them or you try to verify them, it is a matter of words—by collecting relevant facts. (Robbins 1998, p. 58)

I doubt that Popper would have agreed that whether you try to falsify hypotheses or verify them is merely a matter of words.

These quotations suggest that throughout his career Robbins held to the view that the assumptions of economic theory were self-evident and that the role of statistical studies was to test their ‘applicability’; to verify their relevance in a particular context and not to ‘test’, in the sense of ‘falsify’ the theories themselves.

3. The M2T Seminar: Methodological and Empirical Studies¹⁷

The young economists who were seeking to reject Robbins’ methodology needed an alternative and they found their inspiration in the work of Karl Popper. Important here

¹⁶ In this article, a new character appears to replace the wretched Dr Blank: “We should all agree that the mythical Schmoller student, who, after five hundred pages of statistical investigation, decided that the price of pork in the Eastern District of Berlin in the years 1895-1900 was “determined by supply and demand”, had been wasting his time.” (p. 349)

¹⁷ I was an undergraduate at LSE from 1956 to 1960, studying for the B.Sc.(Econ) and specialising in Computational Methods in the Statistics Department. I took the compulsory introductory courses in Microeconomics and Macroeconomics and a further compulsory course in Applied Economics in Part I of the degree and then specialised in Mathematics and Statistics in my Final Year. Upon graduating in June 1960 with First Class Honours, I applied for and obtained an Assistant Lectureship in Economics at LSE and began teaching in October 1960. I was immediately invited to join the M²T Seminar and I remember that in the early sessions we discussed Dick Lipsey’s work on the Phillips Curve.

was the link between Kurt Klappholz in the Economics Department and Joseph Agassi, one of Popper's younger colleagues in the Philosophy Department.¹⁸ Initially, a group of the economists met with Agassi, who expounded Popper's methodology to them.¹⁹ Klappholz and Agassi (1959), while ostensibly a review article criticising Schoeffler (1955) and Papandreou (1958) from a Popperian standpoint, began with a detailed critique of Robbins' methodological position.²⁰

Even though Robbins' aim was "not to discover how Economics should be pursued" (p. 72), it is clear that methodological prescription was prominent in his *Essay*. We note, in particular, his suggestion that there should be an *a priori*, water-tight, separation between economics and other sciences. This amounted to the *a priori* decision to view certain variables of economics (e.g. tastes and technology) as essentially "exogenous", i.e. as not determined within economic models, rather than as not yet explained by any existing economic model. Robbins did not, of course, object to attempts to explain "tastes" psychologically or sociologically, but according to his view no explanation of them should contain (endogenous) economic variables. Secondly, he denied a priori the possibility of discovering "quantitative" laws in economics.

We find these suggestions unacceptable because they are designed to limit the field of argument a priori. As a hypothesis the view that tastes are independent of other economic factors could be subject to critical discussion; it could be countered by an alternative hypothesis describing conceivable relations between income, prices, tastes, etc. Robbins apparently did not advance his view that tastes are exogenous as a hypothesis, but rather as a methodological rule designed to delineate the area of economic discussion. The rule, in its turn, was based on more general considerations—on his general view of economics as a science.

Robbins regarded scientific laws as universal statement known with certainty to be true; laws were statements of the necessary aspects of natural phenomena. ... How was knowledge of these laws obtained? It is derivable neither from history nor from controlled experiments, but rather obtained by a process of "deduction from a series of postulates. And the chief of these postulates are all assumptions involving in some way simple and indisputable facts of experience. ... They are so much the stuff of everyday experience that they have only to be stated to be recognised as obvious" (pp. 78, 79).

Everyday experience might perhaps tell us that tastes are exogenous, but it certainly cannot tell us that tastes, or any other factor, must be regarded as

¹⁸ Klappholz had a strong interest in philosophy and was involved in teaching in the B.A. Honours in Philosophy and Economics degree, which commenced in 1958-59.

¹⁹ By the time I joined the M²T Seminar in 1960, Chris Archibald was keeping a detailed record of the discussion at the seminar for distribution to the participants. I do not know whether he began this process at the beginning of these meetings, as the seminar archives seem to have completely disappeared over time. For many years I kept my papers from the M²T Seminar post-1960, but they were discarded during some office move and I confess that I have forgotten most of the detail of the seminars I attended. As a result, I shall concentrate on giving something of the flavour of the seminar, rather than a detailed history. For a fuller discussion, see de Marchi (1988).

²⁰ They also criticised Terrance Hutchinson, who in Hutchinson (1938) had criticised the *Nature & Significance* from a Popperian point of view. This led to an exchange of views, see Hutchinson (1960) and Klappholz and Agassi (1960).

exogenous in all future theories. The view, however, that economic laws are certain and must be based on everyday experience, does somehow entail that some factor or other must be exogenous. (pp. 60-1)

To carry out the programme in Methodology, Measurement and Testing, it was necessary to examine models to derive predictions to be tested and it was necessary for these predictions to be tested. While Popper had a strong influence on the methodology of the M²T Seminar, the interests of the participants differed, as did their responses; some were mainly interested in a methodological investigation of economic theories, while others were more concerned with the process of testing.

Methodological Studies:

Considering the methodological studies concerned with establishing whether economic models generated testable hypotheses, two are of particular interest:²¹

Archibald versus Chicago:

One of the most ambitious methodological exercises was presented by Christopher Archibald in Archibald (1962) in an analysis of the reaction of Chicago economists to Chamberlin's theory of Monopolistic Competition (Chamberlin 1933). He pointed out that whereas Friedman (1953) had argued that theories should be judged by their predictions and not their assumptions, Chicago economists had criticised Chamberlin on the basis of his assumptions, rather than his predictions. After a considerable amount of mathematical analysis of the comparative statics of the Chamberlin model he concluded that:

It is obviously extremely difficult to prove that a model is empty, and, indeed, this model is not completely empty: all I can claim is that it yields, so far as I can discover, no *qualitative comparative static* predictions, and that this is the consequence of a general defect, the incomplete specification of the demand relationship within the group. (p. 19, italics in the original)

He concluded with a criticism of the Chicago approach to Chamberlin:

Perhaps the most serious criticism of Chamberlin's critics is that they have concentrated upon *a priori* discussion of his assumptions, instead of on

²¹ A third study worth mentioning was Klappholz and Mishan (1962), in which the authors argued that it was not possible to derive testable hypotheses from identities, as some macroeconomic modellers seemed to think was.

discovering what facts were needed to give the theory content, and endeavouring to obtain them so that they might test it. (p. 21)²²

The outcome of Archibald's work here and in his examination of the predictions of Marginal Productivity Theory (Archibald 1960) was that the problem of determining the mathematical signs of the second order partial derivatives involved in comparative static analysis made it difficult to obtain predictions from the models that he had examined.

Lancaster and Qualitative Predictions:

The second study was carried out by Kevin Lancaster, the mathematician in the group²³ and in Lancaster (1962) he carried out a theoretical analysis of the possibility of predicting the signs of the dependent variables in a system of simultaneous equations from a knowledge of the signs of the coefficients in the system. He was concerned with the basic question of whether comparative static analysis could yield predictions that were testable. He showed that if the signs of the coefficients were arranged into a matrix, it was possible to derive mathematical theorems to generate numerical counting rules that would determine the necessary and sufficient conditions for whether the system does yield predictions. Lancaster's results were based on only the functional form of the equations in the model and did not depend on algebraic details, such as whether the equations were in log or linear form, so they were completely general.²⁴ In the context of the aims of the M²T Seminar, this was an important paper, as it provided the theoretical filter for assessing whether the results of comparative static analysis could be tested. That it has not had a long term influence on economic methodology is, I suspect, due to two factors. First, many economic theorists are not interested in testing the predictions of their theories and so have no need to carry out this analysis. Secondly, the introduction of time lags into econometric models made macroeconomic models dynamic and reduced the interest in comparative static analysis.

²² The response from Chicago was brief and somewhat dismissive of Archibald's efforts (see Stigler 1963, Friedman 1963 and Archibald 1963 for his response).

²³ He had a BS in Mathematics and Geology from the University of Sydney.

²⁴ One sign of the times of this paper is that the models chosen to illustrate the theoretical results were based on 'Keynes and the Classics' debate that predated the 'Keynesians *versus* Monetarists' that was to follow.

Empirical Studies:

Turning now to the empirical studies, it is necessary to put them into context in terms of the state of statistical teaching and applied research in the late 1950s. In view of its later emergence as the leading centre for econometrics in the United Kingdom, it is important to note that econometrics had not reached the Economics Department at LSE by the time of the M²T Seminar. It is true that Roy Allen had taught a course entitled 'Problems in Econometrics' in the sessions 1946-47 and 1947-48, the course being listed in the LSE Calendar under 'General Economic Theory' and was 'Recommended for postgraduate students'.²⁵ However, there were very few postgraduates, compared with the number of undergraduates, and no taught Masters degrees, as both PhD and MSc students were examined by thesis. Allen's course was not taught after 1948.

For undergraduates, a course 'Introduction to Econometrics' appeared in the 1951-52 session, but it was taught in the Statistics Department and offered as a course primarily for students in that department who chose the option of Economic Statistics within the Special Subject of Statistics.²⁶ It was open as an option for students in other Special subjects, but it is doubtful if many economics students attended. In the first year it was taught by a theoretical statistician, Geoff Penrice, but when he left at the end of the year, it was taken over by Harold Booker, whose interests were in national income accounting and the sources of economic statistics, and George Morton, who taught game theory and linear programming. Although the course was expanded from 10 to 30 lectures and the syllabus remained the same, the econometric content was significantly reduced. In 1953-54, as there was no econometrician at LSE, Wilfred

²⁵ The syllabus was: "An account will be given of the work of Tinbergen and Frisch on econometric business cycle research and of the work of Leontief on import and output relations in the economic system. The emphasis will be as much on the statistical methods used as on the economic implications of the results." (LSE Calendar 1946-47, p. 170.) This was probably the first formal course in econometrics taught in the UK.

²⁶ The syllabus was: "Scope of econometrics. Derivation of Supply and Demand curves by regression analysis and simultaneous probability equations. Production and Consumption functions. Problems of identification and aggregation. Connection between micro-economic theory and macro-economic models. Problems of obtaining suitable statistical data." (LSE Calendar, 1951-52, p. 353).

Corlett from UCL was brought in to beef up the econometric content²⁷, although economic data sources, game theory and linear programme continued to figure prominently in the course. The course continued in this format into the early 1960s.²⁸

For students in the Economics Department, there was no course in statistical theory that would give an exposure to estimation and testing. Instead, there were courses in ‘Applied Economics’, which involved taking economic problems, developing the relevant theory and then illustrating the application of the theory to the problem by looking at economic data in the form of tables and charts.²⁹ In this context, the idea of testing theories was both novel and being undertaken by economists who had little formal training in what they were doing.

Lipsey and the Phillips Curve:

One early study was Lipsey (1960), a further study of the Phillips Curve that Bill Phillips suggested he carry out. Lipsey (1997, p.ix) recalled. “This I did and spent a year working out regressions (at least my research assistant, June Wickens, did on a mechanical calculator at the rate of about two a day) and trying to understand the curve in terms of microeconomic theory.” (p. ix).³⁰ Lipsey’s paper is interesting from several points of view. First, he showed that by running linear regressions on the full data set with non-linear functions of the rate of unemployment, such as $(1/U)$, $(1/U^2)$ and $(1/U^4)$, it was possible to replicate very closely the results that Phillips had obtained through a very idiosyncratic process of averaging the data and fitting the curve by eye. The fact that a Phillips Curve could now be estimated using standard multiple regression analysis opened up the economics profession to the development of a ‘Phillips Curve Industry’, which generated estimates of the Curve for any country where the relevant data existed.

The second interesting feature of the paper was that, whereas the original Phillips paper had not provided any theory to explain the existence of the relationship,

²⁷ His publications are listed in the References.

²⁸ For further discussion of the econometric situation at LSE at this time, see Gilbert (1989).

²⁹ The nature of these courses may be judged from Phelps Brown (1951), which was the recommended textbook for such a course.

³⁰ June Wickens, a talented mistress of the Doolittle Technique, was the legionary research assistant in the Economics Department who was responsible for producing most of the regression analysis involved in early econometric work at LSE. Regression analysis in the Economics Department almost ground to a halt when she married a graduate student, A.G. (Bertie) Hines, and moved to the University of Bristol.

Lipsey devoted considerable space to developing a theoretical model and attempted to explain the cyclical ‘loops’ in the observations around the fitted curve. The criticism of ‘measurement without theory’ was a characteristic feature of the M²T Seminar.³¹

Lipsey and Steuer testing Kaldor: Nicholas Kaldor’s response to Phillips’ original article was to suggest an alternative theory (Kaldor 1959):

... the rise in money wages depends on the *bargaining strength* of labour; and bargaining strength, in turn, is closely related to the prosperity of industry, which determines both the eagerness of labour unions to demand higher wages and the willingness and ability of employers to grant them. ...

It is when investment is high that profits are high, and it is in periods of rising total production and rising productivity that profits are rising. Such periods in turn are periods of low unemployment, and also periods of falling unemployment. (p. 137 in Lipsey and Steuer)

Kaldor did not supply any evidence to support the suggestion that the correct relationship was between wage changes and profits rather than unemployment. Despite this, Lipsey and Steuer (1961) set out to test Kaldor’s hypothesis against the Phillips Curve as “Kaldor’s counter-explanation gives us a chance to subject the Phillips theory to a serious test in which it has a real chance of being refuted. The test is therefore important from the point of view of our confidence in the Phillips theory.” (p. 139). That is, instead of merely ignoring Kaldor’s hypothesis (or challenging him to produce empirical evidence), within M²T methodology it was seen as an opportunity to ‘test’ Phillips theory.

To do this they first had to formulate Kaldor’s hypothesis in a form that could be tested, as Kaldor had not specified any particular functional form.³² Having settled this matter, regressions were run using equations that included terms representing both profits and unemployment and it was shown that the effect of unemployment dominated. This was true both using aggregate data and, in general, for disaggregated

³¹ Bernard Corry, another active member of the Seminar, recalled this emphasis on theory: “... So we were into applied work, and then any visitors that came to the school were always invited to the M²T. Quite famous Americans, that the Young Turks tried to, I wouldn’t say tear to bits, but show up methodologically. They were always presenting applied work on, I don’t know, measuring productivity growth; and then people would say, ‘What theory are you testing? All you are doing is empirical work.’” (Corry, 1997, p. 189).

³² There is no evidence in the paper that the authors contacted Kaldor to discuss this point. However, he seems to have accepted their formulation, as they conclude: “In conceding the existence of the observed relation, Kaldor has not been ungenerous.” (p. 150).

data covering ten industries. What is of interest here is that although the main theoretical work on testing Nested and Non-Nested Hypotheses did not happen until the 1980s (see Pesaran 1987), Lipsey and Steuer were applying the recommended procedure for dealing with hypotheses that can be nested in this paper. It is unfortunate that this example of testing rival theories against ones own, rather than simply ignoring them, did not receive more attention from other economists carrying out empirical studies.

A potential teaching development:

There were plans to introduce the approach of the M²T Seminar to graduate students and in the 1962-63 Session there appeared a **Course and Seminar: Case Studies in Measurement and Testing in Economics**.³³ After an introductory series of lectures that covered methodological matters, with references to Friedman (1953), Koopmans (1957) and Klappholz and Agassi (1959), there was a one-lecture idiots' guide to hypothesis testing.³⁴ A few of the case studies reflected the work of the M²T Seminar, such as Archibald (1960) and Lipsey (1960), but the rest were a mixed bunch involving a mixture of qualitative and quantitative testing. The course was repeated in 1963-64 and then dropped. Whether this course might have had a lasting impact on graduate teaching at LSE is hard to assess, as it was not continued beyond the initial offering, for reasons to be outlined below.

The decline of the M²T Seminar:

The distinctive Popperite flavour of the work of the M²T Seminar did not last much beyond the mid-1960s as a result of two factors. First, changes in the personnel in the Department of Economics produced a new attitude towards measurement and testing.

³³ The course consisted of ten lectures and seven seminars, to be taught by Bernard Corry, Dick Lipsey, Maurice Peston, Max Steuer and Jim Thomas. The syllabus was "Introduction: the place of measurement and testing in the development of economic theory and a survey of the simple statistical tools used in subsequent case studies; testing the theory of the firm; measuring demand; measuring macro-economic relations and testing macro-economic models of income and employment; testing the Cobb-Douglas production function; testing macro-economic models of distribution; testing theories of international trade." (see the LSE Calendar for 1962-63, p. 302 for details of the Recommended reading).

³⁴ This was my first ever lecture at LSE and it consisted of an attempt to give a non-technical outline of setting up the Null and Alternative Hypotheses and testing statistical theories. The material was obvious to the US graduates present, who all seemed to have taken statistics courses, but new to the British graduates, most of whom were innocent of the subject.

Secondly, there were major changes in the structure of teaching that put more focus on statistical theory and econometrics.

Exeunt (fere³⁵) Omnes:

The process of change was accelerated by the departure of many of the key founder members of the seminar. Dick Lipsey and Chris Archibald moved to the new University of Essex³⁶ and Maurice Peston to a newly formed Department of Economics at Queen Mary College, London, shortly to be followed there by Bernard Corry. Kelvin Lancaster went to Columbia University as a visitor and stayed there. There were also changes in the senior members of the Economics Department, with the semi-retirement of Lionel Robbins and his preoccupation with the work of the Committee on Higher Education (Robbins 1963). Frank Paish and Sir Arnold Plant, neither of whom had been particularly positive with respect to modelling, retired in 1965. The Robbins Seminar ceased to be the main focus for staff and graduates and more specialised seminars appeared, reflecting the division of the subject into narrower areas.³⁷ One important arrival at the LSE in 1963 was Denis Sargan to a Readership in Econometrics in the Statistics Department and his influence will be discussed below.

Changes in degrees and courses:

There were dramatic changes in the structure of both undergraduate and graduate degrees in the early 1960s:

Changes in the BSc(Econ):

Up until 1963, the structure of the BSc(Econ) (the main undergraduate degree) had been a very general two-year Part I with 8 examination papers and a specialised Part II, involving 5 examination papers. In Part II it was possible to take a course in Mathematical Economics, but only as an option along with Public Finance and most students chose the latter. The Corlett econometric course could be taken, but only as an option to the famous three-hour extended Essay that was seen by highfliers as an

³⁵ (almost)

³⁶ As the University of Essex was set up as a direct result of the recommendations of the Robbins Report (Robbins 1963), it might be argued that Robbins was partly responsible for speeding up the demise of the M²T Seminar.

³⁷ One of the earliest of these was an informal seminar set up by Roger Alford, Victoria Chick and Jim Thomas and focussing on Monetary Theory. It was commonly known as the 'Chick Shop'.

opportunity to show First Class quality, so again was not taken by many students. In 1963 a new BSc(Econ) was introduced that reversed the weighting, so that now Part I was a general programme with 5 examinations and Part II and two-year specialist course. There was still some compulsory history and politics courses in Part II, but now students had to take courses in mathematics and statistics. There was more scope for economics students to take outside options, including the Economic Statistics courses that were now taught by Denis Sargan and Bill Phillips for the specialists in the Statistics Department.

The Taught MSc:

Denis Sargan switched to a Chair in the Economics Department in 1964 and began to have a significant effect there. When a new taught MSc in Economics was introduced in 1964, it offered two options: the first (**Economics**), which was taken by most students, involved a compulsory course in basic, non-technical econometrics, while the second (**Economics and Econometrics**) offered a programme of more advanced technical courses.

The changes in both undergraduate and graduate teaching programmes meant that there was now an emphasis on nearly all students having some exposure to statistical theory or econometrics and now courses in Applied Economics tended to have more references to applied econometric work, rather than the examination of charts and tables of statistical data.

The Final Years of the M²T Seminar:

After the departures outlined above, the seminar continued for several years with Max Steuer as the Chair. Over time the nature of the seminar changed. The visitors from the United States who were invited to attend were less interested in discussing methodology and often more experienced in carrying out applied econometric studies. While it continued to look for theoretical underpinning for applied analysis, rather than mere 'empirical work' it lost its early Popperian fervour. With the disappearance of the Robbins Seminar the M²T became the main general economics seminar and continued as such for some time. However, with the growth in the number of special area seminars being developed in the Economics Department, a general seminar lost some of its appeal and the seminar finally closed.

4. Conclusions

The establishment of the M²T Seminar in the late 1950s was very much an LSE phenomenon that reflection of the lasting power of Lionel Robbins methodological position as presented in the *Nature & Significance*. His argument that deductions from self-evident assumptions did not need empirical analysis strongly discouraged an interest in statistical analysis and econometric testing. His further negative attitude to statistical estimation, which seems to have been based on a lack of knowledge of statistical theory, was extreme. To the extent that he did approve of ‘realistic’ studies, these were to ‘verify’ economic theories and not to test them.

The M²T Seminar was an LSE phenomenon in the sense that while the methodological studies represented a novel attempt to apply Popper’s methodology to testing economic models and showed how difficult it was to derive any testable predictions, even from simple economic models, the testing was limited by the statistical knowledge of the participants.³⁸ While some members of the M²T Group, such as Kevin Lancaster, were well trained in Mathematics, none of them had a serious background in Statistical Theory. The result was a degree of ‘learning by doing’ in the empirical work, which often showed considerable ingenuity, as in Lipsey and Steuer’s testing of Kaldor’s Profits Hypothesis.

We live in a time where there a few problems of data shortage, computing power is virtually unlimited and there is a vast output of applied econometric studies. A large proportion start from an equation (or set of equations) that are not derived from a formal theoretical model, but presented as being ‘plausible’ representations of common sense assumptions about what might affect the phenomenon being considered. Looking at many of these studies, one might feel some nostalgia for the days of the M²T Seminar and its attempts to deal with such ‘Measurement without Theory’.

³⁸ As well as by a shortage of time series data and a lack of serious computational power.

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