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Keynes, statistics and econometrics

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Keynes, statistics and econometrics

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"In dealing with the correspondence of Leibniz and Bernoulli, I have not been mainly influenced by the historical interest of it. The view of Leibniz, dwelling mainly on considerations of analogy, and demanding 'not so much mathematical subtlety as a precise statement of all circumstances', is, substantially, the view which will be supported in the following chapters. The desire of Bernoulli for an exact formula, which would derive from the numerical frequency of the experimental results a numerical measure of their probability, preludes the exact formulas of later and less cautious mathematicians"
(Keynes, Treatise on Probability, p. 403)

1. Introduction

Keynes played a central role in the debate on the emerging econometric methods in the late 1930s. In particular, his 1939 critique of Tinbergen's first League of Nations study is considered to have sparked off the debate about the role of econometrics (Hendry and Morgan 1995), which saw him involved in direct exchanges with some of the other leading figures of the emerging field of econometrics. After an initial phase in which his objections were constructively discussed, since the early 1940s they were substantially rejected, and his attitude towards economics considered old-fashioned.

The assessment of Keynes's criticism remains controversial, but the long prevailing view is that Keynes was an *a priori* anti-econometrician (see Samuelson 1948 and Klein 1951). Stone (1978) maintained that Keynes' review was "a model of testiness and perverseness" (p. 61) principally due to his temperamental characteristics. Since the end of the 1970s new contributions have recognised the relevance of Keynes's criticism. However, they concentrated on those remarks of his which dealt with 'technical issues' involved with applying regression (e.g., omitted variable bias,

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simultaneous equation bias, and so on). It was Patinkin (1976) who first found it "somewhat depressing to see how many of [Keynes's criticisms to the use of correlation analysis to estimate equations] are, in practice, still of relevance today" (p. 1095). Hendry (1980) wrote that "[Keynes's] objections make an excellent list of what might be called problems of the linear regression model" (p. 396). Some years later Pesaran-Smith (1985) recognised that Keynes was right on both the technical and logical arguments; and Rowley (1988) maintained that "Keynes' criticisms have been diluted, forgotten or mis-stated rather than absorbed into the prevalent orthodoxy" (p. 25). He regretted that "we have waited too long for econometric methodology to come of age and address its logical bases" (p. 30). Actually, it is in this wider context that Keynes has been considered in the 1990s. McAleer (1994) writes that "some of Keynes's criticisms of Tinbergen's pioneering econometric methodology remain relevant to this day" (p. 332) and that his implicit research program "subsequently led to the development of numerous econometric techniques that are now widely used in applied econometrics" (p. 334). Similarly Keuzenkamp (2000) maintains that Keynes's sceptical attitude remains substantially justified. In conclusion, it is recognised that Keynes's criticism of Tinbergen was sound in many points. However, it is considered too harsh and Keynes is blamed for throwing out the baby with the bath water.

This article reconstructs Keynes's reflections on the issue of the role of econometrics in the economic discourse in a time perspective longer than is usually considered in the literature. In the second section his conception of economics and the role of mathematics and statistics is presented, as a necessary premise to understanding Keynes's criticism. In the third and fourth sections we analyse respectively the Keynes-Tinbergen debate in the period 1938-1940 and the exchange between Keynes and other econometricians in the period 1939-1941. The last section provides some final remarks on Keynes's alleged anti-econometrics attitude and an interpretation of the reasons of the harshness of his criticism.

2. A preliminary note. Keynes on the nature of economics and the role of mathematics and statistics

Keynes's mature methodological conception dates from the early 1930s, when he was constructing the model of his *General Theory*. It can be traced throughout the drafts of his book, in its final text, and in other writings and in his correspondence before and after the *General Theory*. His 1938 correspondence with Harrod is particularly relevant from this point of view. In a letter of the 4th of

July, 1938, Keynes defined economics as "a branch of logic, a way of thinking" (Keynes 1973b, p. 296), and added:

"Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, *unlike the typical natural science, the material to which is applied is, in too many respects, not homogeneous through time*" (ibid. p. 297, italics added).

Changing and unstable factors like "motives, expectations and psychological uncertainties" (ibid. p. 300), which act in a context of limited knowledge of actors and structural uncertainty, make economic material, "in too many respects", "not homogeneous through time". As a consequence "economics is essentially a moral science [i.e. a human science] and not a natural science. That is to say, it employs introspection and judgement of value" (ibid). Hence, the non-homogeneity of the material through time compels economics to take the particular characteristics of the historical world into account and to use introspection and judgement of value in order to discover the relevant factors necessary for building a model. The *relevant* model does not emerge automatically out of empirical study, as if it were a result of a "blind" manipulation of data (see Keynes 1936, p. 297). The adequacy of the model depends on the economist's ability to select the relevant factors. The decision over what part of concrete reality to incorporate into a model was what Keynes termed "judgement of value". The model is the result of a continuous correction of judgement, "a mixture of intuitive selection and formal principles." Keynes emphasised that the selection of the relevant factors begins with the analysis of facts, and that economists must continuously refer to facts:

"The specialist in the manufacture of models will not be successful unless he is constantly correcting his judgement by intimate and messy acquaintance with the facts to which his model has to be applied" (letter to Harrod, 16 July 1938, Keynes 1973b, p. 300).

Keynes was not going to refuse the use of mathematics in economics *per se*. He appreciated the contributions in the field when they make it possible to illuminate economic problems -- that is when the subject matter makes its use appropriate.[2] However, according to him, mathematical generalisations have primarily an instrumental role, especially in order to "disclose gaps and imperfections in your thought" (Keynes 1936, p. 305). This is due to the particular nature of economic material, which , as a rule, makes a complete and exact generalisation not possible. "In a study so complex as economics … we cannot hope to make completely accurate generalisations",

[2] This is the case, for example, with Ramsey's economic-mathematical works. Keynes wrote that "A Mathematical Theory of Saving" (1928) was "one of the most remarkable contributions to mathematical economics ever made, both in respect of the intrinsic importance and difficulty of its subject, the power and elegance of the technical methods employed, and the *clear purity of illumination* with which the writer's mind is felt by the reader to play about its subject" (Keynes, 1928, p. 335-6, emphasis added).

he wrote in the *General Theory* (ibid., p. 247) -. As a consequence the economist's style of exposition has to be *quasi-formal*, as Keynes wrote in an early fragment of the preface of the *General Theory* (Keynes 1973a, p. 296-8), echoing Marshall's statements.

For Keynes statistics has an instrumental role in economics too: "Statistics are of fundamental importance to suggest theories, to test them and make them convincing .. and to eliminate impressionism" (Keynes 1971 [1930], vol.2, p. 366) - that is to increase their accuracy.[3] In the *General Theory* he called for a statistical examination of some key concepts like the propensity to consumption and the multiplier. Keynes himself made some preliminary attempts to verify the stability of the consumption function, using early national income data developed for the United Kingdom by Colin Clark and for the United States by Simon Kuznets. These quantitative research projects are very important in improving economic theory. The economists, as he wrote to Harrod, must not be "reluctant to soil [his] hands" (16 July1938, in Keynes 1973b, p. 300).

On the contrary, Keynes considered that prediction was not the main object of the statistician. To understand this statement, it is necessary to refer to his discussion of statistical inference in Part V of his earlier work, the *Treatise on Probability* (1921)[4]. The starting point of Keynes's analysis was the historical discussion of the theorem that permits to derive a numerical measure of probability from a numerical statistical frequency of similar events that had been observed previously -- that is to infer an exact measure of probability from observed frequency. According to him this theorem "is only valid subject to stricter qualifications … and in conditions which are the exception, not the rule" (ibid. p. 369).[5] Moreover, he emphasised, "it cannot possibly be inferred from a statement of the number of trials and the frequency of occurrence merely, that [these conditions] have been satisfied" (ibid., p. 404-5). In fact, he added, "we must know, for instance, that the examined instances are similar in the main relevant particulars, both to one another and to the unexamined instances to which we intend our conclusion to be applicable. An unanalysed statement of frequency cannot tell us this" (ibid., p. 405). Keynes's conclusion was that "the application of the mathematical methods .. to the general problem of statistical inference is invalid" (ibid. p. 419). He wrote:

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[3] Two years before, reviewing in *Economic Journal* (1928) an important NBER publication by Frederick Mills on the behaviour of prices in United States over the period 1890-1925, Keynes defined this volume as "a pioneer work in that kind of quantitative observation which has provided the basis on which other subjects have been turned into accurate sciences ... *If economic theory was armed with books of this kind ... the hopes of progress would unquestionably increased*" (Keynes, 1928, p. 226, emphasis added).

[4] An exhaustive account and extensive discussion of the inductive reasoning in the *Treatise on Probability* is in Carabelli (1988). See also Lawson (1985), Klant (1989) and Bateman (1990).

[5] They include assumptions like the assumption that a knowledge of what has occurred at some of the trials would not affect the probability of what may occur at any of the others and that probabilities are all equal *a priori*: this implies that the typical example for the valid application of the Bernoulli's theorem is that of balls drawn from a single urn, containing black and white balls in a known proportion, and replaced after each drawing.

Our state of knowledge about our material must be positive, not negative, before we can proceed to such definite conclusions as they purport to justify. To apply these [mathematical] methods to material, unanalysed in respect of the circumstances of its origin, and without reference to our general body of knowledge, merely on the basis of arithmetic … can only lead to error and to delusion (ibid., p. 419)

In his statement, Keynes echoed Leibniz's view against Bernoulli's, that demanded "not so much mathematical subtlety as a precise statement of all the circumstances" (ibid., p. 403). The problem is "part of the general problem of founding judgements of probability upon experience, and can only be dealt with by the general methods of induction" (ibid., p. 400), which he expounded in Part III of *Treatise*. There Keynes maintained that "the validity and reasonable nature of inductive generalisation is ... a question of logic and not of experience, of formal and not of material laws" (ibid., p. 246):

"The validity of every induction depends, not on a matter of fact [the empirical confirmation], but on the existence of a relation of probability. An inductive argument affirms, not that a certain matter of facts is so, but that relative to certain evidence there is a probability in its favour" (ibid., p. 245)

The inductive hypothesis is logically founded on the *principle of limited independent variety*. It states that, as the number of independent constituents of a system, together with the laws of necessary connection, become more numerous, inductive arguments become less applicable (ibid., pp. 279-80). For inductive inference the propositions that constitute the premises of an inductive argument must have a high degree of limited independent variety, or, we may say, homogeneity. In other words, an object of inductive inference should not be infinitely complex (ibid., pp. 286-7). The reason for this fundamental requirement is that strictly positive prior probabilities are assessed by analogy. The importance of analogy in inductive reasoning is strongly emphasised by Keynes, who, in this case, deepened Hume's conception. If every fact has its own cause (or generator), then the method of reasoning by means of analogy breaks down, and induction becomes impossible. Only with regard to finite independent variety systems, "probable knowledge can be validly obtained by means of an inductive argument" (ibid., p. 280).

According to Keynes, the acceptance of the hypothesis that the character of the system of nature is finite necessarily involves the acceptance of an additional assumption, the hypothesis about the atomic character of natural law. This implies that inductive methods are not applicable in those cases where the system is an organic complex. Discussing Edgeworth's *Mathematical Psychics*, Keynes (1973 [1926]) wrote that "the atomic hypothesis ... has worked so splendidly in physics", but it "breaks down in psychics". In fact:

"We are faced at every turn with the problems of organic unity, of discreteness, of discontinuity – the whole is not equal to the sum of the parts, comparisons of quantity fail us, small changes produce large effects, the assumptions of a uniform and homogeneous continuum are not satisfied" (Keynes (1973 [1926]), p. 262).

The points considered here provide a background to understanding Keynes's logical criticism of Tinbergen's econometrics of business cycle. Actually, the most important examples discussed in the *General Theory*, in which the characteristics of non-homogeneity and complexity of the material make it not analysable in a formalised way, are the cases of long-term expectations and the business cycle. Long-term expectations depend on the most probable forecast that the agents can make and on the confidence with which they make that forecast. Confidence is defined in terms of "how highly we rate the likelihood of our best forecast turning out quite wrong". Our knowledge of the future is often "fluctuating, vague and uncertain". In presence of such uncertainty "there is no scientific basis on which to form any calculable probability whatever" -- that is, it is not possible to use a probabilistic theory of expectations. In presence of such uncertainty "it is reasonable to be guided to a considerable degree by the facts we feel somewhat confident about". Agents have to fall back on conventional judgement and animal spirits, or more precisely, to neither rational nor irrational motives (see Marchionatti 1999). Expectations are very important in business cycles phenomena which, in Keynes's view, are determined by investment. As expectations and investment cannot be modelled with probabilistic relations, also the business cycle too seems to be beyond the domain of probabilistic inference.

3. The Keynes-Tinbergen debate on econometric method

3.1. Tinbergen's econometric approach

Tinbergen 1939 report for the League of Nations, *Statistical Testing of Business-Cycle Theories*, represented a fundamental contribution to the contemporary statistical and econometric research on business cycle, an increasingly important subject at that time[6]. It was also an innovative contribution from the point of view of testing procedures (Morgan 1990, p.108-114). The work was expected

[6] In the 1920s institutions like NBER and IFO were established to study business cycles in a descriptive way. Yule (1927), Slutsky (1937), Frisch (1933b) elaborated theoretical models. Yule and Slutsky showed that exogenous shocks can generate cyclical patterns. Frisch proposed a propagation-impulse model of business cycle. Tinbergen build in 1936 a macroeconomic model for the Dutch economy, of which a simplified version was published in English in a small volume entitled *An Econometric Approach to Business Cycle Problems* (1937). Tinbergen's may be considered an intermediate approach in order to close the gap between economists and mathematicians in the statistical study of business cycles.

both to provide general economic forecasts and to guide government policies to control business cycle (Epstein 1987).

The first volume of the report, on which Keynes chose to focus, contained an explanation of the method of econometric testing and a demonstration of what could be achieved in three case studies. Tinbergen presented the method of his econometric study, a synthesis of statistical business cycle research and quantitative economic theory, in the spirit of *Econometrica*'s program. In Chapter 1 he distinguished the role of the statistician from that of economist. The latter hands over the theories to the statistician to submit them to examination. This means that the "responsibility" for the theories lies with the economist. As a consequence "the sense in which the statistician can provide 'verification' of a theory is a limited one" (Tinbergen 1939a, p.12). On the other hand the role of the statistician is not confined to 'verification', but also to the discovery of what causes are operative and how strongly each of them operates. This is the problem of 'measurement'. Secondly, Tinbergen defined the form in which an economic theory must be expressed in order to be verified. It must be expressed in quantitative form, which restricts the inquiry to the examination of measurable phenomena.[7] Moreover in order to inquire about business cycle, it must be a dynamic theory. It must be one which "deals with the short-term reactions of one variate upon others but without neglecting the lapse of time between cause and effect" (ibid., p. 13). The equation in which it is expressed relates to non-simultaneous events: the form taken is described as 'sequence analysis'. To the extent that the additions to static theory are the result of statistical research, we may say that 'the statistician may supply theoretical suggestions to the economist" (ibid., p. 14). In Chapter 2 Tinbergen outlined the technical method of multiple correlation analysis by applying it to an economic business cycle theory translated into a parametrised mathematical-economic model. Then he tested for the plausibility of the parameter estimates. Finally, he checked the outcomes generated by the system as a whole to see whether a theory provides a business cycle mechanism or not. Tinbergen was interested in testing the economic importance of results. Namely, he investigated whether particular effects have a plausible sign and are quantitatively important. If so, significance tests were used to assess the statistical accuracy of estimates. In Chapter 3 Tinbergen discussed the results obtained in applying the method to the relation and indicated the proximate objective causes of changes in investment activity "looked at from the side of entrepreneurs and public authority" (ibid., p. 34). The analysis was made in three case studies -- on general investment, investment in residential building and in railway rolling-stock.

[7]"The inquiry is, by its nature, restricted to the examination of measurable phenomena. Non-measurable phenomena may, of course, at times exercise an important influence on the course of events; and the results of the present analysis must be supplemented by such information about the extent of that influence as can be obtained from other sources" (Tinbergen 1939a, p. 11).

3.2. The Keynes-Tinbergen debate, 1938-1940

3.2.1. The story of the debate

The Keynes-Tinbergen debate went through two different phases. The first one took place in the short period between August and September 1938. It had a semi-private character, and took the form of an exchange of letters between Keynes, Tinbergen and other economists and League of Nations officers. The second phase took place between September 1939 and March 1940 and was marked by Keynes's review of the Tinbergen's first volume of the book, published in the September issue of the *Economic Journal*, and by Tinbergen's reply.

The story begins on August 11, 1938, when Keynes received a letter from R. Tyler, of the League of Nations, who wrote that he was sending him a proof copy of the book written by Tinbergen in order "to obtain from you any criticism you might have" (Keynes Papers, CO/11/291). Keynes was already acquainted with Tinbergen's work – as it is witnessed by letters in July and early August 1938 to Roy Harrod.[8] Unlike Harrod, who seemed to be in favour of Tinbergen's work, Keynes expressed perplexities, essentially based on his view on the appropriate role of mathematics and statistics, and his negative evaluation of the recent evolution in their application in economics. After a first reading of the proofs, Keynes's judgement was negative. In some letters to Kahn and Harrod (respectively in Keynes 1973b, p. 289 and p. 331-2) he declared that, "so far as I can understand the matter", Tinbergen's work was "all hocus" (letter to Kahn, 23 August 1938, in Keynes 1973b, p. 289) -"it is almost pure hocus", Keynes wrote to Harrod (letter of 23 August 1938, in Keynes 1973b, p.332) -, because "there is not the slightest explanation or justification of the underlying logic" (letter to Kahn, 23 August 1938, in Keynes 1973b, p. 289). These early negative impressions were confirmed in a long letter to R. Tyler two weeks later, the 23rd of August (in Keynes1973b, p. 285-289), in which Keynes outlined the fundamental lines of his criticism of Tinbergen's method of analysis. He recognised the importance of testing "the quantitative influence of factors suggested by a theory" (ibid. p. 289), but he pointed out the issue of the correct method to be employed.

On September 12, 1938, Tinbergen - who had received Keynes's comments through A. Loveday, director of the Financial Section of Economic Intelligence Service - wrote to Keynes replying to his critiques. He thought that there was "some misunderstanding behind some of [Keynes'] questions"(Keynes 1973b, p. 291) but recognised that "It is difficult to meet [Keynes'] remarks on methodology in general" (ibid.), thus preferring to discuss technical questions. The reading of

[8] Harrod had taken some part in discussing Tinbergen's work for the League of Nations and participated to a small meeting of experts held in Cambridge in July 1938 and then at the Cambridge meeting of the British Association for the Advancement of Science in early August 1938 (see letters of Harrod to Tinbergen, 20 January 1938, and Loveday to Harrod, 30 May 1938, in Harrod 2003) in which a draft of Tinbergen's book was a subject of discussion.

Tinbergen's letter supported Keynes's critical feeling that the work was methodologically weak, which made the results obtained of little practical value:

"I have had a letter from Tinbergen, which deals very frankly with my points. But the upshot is that *the results are very much what I supposed them to be*. *The mistake he is making*, I think, is in being *too ambitious* in regard to the material to which he his applying his method, and *too premature* in applying it at all until the general question of its validity has been established" (emphasis added) letter to Loveday, September 20, 1938, (Keynes' Papers, CO/11/319)

Replying to Tinbergen on the same day, Keynes wrote:

"I hope you will continue your investigations. But I do emphasise the consideration that very little practical weight ought to be given to your provisional conclusions pending a justification of the application of your general method to statistics of the character and quality in question" (Keynes 1973b, p. 293-4).

Therefore, Keynes concluded that Tinbergen had to demonstrate first of all that his method was applicable, rather than simply applying it. His letter to Harrod, on 13 September 1938 ended the first phase of his criticism to Tinbergen:

"I will await Tinbergen's revised version ... If Tinbergen was a private research student, he would deserve every encouragement. It is certainly worth his while pursuing all this. But I think it very dangerous for a collection of *responsible* economists to give it any sort of *imprimatur* in its present stage" (Keynes 1973b, p. 304, emphasis added).

In the September 1939 issue of the *Economic Journal*, one year after their first exchange, Keynes published a long review of Tinbergen's just published work, "limited to an explanation of the statistical method which is proposed to employ" (ibid., p. 306).
In a letter to Keynes of December 18, 1939, Tinbergen expressed his astonishment for Keynes's harsh reaction to his work:

"I must frankly admit I had expected you to be nearer to this type of work. There are some features in your work, viz. realism and focussing on the broad lines, which I also see as characteristic for this sort of econometric business cycle research".

He extensively replied to Keynes's "serious" questions in the March 1940 issue of the *Economic Journal*.[9] In his "comment" (Keynes 1973b) Keynes defined Tinbergen's reply "very valuable",

[9]In the same 1940, at the invitation of the editors of the *Review of Economic Studies*, Tinbergen also wrote a paper "to go into some more detail concerning the method" of analysis. It offers a restatement of the method and integrates Tinbergen's reply to Keynes. In particular, Tinbergen emphasises the flexibility of his method (Tinbergen 1940b, p. 236).

but not adequate to answer his questions persuasively. Nevertheless, he declared (no doubt a bit ironically) that he was in favour of the continuation of Tinbergen's type of research: "Newton, Boyle and Locke all played with alchemy. So let [Tinbergen] continue" (ibid. p. 320).

3.2.2. Keynes's criticism

Keynes (1973b [1938] and 1939) first posed the central question: the "question of methodology" in general - that is, "the logic of applying the method of multiple correlation to unanalysed economic material, which we know to be non–homogeneous through time" (Keynes 1973b, p. 285). Then, he discussed specific issues: the comprehensiveness of the factors, their independence and measurability, the constancy of the coefficients and the time-lags. He concluded by going back to the methodological question, raising the problem of passing from statistical description to inductive generalisation. On some points Keynes's critique shows his limited knowledge of the developments of the econometric literature in the previous two decades (despite the fact that Keynes was on the editorial board of *Econometrica* since 1933) and a few misunderstandings on technical issues. This fact is well known and widely emphasised (see for example Hendry and Morgan 1995). Here we focus instead on the essential points of Keynes's criticism, which may be considered long-lived in a historical perspective.

Keynes wrote that the logical condition for using the method of multiple correlation is the existence of "numerically measurable, independent forces, adequately analysed" -- that is, "independent atomic factors and between them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time". However, he continued, "we know that every one of these conditions is far from being satisfied by the economic material under investigation". Hence "how far does this impair the validity of the method ? This seems to me to deserve a most careful preliminary enquiry" (ibid., p. 285-6). Unfortunately Tinbergen's discussion appeared "grievously disappointing":

> "it leaves unanswered many questions which the economist is bound to ask before he can feel comfortable as to the conditions which the economic material has to satisfy, if the proposed method is to be properly applicable" (Keynes 1939, p. 306)

Keynes then examined the issue in detail and raised a set of questions about the conditions of validity of Tinbergen's procedures [10].

The first condition Keynes pointed at was the completeness of significant causes: "is it assumed that the factors investigated are comprehensive and that they are not merely a partial selection out of all the factors at work?" (1973b, p. 286-7). If they are not all included, the estimated coefficients suffer from omitted variable bias. If they are included, and if "the economist has correctly analysed beforehand the *qualitative* character of the causal relations", then he can examine their quantitative importance, i.e. how strongly each of them operates. This is for Keynes the primary role of econometrics. It is quite different to affirm, as Tinbergen did, that the statistical test can prove a theory to be *incorrect*, or incomplete – that is to falsify a theory - by showing that it does not cover a particular set of facts.[11] In addition, Keynes (1940) raised the related problem of testing theories when different econometric specifications can be derived from a theory:

"the seventy translators of the Septuagint were shut up in seventy separate rooms with the Hebrew text and brought out with them, when they emerged, seventy identical translations. Would the same miracle be vouchsafed if seventy multiple correlators were shut up with the same statistical material ? And anyhow, I suppose, if each had a different economist perched on his a priori, that would make a difference to the outcome" (ibid. p. 155-6)

The second condition is that all the significant factors are measurable. Keynes wondered what place was left for expectations, for the state of confidence relating to the future and for non-numerical factors, such as inventions, politics, labour troubles, wars, financial crises. He felt the suspicion "that the choice of factors is influenced .. by what statistics are available, and that many vital factors are ignored because they are statistically intractable or unprocurable" (letter to Tyler, 23 August 1938, in Keynes 1973b, p. 287). Keynes noted that according to Tinbergen "the method can be usefully applied if *some* of the factors are measurable, the results obtained from examining these factors being 'supplemented' by other information" (Keynes 1939, p. 309). But "how can this be done ? He does not tell us" (ibid.).

The third question concerned the independence of factors. First of all Keynes raised the problem of spurious correlation: "If we are using factors which are not wholly independent, we lay ourselves open to the .. complications of 'spurious' correlation" – a term introduced by K. Pearson (1897) in a discussion of correlation between indices. He then raised the problems of simultaneity and multicollinearity:

[10] Keynes also cites the inadequacy of statistics – an "obvious" difficulty: "These many doubts are superimposed on the frightful inadequacy of most of the statistics employed, a difficulty so obvious and so inevitable that it is scarcely worth while to dwell on it" (Keynes 1939, p. 317).

[11] The question of whether testing can prove a theory to be correct is not controversial. Both Keynes and Tinbergen agree that testing cannot prove the correctness of a theory, whatsoever amount of empirical evidence is available.

"What happens if the phenomenon under investigation itself reacts on the factors by which we are explaining it ? When he investigates the fluctuations of investment, Tinbergen makes them dependent on the fluctuations of profit. But what happens if the fluctuations of profit partly depend (as, indeed, they clearly do) on the fluctuations of investments ? Professor Tinbergen mentions the difficulty in a general way in a footnote .., where he says .. that <one has to be careful>. But is he ? .. In practice Professor Tinbergen seems to be entirely indifferent whether or not his basic factors are independent of one another" (ibid. p. 309-10).

Then Keynes brought up two questions of technical importance: they concern the functional forms, the time lags and trends. First Keynes maintained the implausibility of the widespread assumption of linearity and argued for the examination of alternative functional forms. About time-lags and trend and the general problem of dynamic specification, Keynes expressed some perplexities and accused Tinbergen of scarce rigour in treating time lags and trends in an *ad hoc* manner by choosing them by a trial and error approach:

"Professor Tinbergen ... invents them [time lags] for himself. This he seems to do by some sort of trial-and-error method. That is to say, he fidgets about until he finds a time lag which does not fit in too badly with the theory he is testing and with the general presuppositions of his method ...The introduction of a trend factor is even more tricky and even less discussed .. In the case of fluctuations in investment, 'trends', Professor Tinbergen explains, 'have been calculated as nine-year moving averages for pre-war periods … and as rectilinear trends for post-war periods " (ibid., p. 315).

This seemed to Keynes inaccurate and arbitrary:

"with a free hand to choose coefficients and time lag, one can .. always cooking a formula to fit moderately well a limited range of past facts. But what does this prove ?" (letter to Tyler, cit. in Keynes 1973b, p. 286-7).

In other terms, Keynes questioned the manipulation of data to "make possible to fit an explanation to any fact" (Keynes 1939, p. 311).

In conclusion, he went back to what he considered the critical condition: the likely structural instability. In 1938 Keynes put the constancy of the parameters into question: "the coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely we know that they are not constant" (Keynes 1973b, p.286). This issue is directly connected with the problem of the inductive generalisation, that is, the inductive and predictive value of the estimates, or the relevance of the estimated model to the future. It is "the slippery problem of passing from statistical description to inductive generalisation", which, Keynes remembered, **"**thirty years ago [in the *Treatise on Probability*] I used to be occupied in examining in the case of simple correlation":

"How far are these curves and equations meant to be no more than a piece of historical curve-fitting and description, and how far do they make inductive claims with reference to the future as well as the past ? ... Put broadly, the most important condition is that the environment in all relevant respects .. should be uniform and homogeneous over a period of time. We cannot be sure that such conditions will persist in the future, even if we find them in the past. But if we find them in the past, we have at any rate some basis for an inductive argument" (Keynes 1939, p. 315-6)

Tinbergen "makes the least possible preparation for the inductive transition" (p. 316). According to Keynes the correct procedure is to break up the period under examination into a series of sub-periods, "with a view to discovering whether the results of applying our method to the various sub-periods taken separately are reasonably uniform" (p. 316)[12]. If this is the case, then there is some grounds for projecting the results into the future". This procedure was not followed by Tinbergen:

"For his pre-war investigations he takes a period of about forty years and makes no attempt to break it up into sub-periods. If he had done so, would his regression coefficients, calculated for each decade taken separately, differ somewhat widely from those calculated as the best fit for the whole period ? This is worth examination. For the main prima facie objection to the application of the method of multiple correlation to complex economic problems lies in the apparent lack of any adequate degree of uniformity in the environment" (ibid. p. 316-7).

The chief dilemma Tinbergen was facing was, Keynes concluded, "that the method requires not too short a series, whereas it is only in a short series, in most cases, that there is a reasonable expectation that the coefficients will be fairly constant" (Keynes 1973b, p. 294): *this is, and will be, the leitmotif of Keynes's criticism*. Actually:

"the broad problem of the credit cycle is just about the worst case to select to which to apply the method, owing to *its complexity, its variability, and the fact [that] there are such important influences which cannot be reduced to statistical form*" (ibid., emphasis added)

This does not mean, Keynes added, that "there may not be problems within the general field of the trade cycle which would provide suitable material". However, "surely there is no general

[12]The genesis of this procedure is in the *Treatise on Probability* (1921). The criticism of the application of mathematical methods to the statistical inference leads Keynes to propose other methods "more consonant with the principle of sound induction". In fact to argue from the mere fact that a given event has occurred invariably in a great number of instances that it is likely to occur invariably in future instances "is a feeble inductive argument, because it takes no account of the analogy" (ibid., p. 445). To strengthen the argument we need to increase the analogy between the instances. This "chiefly consists, Keynes argues, in determining whether the alleged association is stable, where the accompanying conditions are varied" (ibid., p. 427). A technical method that supplies the qualified procedure is, according to Keynes, that proposed by the German statistician and economist William Lexis. It consists in breaking up a statistical series into a number of sub-series, "with a view to analysing and measuring, not merely the frequency of a given character over the aggregate series, but the *stability* of this frequency amongst the sub-series" (p. 428, emphasis added).

presumption that any enquiry one might fix on will be suitable. The presumption is to the contrary". According to Keynes "the method will prove valuable when applied to certain types of problems, [more elementary cases (Keynes 1939, p. 317)] where adequate statistics exist" (ibid.). In his 1939 correspondence Keynes cites in fact at least two examples of types of problems to which multiple correlation methods can be reasonably applied (the demand for automobiles, and the demand for investment in new rolling stock. See section 4.3).

3.2.3. Tinbergen's reply

For Keynes the core of the matter were the logical conditions for applying the method of multiple correlation -- that is, an issue that precedes its application, while and the technical problems were subordinate to this logical question. Tinbergen's reply, instead, avoided as much as possible the logical question and the "slippery problem of passing from statistical description to inductive generalisation", and stressed – with many illustrations of his approach in business cycle research - the flexibility of his empirical method, leaving Keynes's central objection substantially unanswered. As we know, Tinbergen was truly astonished at Keynes's reaction and politely rejected it. On September 12, 1938, , after reading the critical notes Keynes sent to Tyler, Tinbergen wrote to him. He thought that there was "some misunderstanding behind some of [Keynes'] questions", but recognised that "it is difficult to meet [Keynes'] remarks on methodology in general" (Keynes 1973b, pp. 291-3). He said that he preferred to discuss technical questions. However, Tinbergen did not offer any systematic technical methodology for dealing with the problems under discussion, although he seemed to anticipate some contemporary advances (see Dharmapala-Mc Aleer 1996 and Mc Aleer 1994).

Regarding the need for *a complete list* of the relevant factors –which was equivalent to say that the model must be correctly specified -, Tinbergen explained that he assumed that "the factors included are *comprehensive as far as the more important are concerned*" (emphasis added). He added that "it does not matter if non-relevant factors have been forgotten", because "what factors are relevant and what are not will not always be cleared beforehand. It must then be tried out" (Tinbergen 1940a, p. 142). In other words, he maintained that a correct specification is subjected to statistical testing. What is important, according to Tinbergen, is that some conditions (drastic restrictions, as a matter of fact) are met: (a) that the explanatory variables chosen explicitly are the relevant ones; (b) that the non-relevant explanatory variables may be treated as random residuals, not systematically correlated with the other explanatory variables ("this may be tested afterwards - e.g. by calculating

the serial correlation for the residuals and the bunch maps") - and (c) that the mathematical form of the relation is given" (ibid., p. 141).

As regards expectations and the state of confidence, Tinbergen thought expectations are "products of the human mind which are based on past experience, even though they relate to future moments" (ibid., p. 147). They are "hidden" in some systematic variables such as profits, etc. He did not deny that "external events" may also influence expectations. However, he thought that "these external events will be, as a rule, of an unsystematic character, and may thus be part of unexplained residuals" (ibid.).

As for the question whether the explanatory variables should be *independent* of each other, Tinbergen distinguished between the statistical and the economic meaning of the word *independent*. He argued that, for statistical purposes, explanatory factors did not need to be economically independent of each other but only uncorrelated.

As regards the question of the constancy of the coefficients, he explained that he assumed the constancy of coefficients as a first approximation. As to lags and trends, he admitted that "they are sometimes assumed by common sense guessing" and that

*"*In principles both [lags and regression coefficients] *have been determined so as to make the correlation the highest possible and by only admitting such values as seemed to have economic sense*" (ibid. p. 150).

As regards Keynes's observation that it was arbitrary to use nine-year moving averages as trends in pre-war periods and straight lines in post-war years, and that a manipulation makes it possible to fit any explanation to any facts, Tinbergen answered that there were arguments in favour of examining linear models:

"for short periods there is not much difference between a straight trend and a moving average. For long periods there is, and then the moving average is decidedly better .. The advantage of straight-line trends is that no observations are lost in the extremes. This is why they have been preferred for the (short) post war-period" (ibid. p. 251)

About the crucial question of the inductive generalisation, Tinbergen maintained that:

"If there is no reason to suppose that the laws that have governed the reactions of individuals and firms in the past will have changed in the near future, it seems possible to reach conclusions for the near future by measuring as exactly as possible those same reactions in the past" (ibid. p. 152)

Of course, he added, "this is only true if no structural changes take place". However, he concluded, "even if [structural changes] take place, it will, in many cases, be possible to 'localise' their influence - i.e., to indicate which of the elementary or direct causal relations they affect" (ibid.).
On the whole, Tinbergen rejected Keynes's pessimistic view not because he considered his criticism irrelevant, but because in his opinion "the method under discussion promises much more than Mr. Keynes thinks". "The proof of the pudding is in the eating", he concluded. He was mainly interested in getting on with the job.

3.2.4. An appendix to the debate: Rothbarth's review of Tinbergen's second volume

A review of the second volume of *Statistical Testing of Business-Cycle Theories*, entitled *Business Cycles in the United States of America, 1919-32*, was published in the June-September 1941 issue of the *Economic Journal*., which was defined by Kalecki "a model of careful econometric analysis" (Kalecki 1944-5, p. 121). The author was Erwin Rothbarth, a twenty-eight old German economists who after emigrating to England taught economic statistics in Cambridge and worked very closely with Keynes (Cuyvers 1983).
In November 1938 Rothbarth had already reviewed Tinbergen's *An Econometric Approach to Business Cycle Problems*. There he acknowledged the "unassailable" case for the econometric method, but expressed caution as regards how far the econometric approach can go.
Rothbarth praised Volume two of Tinbergen's study as a "brilliant pioneering effort". Before discussing Tinbergen's results, Rothbarh highlighted the relevance of his attempt to demonstrate that a mathematical model of the trade cycle can be constructed which is both sufficiently simple to be tested statistically and a sufficiently good approximation to reality to be useful. Such relevance, Rothbarth wrote, is "independent of the question whether Professor Tinbergen succeeds in explaining the trade cycle in the U.S.A. In my view he fails, but his failure is almost insignificant beside the great merit of the attempt" (Rothbarth 1941, p. 294).
Rothbarth then analysed Tinbergen's findings with painstaking accuracy, questioning in a few cases Tinbergen's reading of his own results. For instance, he pointed out how the econometric findings in themselves do not allow us to decide between two alternative interpretations of the influence of profits on consumption (either through speculative gains or through the increase demand for durables and semi-durables - the acceleration principle). He regarded other results, such as the negligible role of short term interest rate in determining investment in stocks, as not "finally conclusive" in the light of the poor statistics available and behaviours that might not be constant in time (such as the unwillingness to hold speculative stocks in slumps). As regards the treatment of long term interest rates, he highlighted, in a Keynesian line of reasoning, the potential importance of

immeasurable factors: "it is quite possible that variations in confidence and qualitative credit conditions were more important during this period that variations in pure interest rate" (ibid., p. 295). In considering whether the model can account for longer cycles, Rothbarth revealed quantitative-oriented mind by taking into account the possible effect sampling errors. Finally, he also raised the issue of collinearity and the problem of the degrees of freedom, neglected by Tinbergen.

Rothbarth concluded with a recommendation to "professor Tinbergen and his adherents" in favour of smaller models. Such suggestion is very much in the same line of reasoning as Keynes's methodological considerations, as constructing smaller models implies more weight given to economic theory and the investigation of the economic material previous to manipulation of data. The very last paragraph of the review bring us back once again to one of Keynes's main perplexities, the issue of non-homogeneity over time:

"[With a smaller model, Tinbergen] would have needed a separate model for the 1919-22 cycle; but I cannot help feeling that this would have been an additional advantage rather than the reverse. It would have focussed the reader's and Professor Tinbergen's attention on the strong differences existing between this cycle and both the 1929 and 1937 cycles" (ibid., p. 297).

4. The econometricians and Keynes, 1939-1941

The econometricians' initial reactions to the debate consisted in careful considerations of the issues raised by Keynes, although mostly in defence of Tinbergen. Many reviews of Tinbergen's League of Nations study explicitly refer to Keynes's criticism[13]. Keynes's attack also prompted some attempts by eminent econometricians to reconcile his criticism with statistical-econometrical work.

The examples of such attempts presented below appear to be particularly interesting as Keynes's opinion on them was directly expressed in his correspondence between 1939 and 1941.

The first case is the article by Jacob Marschak and Oskar Lange in defence of Tinbergen, submitted to the *Economic Journal* for publication but rejected by Keynes. The second one is an exchange of letters with Tjalling Koopmans over his 1941 paper "The Logic of Econometric Business-Cycle Research", where he provided a clear restatement of Tinbergen's method. Finally we present two specific applications of multiple correlation analysis which Keynes recognised as suitable: the study

[13] Allen (1940) considered Keynes's questions "pertinent". Tintner (1941) agreed with Keynes that expectations "are not introduced explicitly enough" in the study (p. 622). J.E.W. (the reviewer for the *Journal of the Royal Statistical Society*) (1940), raised some of Keynes's methodological questions (albeit without quoting him) on factors measurability, the constancy of coefficients, the linearity, etc.

of automobile demand (correspondence with Victor Szeliski of the Institute of Applied Econometrics, New York[14]) and the demand of investment in new rolling stock (analysed by E.J. Broster).

4.1. In defence of Tinbergen: Lange and Marshack, 1940

Shortly after publishing Tinbergen's rejoinder, Keynes received a journal submission from Oskar Lange and Jacob Marschak entitled "Mr. Keynes on the statistical verification of business cycle theories".[15] (It was sent from Chicago, 15 February 1940, immediately before Tinbergen's reply and Keynes's final comment were published). Keynes decided not to publish the paper in the *Economic Journal*. It was then published for the first time in Hendry and Morgan eds. (1995), where it is presented as an example of the "more constructive criticism that emanated from those in favour of Tinbergen's approach, who saw problems with it but wished to advance the methods adopted" (p. 56). Such assessment, in our opinion, overrates the paper.

Lange and Marschak start by claiming themselves in "profound agreement with the economic theories of Mr Keynes", and are therefore keen in supporting the idea they capable of empirical and statistical verification (p. 390). However, the case for statistical verifiability is not discussed in depth before moving on to the importance of giving quantitative precision to what is already known in qualitatively terms. Lange and Marschak agree on some of the weaknesses of Tinbergen's work pointed out by Keynes, and even add a few to the list. They criticise Tinbergen's treatment of regression coefficients as exact numbers, rather than as estimates, and his subsequent failure to compute standard errors[16].

The rest of their essay is devoted mainly to remarks of a more technical nature, some of which are dealt with acutely, while on others the line of argument appears seriously flawed. The cobweb model is presented in order to show how cyclical movements can be generated by linear relationships. The issue of the measurability of variables is addressed by noting that many qualitative dimensions can be treated statistically (e.g. through the use of dummy variables). Far less convincing is the defence of the use of trends, which Lange and Marschak interpret as a mean

[14] The Institute was founded in 1938 by Charles F. Roos, one of the founders of the Econometric Society in 1930 and the director of the Cowles Commission for Research in Economics from 1934 to 1937. In 1937 he left for New York to engage in the practical application of econometrics to the problems of business. Here he founded the Econometric Institute, of which he was the president and director of research from 1938 until his death in 1958. Victor S. Szeliski was co-author with Roos of many papers between 1934 and 1943, published in *Econometrica*, *Journal of American Stastistical Association*, *Journal of Political Economy*.

[15] From a history of economics point of view, the relevance of this paper lies also in the fact that it was written with the help of Trigve Haavelmo, Jacob Mosak and Theodore Yntema.

[16] However, Lange and Marshak seem inaccurate on this point: Tinbergen apparently did compute standard errors, as explained in the paragraph on "Significance calculations" for at least some of the regression equations. See table III.10, p. 78-79, and Graph III.12, p.84.

both of capturing the gradual variation in time of parameters and of eliminating the "nonsense correlations" arising in time series.

On the whole, the tone of the article was overall conciliatory, but the defence of the method appears weak and unaware of the deep methodological issues. Hendry and Morgan hypothesise that Keynes decided not to publish it because he thought that the issue had already been discussed enough. In a letter to Harrod of August 27, 1935, Keynes expressed his worries about the tendency to accept part of his work by accommodating it to views that were incompatible with it. In this light it certainly seems legitimate to conjecture that Keynes was likely to be irritated by the real eagerness expressed by the authors to reconcile his theories with the methods of empirical verification. In any case, in our opinion Keynes's decision appears justified by the analysis of the contents of the article, which add little substance to the debate. Actually, in a letter sent to Pigou on 29 March 1940 (EJ/1/6), Keynes maintained that Tinbergen's reply was of far higher quality as Lange and Marschak "tell us what their view is but do not give their reasons". He confirmed that he criticised Tinbergen's method and not the idea that business cycle theories can be tested[17]:

"I have, of course, never said anything to the effect that no business cycle theory can be tested statistically. I was dealing solely with Tinbergen's very special method of analysis".

4.2. In defence of Tinbergen: Koopmans, 1941

A deeper and more complete consideration of Keynes's criticism is provided one year later by Tjalling Koopmans's article "The logic of econometric business cycle research" (Koopmans 1941).

On May 23, 1941, Keynes received a letter from Koopmans, announcing that he was sending him an offprint of said article, which attempted "to answer some of the questions raised in your review of Tinbergen's investigation for the League of Nations" (CO/4/155). In fact, it was intended as a contribution to a more systematic exposition of the logic of methods applied in econometric business-cycle research.

The article aims to address the issue "to what extent business cycle econometric results derive from statistical observation and to what extent they depend on other hypothesis or information?" (p. 158). Koopmans starts off by enumerating "the elements of the logical situation facing the student of that problem" (ibid.). The first one is the availability of time series data. He noted how from "the

[17] O'Donnell's comment (1997) to this letter is analogous: "The letter .. demonstrates .. two important propositions. The first is that Keynes's critique of Tinbergen's work was only a critique of a 'very special method of analysis'. Although this proposition may be inferred from Keynes's previously published writings, it is unambiguously confirmed by the letter… The second is that the object of his attack was *not* the validity of all conceivable statistical methods, including those for statistically testing the business cycle … Both propositions are also abundantly clear in Keynes' reply to Lange" (p. 155-6).

combination of uniqueness and manifold interrelation of data" (which are two crucial characteristics of economic data) some "fundamental difficulties and limitations" arise that are specific to the application of these methods to economic problems (p. 160).

The second element is the adoption of the "general working hypothesis": that causal connections between the variables dominate "mere chance fluctuations" in determining the fluctuations of the internal variables (apart from "recognised but unmeasurable external factors" such as earthquakes or strikes)[18]. Koopmans recognised the possibility of unmeasurable internal factors acting as a cause on other variables, thus addressing one of Keynes's more important questions. He maintained that the only way to make sense of this concept is if non-measurable phenomena like "expectations" or the "state of confidence" are regarded as themselves determined mainly by measurable internal and/or recognizable external phenomena.

The need for introducing additional information[19] – the third element – stems from the fact that the high degree of interrelation allows for different ways in which fluctuations of one variable may be reconstructed by combining some others. In the absence of additional information, the only unconditional inference one may draw is negative (that is to say, proving a theory incorrect) and inconclusive.

Koopmans then discussed the relevant features of Tinbergen's investigations, identifying three sets of premises in his study:

1) that all influence on variable x_1 (dependent) not emanating from a set of "determining" variables $x_2,...,x_n$ is attributable either to influences adding up to a random component, or to an function of time (trend), or stem from recognised un-measurable external forces affecting only a few observations;

2) that the influence exercised by $x_2,...,x_n$ can be represented by mathematical functions;

3) assumptions on the sign or value range or on the relative proportions of coefficients, and on value range for lags.

The method prescribes that the list of premises is produced by the economist and then passed on to the mathematical statistician who first applies the principle of statistical censorship, which requires that "the additional information should not imply statements which can be unconditionally rejected because they are contradicted by the data" (p. 163). He will then investigate "whether at least one set of coefficients and lags exists which is compatible with all three set of premises"[20]. Koopmans

[18] Internal/external correspond to endogeneous/exogeneous in today terms.

[19] This may take the form of observations not expressible as statistical time series, experiences from other countries or periods of time, deductions from economic theory or "mere working hypothesis with a certain degree of plausibility".

[20] .i.e., that: "(i) has the properties specified in the third set of premises and (ii) when combined with the series $x_2,\ldots,x_n$ … (according to the prescriptions given in the second set of premises) leaves only such 'unexplained residuals' … as do not contradict the premises adopted in the first set." (p. 166).

seems to accept some of Keynes's concerns in highlighting the crucial centrality of economic premises:

"Knowing how easily a *statistically undetectable omission* of one relevant determining variable, or an incorrect specification of an a priori known lag, may … distort the values and even the signs of the other coefficients, *the investigator will devote a full share of his suspicion to the less technical part of the procedure: the choice of the premises.*" (p. 167, emphasis added).

If the statistician finds a good fit, this does not confirm that the list of premises is valid, but merely suggests the conditional conclusion that takes the form of "best estimates". The validity of these estimates needs to be assessed against the width of margins of error and problems such as the presence of multiple collinearity. After the statistician's verdict on the premises as a whole (they may be contradicted by the data, or not; if not, they might provide sufficient basis for quantitative precision, or not), it is again the economist's task to divide premises into acceptable and dubitable ones. It can then be the case that the statistician is able to confirm the dubitable premise.

Koopmans stressed the importance of expressing the alternative to a dubitable premise in terms of a subsidiary premise such that it is mutually exclusive to the dubitable one and that either one or the other could be true. He then choose to illustrate this by discussing the two premises that Keynes found most problematic: the use of linear relations and the constancy of coefficients. In order to test the linearity assumption, Koopmans suggested technical devices such as including in the equation the squares or other curvilinear functions of the explanatory variables as a conclusive test that Tinbergen failed to perform. Matters are far more complicated in reference to the constancy of the coefficients: "Here I appeal to economists to specify the criticism in order to make its relevance liable to statistical test" (p. 175). In some cases "abrupt change at specific moment in time" might be identified, while in order to allow for "gradual and smooth change" (p. 175), if a sufficient number of observations is available, one may break up the period in two or more sub-periods. A different case arises when the influence of a determinant variable x_2 on x_1 depends on the value of x_3 (due to bottlenecks in the economy or to unmeasurable factors), with the result that the additivity of influences should be abandoned. He admitted having no suggestions as to how to test for constancy of lags: "Purely technical study is urgently required on this important point" (p. 177). Koopmans's conclusion was that:

"No single clear-cut answer can be given to our initial question… [the combination of data and additional information] is a complicated process, the result of a continuous dialogue of a game of give and take, between economist and statistician." (p. 178)

While he looked at Tinbergen's results in the light of his rigorous description of the method, he nevertheless basically defended and reaffirmed the validity of the method itself, regarded as:

"the only method by which the relevant information contained in statistical time series can be extracted and made available for giving such quantitative precision to the supposed relationships of business-cycle theory as it truly supports." (ibid.)

He maintained that in the cases where "a basis of premises both solid and sufficient has been reached with respect to each variable to be explained" (p. 179), extrapolations could be made from the knowledge gained for two kind of purposes: policy and prediction. As regards policy, the objective is to quantify the effect a certain measure would have within the studied period in the country analysed: using it as a guide to actual policy presupposes "the *persistence* of main dynamic features of the economy in the future" (ibid., emphasis added). Prediction represents a "much more hazardous undertaking" (ibid.).

Koopmans's conclusion was that Tinbergen's results fall instead under the cases where "a basis both solid and sufficient … could not be established" (p. 180). A number of unsolved uncertainties[21] allow thus "not one single reconstruction of the fluctuations of the period but a set of possible explanations" (ibid.). Koopmans reveals his strongly policy-motivated concern in defining "the important mathematical problem": "to detect and analyse common characteristics of all possible explanations and to discover whether certain types of policy may have favourable effects on stability whichever explanations corresponds best to reality" (ibid.).

On May 29th, 1941, Keynes answered Koopmans, He expressed great appreciation for his work, but also reaffirmed his fundamental criticism, emphasising as the main dilemma the issue of stability of the environment in the long run:

"Many thanks for sending me your article … I enjoyed it very much. *I am sure these matters need discussing in that sort of way*. *There is one point, to which in practice I attach a great importance, you do not allude to*. *In many of these statistical researches, in order to get enough observations they have to be scattered over a lengthy period of time; and for a lengthy period of time it very seldom remains true that the environment is sufficiently stable*. *That is the dilemma of many of these enquiries*, which they do not seem to me to face. Either they are dependent on too few observations, or they cannot rely on the stability of the environment. It is only rarely that this dilemma can be avoided" (CO/4/170, emphasis added).

[21] He explicitly mentioned the "relative influence of retail prices and income on consumption expenditure" and referred to other influences whose recognition is limited by the lack of calculation margins of error.

4.3. Two examples of the method 'properly in place': the correspondence with Broster and Szeliski , 1939

Keynes's correspondence in the years of the debate also offers examples of applied studies in which he believed that the use of econometric tools was justified by the specific features of the object of analysis.

One example is cited in his letter to Tyler of 23 August 1938: the case of the demand for investment in new rolling stock. At that time Keynes was publishing in the *Economic Journal* an article by the English statistician E. J. Broster that applied the multiple-correlation method to the question of the relation between the volume of traffic and operating costs on the British Railways in the years 1928-1937 (Broster 1938). He introduced multiple linear regression equations expressing total operating costs as a function of passenger-miles, ton-miles and coaching train-miles, and freight-train-miles: "That is the sort of case – Keynes remarked - where one has at any rate a modest expectation of useful results". He continued:

"On the other hand, the question of what determines the volume of investment itself I should regard *prima facie* extremely unpromising material for the method" (Keynes 1973b, p. 295).

In his correspondence with him Keynes agreed with the methodological line taken by Broster, however adding:

"I was raising the logical difficulties. You say in effect that, *if one was to take these seriously, one would give up the ghost in the first lap, but that the method, used judiciously as an aid to more theoretical enquiries and as a means of suggesting possibilities and probabilities rather than anything else, taken with enough grains of salt and applied with superlative common sense, won't do much harm*. I should quite agree with that. *That is how the method ought to be used*. Though, even so, I think it requires more careful selection of topics than Tinbergen has made. He, however, is really claiming much more of it, - as though it was of more demonstrative character than other methods of approach" (letter to E.J. Broster, December 19, 1939, CO/11/447, emphasis added)

The second case is illustrated by an exchange with Victor Szeliski, from whom Keynes received a letter in November 1939. Szeliski had read Keynes's review of Tinbergen's League of Nations study "with considerable interest and approval", and he 'naturally' wondered to what extent Keynes thought that "the same criticisms apply to Roos's and my study of automobile demand". He added:

"Of course our purpose was narrower that his; we were not trying to prove or disprove business cycle hypotheses, but to develop a "law" connecting retail automobile sales with factors which, a priori, are causes of sales"(CO/11/444)[22].

Their study, part of a research project commissioned by General Motors, investigated the determinants of demand for automobiles and, among other things, estimated its price-elasticity. It was critically reviewed by Willford I. King, president of the American Statistical Society (King 1939a). King raised questions on the suitability of the data series used, on the neglect of the effects of the movement of the supply curve, and on the identification problem - how the shape of one curve can be reconstructed from data on the intersections of demand and supply. King expressed a general distrust in inductive methods.

Roos's and Szeliski's reply (1939b) begins with a reflection on the need for a shift to take place in economics towards defining concepts "not in terms of properties, but in terms of the series of operations by which they are measured" an in the physical sciences (p. 652). They praised the development of econometric methods as a step in such direction (making the deductive process more rigorous), but they deplored the excessive attention devoted in this field to mathematical technicalities and counted Keynes (together with Evans, Fisher, Schultz, and Robertson) among the few who explored the underlying theoretical premises upon which econometric investigation should rely.

They reject King's point of view, which they take as representative of the classical and neoclassical tradition, and argue for general demand functions including many arguments (such as prices of other goods and time) from which the classical (Cournot-Marshall) demand function $D=F(p)$ is derived as a special case (by holding other things, including time, constant). As for the identification problem they wrote correctly: "unless the supply curve shifts, it is impossible to determine the demand curve at all".

In his rejoinder Kings (1939b) expressed a clear *a priori* anti-econometrics position:

"I consider that statistical and mathematical processes can, by themselves, but rarely be relied upon to establish economic laws or relationships, and that when findings are based purely upon the results of such procedures they are even more likely to be invalid than when they are based solely upon deductions drawn form everyday observations… in the economic field, statistics and mathematics are mainly useful for verifying and reducing to quantitative terms concepts which have first been worked out thoroughly by a process of deduction from facts commonly observed. [Roos and von Szeliski], on the other hand, believe in relying almost entirely upon the inductive method" (p. 664)

[22] The reference was to: Roos C.F. and V. von Szeliski, *The Dynamic of Automotive Demand*, published by General Motors Corporation in 1939, and "The concept of demand and price elasticity. The dynamics of automobile demand", *Journal of the American Statistical Association*, 34, 1939, pp. 652-664.

In a further reply Roos and Szeliski (1939c) reaffirmed their methodological standpoint, and the need for statistical analysis in demand studies:

"The issue here is above all how the method of analysis can be related to the theoretical background or what kind of techniques are required by the theoretical background. […]We do not rely almost entirely on induction and[…]we regard every-day observations pertaining to the industry as of utmost importance." (p. 665)

Keynes's reply to Szeliski of December 19, 1939 provides a restatement his views on the proper role for econometric methods and thus supports our view that Keynes is not an *a priori* anti-econometrician.

"In reply to your letter of November 1921, it is now some time since I looked through your study of automobile demand, and only a general impression is left in my mind. This general impression, however, is to the effect that *you have chosen just the sort of problem where multiple correlation methods may be useful. You are dealing with details of a specific problem where the main causes are pretty well known a priori, and where the statistics are definite and precise.* The method is always full of danger, but, in my opinion, *it is the kind of problem to which you have applied it rather than in those to which Tinbergen has applied it that the method is properly in place*" (CO/11/445, emphasis added)

5. Concluding remarks

Keynes is not a critic and an opponent of econometric work *per se*. Our reconstruction shows that there is no evidence for this interpretation. What he opposed were the attempts at statistical inference without any prior effort of ascertaining the suitability of the economic material for making such inferences. At the core of Keynes's criticism of Tinbergen's work there is the question of methodology. He disputed the legitimacy of inductive methods in the form of correlation analysis applied to economic matters. He argued that there was no reason to expect the behaviours were stable over the long run, and so there was no reason to infer stable correlations. Keynes's stance in relation to Tinbergen is fundamentally similar to the one adopted in his *Treatise on Probability*.
"The slippery problem" of passing from statistical description to inductive generalisation, which Keynes had shown nineteen years earlier to be relevant for the case of simple correlation, [*prima facie*] seemed to him to arise also for the multiple correlation method.. Keynes focused upon the inductive aspects of Tinbergen's analysis and examined whether the implicit 'fundamental assumptions' he made - uniformity and homogeneity of the environment over a period of time, completeness of the list of the significant causes, measurability of all the significant factors, mutual

independence of factors - were legitimate. Only if the conditions for inductive generalisation are met does it become possible to use the method of multiple correlation for disentangling the laws of action of the forces at the work. In Keynes's opinion "every one of these conditions is far from being satisfied" (p. 286) in the field of business cycle:

"The successful application of this method to so enormously complex a problem as the business cycle does strike me as singularly unpromising project *in the present state of our knowledge*" (emphasis added).

Econometricians at first took Keynes's criticism in earnest. The debate went on at least until Koopmans (1941), but came to a rupture , when Haavelmo (1943) wholly restated econometric methods in probabilistic terms. Haavelmo began his "Statistical Testing of Business Cycle Theories" by remarking that the criticisms directed at Tinbergen's study went beyond technical matters but often implied instead that Tinbergen "had tried to go too far with statistical methods"(ibid.). Keynes is explicitly (and incorrectly) identified as a believer in the supremacy of "the noble art of theoretical deductions based on 'general economic considerations'". Rather than focussing on the more technical issues, and discussing them one by one, Haavelmo cut the ground beneath them by a change of paradigm that makes them irrelevant.

The first key point is that any model is seen as a formal logical construction, such that a non-logical jump is always needed in the end: however complex the formal construction is, "we shall not, by logical operations alone, be able to build a complete bridge between our model and reality". Actual data series are to be somehow arbitrarily chosen as counterparts of theoretical variables, and a statement deduced for the latter is made about the former. Verifying such a statement does not imply accepting the theory, though, as "the same statement might usually be deduced from many different constructions". In this context, Haavelmo gets rid of the worry about the completeness of the list of causes: a regression equation containing an incomplete list of causes "means only the testing of a somewhat simpler hypothesis" and is likely to produce "an addition to our knowledge".

Haavelmo's second key point is that both theoretical and observed variables should be redefined as stochastic objects. He claims that this is necessary for "an objective and intelligent discussion of such questions as those of Lord Keynes." The objective of statistical testing becomes "to draw some inference […] as to which of these mechanisms (probability laws) actually produced the data" (p.17). In order to be tested, a business cycle theory must then take the form of hypotheses regarding joint probability laws and allowing for probability statements about facts, which leave room for type I and type II errors:

"We now have the possibility that the theory might be true even when the deduced statement about the facts turn out to be wrong. Also, the theory might be wrong […] while the statement it makes about the facts might sometimes be true" (ibid)

In fact, it is accepted theories are undistinguishable from the point of view of observations and that such problem cannot be eliminable: "Theories with *different economic meaning* might lead to exactly the same probability law… just as different pairs of supply and demand curves might have the same intersection point." [italics in the original] (p. 18)

Haavelmo provided the basis for much of the methodology of the Cowles Commission. With the establishment of this approach, Keynes-type discussions were increasingly ignored and the slippery issue of causal inference was kept in the econometric closet for over thirty years (see Leamer 1983). Today, however, econometricians recognize that most of the problems Keynes raised were real and his warnings on the specific question of business cycle are still relevant, even if econometrics has made considerable efforts to overcome the difficulties. It is also the general opinion that Keynes's criticism seems to be overly harsh within the context of contemporary econometrics of 1939-40.

What motivated such harshness? The answer does not lie in Keynes's temperamental characteristics, as suggested by Stone (1978). Rather, on the fact that from the mid-1930s on Keynes had noticed that a new anti-Marshallian conception on the nature, method and style of economics – opposed to what he considered correct – was meeting with increasing success (see Marchionatti 2002 and 2004). What's more, this new approach was appropriating his own work. Keynes was seriously worried about the emerging tendency to use statistical and mathematical methods to formalise economic analysis. This "large proportion of recent mathematical economics… assumes strict independence between the factors involved and lose all their cogency and authority if this hypothesis is disallowed", he wrote in the *General Theory* (Keynes 1936, p. 297). On this basis, his judgement was strongly negative:

"Too large a proportion of *recent mathematical economics* are merely concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols" (ibid., p. 296)

By "recent mathematical economics," Keynes was referring to those economists who subscribed to the *Econometric Society* program. This can be asserted on the basis of the little explicit evidence available – his correspondence with Harrod and, above all, with Ragnar Frisch in the 1930s[23]. The *Econometric Society* was founded in 1930. Its program – set out in the editorial of *Econometrica* by

[23] The correspondence with Ragnar Frisch is concentrated in the period 1932-1936. A discussion of it is in Louçã 1999.

Ragnar Frisch – was "to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems" and "that are penetrated by constructive and rigorous thinking similar to that which has come to dominate in the natural sciences" (Frisch 1933, p. 1). As emerges from his correspondence with Frisch, Keynes's mistrust in "recent mathematical economics" concerned:

a) the imprecision of assumptions, which are often 'special', but covered by a maze of symbolism; for example, the assumption of strict independence between the factors (often present in mathematical works), excludes the consideration of complexity;
b) b) the un-clear application of conclusions.

Keynes's concern was due to the fact that his own ideas – from the *Treatise on Money* to the *General Theory* –were having a relevant impact on young econometricians.[24] Many of them, such as Frisch and Tinbergen, thought that an important goal of economics was to create a basis for practical measures to be implemented in order to fight economic crisis and unemployment. Keynes's theoretical analysis in *General Theory* and his emphasis on monetary and fiscal policies made his work extremely important for the econometricians as a theoretical structure suitable for quantitative analysis of those problems. In this sense it met the early Keynesians who saw the *General Theory* as a "machine for policy" (Skidelsky 1992, p. 538). According to econometricians Keynes's theory, originally expressed in literary form, had to be translated into mathematical form as a system of equations to emphasise the basis hypotheses in a formal and simpler framework. Immediately after its publication, Keynes's *Theory* was discussed in *Econometrica*'s circle. The first version of Hicks' paper, which contained the famous IS-LM model of Keynes's theory, had been presented and discussed to the sixth European meeting of the Econometric Society at Oxford in September 1936 and published in *Econometrica* in April 1937, just after Harrod's and Meade's papers. After that, this simultaneous equation interpretation became its dominant interpretation – i.e. a simplified version of the *General Theory* in the form of a specified model that offered a mathematical framing for the theory -, even though this was at odds with Keynes's original formulation. The tendency to accept only a part of his work while rejecting the rest had already worried Keynes, when he was discussing various issues of the *General Theory* with Harrod:

"I am frightfully afraid of the tendency of which I see signs in you [Harrod], to appear to accept my constructive part and to find some accommodation between this and deeply cherished views which would in fact be only possible if my constructive part had been partially misunderstood" (Keynes to Harrod, 27 August 1935, in Keynes 1973a, p.548).

[24] Tinbergen himself, reviewing in 1935 the recent business cycle theories, devoted great attention to the parts of Keynes's *Treatise of Money* "which give very pertinent remarks on the business cycle problems" (p. 266). Tinbergen classifies Keynes's theory as a semi-mathematical one and argues for its mathematical treatment.

Keynes reacted against the dangers of encapsulating his theories in a limited formal model in his famous 1937 article on the *Quarterly Journal of Economics*. There he emphasised the importance of factors like uncertain knowledge, in presence of which ”there is no scientific basis on which to form any calculable probability whatever” (p. 113) and rational calculation is of little use. At that time Keynes probably became aware that a convergence was to be realised among what we can call the early ‘neoclassical synthesis’ interpretation of the *General Theory* and the interpretation given by the econometricians. Tinbergen epitomised this tendency at its best. On the one hand he reintroduced a conception of economics and its method that Keynes (as Marshall before him) had rejected, on the other hand he put forward a usage of statistical inference that Keynes had criticised. Enough to explain Keynes’s virulence.

REFERENCES

Allen, R.G.D. (1940), “Review of *Statistical Testing of Business –Cycle Theories, Vol. I: A Method and Its Application to Investment Activity: Statistical Testing of Business-Cycle Theories. Vol. II: Business Cycles in the United States of America, 1919-32*”, *Economica*, 7, 335-339.

Anonymous (J.E.W.) (1940), “Review of *Statistical Testing of Business –Cycle Theories, Vol. I: A Method and Its Application to Investment Activity: Statistical Testing of Business-Cycle Theories. Vol. II: Business Cycles in the United States of America, 1919-32*”, *Journal of the Royal Statistical Society*, 103, 256-259.

Bateman, B. W. (1990), "Keynes, induction and econometrics", *History of Political Economy*, 22:2, 359-379.

Broster, E.J. (1938), “Variability of Railway Operating Costs”, *Economic Journal*, 48 (192), December, pp. 674-684

Carabelli, A. (1988), *On Keynes’s Method*, London: Macmillan.

Cuyvers, L. (1983), “Keynes collaboration with Erwin Rothbarth”, *The Economic Journal*, 93, 629-36.

Dharmapala, D. and McAleer, M. (1996), “Econometric Methodology and the Philosophy of Science”, *Journal of Statistical Planning and Inference*, 49, pp. 9-37.

Epstein, R. (1987), *A History of Econometric Ideas*, Amsterdam: North Holland

Frisch, R. (1933a), “Editorial”, *Econometrica*, 1, p. 1-4.

Frisch, R. (1933b), "Propagation and Impulse Problems in Dynamic Economics", in *Economic Essays in Honor of Gustav Cassel*.

Garrone, G. and Marchionatti, R. (2004), “Keynes on econometric method. A reassessment of his debate with Tinbergen and other econometricians, 1938-’43”, *CESMEP Working Paper*, Department of Economics, University of Torino.

Haavelmo, T. (1943), "Statistical Testing of Business Cycle Theories", *Review of Economics and Statistics*, 25: 13-18.

Hamouda, O.F. and Smithin, J.N. (eds), *Keynes and Public Policy after Fifty Years*, vol. 2, E.Elgar.

Harrod, R. (1938), “Scope and Method in Economics”, *Economic Journal*, 48, September, pp. 303-412.

Harrod, R. (2003), *The Collected Interwar Papers and Correspondence of Roy Harrod*, edited by D. Besomi, two vols. Cheltenham, Edward Elgar.

Hendry, D.F. (1980), "Econometrics - Alchemy or Science ?", *Economica*, 47, August, 387-406.

Hendry, D.F. and Morgan, M.S. (1995) eds. *The Foundations of Econometric analysis*, Cambridge University Press.

Hicks, J.R. (1937), "Mr. Keynes and the 'Classics': A suggested interpretation", *Econometrica*, 5, pp. 147-159.

Kalecki, M. (1944-5), "The Work of Erwin Rothbarth", *The Review of Economic Studies*, 12 (2), pp. 121-2.

Keuzenkamp, H. A. (2000), *Probability, Econometrics and Truth. The methodology of econometrics*, Cambridge: Cambridge University Press.

Keynes, J.M. (1921), *The Treatise on Probability*. Now in *The Collected Writings of J.M. Keynes*, vol. VIII, London, Macmillan for the Royal Economic Society, 1973.

Keynes, J.M. (1926), "Francis Ysidro Edgeworth, 1845-1926", *The Economic Journal*, March. Now in *The Collected Writings of J.M. Keynes*, vol. X, *Essays in Biography,*, Macmillan for the Royal Economic Society, 1973.

Keynes, J.M. (1928), "Review of Frederick C. Mills, *The Behaviour of prices. A report of an investigation*", *The Economic Journal*, 38, December. Now in *The Collected Writings of J.M. Keynes*, vol. XI, Macmillan for the Royal Economic Society, 1973.

Keynes, J.M. (1930), *A Treatise on Money*, Macmillan Press. Now in *The Collected Writings of J.M. Keynes*, vols. V and VI, Macmillan for the Royal Economic Society, 1971.

Keynes, J.M. (1936), *The General Theory of Employment, Interest and Money*, London, Macmillan Press. Now in *The Collected Writings of J.M. Keynes*, vol. VII, London, Macmillan for the Royal Economic Society, 1973.

Keynes, J.M. (1937), "The General Theory of Employment", Quarterly Journal of Economics, 51 (2), pp. 209-223. Now in *The Collected Writings of J.M. Keynes*, vol. XIV.

Keynes, J.M. (1939), "Professor Tinbergen's Method", *Economic Journal*, 49, pp. 558-568. Now in *The Collected Writings of J.M. Keynes*, vol. XIV, *The General Theory and After. Part II. Defence and Development*, Macmillan for the Royal Economic Society, 1973.

Keynes, J.M. (1940) "Comment", *The Economic Journal*, March. Now in *The Collected Writings of J.M. Keynes*, vol. XIV, *The General Theory and After. Part II. Defence and Development*, Macmillan for the Royal Economic Society, 1973.

Keynes, J.M. (1973a), *The Collected Writings of J.M. Keynes*, vol. XIII, *The General Theory and After. Part I. Preparation*, Macmillan for the Royal Economic Society.

Keynes, J.M. (1973b), *The Collected Writings of J.M. Keynes*, vol. XIV, *The General Theory and After. Part II. Defence and Development*, Macmillan for the Royal Economic Society.

King, W.I. (1939), "Can production of Automobiles be Stabilized by Making Their Prices Flexible?", *Journal of the ASA*, December 1939, pp. 641-651.

King, W.I. (1939b), "The concept of demand and price elasticity. The dynamics of automotive demand: A Rejoinder ", *Journal of the ASA*, December 1939, pp. 664-5.

Klant, J. (1989), "The Slippery Transition", in T. Lawson and H. Pesaran (eds) *Keynes' Economics: Methodological Issues*, London: Croom Helm.

Klein, L. (1951), "The Life of J. M. Keynes", *Journal of Political Economy*, 59, pp. 443-51.

Koopmans, T.C. (1941), "The Logic of Econometric Business-Cycle Research', *Journal of Political Economy*, 49: 157-181.

Lawson, T and Pesaran, H. (eds) (1985), *Keynes' Economics: Methodological Issues*, London: Croom Helm.

Lawson, T. (1985), "Keynes, prediction and econometrics", in T. Lawson and H. Pesaran (eds), *cit.*

Lawson, T. (1989), "Realism and Instrumentalism in the Development of Econometrics", in N. de Marchi and Gilbert (eds), *History and Methodology of Econometrics*, Oxford, Clarendon Press.

Leamer, E. (1983), “Let’s Take the Con Out of Econometrics”, *American Economic Review*, 73, pp. 31-43.

Leeson, R. (1998), “The Ghosts I Called I Can’t Get Rid of Now: the Keynes-Tinbergen-Friedman-Phillips Critique of Keynesian Macroeconometrics”, *History of Political Economy*, 30(1), pp. 51-94.

Louçã, F. (1999), “The econometric challenge to Keynes: arguments and contradictions in the early debate about a late issue”, *The European Journal of the History of Economic Thought*, 6, 404-438.

Marchionatti, R. (1999), “On Keynes’s animal spirits”, *Kyklos*, 52: 415-39.

Marchionatti, R. (2002), “Dealing with Complexity: Marshall and Keynes on the Nature of Economic Thinking”, in *The Economics of Alfred Marshall. Revisiting Marshall’s Legacy*, edited by R. Arena and M. Quéré, London: Palgrave-Macmillan.

Marchionatti, R. (2004), “What *don’t* economists know now that a century ago Marshall did ? A note on Marshall sophisticated informality”, *Journal of Post-Keynesian Economics*, Spring 2004, pp. 441-460.

Marschak, J. and Lange, O. (1940), "Mr. Keynes on the Statistical Verification of Business Cycle Theories" in Hendry, D.F. and Morgan, M.S. (1995), cit. pp. 390-398.

McAleer, M (1994), “Sherlock Holmes and the Search for Truth: A Diagnostic Tale”, *Journal of Economic Survey*, 8, pp. 317-70.

Moggridge, D.E. (ed, *Maynard Keynes: An Economist’s Biography*, London, Routledge.

Morgan, M.S. (1990), *The History of Econometric Ideas*, Cambridge, Cambridge University Press.

O’Donnell, R. (1997), “Keynes and Formalism”, in G.C. Harcourt and P.A. Riach, *A ‘Second Edition’ of the General Theory*, vol. 2, London: Routledge.

Patinkin, D. (1976), “Keynes and econometrics: on the interaction between the macroeconomics revolutions in the interwar period”, *Econometrica* 44: 1091-1123.

Pearson, K. (1897), “Mathematical contributions to the theory of evolution - On a form of spurious correlation which may arise when indices are used in the measurement of organs”. Proceedings of the Royal Society of London, 60, 489-497.

Pesaran, H. and Smith, R. (1985), “Keynes on Econometrics”, in T. Lawson and H. Pesaran (eds), *cit.*

Roos, C.F and Szeliski, V. (1939), *The Dynamic of Automotive Demand*, General Motor Corporation.

Roos, C.F. and Szeliski, V. (1939b), “The concept of demand and price elasticity. The dynamics of automotive demand”, *Journal of the American Statistical Association*, 34, pp. 652-664

Roos, C.F. and Szeliski, V. (1939c), “The concept of demand and price elasticity. The dynamics of automotive demand: A Further Reply”, *Journal of the American Statistical Association*, 34, pp. 665-6.

Rothbarth, E. (1938), “An Econometric Approach to Business Cycle Problems”, *Economica*, 5 (20), pp. 488-91.

Rothbarth, E. (1941), “ Review of Statistical Testing of Business-Cycle Theories: II. Business Cycle in the United States of America, 1919-32”, *Economic Journal*, 51, 293-7.

Rowley, R. (1988), “The Keynes-Tinbergen Exchange in Retrospect”, in O.F. Hamouda and J.N. Smithin (eds), *Keynes and Public Policy after Fifty Years*, vol. 2, E.Elgar, pp. 23-31.

Samuelson, P. (1946), ‘Lord Keynes and the *General Theory*’, *Econometrica*, 14 (July), pp. 187-200.

Skidelsky, R. (1983), *John Maynard Keynes*. Vol. I, London: Macmillan.

Slutsky, E. (1937), "The Summation of Random Causes as the Source of Cyclical Processes", 1927 (in *Econometrica*, 1937)

Staehle, H. (1937) “Short-period Variations in the distribution of incomes and the ‘Propensity to Consume’”, *Review of Economic Statistics*, 19, pp. 133-143.

Staehle, H. (1938), "New considerations on the distribution of incomes and the 'Propensity to Consume'", *Review of Economic Statistics* , 20, pp. 134-141

Staehle, H. "A rejoinder", *RES*, 21, 1939, pp. 129-130

Stone, R. (1978), *Keynes, Political Arithmetic and Econometrics*, Proceedings of the British Academy, Vol. 64, Oxford: Oxford University Press.

Tinbergen, J. (1935), "Annual Survey: Suggestions on Quantitative Business Cycle Theory", *Econometrica*, pp. 241-308.

Tinbergen, J. (1937), *An Econometric Approach to Business Cycle Problems*, Impasses Economiques,Herman &C., Paris.

Tinbergen, J. (1939a), *Statistical Testing of Business-Cycle Theories*, vol. I: *A Method and Its Application in Investment Activity*, Geneva: League of Nations.

Tinbergen, J. (1939b), *Statistical Testing of Business-Cycle Theories*, vol. II: *Business Cycles in the USA, 1919-1932*, Geneva: League of Nations.

Tinbergen, J. (1940a), "On a Method of Statistical Business Research. A Reply", *Economic Journal*, March, pp. 141-54.

Tinbergen, J. (1940b), "Econometric Business Cycle Research", *The Review of Economic Studies*, 7, 73-90.

Tintner, G. (1941), "Review of *Business Cycles in the United States of America, 1919-1932*", *Journal of Political Economy*, 4, 618-22.

Yule (1927), "On the Method of Investigating Periodicities in Disturbed Series", 1927, *Philosophical Transactions of Royal Society.*