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WORKING PAPER SERIES

Keynes on econometric method.

A reassessment of his debate with Tinbergen and other econometricians, 1938-1943

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1. Introduction

In the 1920s and 1930s a radical change occurred in the theoretical and methodological approach in economics, which laid the foundations for the mainstream of economic science in the second part of twentieth century. This change was essentially the result of a criticism of the "classical situation" – to use Schumpeter's expression –, represented by Marshall's work and legacy, by a new generation of economists who called for a reconstruction of the economic science along more rigorous lines. They conceived economic theory as the field of application of exact logic and adopted the methods of natural science, which they thought would alone guarantee the clearness and rigour necessary for the theory as well as for the empirical research in economics (Marchionatti 2001). In this context econometrics emerged as part of the 'modern models', which were conceived as an applied development of Walras's and Pareto's mathematical economics (Tinbergen 1949, Schumpeter 1954). Many economists expressed doubts and objected to this new approach. Keynes was one of them. In the late 1930s he debated on method with Jan Tinbergen and other leading figures of the emerging field of econometrics. Since the 1940s his criticism was substantially rejected, and his conception of economics considered old-fashioned. Recently, more and more attention has been devoted to reflecting on the performance of the twentieth-century economics, particularly after what Bowles and Gintis (2000) have termed the "Walrasian detour". In particular, the question of the appropriate style of economic discourse has again been considered a topic worth discussing, along with the role of formalism and mathematics. (See: Krugman (1998), Mc Closkey (1997), Bowles and Gintis (2000), Marchionatti (2002). The 1930s discussion on mathematical economics and econometrics should be reconsidered in this 'revisionist' context.

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[1]. It was also an innovative contribution from the point of view of testing procedures (Morgan 1990, p.108-114). The work was expected both to provide general economic forecasts and to guide government policies to control business cycle (Epstein 1987, Hallett 1989). The first volume of the report, on which Keynes chose to focus, contained an explanation of the method of econometric testing. Tinbergen presented it as a

[1] In the 1920s institutions like NBER and IFO were established to study business cycles in a descriptive way. Yule (1927), Slutsky (1937, a revised English version of a Russian paper of 1927), 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 built 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 aiming at closing the gap between economists and mathematicians in the statistical study of business cycles.

synthesis of statistical business cycle research and quantitative economic theory, in the spirit of *Econometrica*'s program. 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 the theory provides a business cycle mechanism or not. The first volume was followed by a second one, entitled *Business Cycles in the United States of America, 1919-1932* (1939), where Tinbergen applied his method to annual data for the United States and endeavored to construct an economic model of the economic system taking into account all the important factors influencing business cycles in the country in the post-war period.

Tinbergen's work raised immediately a lively debate, known as the Tinbergen debate, with contributions of Keynes, Frisch, Friedman and many others (see Leeson 1998, and Louçã 1999). Keynes's 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). The assessment of Keynes's criticism remains controversial, but the long prevailing view is that Keynes was an *a priori* anti-econometrician. Samuelson (1946) maintained that he was technically incompetent. Klein (1951) called his review "one of his sorriest professional performance" (p. 450). Stone (1978) maintained that Keynes had little or no awareness of the economic literature. He wrote that his 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 instead recognised the relevance of Keynes's criticism. 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). Pesaran and Smith (1985) and Rowley (1988) went beyond the simple consideration of technical issues. Pesaran and Smith (1985) recognised that Keynes was right on both the technical and logical arguments: on the technical level "the problems that Keynes raised were real, despite his occasional technical confusion" (p. 144). At a logical level "econometric inference .. is insupportable" (p. 147). 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; see also Dharmapala and McAleer 1996) writes that some of Keynes's criticisms of Tinbergen's 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 now recognised that Keynes’s criticism of Tinbergen was sound in many points. However, it is considered overly harsh and Keynes is blamed for throwing out the baby with the bath water.

This article reconstructs Keynes’s discussion of the role of econometrics in the economic discourse in a time perspective longer than is usually considered in the literature. In the second and third 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 conception of economics, his alleged anti-econometrics attitude and proposes an interpretation of the harshness of his criticism.

1. The Keynes-Tinbergen debate on econometric method, 1938-1940

2.1. The story of the debate

The Keynes-Tinbergen debate went through two different phases. The first phase 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 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 witnessed by letters in July and early August 1938 to Roy Harrod.[2] While Harrod looked favourably at Tinbergen’s work, Keynes expressed perplexities, essentially based on his view on the appropriate role of mathematics and statistics in economics, 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

[2] 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 discussed.

matter", Tinbergen's work was "all hocus" (letter to Kahn, 23 August 1938, in Keynes 1973b, p. 289, see also the letter to Harrod of 23 August 1938, in Keynes 1973b, p.332), because "there is not the slightest explanation or justification of the underlying logic" (ibid., p. 289). These early negative impressions were confirmed in a long letter to R. Tyler two weeks later, in which Keynes outlined the fundamental elements of his criticism of Tinbergen's method of analysis. While he recognised the importance of testing "the quantitative influence of factors suggested by a theory" (ibid. p. 289), 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. Tinbergen's letter supported Keynes's critical feeling that the work was methodologically weak, which made the results obtained of little practical value. 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). Although based on questions he had already raised and employing some previous reasoning, his critical discourse on the whole appeared more complete, and very effective from a rhetorical point of view.

Tinbergen was astonished by Keynes's harsh reaction to his work (see the letter to Keynes of December 18, 1939). He extensively replied to Keynes's "serious" questions in the March 1940 issue of the *Economic Journal*.[3] In his "comment" (Keynes 1973b) Keynes defined Tinbergen's reply "very valuable", 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 him continue" (ibid. p. 320).

2.2. Keynes's criticism: "This brand of statistical alchemy [econometrics] is[not] ripe to become a branch of science"

Keynes (1973b [1938] and 1939) stated first 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. Finally, turning back to methodological grounds, he raised the problem of passing from statistical description to inductive generalisation.[4]

The logical condition for using the method of multiple correlation, Keynes wrote, 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, Keynes 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)

[3]In the same year 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 (see p. 236).

[4] 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). In our paper we focus instead on the essential points of Keynes's criticism, which may be considered long-lived in a historical perspective.

Then Keynes raised a set of detailed questions about the conditions of validity of Tinbergen's procedures[5].

The first condition Keynes enunciated was the completeness of significant causes. Keynes asked: "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 what is called today omitted variable bias. Only if they are included, and if "the economist has correctly analysed beforehand the *qualitative* character of the causal relations", he can then examine their quantitative importance, i.e. how strongly each of them operates. For Keynes this is the primary role of econometrics. It is quite different from affirming, 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.[6] 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). Tinbergen claimed that "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 issue was the independence of factors. First Keynes raised the problem of spurious correlation: "If we are using factors which are not wholly independent, we lay ourselves open to the

[5] 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).

[6] 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. As noted in Keuzenkamp (1995), the idea that scientists cannot prove a theory but may be able to falsify it was "a common sense notion in the statistical literature since (at least) the turn of this century" (p. 240).

.. complications of ‘spurious’ correlation” – a term introduced by K. Pearson (1897) in a discussion of correlation between indices. Then he drew attention to the problem of simultaneity:

“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 raised two questions of technical importance concerning the functional forms, the time lags and trends. First Keynes maintained the implausibility of the widespread assumption of linearity and called for the examination of alternative functional forms. About the general problem of dynamic specification, Keynes accused Tinbergen of scarce rigour in treating time lag 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 him 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, Keynes went back to what he considered the critical condition, that of the likely structural instability putting the constancy of the parameters into question (Keynes 1938):[7] “the

[7] Pesaran and Smith (1985) re-estimated some of Tinbergen’s relations by the OLS method using the original undetrended series. (Their purpose was to examine the effect of de-trending on Tinbergen’s results). They found that “the un-detrended OLS results suffer from a significant degree of residual autocorrelation which sheds considerable doubt on the size and the statistical significance of the estimated regression coefficients … The method of de-trending employed by Tinbergen can, and often does, deal with the problem of residual autocorrelation. *But … its application can also introduce erroneous dynamics into the relation and its residuals*” (p. 141-2, *emphasis added*). The presence of residual autocorrelation can be due to the factors stressed by Keynes: omitted variables, functional form misspecification, structural change and a host of other factors “all of which are highlighted in Keynes’ review” (p. 143).

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" (p.286). This issue is directly connected with the problem of 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 I used to be occupied in examining in the case of simple correlation". He was referring to his work then published in the *Treatise on Probability* (1921), in which he 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), that is, it depends "not on a matter of fact [the empirical confirmation], but on the existence of a relation of probability" (ibid., p. 245). In fact "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.). Inductive reasoning makes use of analogy. Keynes demonstrated that the method of reasoning by means of analogy breaks down if the system analysed is not-homogeneous and an organic complex: as a consequence induction becomes not possible. According to Keynes, a low degree of homogeneity and a high degree of complexity are a peculiarity of an economic system.[8],[9]

Let's now go back to the criticism of Tinbergen on problem of inductive generalisation. Keynes asked:

[8] The inductive hypothesis is logically founded, for Keynes, on the principle of limited independent variety. It stated 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. Only with regard to finite independent variety systems, "probable knowledge can be validly obtained by means of an inductive argument" (ibid., p. 280). 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. However, as Keynes (1933) wrote discussing Edgeworth's *Mathematical Psychics*, "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 1933, p. 262). An exhaustive account and extensively discussion of the inductive reasoning in the *Treatise on Probability* is in Carabelli (1988). See also Klant (1989), Lawson (1989), Bateman (1990), Keuzenkamp (2000).

[9] 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 probabilistic way, are the cases of long-term expectation and the business cycle. Long-term expectation depends 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. If expectations and investment cannot be modelled with probabilistic relations, also the business cycle too has to be beyond the domain of probabilistic inference.

"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)

Keynes maintained that Tinbergen made "the least possible preparation for the inductive transition" (p. 316). The period under examination should have broken up 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)[10]. If this is the case, then "there is some grounds for projecting the results into the future" (ibid.). Tinbergen failed to follow this procedure:

"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

[10]The genesis of this procedure is also 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 more elementary cases "where adequate statistics exist" (ibid.). A type of problem to which the multiple correlation method can be applied is cited in his letter to Tyler: the case of the demand for investment in new rolling stock. At that time he was publishing an article written by the English statistician E. J. Broster in the *Economic Journal* 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-mil[illegible]n-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".[11]

Keynes's conclusion was that Tinbergen needed to demonstrate that his method was applicable, rather than simply applying it. For, when applied inappropriately, the method could result in "a false precision", beyond "what either the method or the statistics actually available can support" (Keynes 1973b, p. 289).

2.3. Tinbergen's reply: "The proof of the pudding is in the eating"

The core of Keynes's discussion was the issue of the logical conditions for applying the method of multiple correlation -- that is, a problem that precedes its application, and to which the technical questions were subordinate. Tinbergen's reply avoided instead, 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. However, he did not offer any systematic technical methodology for dealing with the problems under discussion, although he seemed to anticipate some contemporary advances (see Dharmapala and Mc Aleer 1996 and Mc Aleer 1994).

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[11] On December 19, 1939, Keynes, answering to a letter of E. J. Broster, wrote: ""The general line you take is interesting and useful. It is, of course, not exactly comparable with mine. 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*)

Regarding the need for *a complete list* of the relevant factors – i.e., for a correct specification -, Tinbergen assumed that "the factors included are *comprehensive as far as the more important are concerned*" (*italics 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 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*, arguing that, for statistical purposes, explanatory factors needed to be uncorrelated rather than independent in an economic sense.
Regarding the constancy of the coefficients, he explained that it was assumed 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 with 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)

Finally, 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”. Mainly interested in getting on with the job, he concluded: “The proof of the pudding is in the eating”.

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*, published in the June-September 1941 issue of the *Economic Journal*, sheds further light in understanding Keynes’s real attitude towards statistical-econometric work. Kalecki defined it “a model of careful econometric analysis” (Kalecki 1944-5, p. 121). The author was Erwin Rothbarth, a twenty-eight old German economist who had emigrated to England after Hitler’s rise to power. At that time he 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*. His highly competent discussion of Tinbergen’s analysis of the Dutch economy was preceded by the acknowledgement of the “unassailable” case for the econometric method, “forced on the economist” by the fact that “the system as a whole acquires a certain measure of stability by the interactions of a fairly large number of not very stable relationships” (p. 489) and by the need “to be able to exclude some possibilities at least on empirical grounds”. However, Rothbarth expressed caution as regards how far the econometric approach could go: “the advance of this branch of economics seems to be bound up with the advance realised in the theory of time series”.

Rothbarth considered Volume two of Tinbergen's study a "brilliant pioneering effort". Again, before discussing Tinbergen's results, Rothbarh highlighted the relevance of his attempt to demonstrate that it is possible to construct a mathematical model of the trade cycle which is 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 1940, p. 294). In fact, Rothbarth 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. As regards the treatment of long term interest rates, he highlighted, in a Keynesian line of reasoning, the potential importance of immeasurable factors. In considering whether the model can account for longer cycles, Rothbarth once more revealed his familiarity with the econometric methods and his quantitative-oriented mind, checking whether an increase in the period of the system could be caused by "a moderate variation in the coefficients … - such as might arise from sampling error" (ibid., p. 296). Finally, he also raised the issue of collinearity and the problem of the degrees of freedom, which Tinbergen "seems entirely to neglect". Rothbarth concluded by stressing a recommendation to "professor Tinbergen and his adherents" in favour of smaller models - which is consistent with Keynes's methodological suggestions. In fact, constructing smaller models implies that a greater responsibility is placed on the economist before he passes the material to be analysed on to the statistician, that is, more weight given to economic theory and the investigation of the economic material previous to manipulation of data. The very last paragraph 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).

3. The econometricians and Keynes, 1939-1943. From reconciliation to rejection

The econometricians' first reactions to the debate consisted in careful considerations of the issues raised by Keynes. The reviews of Tinbergen's League of Nations study often mentioned Keynes's criticism. Allen (1940) considered Keynes's questions "pertinent". Tintner (1941) agreed with Keynes that the 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 (without quoting him) on factors measurability, the constancy of coefficients, the linearity, etc. Bartlett (1940) noted that Keynes "set out with gusto the host of statistical difficulties that still remain [in investigations of Tinbergen-type] – the validity of the data, the measurability of all relevant variables, the linearity of the relations, the absence of specified time-lags, the stability of the series" (p. 18). Keynes's attack also prompted some attempts by eminent econometricians to reconcile his criticism with statistical-econometrical work. Three cases appear to be particularly interesting because Keynes was directly involved in expressing his opinion on them in his correspondence between 1939 and 1941. The first case is an exchange of letters with Victor Szeliski of the Institute of Applied Econometrics, New York,[12] about the use of the multiple correlation methods in the study of automobile demand. The second case is the well-known article written by Jacob Marschak and Oskar Lange in defense of Tinbergen, submitted to the *Economic Journal* for publication but rejected by Keynes. The third case is an exchange of letters with Tjalling Koopmans over his 1941 paper "The Logic of Econometric Business-Cycle Research", which was a clear restatement of Tinbergen's method. The attempts at reconciliation end with Trygve Haavelmo (1943), who introduced full probability reasoning in econometrics.

3.1. Three exercises in reconciliation

3.1.1. Szeliski, 1939: the method 'properly in place'

On November 1939 Keynes received a letter from Victor Szeliski who had read Keynes's review of Tinbergen's study "with considerable interest and approval", and "naturally" wondered to what

[12] 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*.

extent Keynes though “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)[13].

This study on “Factors Influencing Automobile Demand”, part of a research project commissioned by General Motors, investigated the determinants of demand for automobiles and estimated its price-elasticity, among other things. The study was critically reviewed by Willford I. King, president of the American Statistical Society. On top of raising 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 (1939a) expressed a general distrust in inductive methods. Roos’s and Szeliski’s reply (1939b) throws light on their approach to the role and application of econometric methods and to their relation to the premises of economic theory. They begin by wishing for economics an analogous shift as took place in physical sciences, where “concepts are to be defined, not in terms of properties, but in terms of the series of operations by which they are measured” (p. 652). They then praised the development of econometric methods as a step in such direction, but lamented the focus on mathematical technicalities and counted Keynes among the few who explored the theoretical premises upon which econometric investigation should rely. They 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 by holding other things (including time) constant. They claimed that, far from “eliminating” the effects of external influences, they had determined several dynamic demand functions, each of which “is a family of curves, not *the* curve” [italics in the original]. As for the identification problem they correctly pointed out that “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

[13] The reference was to Roos C.F. and V. Szeliski, (1939a) and (1939b).

concepts which have first been worked out thoroughly by a process of deduction from facts commonly observed. [Roos and Szeliski], on the other hand, believe in relying almost entirely upon the inductive method" (p. 664)

In a further reply Roos and von Szeliski (1939c) reaffirmed their methodological standpoint:

"Actual demand schedules can only be found by analysis of statistics. The issue here is *how* they shall be analysed, and 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 (December 19, 1939), expressing approval for his and Roos's study, supports our claim that his view was not one of 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*)

3.1.2. Lange and Marshack, 1940: 'failed reconciliation'

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".[14] (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. It appeared for the first time in Hendry and Morgan eds. (1995), who praised it 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). Hendry and Morgan's assessment seems to overvalue the paper.

Lange and Marschak were keen on asserting that Keynes's theories, with which they declared themselves in "profound agreement", were capable of statistical verification. However, in the first pages of their article, Marschak and Lange argued only quite vaguely in favour of statistical verifiability. On the other hand, they agreed that Tinbergen's work had some of the weaknesses of

[14] From an 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.

that Keynes had pointed out and even add a few to the list. They criticised Tinbergen's treatment of regression coefficients as exact numbers rather than as estimates and his subsequent failure to compute standard errors.[15] The rest of their essay is devoted mainly to remarks of a more technical nature. Some of these are dealt with acutely, but on others the line of argument appears seriously flawed. The cobweb model is enunciated in order to show how cyclical movements can be generated by linear relationships. They address the issue of the measurability of variables by noting that many qualitative dimensions can be treated statistically (e.g. through the use of dummy variables). Far less convincing, in our view, was Marschak's and Lange's defence of the use of trends (which they interpret as a mean of both capturing the gradual variation in time of parameters and of eliminating the "nonsense correlations" arising in time series). Even more obscure is the passage which deals with the unit of measurement of profits and the correct shape of the relation to be estimated.[16] On the whole, their tone was overall conciliatory[17].

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[15]However, Marshak and Lange did not seem accurate on this point. Tinbergen apparently did compute standard errors, for at least some of the regression equations, as explained in the paragraph on "Significance calculations". See p. 80. see table III.10, p. 78-79, and Graph III.12, p.84.

[16] The explanatory variable influencing investment should be "*the difference between profits measured as a percentage on current* cost *of capital goods and the rate of interest*" (p. 394). Writing *P* for profits, *C* for the cost of capital goods, *R* for the interest rate and *I* for investment, Marschak and Lange started by conceding that while Tinbergen fits his data to: $I = aP - bC - cR + d$

(where small letters indicate coefficients to be estimated), the correct estimation equation implicit in Keynes' remark is:

$$I = m\left(\frac{P}{C} - R\right) + n$$

Marschak and Lange introduced then a measurement error arguing that, in the absence of direct measurements of profits, indicators such as the non labour income must be used: $P = hP' + k$

By substitution, they transformed "Keynes' equation" into:

$$I = m\left(\frac{hP' + k}{C} - R\right) + n$$

and thus: $I = mh\frac{P'}{C} - m\frac{k1}{C} - mR + n$

Their line of argument is that this formula, they claimed, "*resembles Tinbergen's equation*". It seems to us that the only obvious resemblance lies in the variables that enter it (*P*, *C*, *R*), while the shape of the relationship being estimated is crucially different. Introducing the measurement error convincingly accounts for different (absolute value) coefficients for *P* and *R* (*mh* and *m* respectively), but the first regressor is still a profit *rate* with C in the denominator, and the second one is the inverse of C. However, Marschak and Lange went even further: "*the resemblance becomes complete if we remember that he measures each variable as so much per cent excess of its average (or trend)… The ratio between profits and cost can be approximated by the difference between the deviation percentages*" (p. 395). This passage raises the suspicion that they are mistaking the mathematical form of a *percentage rate* with the meaning of a profit rate as a *ratio* between variables. What can be approximated by the difference between the deviation percentages is *the percentage deviation* of the profit rate, not the profit rate itself. There is no ground on which to substitute such difference into the above equation (which is not explicitly done in Marschak and Lange's paper, but seems to be the following logical step) in order to end up with the one used by Tinbergen. Besides, they dropped the subject without explaining what happens to the second term, or whether the other variables (I, R) would need to be transformed at all.

[17] Hendry and Morgan express their surprise, though, at Marschak and Lange's apparent ignorance of the tests of homogeneity carried out by Tinbergen, and hint that the authors here might have felt the need to concede something to Keynes as they were submitting the paper to the *Economic Journal*. Hendry and Morgan's claim that Tinbergen did test for homogeneity over time seems to based on a table (III.6, on pp. 70 –71 in the original edition), in which Tinbergen does in fact present the results of a rudimentary test of structural change. This is performed by running separate regression for different time periods: "*(i) before 1895, the turning point of the "long cycle", (ii) between 1895 and the*

Hendry and Morgan hypothesise that Keynes decided not to publish this paper because he thought that the issue had already been discussed enough. One might speculate whether the slight touch of sycophancy, hinted at by Hendry and Morgan, may have contributed to the decision not to publish it. 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 revealed by the authors to reconcile his theories with the methods of empirical verification. In any case, we think that Keynes's decision appears justified by the analysis of the article's contents, which add little substance to the debate. Actually, in a letter to Pigou of 29 March 1940 (EJ/1/6), Keynes maintained that "Tinbergen's reply was of far higher quality than this one":

"He really does try to meet my specific points to the best of his ability and says some very interesting and important things about them, whether or not one considers him convincing. This document, on the other hand, seems to me very largely a mere expression of opinion. On most of the main issues the authors tell us what their view is but do not give their reasons"

The only valuable point, according to Keynes, regarded the issue of linearity:

"One of the most interesting point they raise, which is definitely not in Tinbergen[18], is on p. 12, where they attempt to deal with my 'suspicion that the assumption of linearity rules out cyclical factors'. I think there may be something in what they say there".

In conclusion, he made explicit what the object of his criticism was:[19]

"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".

war, and (iii) after the war " for Germany, USA and UK and by comparing the estimated coefficients. No clear-cut conclusion is drawn from this exercise, though. In first place, Tinbergen reports the difficulty to obtain comparable figures for the pre-war and the post-war periods, especially as far as profit figures for the US and UK are concerned. Using different series (share prices for the US, estimated non-labour income for the UK) leads to quite different coefficient estimates than those obtained with profits for the post-war years. Moreover, considerable differences in estimates are found for the UK and Germany in the comparison of the two pre-war periods. Tinbergen seems to interpret these results as evidence of some structural change taking place, without being troubled by any methodological implication.

[18] It is not in Tinbergen book but it is in his reply, although only mentioned and not explained at length as in Lange and Marschak.

[19] 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).

3.1.3. Koopmans, 1941: a comprehensive reply

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

The stated aim of the article is to investigate "the possibilities and limitations" of extracting information from statistical observations regarding the relations underlying short-run economic movements, by addressing 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 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)[20]. Koopmans recognised the possibility of unmeasurable internal factors acting as a cause on other variables - one of Keynes's main questions - and maintained that the only way to make sense of this concept was to regard non-measurable phenomena like "expectations" or the "state of confidence" as themselves determined mainly by measurable internal and/or recognizable external phenomena. The need for introducing additional information[21] – the third element – stems from the fact that a 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 and identified the sets

[20] Internal/external correspond to endogeneous/exogeneous in today terms.
[21] Additional information 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".

of premises in his study.[22] The method prescribes that the list of premises produced by the economist then goes to the mathematical statistician who 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 investigate "whether at least one set of coefficients and lags exists which is compatible with all … sets of premises"[23]. Koopmans seems to take in 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 be contradicted and provide sufficient basis for quantitative precision, or not be contradicted but provide insufficient basis for conclusions -, 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 illustrated this by discussing two premises that Keynes found most problematic: the use of linear relations and the constancy of coefficients. For testing the linearity assumption, Koopmans prescribed 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

[22] They are: 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.

[23] .i.e., that: (i) has the properties specified in the third set of premises and (ii) when combined with the series $x_2,…,x_n$ … (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).

specify the criticism in order to make its relevance liable to statistical test" (p. 175).[24] 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 definition of the method, he nevertheless basically defended and reaffirmed the validity of the method itself:

"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), it is legitimate to extrapolate for policy and prediction purposes. 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., italics added). Prediction represents a "much more hazardous undertaking" (ibid.). Koopmans concluded that Tinbergen's results fall instead under the cases where "a basis both solid and sufficient … could not be established" (p. 180).

On May 29th, 1941, Keynes answered Koopmans. He seemed to appreciate his work, but he reaffirmed his fundamental criticism, emphasising "the dilemma of many of these enquiries" relative to the stability of the environment over 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*).

[24] 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), number of observations permitting, 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.

3.2. Haavelmo, 1943: the probabilistic approach takes over

The debate comes to a rupture with Haavelmo (1943), when econometric methods are wholly restated in probabilistic terms.

Haavelmo (1943) began 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) singled out among those critics who apparently believe in the supremacy of "the noble art of theoretical deductions based on 'general economic considerations'". Haavelmo took quite a different track with respect to reactions to Keynes's criticism by other econometricians, discussed above. Instead of focussing on the more technical issues, and discussing them one by one, he 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 then made about the former. However, verifying such statement does not imply accepting the theory, because "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". The second key point is to redefine both theoretical and observed variables as stochastic objects. This is necessary, he claimed, for "an objective and intelligent discussion of such questions as those of Lord Keynes." The aim of statistical testing becomes "to draw some inference …, as to which of these mechanisms (probability laws) actually produced the data" (p.17). Prediction relies on the hope of the persistence of such mechanism. 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)

That theories are undistinguishable from the point of view of observations is accepted as an in eliminable problem:

“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 are increasingly ignored and, as Leamer (1985) writes, the slippery issue of causal inference will be “kept in the econometric closet for over thirty years”.

4. Concluding remarks

Our reconstruction shows that there is no evidence for regarding Keynes as a critic and an opponent of econometric work *per se*. 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 lies the question of methodology. He doubted 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 system to be stable over the long run, and so there was no reason to infer stable correlations.

Keynes raised a question that *prima facie* seemed to him to have a positive answer: whether “the slippery problem” of passing from statistical description to inductive generalisation (which he showed to be relevant in the case of simple correlation in his *Treatise on Probability*) arose also for the multiple correlation method. Keynes focused upon the inductive aspects of Tinbergen's analysis and examined the legitimacy of its implicit 'fundamental assumptions' - 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, independence of the different factors of one another. According to Keynes “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. Then, with Haavelmo and the establishment of the Cowles Commission approach, they abandoned the debate. 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 was overly harsh within the context of contemporary econometrics of 1939-40.

Why was it so harsh ? This did not depend – this is our interpretation - on Keynes's temperamental characteristics, as Stone (1978) suggested. Rather, Keynes was harsh because from the mid-1930s on he began to notice that a new conception on the nature, method and style of economics, opposed to what he considered correct, was meeting with increasing success. According to Keynes, "economics is a branch of logic, a way of thinking" (Keynes 1973b, p. 296):

> "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*" (letter to Harrod of the 4th of July, 1938, ibid. p. 297, italics added).

The non-homogeneity of the material through time – due to the existence of changing and unstable factors like "motives, expectations and psychological uncertainties" (ibid., p. 300) - 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 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." In this context, mathematical generalisations essentially have 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: as a rule it makes a complete and exact generalisation not possible - "In a study so complex as economics … we cannot hope to make completely accurate generalisations" (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: it is of fundamental importance "to eliminate impressionism", that is to increase the accuracy of the theories. In the *General Theory* he called for a statistical examination of some key concepts like the propensity to consumption and the multiplier.[25] The economists, as he wrote to Harrod, must not be "reluctant to soil [his] hands" (letter to Harrod, 16 July1938, in Keynes 1973b, p. 300). Prediction, instead, was not the main object of the statistician.

This conception of the nature and method of economics made Keynes seriously worried about the emerging tendency to use statistical and mathematical methods to formalise economic analysis. A "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 agreed to the *Econometric Society* program. This can be asserted on the basis of the little explicit evidence available – his correspondence with Harrod and with Ragnar Frisch in the 1930s[26]. The *Econometric Society* was founded in 1930. Its program – set out in the editorial of *Econometrica* by 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 1933a, p. 1). As emerged from his correspondence with Frisch, Keynes's mistrust in "recent mathematical economics" concerned: a) the imprecision of assumptions, often 'special', but covered by a maze of symbolism; e.g., the assumption of strict independence between the factors (common in mathematical works), excludes the consideration of complexity; b) the unclear application of conclusions. Keynes's concern on this "recent mathematical economics" was reinforced by the fact that his own ideas – from the *Treatise on Money* to the *General Theory* – had had a relevant impact on young econometricians.[27] 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

[25] 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 Oxford economist Colin Clark and for the United States by Simon Kuznets.

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

[27] 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.

valuable as a theoretical structure suitable for quantitative analysis of those problems. According to econometricians Keynes's theory, originally expressed in literary form, needed to be translated into 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 on the same subject. This simultaneous equation interpretation of the *General Theory* – i.e. a simplified version offering a mathematical framing in the form of a specified model -, became its dominant interpretation even though this was at odds with Keynes's original formulation.[28] 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).

In the mid 1930s Keynes became aware that a convergence was to be realised among those which we can call the early 'neoclassical synthesis' interpretation of the *General Theory* and the interpretation given by the econometricians. Keynes's virulence against Tinbergen can be explained by the fact that the latter epitomised this tendency at its best. On the one hand Tinbergen reintroduced a conception of economics and its method that Keynes, as Marshall before him, had rejected, on the other he proposed an usage of statistical inference that Keynes had criticised.

Many contemporary economists are disappointed by the unsatisfactory achievements left by the "Walrasian detour", which dominated a great part of the post-war economics. They recognise that the Keynesian (and Marshallian) issue of the appropriate style for economics – and therefore the reflection on the role of mathematics, statistics and econometrics in economics – does still matter. In this thoughtful context we may justify Keynes's concern and appreciate his methodological contribution, whose criticism of Tinbergen's econometric method is an important part.

[28] The new econometric approach appropriated not only Keynes's work, but also, in a sense, Hicks's 1937 paper. It is noteworthy that in his review of Davis's *Theory of econometrics* (1941) Hicks criticized the statement that Marshall's mathematical appendix came to be regarded by many as his most valuable contribution to the subject. Hicks replied in Keynes's mood: "Hardly, we must surely reply, by those who know their Marshall. This statement his mathematician wishful thinking. Pur mathematician will not have become an economist until he has learned that there are vital things in economics which are not applied mathematics; and that there is much else which could be stated mathematically but which anyone with a sense of mathematical elegance would prefer to state in prose" (p. 352).

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