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The Appropriate Style of Economic Discourse. Keynes on Economics and Econometrics

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The appropriate style of economic discourse. Keynes on economics and econometrics

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This paper reconstructs Keynes's reflections on the issue of the role of econometrics in the economic discourse. It analyses the Keynes-Tinbergen debate in the period 1938-1940 and the exchange between Keynes and other econometricians in the period 1939-1941. We argue 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. We offer also an interpretation of Keynes's harshness in his criticism of Tinbergen.
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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 guarantee the clearness and rigour necessary

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for the theory as well as for the empirical research in economics. In this context econometrics emerged as part of the 'modern models', which were conceived of as an applied development of Walras's and Pareto's mathematical economics (Tinbergen, 1949; Schumpeter, 1954). Many economists expressed doubts and objections 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. In recent years, an increasing attention has been devoted to reflecting on the performance of the twentieth-century economics and to 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 for example: McCloskey, 1997; Krugman, 1998; Bowles and Gintis, 2000; Chick and Dow, 2001; Marchionatti, 2002; O'Donnell 2006). The 1930s discussion on econometrics should be reconsidered in this 'revisionist' context.

Tinbergen's 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. It was also innovative from the point of view of testing procedures (Morgan, 1990, pp.108-114). The work was expected both to provide general economic forecasts and to guide government policies to control business cycles (Epstein, 1987; Hallett, 1989). The first volume of the report, on which Keynes's critique focused, contained an explanation of the econometric method, presented it as a 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 parametrized 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 publication of the report immediately raised what is known as the Tinbergen debate, to which Keynes, Frisch, Friedman and many others contributed (see Hendry and Morgan 1995, Leeson 1998 and Louçã 1999). The assessment of Keynes's critique of Tinbergen's first League of Nations study remains controversial. In the first post-war decades the prevailing view was that Keynes was an *a priori* anti-econometrician (for example Samuelson 1946 and Klein 1951). At the end of the 1970s some contribution began to recognise the importance of the problems raised by Keynes regarding the use of correlation analysis (Patinkin 1976, Hendry 1980)[i]. In the following years an increasing number of contributions recognised that Keynes's criticism was sound in both the technical and logical arguments and remains relevant to this day (Lawson 1985, Pesaran and Smith 1985, Gillies 1988, Rowley 1988, McAleer 1994, Dharmapala and McAleer 1996, Keuzenkamp 2000). However, it is generally considered overly harsh and Keynes has been blamed for throwing out the baby with the bath water.

This paper reconstructs Keynes's view of the role of econometrics in the economic discourse as it emerges in his debate with Tinbergen. In section two his conception of economics and the role of mathematics and statistics is presented as a necessary premise to understanding his criticism of Tinbergen. In the third section we analyze the Keynes-Tinbergen debate, with some reference also to the exchange between Keynes and other econometricians in the period 1939-1941 and we propose an interpretation of his harshness. The last section provides some final remarks on Keynes's alleged anti-econometrics attitude.

2. **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*. In his 1938 correspondence with Harrod, particularly relevant from this point of view, Keynes defined economics as "a branch of logic, a way of thinking" (letter of the 4th of July, 1938, in 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 to facts economists must continuously refer:

"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.[ii] However mathematical generalisations have in his view primarily an instrumental role, especially useful 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 (see *General Theory*, 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.

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.[iii] 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. He considered these quantitative research projects very important in improving economic theory. An economist, as he wrote to Harrod, must not be "reluctant to soil [his] hands" (16 July 1938, in Keynes 1973b, p. 300).

On the contrary, Keynes considered that prediction was not the foremost 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)[iv]. The starting point of Keynes's analysis was the historical discussion of the Bernoulli's theorem that permits to derive a numerical measure of probability from a numerical statistical frequency of previously observed similar events -- that is to infer an exact measure of probability from observed frequency. He claims that such theorem "is only valid subject to stricter qualifications … and in conditions which are the exception, not the rule" (ibid. p. 369).[v] 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:

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,[vi]. 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 become more numerous, inductive arguments become less applicable (ibid., pp. 279-80). Inductive inference requires that the premises of an inductive argument must have a limited independent variety, or, we may say, a high degree of homogeneity. In other words, an object of inductive inference should not be 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 example provided in the *General Theory*, in which the non-homogeneity and complexity of the material make it not suitable to be analysed in a formalised way, is the case 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. 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 – and "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 (see Marchionatti 1999). Expectations are very important in business cycles phenomena which, in Keynes's theory, are determined by investment. As expectations and investment cannot be modelled with probabilistic relations, also the business cycle seems to be beyond the domain of probabilistic inference.

3. The debate with Tinbergen and with other econometricians on econometric method

3.1. Keynes's criticism to Tinbergen

In the September 1939 issue of the *Economic Journal* 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).[vii] The debate had actually started one year before, in the form of an exchange of letters between Keynes Tinbergen and two officers of the League of Nations, Alexander Loveday and Royall Tyler. Our reconstruction of Keynes's critique and of Tinbergen's reply is based on both published material and correspondence.

Keynes (1973b [1938] and 1939) posed the central question first: 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 - as is well known and widely emphasised (see for example Hendry and Morgan 1995)- 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. Here we focus instead on those 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 raised a set of issues about the conditions of validity of Tinbergen's procedures [viii].

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.[ix]

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:

"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 issues 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. Secondly, 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, as he had already stressed in a letter to
Tyler:

"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. Keynes put the constancy of the parameters into question - they
were assumed to be constant for 10 years or for a larger period -. 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 his dissertation then
published in a revised version 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)[x]. 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 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 correspondence Keynes cites in fact at least two examples of types of problems in which the specific features of the object of analysis justify. the use of multiple correlation analysis: the demand for investment in new rolling stock and the demand for automobiles.

The first one is cited in the letter to Tyler of 23 August 1938. At that time Keynes was publishing in the *Economic Journal* an article by the English statistician E. J. Broster that studied 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 direct correspondence with Broster Keynes, while approving the methodological line of his work, however added:

"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 other case is illustrated by an exchange with Victor Szeliski of the Institute of Applied Econometrics, New York, from whom Keynes received a letter in November 1939[xi]. Szeliski had read Keynes's review of Tinbergen's League of Nations study "with considerable interest and approval", and he wondered to what extent Keynes thought that the same criticisms apply to Roos's and his 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)[xii].

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. Keynes's reply to Szeliski of December 19, 1939 provides a restatement his idea 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)

3.2. The econometricians' reaction, 1940-41: Tinbergen, Lange&Marschak, Koopmans

The initial reactions by the econometricians were aimed at reaffirming and defending the validity of their method while taking into serious account some of the points of Keynes's criticism.

Such attitude is reflected in Tinbergen's reply, but also in contributions by others: Lange and Marshack in 1940 and Koopmans in 1941, which can be seen as a sort of exercises in reconciliation. Tinbergen ìwas astonished by Keynes's harsh reaction. In a letter to Keynes of December 18, 1939, he wrote:

"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".

In his reply he addressed carefully the technical aspects of Keynes's critique – as recognised by Keynes himself in a letter to Pigou of March 29, 1940 [EJ/1/6]:

"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."

However, he avoided the logical question and the "slippery problem of passing from statistical description to inductive generalisation". He stressed – with many illustrations of his approach in business cycle research – the flexibility of his empirical method, leaving Keynes's central objection substantially unanswered.

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 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 technical 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)

As regards the question of the constancy of the coefficients, he explained that he assumed the constancy of coefficients as a first approximation. 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.

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 [Tinbergen] continue" (ibid. p. 320).

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" which was not published.

Lange and Marschak claimed themselves in "profound agreement with the economic theories of Mr Keynes", and weretherefore keen in supporting the idea that they were 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. In particular, they criticise Tinbergen's treatment of regression coefficients as exact numbers, rather than as estimates. The rest of their essay is devoted mainly to remarks of a more technical nature. 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 at stake.

At the beginning of 1941, one of the major exponents of post-war econometrics intervened in the debate. On May 23, 1941, Keynes received a letter from Koopmans, announcing that he was sending him an offprint of his 1941 paper "The Logic of Econometric Business-Cycle Research", which provided a clear restatement of Tinbergen's method aimed at answering "some of the questions raised in your review of Tinbergen's investigation for the League of Nations" (CO/4/155). Koopmans addressed 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). He starts by enumerating three crucial in econometric enquiry. The first one is the availability of time series data. 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)[xiii]. 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 – 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. The method prescribes that a list of premises is produced by the economist and then passed on to the mathematical statistician who investigates "whether at least one set of coefficients and lags exists which is compatible with all three set of premises" (p. 163). Koopmans seems to accept some of Keynes's concerns in highlighting the crucial centrality of economic premises. 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 (even if not contradicted by the

data, they might or not provide sufficient basis for quantitative precision), 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 the two are mutually exclusive. He then illustrated this by discussing two of the 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 some tests 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. In conclusion Koopmans recognised that the combination of data and additional information is "the result of a continuous dialogue of a game of give and take, between economist and statistician." (p. 178).

Looking at Tinbergen's results in the light of his rigorous definition of the method, 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).

In his answer to Koopmans, Keynes expressed great appreciation for his work, but also reaffirmed his fundamental criticism, emphasising the issue of stability of the environment in the long run as the main dilemma:

"I enjoyed [your article] 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" (letter of May 29th, 1941, in CO/4/170, emphasis added).

3.3. An alternative interpretation of Keynes's harshness

It is the general opinion that Keynes's criticism was overly harsh within the context of contemporary econometrics of 1939-40. Such harshness did not depend – this is our interpretation – on Keynes's temperamental characteristics, as Stone (1978) suggested. Rather, it was due to the fact that since the mid-1930s on he had been aware that a new conception on the nature, method and style of economics, opposed to what he considered correct, was meeting with increasing success.[xiv] His 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)

The little explicit evidence available – his correspondence with Harrod and with Ragnar Frisch in the 1930s[xv] – allows to assert that by 'recent mathematical economics', Keynes was referring to those economists who agreed to the programme of the *Econometric Society*, founded in 1930. Such programme – 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.[xvi] Keynes's concern about this 'recent mathematical economics' was reinforced by the fact that his own ideas – from *A Treatise on Money* to *The General Theory* – had had a relevant impact on young econometricians.[xvii] Many of them, such as Frisch and Tinbergen, believed 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 *The General Theory* and his emphasis on monetary and fiscal policies made his work extremely valuable as a theoretical structure suitable for quantitative analysis of those problems. The econometricians believed that Keynes's theory, originally expressed in literary form, needed to be translated into a system of equations to emphasise the basic hypotheses in a

formal and simpler framework. Immediately after its publication, Keynes's *Theory* was discussed in the *Econometrica*'s circle. The first version of Hicks' paper, containing the famous IS-LM model of Keynes's theory, had been presented and discussed at the sixth European meeting of the Econometric Society in 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.[xviii] 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 between those which we can call the early 'neoclassical synthesis' interpretations 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.

4. Concluding remarks

Our reconstruction of the Keynes-Tinbergen debate shows that there is no evidence for considering Keynes a strong 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 there is the question of methodology. He argued that, in applying correlation analysis to economic matters, there was no reason to expect the behaviours were stable over the long run, and thus 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*.

Keynes felt that "the slippery problem" of passing from statistical description to inductive generalisation, which he had shown nineteen years earlier to be relevant for the case of simple correlation, arised 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 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 tried to make 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 that theories are undistinguishable from the point of view of observations and that such problem is not 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.

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 Keynes' (and Marshall's) issue of the appropriate style for economics – and therefore the reflection on the role of mathematics, statistics and econometrics in economics – still does matter. In this thoughtful context we may justify Keynes's concern and appreciate his methodological contribution, of which the criticism of Tinbergen's econometric method is an important part.

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Notes

[1] The importance of the objections raised by Keynes had already been recognized by Henri Theil in a little-known article published in the Italian journal L'*industria* in 1963.

[1] 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, 1973c, p. 335-6, emphasis added).

[1] 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).

[1] 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).

[1] They include assumptions such as the following: 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.

[1] Keynes writes: "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] On August 11, 1938, Keynes received a letter from Tyler 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). After a first reading of the proofs, Keynes's judgement was negative (see the letters to Kahn and Harrod of 23 August 1938, in Keynes 1973b) based on the lack of " the slightest explanation or justification of the underlying logic". In a letter to R. Tyler of the 23rd of August (in Keynes1973b, p. 285-289) 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.), and preferred to discuss technical questions. The reading of Tinbergen's letter supported Keynes's critical feeling that the work was methodologically weak, which made the results obtained of little practical value (see the letter to Loveday, September 20, 1938 in Keynes' Papers, CO/11/319). Replying on the

same day, Keynes wrote to Tinbergen that he had to demonstrate first of all that his method was applicable, rather than simply applying it.

[1] 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).

[1] 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.

[1] 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).

[1] The institute was founded in 1938 by Charles F. Roos, one of the founders of the Econometric Society and the director of the Cowles Commission for Research in Economics from 1934 to 1937.

[1] 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.

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

[1] Jolink (2000) explains Keynes's strong opposition to Tinbergen's work by the fact that 'Tinbergen's statistical results explicitly challenged Keynes's [investment] theory' (p. 3), 'by downgrading the theoretical influence of interest fluctuations on investment fluctuations' (p. 11). While these considerations are important in showing the wide range of topics involved in the debate, in our opinion they are of minor importance in explaining the harshness of Keynes's criticism.

[1] The correspondence with Roy Harrod is published in Keynes's *Collected Writings*; the (un-published) correspondence with Ragnar Frisch, concentrated in the period 1932-1936, is kept at the National Library of Norway, Oslo Division. A discussion of the latter is in Louçã 1999.

[1] In a letter of February 1932 to Frisch Keynes writes: 'Mathematical economics is such risky stuff as compared with non-mathematical economics, because one is deprived of one's intuition on the one hand, yet there are all kinds of unexpressed unavowed assumptions on the other. Thus I never put much trust in it unless it falls in with my own intuition' (Frisch papers, 76/A). In a later letter of November 28, 1935, Keynes adds: 'I cannot persuade myself that this sort of treatment of economic theory has anything significant to contribute. I suspect it of being nothing better than a contraption proceeding from premises which are not stated with precision to conclusions which have no clear application. I may misjudge the situation. But I am convinced that this mode of

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[i] The importance of the objections raised by Keynes had already been recognized by Henri Theil in a little-known article published in the Italian journal L'*industria* in 1963.

[ii] 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, 1973c, p. 335-6, emphasis added).

[iii] 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).

[iv] 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).

[v] They include assumptions such as the following: 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.

[vi] Keynes writes: "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)

[vii] On August 11, 1938, Keynes received a letter from Tyler 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). After a first reading of the proofs, Keynes's judgement was negative (see the letters to Kahn and Harrod of 23 August 1938, in Keynes 1973b) based on the lack of " the slightest explanation or justification of the underlying logic". In a letter to R. Tyler of the 23rd of August (in Keynes1973b, p. 285-289) 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.), and preferred to discuss technical questions. The reading of Tinbergen's letter supported Keynes's critical feeling that the work was methodologically weak, which made the results obtained of little practical value (see the letter to Loveday, September 20, 1938 in Keynes' Papers, CO/11/319). Replying on the same day, Keynes wrote to Tinbergen that he had to demonstrate first of all that his method was applicable, rather than simply applying it.

[viii] 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).

[ix] 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.

[x]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).

[xi] The institute was founded in 1938 by Charles F. Roos, one of the founders of the Econometric Society and the director of the Cowles Commission for Research in Economics from 1934 to 1937.

[xii] 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.

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

[xiv] Jolink (2000) explains Keynes's strong opposition to Tinbergen's work by the fact that 'Tinbergen's statistical results explicitly challenged Keynes's [investment] theory' (p. 3), 'by downgrading the theoretical influence of interest fluctuations on investment fluctuations' (p. 11). While these considerations are important in showing the wide range of topics involved in the debate, in our opinion they are of minor importance in explaining the harshness of Keynes's criticism.

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