Keynes and the logic of econometric method

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Abstract.
This paper analyzes the controversy between Keynes and Tinbergen on econometric testing of business cycle theories. In his writings, Keynes repeatedly emphasizes a 'logical' objection to Tinbergen's work. It is clarified what exactly this logical objection is, and why it matters for a statistical analysis of investment.
Keynes' arguments can be traced back to his *Treatise on Probability*, where the 'principle of limited independent variety' is introduced as the basic requirement for probabilistic inference. This requirement is not satisfied in case of investment, where expectations are complex determinants. Multiple correlation, sometimes thought to take care of required *ceteris paribus* clauses, does not help to counter Keynes' critique.

Keywords:
Keynes-Tinbergen controversy; testing; multiple correlation; expectations, *ceteris paribus*, econometric inference.

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> ‘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’ (Keynes, 1939, p. 567)

1. Introduction[1]

Keynes disliked econometrics. Moreover, he did not understand much of it. This, at least, is the view of many economists and econometricians who recall their vague, and usually indirect, knowledge of the Keynes-Tinbergen controversy (Keynes, 1939, 1940; Tinbergen, 1940). Stigler *et al.* (1995, p. 344) add that ‘Keynes’ long reign at the *Economic Journal* probably discouraged its publication of econometric work, of which he was a sceptic, again a subsidy to *Econometrica*, and his policies also helped the *Review of Economic Studies*.’

The controversy with Tinbergen is frequently regarded as a deplorable clash between an old and a new era in economics (e.g. Klein, 1951; Stone, 1978; Morgan, 1990; Malinvaud, 1991).[2] Occasionally, Keynes is credited with raising the problem of mis-specification, without having an established vocabulary in which to formulate the problem (Patinkin, 1982; Pesaran and Smith, 1985; McAleer, 1994). However, in this paper I will argue that Keynes’ critique is not primarily one of mis-specification. It is neither based on an objection to econometrics and probabilistic inference in general, nor does it follow from an outdated misunderstanding of the crucial issues at stake.

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[2] Leeson (1995, p. 3) contains a number of additional references.

Keynes understood them all too well.

The debate between Keynes and Tinbergen has its roots in one of the first and most ambitious efforts to test economic theories statistically. On request of the League of Nations, Haberler ([1937] 1958) had provided an overview of business cycle theories. As a follow-up, Tinbergen was asked to confront those rival theories with data. The resulting work (Tinbergen, 1939a,b) roused strong sentiments. The debate concentrated on the question whether statistical methods are proper tools for testing economic theories.

The basic issue at stake was: is the multiple regression model,

$$y_i = a_1 x_{i,1} + \ldots + a_k x_{i,k} + \mu_i \ , \ (\mu_1,\ldots,\mu_n)' \equiv \mu \sim (0,\Sigma), \ i=1,\ldots,n, \qquad \textbf{(1)}$$

a valid or fruitful tool for testing economic hypotheses? This question has to be answered taking into consideration the possibility of serious specification uncertainty and a limited set of (unique, non-experimental) data, where k (the potential number of regressors) may well exceed n (the given number of observations). Is this a practical, or is it a logical problem? Once started, the debate on those questions never concluded.[3]

Unlike Tinbergen, who was a very pragmatic research worker, Keynes was preoccupied with the *logical* conditions for probabilistic inference, as may be clear from his earlier work, Keynes ([1921] 1973a). Keynes argued that the application of statistical methods to the analysis of investment behaviour (the example presented by Tinbergen, 1939a, to clarify his method) was the least promising starting point as this is a case where those logical conditions were not even remotely met. In this paper I aim at clarifying why not. I will focus on the logical issue raised by Keynes, which does not seem to be generally understood or appreciated. This logical point may have been phrased obscurely, but it is worth further investigation for a better understanding of the foundations of econometric inference—-even today.

[3] See Leamer (1978) for a recent perspective.

To get a feeling for Keynes' argument, it briefly runs as follows. Statistical testing of economic theories is a form of induction. Induction needs a justification, an 'inductive hypothesis'. A 'principle of limited independent variety' may be invoked for this purpose, but has to be justified as well. The justification depends on the issue whether the number of causes or generators of phenomena of interest is limited or, to the contrary, unlimited or complex, and whether they can be known *a priori* (Section 2). In cases where interdependent expectations are involved, this is not the case (Section 3). Investment is an instance where such expectations matter more than anywhere else. Disregarding the issue by invoking a *ceteris paribus* clause (in a statistical model represented by conditioning and adding a stochastic error with known properties) is not warranted (Section 4). In short, Tinbergen does not (and would be unable to) justify the inductive hypothesis. In fact, his inductive claims are very modest but, therefore, one may wonder what use his effort is, and this indeed Keynes does (Section 5). Section 6, on the implications of lack of homogeneity and limited independent variety, concludes the paper. The fact that econometric modelling of investment has turned out to be a notoriously difficult issues in applied econometrics, even today, suggests that Keynes' objections were not altogether misguided.

2. Limited independent variety and induction

Keynes ([1921] 1973a) formulates an inductive theory of probability which has become known as a 'logical' Bayesian probability theory. A central problem in the *Treatise on Probability* is to obtain a probability for a proposition (hypothesis) h, given some premise (concerning some event or evidentional testimony), e. The posterior probability of h given e is given by Bayes' rule,

$$P(h|e) = \frac{P(e|h)P(h)}{P(e)}. \qquad \textbf{(2)}$$

In order to obtain this posterior probability, a strictly positive prior probability for h is needed: if $P(h)=0$, then $P(h|e)=0\ \forall\ e$.[4] It is not obvious that such a prior does exist. If infinitely many propositions may represent the events then, without further prior information (like informative analogies, being an important source for Keynesian prior probability), their logical *a priori* probability obtained from the 'principle of indifference' goes to zero.[5] In philosophy, this is known as the problem of the zero probability of laws in infinite domain (Watkins, 1984).

Analogy is important in inductive reasoning. This was already emphasized by Hume, and further elaborated by Keynes. Keynes ([1921] 1973a, p. 247) cites Hume who argues that reasoning is founded on two particulars, 'the constant conjunction of any two objects in all past experience, and the resemblance of a present object to any of them'. Keynes ([1921] 1973a, p. 243) makes a crucial addition by introducing 'negative analogy'. This negative analogy plays a similar role in his theory of probability as identification does in contemporary econometrics. Using the example 'are all eggs alike?', Keynes (*ibid.*) argues that an answer to this question depends not only on tasting eggs under similar conditions.[6] To the contrary, the experiments should not

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[4] What exactly constitutes a hypothesis relative to the multiple regression model (1) is not yet made precise. Possible candidates may be a hypothesis about (i) value or sign of a_i; (ii) the validity of the statistical assumptions; (iii) the validity of the complete model; or (iv) a prediction generated from the model. Note, furthermore, that Keynes does not claim that probabilities are or should be numerically measurable (for a discussion of his ordinal theory of probability see Keuzenkamp forthcoming).

[5] There is no need here to deal with Keynes' deeper concerns about the principle of indifference. Another famous Cambridge probability theorist, Harold Jeffreys, who like Keynes was educated by the Cambridge philosopher W.E. Johnson, was also bothered with the problem of obtaining strictly positive prior probabilities if the number of hypotheses is large. Wrinch and Jeffreys (1921) attack this problem by means of a 'simplicity postulate'. This postulate entails that prior probability depends inversely on the complexity of hypotheses.

[6] The question of eggs represents abstractly Keynes' (1983, pp. 186-216) argument with Karl Pearson during 1910-11. Here, the issue was the influence of parental alcoholism on the offspring. Anticipating the argument presented below, Keynes ([1910] 1983, p. 205) claims that 'The methods of "the trained anthropometrical statistician" need applying with much more care and caution than is here exhibited before they are suited to the complex phenomena with which economists have to deal.'

be ‘too uniform, and ought to have differed from one another as much as possible in all respects save that of the likeness of the eggs. He [Hume] should have tried eggs in the town and in the country, in January and in June.’ New observations are valuable in so far they increase the variety among the ‘non-essential characteristics of the instances’ (*ibid.*). The question then is whether in particular applications of inductive inference, the addition of new data adds simultaneously essential plus non-essential characteristics, or only non-essential characteristics. The latter provides promising area for inductive inference.

Keynes ([1921] 1973a, p. 277) argues that, if every separate configuration of the universe were subject to its own governing law, prediction and induction would become impossible. For example, in the regression equation (1) every observation i of y_i would have its own ‘cause’ or explanatory variable, x_j. The same might apply to new observations, each one involving one or more additional explanatory variables. Alternatively, the parameters α might change with every observation, without recognizable or systematic pattern. Those are examples of a lack of ‘homogeneity’ where negative analogy does not hold and, hence, new observations do not generate inductive insights.[7] The universe should have a certain amount of homogeneity in order to make inductive inference based on (2) possible.

To justify induction, the fundamental logical premise of inference has to be settled first. The *principle of limited independent variety* serves this purpose. It is introduced by Keynes ([1921] 1973a) in a chapter significantly entitled ‘The justification of these [inductive] methods’. Keynes ([1921] 1973a, p. 279) defines the independent variety of a system of propositions as the ‘ultimate constituents’ of the system (the indefinable

[7] Keynes’ definition of homogeneity (in so far he really defines this notion) is clearly different from the contemporary statistical definition (e.g. Mood *et al.* 1974), which refers to the equality of probability distributions. Their existence is a presumed part of the ‘maintained hypothesis’. Keynes’ notion deals with this maintained hypothesis itself, more precisely with the existence of a meaningful conditional probability distribution. Homogeneity, in Keynes’ sense, is related to the notion of independent variety.

or primitive notions) together with the 'laws of necessary connection'.[8] The eggs example may be used to clarify what Keynes seemed to have meant. The yolk, white and age of the egg are the (perhaps not all) ultimate constituents of the egg, while the chemical process of taste may be interpreted as the laws of necessary connection. The chemical process is not different in space or time, and the number of ultimate constituants is low. Hence, independent variety seems limited.

If independent variety increases (for example by increasing the number of regressors, changes in functional form *etc.* in equation 1), inductive arguments become less applicable. Keynes (*ibid.*) argues that the propositions in the premise of an inductive argument should constitute a high degree of homogeneity:

> Now it is characteristic of a system, as distinguished from a collection of heterogeneous and independent facts or propositions, that the number of its premisses, or, in other words, the amount of independent variety in it, should be less than the number of its members.

Even so, a system may have finite or infinite independent variety. It is only with regard to finite systems that inductive inference is justified (Keynes, [1921] 1973a, p. 280). An object of inductive inference should not be infinitely complex, so 'complex that its qualities fall into an infinite number of independent groups', i.e. generators or causes (Keynes, [1921] 1973a, p. 287; note that the complexity referred to in the epigraph should be interpreted along those lines). If there is reason to believe that the condition of limited independent variety is met, then inductive inference is, in principle, possible. Keynes dubs this belief the *inductive hypothesis*. It is a sophisticated version of the

[8] The Cambridge tradition of philosophy, following G.E. Moore, in which Keynes operated, was preoccupied with analyzing the meaning of words in terms of their simplicity or complexity. A quintessential example is the notion 'good', which was thought to be a simple, non-natural property to be grasped by intuition. This means that 'good' cannot be reduced to other terms but can be understood intuitively (see Davis, 1994). This approach in Moore's theory of ethics was extended to a theory of induction.

principle of the uniformity of Nature.[9] If independent variety is limited, the number of hypotheses h_i (i=1,...,n) that may represent e is finite. Lacking other information, they are assigned a prior probability of 1/n in accordance with the principle of indifference.

Induction depends on the validity of the inductive hypothesis. The question is, how to asses its validity. This cannot be done on purely logical grounds (as it pertains to an empirical matter), or on purely inductive grounds (this would involve a circular argument).[10] But Keynes argues that there is no need for the hypothesis to be true. It suffices to attach a non-zero prior probability to it and to justify such a prior belief in each application. The domain of induction cannot be determined exactly by either logic or experience. But logic and experience may help to give at least some intuition about this domain: we know from experience that we may place some (but not perfect) confidence in the validity of limited independent variety in a number of instances.[11] 'To this extent', Keynes ([1921] 1973a, pp. 290-1) argues, 'the popular opinion that induction depends upon experience for its validity is justified and does not involve a circular argument.' But one should b e careful with applying probabilistic methods. In particular, Keynes ([1909] 1983, p. 158; letter to C.P. Sanger discussing the problem of index numbers) emphasizes,

> when the subject is backward, the method of probability is more dangerous than that of approximation, and can, in any case, be applied only when we have information, the possession of which supposes some

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[9] Stigum (1990, p. 567; see also p. 545), provides a formal version of the principle of limited independent variety. He argues that it is needed in conjunction with the principle of the uniformity of nature. My view is that it is a weaker version of the principle of uniformity of Nature (see Keuzenkamp, forthcoming, for discussion).

[10] In this sense, the inductive hypothesis resembles a Kantian synthetic *a priori* proposition.

[11] Carabelli (1985) argues that Keynes' intuitionism makes his probability theory anti-rationalistic, and his logicism makes his theory anti-empirical. I do not concur in these views. It is better to interpret Keynes' theory as one of bounded rationality. The rules of probabilistic inference are logical in kind, but applications may be empirical. The judgement about the validity of the application itself is not empirical, but logical. The decision to accept the validity may be regarded as boundedly rational.

> very efficient method, other than the probability method, available.

As an example of such an alternative method, not yet available when Keynes wrote these comments, one might think of R.A. Fisher's theory of experimental design. Indeed, this is arguably the only method (or at least a rare one) which has proved to be effective and successful in cases of inference where the list of causes is likely to exceed the few ones which may be captured by a regression model.

Keynes clearly was not an *a priori* opponent to statistics. Rather, he was cautious. He enabled the appointment of Karl Pearson's student George Udny Yule to a lectureship in statistics in Cambridge in 1912 (Skidelski, 1983, p. 222). As editor of the Economic Journal, he accepted for publication a number of statistical investigations using multiple correlation techniques (although not on business cycle phenomena). He had a strong interest in the collection of data (e.g. national accounts) and actively stimulated this activity.

Finally, he was tempted by the statistical analysis of economic data himself--although this may be regarded as a youthful sin. This occurred when Keynes was working on the Treatise on Probability and was employed at the London India Office. In his first major article, published in the Economic Journal (March 1909, reprinted in Keynes, 1983, pp. 1-22), Keynes makes an effort to test the quantity theory of money by comparing estimates of the general index number of prices with the index number of total currency. The movements were surprisingly similar. In a letter to his friend Duncan Grant, Keynes (18 December 1908, cited in Skidelski, 1983, p. 220) writes that his 'statistics of verification' threw him into a

> tremendous state of excitement. Here are my theories--will the statistics bear them out? Nothing except copulation is so enthralling and the figures are coming out so much better than anyone could possibly have expected that everybody will believe that I have cooked them.

Thirty years later, Keynes turns to another economist who uses more advanced techniques for doing a similar thing: Tinbergen's statistical testing of business cycle theories. Although it has been argued that Keynes' views on probability changed significantly during and after the writing of the *Treatise on Probability*, I think that there is a high degree of consistency between the relevant points in Keynes ([1921] 1973a) and Keynes (1939).[12] Before discussing the Keynes-Tinbergen debate, I will have a closer look at an issue which is essential for appreciating Keynes' argument.

3. The logic and mathematics of expectations

One of Keynes' important contributions to economics is his recognition of the importance of expectations in economic behaviour. However, Keynes does not have a theory of expectations formation in economics, or it must be the theory that such a theory is impossible in view of the interdependency of beliefs. Consider Chapter 12 of the *General Theory* (Keynes, 1936), which discusses 'The state of long term expectation'. This chapter contains the famous beauty contest metaphor to illustrate some problems of an investor (Keynes, 1936, p. 156):[13]

> It is not a case of choosing those which, to the best of one's judgement, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practice the fourth, fifth and higher degrees.

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[12] O'Donnell (1990) concurs in the continuity thesis. Bateman (1990) presents a different view. See Moggridge (1992, p. 623) for a brief discussion.

[13] The beauty contest already appears in Keynes ([1921] 1973a, pp. 27-9). This is not a coincidence. In the *Treatise on Probability*, the contest is used to illustrate the impossibility of calculating an appropriate prior (logical) probability in situations where expectations of other persons' expectations are at stake.

The investor will not be able to apply probability calculus to guide investment decisions, ‘our existing knowledge does not provide a sufficient basis for a calculated mathematical expectation.’ (Keynes, 1936, p. 152). Henri Poincaré (1905, pp. 146-7) argued that induction depends on considerations of simplicity, and that ‘if all things are interdependent, the relations in which so many different objects intervene can no longer be simple’. Keynes would concur that the inductive hypothesis is not valid in this case, because of excessive independent variety. Human nature, Keynes (1936, p. 161) argues, invalidates a mathematical treatment of expectations:

> Even apart from the instability due to speculation, there is the instability due to the characteristic of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than on a mathematical expectation, whether moral or hedonistic or economic. Most, probably, of our decisions to do something positive, the full consequences of which will be drawn out over many days to come, can only be taken as a result of animal spirits--of a spontaneous urge to action rather than inaction, and *not as the outcome of a weighted average of quantitative benefits multiplied by quantitative probabilities*. (emphasis added)

Keynes (*ibid.* pp. 162-3) continues:

> We should not conclude from this that everything depends on waves of irrational psychology. On the contrary, the state of long term expectation is often steady, and, even when it is not, the other factors exert their compensating effects. We are merely reminding ourselves that human decisions affecting the future, whether personal or political or economic, *cannot depend on strict mathematical expectation, since the basis for making such calculations does not exist*; and that it is our innate urge to activity which makes the wheels go round, our rational selves choosing between the alternatives as best as we are able, calculating where we can, but often falling back for our motive on whim or sentiment or

> chance. (emphasis added)

In practice, the economic agent has to fall back on a *convention* (rather than an inductive argument), that the existing state of affairs will continue indefinitely (Keynes, 1936, p. 152). Conventions last as long as they do, but have no logical, constant ground of existence.

Keynes ([1921] 1973a, p. 428) notes that only in exceptional cases, statistical techniques can be used to analyze the complex material like the one that can be found in economics. In a correspondence with his former pupil, Hugh Townshend, Keynes (1979, p. 294) explicates his objections to a formal probabilistic theory of economic expectations:

> Generally speaking, in making a decision we have before us a large number of alternatives, none of which is demonstrably more "rational" than the others, in the sense that we can arrange in order of merit the sum aggregate of the benefits obtainable from the complete consequences of each. To avoid being in the position of Buridan's ass, we fall back, therefore, and necessarily do so, on motives of another kind, which are not "rational" in the sense of being concerned with the evaluation of consequences, but are decided by habit, instinct, preference, desire, will, etc.

This is clearly at odds with a rational expectations hypothesis in the sense of Muth (1961): 'we should be very chary of applying to problems of psychical research the calculus of probabilities' (Keynes, [1921] 1973a, p. 334).

The digression on probability and expectations is important for understanding Keynes' critique of 'Professor Tinbergen's method'. The reason is that, especially in business cycle phenomena, expectations are very important (this, after all, is one of the main arguments of the General Theory). Expectations determine investment, investment determines the business cycle. If expectations cannot be modelled with probabilistic relations (they are not independent of time and place due to an excess of independent variety), then this applies to investment as well. Business cycle phenomena are beyond the domain of probabilistic inference--as Tinbergen would hear from Keynes (1939, p. 561). Before discussing the econometric debate, I will deal briefly with Tinbergen's very different analysis of expectations (see Keuzenkamp, 1991, for details).

Unlike Keynes, Tinbergen had no problem in using probabilistic techniques to model expectations. Tinbergen's 1932 paper, *Ein Problem der Dynamik* (A problem of dynamics), is the first that explicitly uses rational expectations in an economic model (Keuzenkamp, 1991). Tinbergen (1932, p. 172) assumes that expectations of economic agents are rational ('*vernünftig*') in the sense that they should be consistent with the relevant economic model. In addition, Tinbergen (1932, p. 172) makes the crucial assumption that these subjective expectations coincide with the objective expectations:

> In certain cases--which probably will be the most fruitful ones for analysis--one can replace these 'expectations' by economic-theoretical deductions (*durch wirtschaftstheoretischen Deduktion*), certain constants or real variables. For example, in case of a random variable, the rational expectation is the mathematical expectation (*so ist die vernünftige Erwartung die mathematische*)

Tinbergen applies this insight to a model of dynamic inventory allocation with stochastic harvests.

The inventory model itself is not of particular interest here, but some of Tinbergen's conclusions are. The first is that, in a situation of uncertainty, lags should be included in statistical equations of an economic model. Current realizations of economic data (in particular, agricultural prices and stock prices) are often good forecasts of future realizations (this is empirically vindicated in Tinbergen, 1933). In later work on dynamic macro-economic models Tinbergen does not refer to his theory of expectations. On one occasion, however, there is a hint. In Tinbergen (1937), expected profit is argued to be an important factor for explaining investment. Still, no use is made of a rational expectations theory to defend the use of profit and profit lagged as proxies for expected profit. However, Tinbergen (1937, p. 25) notes that

> It could be asked whether profit expectations rather than past profits should be considered as determinants of investment. In principle this is no doubt correct, but it seems to me that the chief factors in expectations are the actual profits that have been made.

The question then is whether, apart from this chief factors, others have a logical or empirical bearing on the analysis of investment as well. This kind of problem is the topic of the next section.

4. *Ceteris paribus* and multiple correlation

In order to understand the controversy between Keynes and Tinbergen, the relation between limited independent variety, expectations, *ceteris paribus* conditions and the method of multiple correlation should be clear. Consider the role of *ceteris paribus* in econom(etr)ic inference. Economic theory relies strongly on *ceteris paribus* propositions. A typical example is that, if price p_i goes up, demand for good *i*, x_i^d goes down, *ceteris paribus*. The 'law of demand' is a refined version of this example. Some of the other circumstances assumed to be constant may be listed explicitly (like income, y, or other prices, p_{-i}), but many remain unspecified (weather, political upheaval, consumer panic).

One of the first applications of multiple correlation methods in economics can be found in Moore (1914). Moore (1914, p. 66), discusses the implausibility of *ceteris paribus* conditions:

> the assumption that, other unmentioned and unenumerated factors remaining constant, the law of demand will be of a certain type, is really tantamount to saying that under conditions which are unanalyzed and unknown, the law of demand will take the supposed definite form.

He continues, ‘the assumption *caeteris paribus* involves large and at least questionable hypotheses’ (p. 67). Reconsider equation (2). Let *h* be the law of demand $\{p_i\uparrow, x_i^d\downarrow\}$ which implicitly figures in conjunction with the relevant *ceteris paribus*, and let *e* be observational evidence. Those *ceteris paribus* are extremely improbable. The evidence on *p* and *x* is, therefore, unlikely to support the law of demand unless the *ceteris paribus* are scrutinized or incorporated explicitly. Indeed, Moore (1914, p. 67) argues, it would be useless speculation to analyze what the effect of rainfall on crops would be, other unenumerated variables being constant. Multiple correlation, Moore (1914, pp. 67-8) argues, solves this problem:

> The method of multiple correlation formulates no such vain questions. It inquires, directly, what is the relation between crop and rainfall, not *caeteris paribus*, but other things conforming to the observed changes in temperature; and, finally, what is the relation between crop and rainfall for constant values of temperatures.

If it can be shown that those variables are the relevant ones for explaining crop size, and if it can be shown that variations in time and place are irrelevant, then the ‘classification problem’ of statistics (as it has become known; see Stigler, 1986) in this application would have been solved. The principle of limited independent variety might be invoked, and inductive claims may be justified.

The method of multiple correlation is due to Yule. He made another major contribution: the analysis of spurious and nonsense correlation.[14] A vivid introduction to the

[14] Spurious correlation is a term introduced by Karl Pearson in 1897 in a discussion of correlation between indices (see Yule, 1929, p. 226, for references). It can be regarded as a general problem of omitted variables (this is the perspective of

problem of spurious correlation is given in Neyman (1952, pp. 143-50), who discusses the statistical 'discovery' that storks bring babies. The problem is straightforward. A cross section sample of raw data for 54 counties is given, presenting for each county the number w of fertile women, the number of storks, s, and the number of births b. As the number of births is likely to be higher in larger counties, where more storks and a higher number of fertile women live, regressing b on s will not yield a valid inference. One should correct for county size. This can be done by creating the transformed variables $x = s/w$, and $y = b/w$. These variables are clearly correlated in Neyman's sample.[15] Of course, the correlation is not very 'plausible'.

The solution is to 'control' for country size in a multiple regression framework.[16] In this sense, multiple correlation may generate the statistical framework for explicitly dealing with the *ceteris paribus*, as Moore and, thirty years later, Haavelmo (1944, p. 17) argued:

> we try to take care of the *ceteris paribus* conditions ourselves, by statistical devices of clearing the data from influences not taken account of in the theory (e.g. by multiple correlation analysis)

This line of reasoning, I think, constitutes what Morgan (1990, p. 122) argues to be the justification (in their argument with Keynes) of her so-called 'thoughtful

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Pearson and, later, Neyman). Spurious correlation is distinct from nonsense correlation. Nonsense correlations is the standard terminology for high but meaningless correlations between integrated (or strongly autocorrelated) time series. If time is the relevant omitted variable, then nonsense correlation becomes a special case of spurious correlation.

[15] This example nicely illustrated what Keynes (1939, p. 561) meant with his remark that, 'if the variables are not independent of one another, we lay ourselves open to the extraordinarily difficult and deceptive complications of "spurious" correlation'.

[16] Alternative options are sometimes available as well. For example, Moore (1914, p. 69) argues that transforming variables to relative changes may eliminate the effect of increasing population, and using relative (instead of nominal) price controls for general changes in the price level.

econometricians' for using methods of multiple correlation in economics. Multiple correlation allows one to deal explicitly with *ceteris paribus*, and to take care of the problem of spurious correlation.

Was Keynes badly informed about the accomplishments of statistics after the appearance of his *Treatise on Probability* in 1921, as suggested by Morgan (1990)? I do not think he was. Keynes knew very well about Yule's work (on multiple as well as spurious correlation). He simply was not satisfied with the validity of the assumptions necessary for applying the methods to the specific example that occupied Tinbergen.

Therefore, consider the question whether multiple correlation can solve the problem of unenumerated *ceteris paribus* conditions in explaining investment demand. Reconsider the multiple regression model (1). Advancing the argument of the next section, let y be investment, and x_i (i=1,...,n) be the relevant variables to explain investment. The problem is the choice of relevant variables. Tinbergen (1939a) considers the price of capital, the general price level, the volume of production of consumption goods, lagged profit (and profit margins), and the interest rate. Is this an exhaustive set of variables for a valid investment equation? Keynes (1939, p. 560) argues that, for valid inference or induction, such an exhaustive set of *verae causae* is required. An obvious omitted variable is the expectation of future profit. Note that nothing in Keynes' writings suggests that such a true cause should turn out to be statistically significant in the context of a specific set of data. It is a matter of logic, not a matter of the sign, magnitude or magnitude of the standard error of an estimated parameter. The justification of inductive inference relies on logical, not empirical, arguments. In a letter to R. Tyler of the League of Nations concerning Tinbergen's research project, Keynes ([1938] 1973, pp. 285-6) complains that

> There is first of all the central question of methodology,--the logic of applying the method of multiple correlation to unanalyzed economic material, which we know to be nonhomogeneous through time.

Keynes criticizes Tinbergen's neglect of logical analysis. In particular, before one applies probabilistic inference, one should know (analyze) whether the principle of limited independent variety is satisfied. As this is not done, Keynes has strong reservations about the validity of the 'inductive hypothesis' in this case.

Tinbergen (1940, p. 141) replies to the criticism expressed by Keynes (1939) concerning the enumeration of the *verae causae*:

> *in so far as one agrees*
> (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; and
> (c) that the mathematical form of the relation is given,
> *certain details on the probability distribution of their "influences" can be given*. (emphasis in original)

The details referred to are mean and standard deviation of the estimates. The second condition, Tinbergen argues, can be tested (*e.g.* tests on residual correlation), while the third can be justified in mathematical terms (*e.g.* linearity of equations is a first order approximation to a non-linear equation). Note that 'relevance' in Tinbergen's writings does not coincide with 'statistical significance'. A parameter may be significant but, from an economic point of view, unimportant. Both Keynes and Tinbergen were aware of such limitations of significance tests.

For the time being, concentrate on condition (a). Previous work of Tinbergen suggested that lagged profit is a reasonably good forecaster of future profit (see the previous section). Keynes ([1936] 1973b, p. 154) does not deny that investors will assume that the future will resemble the recent past--indeed this is part of his theory of conventional behaviour (based on habits rather than rational, mathematical calculations). However, under changing circumstances this expectation may not be justified.[17] The true determinants of investment cannot be listed. Too many variables, captured by 'animal spirits' and irrational sentiments, play a role. They cannot be put into the error term of less relevant variables, as they are the ones that matter. Unlike in games of chance, one should be suspect to claims of a limited number of stable 'permanent causes' in the social sciences (Keynes [1921] 1973a, p. 458). The logic of induction then warrants scepticism about econometric exercises in such cases.

Tinbergen's multiple correlation model is seriously incomplete, from Keynes' perspective. The model is an oversimplification of an intrinsically complex economic problem (see the epigraph of this paper). For similar reasons, Liu (1960) formulated a devastating critique on the structural econometrics program of the Cowles Commission, which may be regarded as the heir to Tinbergen. Keynes did not have the econometric vocabulary (or mathematical skills) to make this point as clear as Liu did, but Liu's attack is consistent with Keynes' critique (although Liu's remedy is not).

Liu (1960, p. 858 fn. 8) analyzes an investment equation to illustrate the point. Consider the regression equation (1), where y is investment, x_1 profits, x_2 year-end capital stock, x_3 liquid assets, and x_4 the interest rate:

$$y = a_0 + a_1 x_1 + a_2 x_2 + a_3 x_3 + a_4 x_4 + \mu. \tag{3}$$

Following Liu (*ibid.*), suppose that the estimates for a_2 and a_4 yield 'wrong' signs, after which x_4 is dropped. The new regression equation is:

$$y = a_0' + a_1' x_1 + a_2' x_2 + a_3' x_3 + \mu. \tag{4}$$

However, unknown to the investigator, the 'true' investment equation is a more complex one, for example:

[17] One may complain that Keynes does not provide a viable alternative to a mathematical expectations theory, his theory merely being that such a theory is not possible. I will not discuss this complaint or possible defence of Keynes but rather refer to the Post-Keynesian literature.

$$y = b_0 + b_1x_1 + b_2x_2 + b_3x_3 + b_4x_4 + b_5x_5 + \mu. \qquad \textbf{(5)}$$

Textbook econometrics teaches that the expected values of the vectors of parameters ***a*** and ***a'*** are functions of the vector of the 'true' parameters, ***b***. Those functions depend on the size of the parameters in ***a***, and the correlation between the omitted and included regressors. The simplification from (3) to (4) may be exactly the wrong strategy, even if the deleted variable is insignificant in (3). Liu concludes that overidentifying restrictions in structural econometrics are very likely to be artifacts of a specification search and not justifiable on theoretical grounds. The investment equation of the Klein-Goldberger model is not identified, despite the apparent fulfilment of the identifying conditions. Liu's advice is to concentrate on reduced form equations if prediction is the purpose of modelling.[18] This, however, was not Tinbergen's purpose. His aim reached further: to test business cycle theories. Liu's sceptical argument provides analytical supports for Keynes' critique of Tinbergen's method. Even decades after Tinbergen's pathbreaking exercise, econometricians remain sceptical about parametric identification.

Explicit '*ceteris*' which are thought to be relevant and known to fluctuate are included by means of additional regressors. In common parlance, the econometrician 'controls' for the effects of those variables. The disturbances of the model are supposed to take care of 'implicit' *ceteris paribus* conditions. The problem of independent variety, it seems, is resolved. This, indeed, is the contention of Stigler (1986). The example presented above shows the difficulties involved with the choice of regressors.

5. Econometrics and business cycle inference

There have been numerous discussions of the Keynes-Tinbergen controversy. Many of them miss the point of Keynes' critique by neglecting his view on the logic of

[18] Sims (1980) can be regarded as an extension of Liu's argument.

probabilistic inference and its implications for modelling phenomena in which expectations matter crucially.[19] Morgan (1990, p. 124) provides, in my view, an interpretation which does not do justice to Keynes' argument, by asserting that 'Because Keynes "knew" his theoretical model to be correct, logically, econometric methods could not conceivably prove such theories to be incorrect.'

Keynes may be accused of arrogance but not of dogmatism (in fact, one will have a rather difficult case to defend the thesis that the moral-philosopher Keynes viewed his theories as 'true').[20]

Indeed, after supporting Tinbergen's (1939a, p. 12) acknowledgement that econometrics cannot verify a theory,[21] Keynes (1939, p. 559) questioned whether the more modest aim, to 'prove' a theory incorrect, could be attained by Tinbergen's methods. This question did not arise from a dogmatic belief in the truth of theory, but from the logical limitations of statistical inference.

Keynes' main problem with the usage of statistical analysis for inductive inference on the business cycle is that either not enough is known to ascertain the validity of the

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[19] For example, the basic point at stake is not whether Tinbergen's model is mis-specified (although this may be of additional interest). Modern econometricians who are trained to search for mis-specifications are inclined to interpret Keynes' criticisms as being primarily related to today's' concepts like 'simultaneity bias', 'structural stability', 'nonlinearity' and 'autocorrelation'. Of course, these are very legitimate and important issues to consider, but the logical point emphasized by Keynes has been understated in such investigations. See also Pesaran and Smith (1985) and McAleer (1994).

[20] See for a supporting view and further discussion Moggridge (1992, p. 163).

[21] 'no statistical test can prove a theory to be correct. It can, indeed, prove that theory to be incorrect, or at least incomplete, by showing that it does not cover a particular set of facts: but, even if one theory appears in accordance with the facts, it is still possible that there is another theory, also in accordance with the facts, which is the "true" one, as may be shown by new facts or further theoretical investigations. Thus the sense in which the statistician can provide "verification" of a theory is a limited one.' One may add that 'prove' is a misleading term, as it relates to logic, while probabilistic inference does not yield logical proofs or refutations. Keynes was well aware of the distinction but may be accused of sloppy terminology.

application of the principle of limited independent variety, or it may be concluded that the principle is not applicable. If the principle does not apply, it follows that inductive claims based on the use of multiple correlation for testing business cycle theories are not valid. The assertion that Tinbergen and other 'thoughtful econometricians' were already concerned with this logical point does not hold ground: the debate between Tinbergen and Keynes shows that they are talking at cross purposes. Tinbergen never seriously considers the problem of induction.[22]

How then does Tinbergen (1939a) proceed? A statistical apparatus for testing rival (possibly non-nested) theories did not yet exist. But even the methods of statistical hypothesis testing available in the late thirties were not of immediate use. Tinbergen was interested in importance testing, *i.e.* in testing the *economic* significance of specific variables. The issue is whether particular effects have the right sign and are quantitatively important. Tests of significance are used to test the 'accuracy of results' or weight of evidence (1939b, p. 12). They are not used as tests of the theories under consideration.

The procedure to test theories consists of two stages. First, theoretical explanations for the determination of single variables are tested in single regression equations. Secondly, the equations are combined in a system and it is considered whether the system (more precisely, the final reduced form equation obtained after successive substitution) exhibits the fluctuations that are found in reality. In order to test rival theories, Tinbergen combines the most important features of the different theories in one 'grand', comprehensive model. Tinbergen's inferential results are like the finding that interest does not matter very much in the determination of investment, or that the acceleration mechanism is relatively weak.

Tinbergen (1939b, p. 12) acknowledges that certain conditions have to be satisfied for

[22] Tinbergen's methods of inference are primarily based on pragmatic applications of Yule's multiple regression, supplemented by methods of Frisch (bunch maps) and, to a lesser degree, R.A. Fisher. The methods of testing are not (the then increasingly popular) significance tests, but rather economic importance tests.

successful application of the multiple correlation method. The model specification problem is dealt with by *a priori* considerations concerning which variables to include, and trial and error if the *a priori* convictions are not strong. Linearity of the relations is in most cases assumed as a convenient first-order approximation. Those problems of specification raised Keynes' suspicion. A serious complaint of Keynes deals with Tinbergen's choice of time lags and, more generally, the choice of regressors. Keynes (1940, pp. 155-6) suggests a famous experiment to Tinbergen:

> It will be remembered that 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?

This rhetorical question has been answered by the postwar practice of empirical econometrics: there is no doubt that specification uncertainty and the coexistence of rival, conflicting theories raise fundamental problems of statistical inference that have not yet been solved analytically.[23] In particular, this applies to econometric modelling of investment- -a case where expectations are of tremendous importance.

According to Keynes ([1938] 1973, p. 296, letter to Harrod), statistics is to be used to test the validity of a model . This is infeasible in the case of the business cycle, because the condition of limited independent variety is not satisfied. The parameters found are not the 'necessary' ones that follow from a correct prior specification. Such a correct specification is not available in case of a complicated problem like business cycle analysis. On top of all, it may even be the purpose of inference to influence economic policy and to *change* the circumstances driving the econometric relations.[24]

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[23] Magnus and Morgan (1995) propose an 'experiment' which takes up Keynes' suggestion. Would they have done so were Keynes wrong?

[24] This point was first expressed on a meeting of the Dutch Economic Association in 1936. This meeting discussed the first macro-econometric model, developed by Tinbergen (1936) for the Dutch economy. Van Dorp (Vereeniging voor de

6. Conclusion: homogeneity and limited independent variety

The principle of limited independent variety forms the basic source of Keynes' attack on Tinbergen. In previous discussions of the Keynes-Tinbergen debate, this principle has been neglected. If the object of inference is not uniform over time, the principle is violated, hence, the 'inductive difficulties arise' (Keynes, 1939, p. 316). In a short memo to Harrod, Keynes ([1938] 1973, p. 305) implicitly refers to this point (emphasis added):

> TINBERGEN - YOUR LETTER OF THE 18TH
> If the three influences on investment are variable, it is *logically impossible* to discover by T.'s method the comparative dependence on profit lagged. He has to assume that they are constant. In fact we know that they are variable both in character and degree.
> I complain that this sort of logical point is not first discussed--or even mentioned.

Keynes does not reject statistics as such, but objects to assuming stable relations where they are likely to be changing (an interesting problem, not solved by Keynes, is how this can be known and, if this is not possible, what the best way forward might be). In a letter to Tinbergen (dated 20 September 1938), Keynes points to a dilemma: '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, 1973, p. 294). The arguments in Keynes' correspondence with Harrod and the review article in the *Economic Journal* are just the same as the ones given in the *Treatise on Probability*: without indication for the constancy of parameters,

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Staathuishoudkunde en Statistiek, 1936, p. 61) criticizes the assumed stability of the mathematical relations as an (undesired) peculiarity of the estimation period (1923-1933). Keynes ([1938] 1973, p. 299, letter to Harrod) makes a similar point: 'to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought'.

the inductive (probabilistic) method is invalid (see, e.g., Keynes, [1921] 1973a, p. 434).[25] Even if parameters can be shown to be constant for the past, it is not yet clear whether they may be expected to remain constant in the future. This comes close to a radical form of Humean scepticism, but indeed cases where expectations matter strongly seem to be the ones where Keynes would deny the validity of the 'inductive hypothesis'. The complexity of the material is high, therefore, positive *a priori* probability is difficult if not impossible to establish. The logical importance of simplicity and complexity is clearly illustrated by this example (see Keuzenkamp and McAleer, 1995).

On July 4, 1938, Keynes wrote to Harrod that economic models should be flexible and one should 'not fill in the real values for the variable functions. (...) The object of statistical study is not so much to fill in missing variables with a view to prediction, as to test the relevance and validity of the model' (Keynes, 1973, p. 296). In a subsequent letter to Harrod, Keynes ([1938] 1973, p. 299) objects to the basis of Tinbergen's work, the formulation of economic theories as mathematical models:

> In chemistry and physics and other natural sciences the object of experiment is to fill in the actual values of the various quantities and factors appearing in an equation or a formula; and the work when done is once and for all. In economics that is not the case.

Models are not static tools that can be pinned down after a sufficient amount of empirical research. Economics should not be converted to a pseudo-natural science, in short. Why does Tinbergen's method destroy its usefulness as an instrument of thought? The reason can be found in the same letter to Harrod, in which Keynes argues that economics is not only a 'science of thinking in terms of models', but also

[25] Today, dealing with varying parameters is straightforward: popular methods are recursive least squares, the Kalman filter, or Bayesian updating. Testing for constancy is also standard (an example is the Chow test). These dynamic estimation methods rely on a stable pattern in the change of the parameters. Hence, if such stability again is unlikely to obtain (as it is in case of 'credit cycle' expectations), Keynes' critique remains valid.

an art of choosing the relevant models, i.e. relevant to the contemporary world. This world changes continuously, also as a result of the economist's actions. Unlike natural scientists, the economist has to be permanently aware of the changing, non-homogeneous, environment, and adopt new models as soon as necessary (Keynes [1938] 1973, p. 296, letter to Harrod).

Keynes adds that Tinbergen could not have selected a worse case for applying statistical inference: the credit cycle is complex and variable, many of its influences cannot be reduced to statistical form. Explaining the volume of investment, 'I should regard as prima facie extremely unpromising material for the method.' (Keynes, letter to Tinbergen, [1938] 1973, p. 295). Keynes' scepticism has been supported by the experience of applied econometricians: investment is known as a notoriously difficult and complex variable to model. Keynes, although occasionally mistaken on details, has a consistent and tenable position in his debate with Tinbergen. I do not conclude, however, that Tinbergen's work is meaningless--far from that. The issue is that the philosophical underpinning of his results is defective. But defective philosophy does not imply defective results. Indeed, Keynes (1940, p. 156) concluded his debate with Tinbergen, 'Newton, Boyle and Locke all played with alchemy. So let him continue.'

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An interesting distinction between the first econometric models and the postwar generation of such models is the way in which government policy is treated. In the postwar models, budgetary and monetary policy are usually represented by exogenous variables which can be tuned to the desired outcome of the model. They may be regarded as intercept terms. In Tinbergen's (1936) model of the Dutch economy, policy enters via changes in the parameters of the model, i.e. via slope terms (occasionally, slope terms like tax rates still figure as instruments). Economic policy entails a change in parameters (e.g. propensity to consume), which may have been a natural way of dealing with the issue for a physicist. The discussion between Tinbergen and Keynes in the *Economic Journal* is related to this point. If policy aims at changing the economic model (changing the parameters), as both Tinbergen and Keynes understood it at that point of time, Keynes may have been right in asking for a justification of the stability of the parameters in Tinbergen's model. Keynes opposed Tinbergen's engineering approach to economics, although Tinbergen held very similar views about economic policy. While Keynes' critique was primarily on the logical foundations of Tinbergen's project, Tinbergen's justification was entirely pragmatic.