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THE ECONOMETRIC CHALLENGE TO KEYNES:
ARGUMENTS AND CONTRADICTIONS IN THE EARLY DEBATES ABOUT A LATE ISSUE

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Abstract. [1]

It has been thoroughly argued that one of the reasons for the impressive, general and initially unopposed acceptance of the 'synthetic' interpretations of the 'General Theory' was the strategic choice of the 'reconcilers', wishing for a theoretical truce with neoclassical economics in order to concentrate in the urgent policies against the Great Depression. Keynes did not campaign against those choices, although he resisted in some private letters and argued for a radical interpretation of his message, at least in the QJE piece in 1937.

The current paper investigates new elements highlighting some features of those debates. Namely, it focuses on the attitude of the econometricians and, in particular, of Ragnar Frisch and his closest associates in the 1930s, those men engaged in the foundation of the *Econometric Society* and in the mathematical reconstruction of economics.

One of the skirmishes between Keynes and the econometricians is well known and researched: the review he provided in 1938 of Tinbergen's work for the League of Nations and the subsequent debate. By that time, Keynes was deeply hostile at least to use of the current mathematical formalism, and made no secret of that. As a consequence, he hastily dismissed Tinbergen's research, which was intended at giving his theories the empirical authoritative content allowing for their imposition as policy rules. The paper emphasizes the importance of the action the fellow econometricians took at the time in relation to this polemics, and provides new evidence about their organized attempt to counter-balance Keynes's critique.

Yet, in spite of the fact that they rapidly joined Tinbergen at the gates of the threatened citadel, a debate was going on among the econometricians about the applicability of the new methods. In that regard, the contradictions and the evolution of the econometricians themselves on these issues are frequently misread or wholly ignored. The paper presents documentary evidence about this landscape of a pluralistic and live technical and epistemological discussion, in which Frisch, Tinbergen, Lange, Marschak, Divisia and others intervened in order to use Keynes's theories, or to address the same problems, and to use the new methods for improving social policies.

After the marginalist breakthrough of the 1870s, the scene was set by the end of the first quarter of the century for a new approach to economic theory and measurement,

giving the signal for the emergence of a method which extended its empirical capacity and analytical scope – that was the role and the ambition of econometrics. The ideology, the methods and the new breath of confirmationism they made possible, and the theories of the emerging research program were getting available, comprehensive and eventually coherent. Yet, they were invited to cross a complex, pluralistic, and diversified landscape of theories and traditions. On the one hand, large part of the empirical and concrete investigation on economic series was developed under the auspices of Wesley Mitchell, frankly hostile to the general equilibrium paradigm. The Keynesian school, on the other hand, developed in the late 1930s an effective critique of 'classical' economics, pushed through by the impact of the Great Depression, which destroyed the charm of equilibrium as an accurate description of reality and dramatically required new policies.

This paper investigates the conditions for the emergence of econometrics in relation to the debates it generated in the 1930s. It points out some evidence for the argument stating that the very motivation for the theoretical and practical intervention of economists of Keynesian inclination eased the victory of the second neoclassical revolution, under the form of the 'synthesis'. In this sense, the acceptance of the epistemological primacy of a very peculiar brand of a simple mathematical formalism for the macro-theories led to the wiping out of the major theoretical alternatives of the first half of the century. Evidence shows that the endorsement of the urgent political agenda for action against unemployment was instrumental for the victory of the econometric program as it came to be conceived of in these incipient years, and that that primacy facilitated the abolition of the reformist agenda itself. In that framework, Keynes was more hostile and certainly more aware of the dangers of the mechanistic and dominant simplistic mode of mathematical expression of the economic models than of the effects of the reconcilers' synthetic efforts, whereas some of the leading econometricians were eventually more inclined than Keynes to the structural attack against the capitalist inequalities and imbalances. The final result was a Phyrric victory - or defeat - of both the Keynesian program and the original intentions of the founders of econometrics, as most of them recognised with sorrow.

The first section presents a very brief outline of the argument, centred on the equilibrium reinterpretation of the 'General Theory' (Oxford, 1936), the next section discusses the emergence of econometrics and the main consequences of the Keynes-Tinbergen debate (Cambridge, 1938) and, finally, some conclusions are presented.

1. Oxford, 1936

The Keynesian heterodoxy matured for some time, while its developments were publicly discussed and followed by a large number of scholars, and attracted quite a lot of attention since its early formulations. In the crucial years from 1930 (*Treatise on Money, TM*) to 1936 (*General Theory, GT*), this movement eventually generated a large consensus in the profession,[2] as Carabelli, Young, O'Donnell, Moggridge, Skidelsky and others indicated. Keynes was indeed considered to be the more important and was certainly the most influential economist in the early thirties, and Keynesianism became the theoretical framework for the analysis of the business cycle, of unemployment and of the distribution of income.

Through this paper, one of the characteristics of his new vision is emphasized. That was the option for a causal, sequential, deductive and dominantly literary mode of analysis, which was, at least for Keynes, the conclusion of his own intellectual trajectory since the early days of his research on the logic of probability, namely the preparation of the *Treatise on Probability* (TP) and the polemics with Karl Pearson. Instead, the econometric program endeavoured to challenge that tradition, favouring simultaneous causation and a simplistic framework as encapsulated in the simultaneous equation approach. And an important contribution for it came from the internal neutralization of the implicit and explicit philosophical implications of Keynes's work and its reduction to elementary mechanical models.

1.A. The breaking of the old consensus

There is large evidence to prove that the submission of economic arguments to mathematical formulations was still seen by the early thirties as a difficult, dangerous and eventually unwise step. Not least than the workings of the recently created (1930) *Econometric Society* provide indicative examples of that conflicting *état d'esprit* among the mathematically inclined economists. For instance, in a 25 November 1935 letter to Schumpeter, the secretary of the Society, Charles Roos, complained about the difficulty to get grants and namely about the possible negative assessment of most of the eventual referees if consulted by the financing institutions. According to Roos, the mathematicians would be hostile to the projects of the *Econometric Society* but, worse, some of its own members could take an unpredictable attitude. That would be, in Roos's opinion, the case of Snyder, 'quite unfavourable' to such projects, and of Wesley Mitchell, both founders of the Society.

Mitchell was one of the famous and more respected of the first members. When in February 1933 the members of the Society elected for the first time their Fellows, the single most voted candidate was precisely Mitchell (57 votes), whereas Fisher, the first elected President of the Society, as well as other founders as Frisch, Schumpeter, Divisia and Roos, got only 54 ballots each. And the second doubtful referee, Snyder, was nominated by the *Econometric Society* to the Advisory Board of the *Cowles Commission*,[3] in spite of the general mistrust as indicated in the letter by Roos (and, according to Schumpeter, Snyder did 'not know an integral from a radio'[4]).

One may interpret that situation as the result of the underdevelopment of mathematical economics, and that was certainly the case. But the point is also that it corresponded, at least for some of the economists, to a radical hostility against the reduction of the subject matter of economics to the constraints of the available techniques. For some of these economists, that reduction implied the acceptance of a rather poor set of assumptions, far away from what the theory was asked to address. Furthermore, the statistical treatment of economic material was still based on rather unclear hypotheses and restrictions on data.[5] Keynes time and again expressed that point, namely in his private correspondence to Frisch:

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 with my own intuitions; and I am therefore grateful for an author who makes it easier for me to apply this check without too much hard work. (Letter from Keynes to Frisch, 24 February 1932, OU)

It is quite obvious that Keynes's scepticism about the mathematical proof of economic theories increased with his experience of policy making and was based on his intuition of the organic unity and complexity of society, and his awareness of the dangers of the general fallacies of composition. The point is also that this scepticism was widely shared in the profession, for the more disparate reasons, and was certainly accepted by some of the forerunners of econometrics (the cited examples of Mitchell and Snyder, or that of Amoroso, to take another founder of the *Econometric Society*). But not by all: Frisch, Tinbergen and the youngest generation were deeply dedicated to the mathematization of the discipline, and they were the driving forces of the new organization.

The subsequent destiny of these crucial Keynesian topics - the choice of the policy approach to solve the unemployment problem and consequently the determination of the subject and purpose of economics as a moral science - decided the fate of the science for the next decades. In fact, the acceptance of the urgency of providing the adequate economic advice eased the imposition of toy-macro-models, either in the simple IS-LM mood or in the more elaborated low-dimension system of linear equations, as Tinbergen used. Thus, this reconceptualization of economics was in turn influential in the subsequent dilution of the reformist aims of the Keynesian agenda.

1.B. Normative economics: Keynes and Frisch

In spite of the very vague contours of the epistemological discussion about the scientific status of economics in the thirties, it is fair to state that the gross divide opposed a very disparate camp, with Keynes, Hayek or Mitchell, who fought against the adoption of the physical metaphor for economics, and those willing to acquire the concepts and to mimic the rigour and the standing of physics as the means for delivering exactness and certainty. Indeed, the role and the centrality of the analogy with physics - and of the ambition to reproduce and to imitate the methods of the natural or 'exact' sciences - was acknowledged by the users of the new brand of methods and was vindicated as part of their specific contribution to the progress of the discipline.[6]

But, unlike the previous generation – of the general equilibrium economists –, the early econometricians presented their case as an argument and the means for the necessary intervention in the conjuncture: some of these influential young economists were convinced activists. While considering the social problems of their times and consequently the tasks of economics, a vast majority of the protagonists of this story accepted the centrality of the problem of unemployment and even of most of Keynesian remedies. That was certainly the case of Frisch in the early thirties. In his inaugural lecture as professor of Oslo University, as soon as 1932, he explained that: 'quantitative formulation of laws and concepts is very nearly as important in economics [as in natural sciences]. This can be seen most clearly if we consider the final goal of economic theory, which is to clarify the inter-relationship between the various factors, and to do so in such a way as to secure a basis for evaluating what practical measures are most suitable to promote socio-economic aims' (Frisch, 1932). In other words, exactness and mathematical rigour was necessary in order to provide better policies and sounder economics.

Furthermore, State intervention was required since market self-regulation was supposed to imply social disaster. In the draft for a previous speech, prepared in the autumn 1931, Frisch wrote: ‘The depression is a sum of unhappiness and misery, and that is why something has to be done in order to stop this crazy and undignified dance that is the business cycle in a modern capitalist society’ (quoted by Andvig, 1992: 299). The point was therefore that, in order to avoid the ‘undignified dance’, the modern market should be regulated. Later in the same year, Frisch published an article in *Tidens Tegn*, a conservative newspaper, under the title “Plan eller kaos” (“Plan or Chaos”), in which he analysed the crisis as the intrinsic consequence of capitalist organization and argued even clearer:

> One has to understand that the ongoing crisis is not a crisis of real poverty, but an organizational crisis. The world is like a ship loaded by the goods of life, where the crew starves because it cannot find out how the goods should be distributed. Since the depression is not a real poverty crisis, but one of organization, the remedy should also be sought through effective organizational work inside the apparatus of production and distribution. The great defect of the private capitalist system of production as it is today is its lack of planning, that is, planning at the social level. This cardinal point cannot be disputed. (5 November 1931, quoted in Andvig, ibid.: 287)

For Frisch, the crisis was a consequence of the inequality and skewness of distribution, both among branches of industry and among social classes.[7] The solution, therefore, was a managed change in social organization combined with expansive monetary and fiscal policy of a Keynesian type. As a consequence, Keynes’s work was attentively followed and discussed in Frisch’s circles: from his publication and for the next decade, Keynes’s GT was taught as the basic course of macroeconomics in Norway (Bjerve, 1995: 20), and Frisch had previously used TM for his lectures.

But Frisch’ interest in Keynesianism faded away during the thirties: he never thought GT to be a truly original work and did not consider it very much,[8] and furthermore he sincerely thought Keynes failed to meet the expectations, given the equilibrium condition adopted in GT. In his tribute to Wicksell, Frisch wrote that when Keynes told him in Cambridge, when they met the 12 March 1934, that he finally decided to equilibrate S=I in his model, he felt deeply disappointed: ‘I vividly remember the deception I felt one evening when Keynes told me that he had finally decided to make actual investment by definition equal to actual saving. I am sure this was a step backwards in the GT as compared with his TM’ (Frisch, 1952: 669).

By that time, Frisch was already moving to the advocacy of other forms of economic policy, rather than those suggested by Keynes. The reason was his understanding of the urgency of effective action against poverty and unemployment, and his awareness of the technical limitations of the current models in order to provide rigorous policies. Consequently, he looked elsewhere and developed a new policy proposal in his long 1934 paper in *Econometrica*, emphasising again and again the ‘monstrosity’ of the situation:

The most striking paradox of great depressions, and particularly of the present one, is the fact that poverty is imposed on us in the midst of a world of plenty. Many kinds of goods are actually present in large quantities, and other kinds could without any difficulty be brought forth in abundance, if only the available enormous productive power was let loose. Yet, in spite of this technical and physical abundance, most of us are forced to cut down consumption. (...) A full recognition of the monstrosity of this situation is the first and basic condition for any intelligent discussion of ways and means to get out of the depression. Of course this implies the conclusion that the cause of great depressions, such as the one we are actually in, is in some way or another connected with the present form of organization of industry and trade. The depression is not a real poverty crisis, [is] not due to an actual shortage of real values. This must be admitted by everybody, regardless of political color. (Frisch, 1934: 259)

This paper, 'Circulation Planning', went further than any of his previous contributions, arguing for a voluntary scheme of direct and moneyless exchange among the economic agents, under some 'organizer's' supervision. In the following years, Frisch always maintained the same analysis of the Great Depression[9] and even extended it as the rationale for the social and economic engineering he was arguing for, progressively abandoning the indirect steering mechanisms of Keynesian flavour and consequently favouring direct planning and control.

For Frisch, and obviously at least for some of his colleagues engaged in the econometric program, the questions of unemployment and distribution of income were decisive: very soon, the World War was seen as the confirmation of their darkest prognosis. Most of them moved in the framework of these problems,[10] and this was indeed why it was so easy for them to eventually dominate the economic research. But, paradoxically, the fact that they wanted desperately to avoid these economic horrors and to prevent the 'monstrosity' caused by 'poverty amidst a world of plenty', a new 'disaster for millions' of human beings, opened the way for the revision of the Keynesian agenda. Indeed, it allowed for the imposition of equilibrium economics, associated as it was to the most important, the only well known and available tools for quantification and estimation – and quantification was required by their approach to the economic problems. The inductive statistical treatment of economic data, the confirmationist estimation of systems of simple linear equations, all that paraphernalia was being made available and the early econometricians were eager to use it.

From their experience and from their theoretical foundations - both the evidence of the crisis of the 1930s and Wicksell's influence, as far as Frisch was concerned – these men knew that disequilibrium was the crucial enigma for real life economics. But their desire to avoid it facilitated the recourse to easily computable models and to modes of theorizing dominated by the mathematical expertise of the current time, and which assumed or desired equilibrium. In other words, their endeavour was finally dominated by one of the available answers and not by the question itself - somehow, the answer changed the nature of their own question.[11]

Two major events, the Oxford meeting in 1936 and that in Cambridge in 1938, illustrate this evolution.

1.C. The 'frightful tendency to compromise'

The publication of GT caused an expected and large impact in the profession. It was an impressive achievement: a synthesis of a large experience in economic observation, explanation and policy making; a recapitulation of some of the most advanced and fruitful conjectures of the time; and an authoritative voice for the economic activism most economists were wishing to engage in. But its flaws, its unexplained innovations and changes in relation to TM and its style undermined its influence: a considerable confusion between the dynamical properties of the model — implying disequilibrium — and the comparative static account in which it was framed, allowed for many different and contradictory interpretations. Even worse, some of them were not clearly unauthorized by Keynes himself, such as the influential IS-LM equilibrating mechanism: based on this, the 'Keynesian-classical synthesis' reintroduced equilibrium in a matter of years. As authoritative researchers already investigated this story, the current section is limited to indicating some of the evidence on the econometricians' reaction to it.

As it is well known, the first version of what came to be known as the IS-LM scheme was presented by Hicks to the sixth European meeting of the *Econometric Society* at Oxford in September 1936, just some months after the publication of the *General Theory*, together with other papers on the topic by Harrod and Meade. This was a very important meeting, where the most distinguished econometricians presented their research. Frisch presented a paper on 'Macrodynamic Systems Leading to Permanent Unemployment', on the role of profit in business cycle, written 'in a quite non-Keynesian vein' (Bjerkholt, 1995: 20). Haavelmo presented then his first paper to an *Econometric Society* meeting, while Jerzy Neyman presented the Neyman-Pearson theory, which was decisive for the future course and standardization of econometric confirmationism.

Acquainted with Meade and Harrod's papers to be presented to the Econometric meeting - and eventually also with Champernowne's (Darity, Young, 1995: 7) - Hicks suggested a formal and geometric representation which established the success of the paper. It was a clear and useful tool; it could be easily adapted to several pedagogic and practical purposes; nevertheless, it was at odds with Keynes's original formulation. In spite of this, the IS-LM and the simultaneous equation interpretation become dominant in the reception of Keynesianism. The general explanation for that evolution, as provided by Skidelsky, Moggridge and others, is that the main followers and disciples of Keynes wanted that to be so, and that those who reacted - Joan Robinson, Kahn and G. Shackle - were very few and very late, since they did not take that position neither in the Oxford meeting (in which they did not participate) nor in the immediately subsequent moments, as they later regretted. The early Keynesians saw the GT as a 'machine for policy, and interpreted it primarily as providing a rationale for public spending' (Skidelsky, 1992: 538). In that sense,

Hicks, Harrod, Meade and Hansen in America, the leading constructors of "IS-LM" Keynesianism, had a clear motive: to reconcile Keynesians and non-Keynesians, so that the ground for policy could be quickly cleared. These early theoretical models incorporated features which were not al all evident in the magnum opus, but which conformed more closely to orthodox theory. The constructors of these models also thought they were improving the original building. (ibid.)

This explanation is here unreservedly accepted. But one must add another point, which deals with the powerful force of formalization explaining both the rapid spread of these versions and the lack of concern of Keynes. And that is emphasized by the very observation by Hicks, when he later became disappointed with the scheme, that the diagram was only designed for 'expository purposes'. He then added a crucial point, namely that 'I am sure that if I had not done it, and done it in that way, someone else would have done it very soon after' (Hicks, 1979: 73 n.).

It was certainly true that, after a long and somewhat public preparation, the core ideas of GT on methodology, on uncertainty and on evolution challenged those of the newly formed group of econometricians. But, since many of them shared the overall vision of GT, they intended to prove it could be framed as an exact model, surpassing Keynes's hesitations in relation to the mathematical formulation of economic theories. Indeed, they thought that such was the only way to move forward, with or without Keynes.

The econometricians – or at least Frisch taken as a reference point for the movement as a whole – did not all share the idea that formulating formal equilibrium models was the only legitimate way for developing macro-theories. But they were strongly devoted to the claim that their mathematical treatment was the only adequate means for explanation - and, therefore, were not able to depart from the equilibrium framework, in spite of the contradictory fact that some of them did not hesitate to criticize the paradigm. As in the case of Frisch, disequilibrium was part of his dramatic vision of world events and dangers, although his scientific effort was on that point divorced from that view and dedicated to the formulation of exact models whose equilibrium conditions were so decisive for computation. In short, the attention to real world disequilibrium justified the use of thought experiences with equilibrium models.

In that sense, it is indeed probable, as Hicks implied, that Marschak, Leontief and Frisch were ready to put a simplified version of GT in the mould of a formal model and to proceed to its discussion, as revealed by the preparations for the Econometric meeting. One outstanding piece of evidence for that is the illuminating letter Marschak wrote to Frisch, while organising the meeting in his by then hometown of Oxford:

> Incidentally, I had a few days ago a somewhat similar idea - that it would be a good thing to ask one of Keynes's adherents to explain to us in a clear (i.e., mathematical) way the substance of his new book [<u>this sentence was underlined by Frisch, "excellent!"</u>] which now creates a sensation among English economists.
>
> I hope that it would be possible to get reporters for at least the following subjects: 1) the main ideas of <u>Keynes' new book</u>; I shall ask Kahn, or Meade, or, if you prefer to have Keynes himself I should suggest that you should write him. 2) on elasticities of substitution (...), R. Allen or Hicks; 3) on imperfect competition, M. Allen or Joan Robinson, or Hicks; 4) definition of income, savings, etc., Lindhal; 5) international relations, by Ohlin, or Harrod, or Lerner. (...)
>
> On pp. 297-8 of his new book Keynes makes some nasty and unfounded remarks against mathematical economics. Owing to his enormous influence, that makes our task even more urgent. (Marschak to Frisch, 8 February 1936, OU)

Finally, the papers on Keynes were presented by Hicks, Harrod and Meade, and were immediately published in the following issues of *Econometrica* (Harrod's in

January, Meade's in February and Hicks's in April 1937). They were warmly welcome, in particular Hicks's: 'I am very glad to have this for *Econometrica*. I think it is a exceedingly valuable paper' (Frisch to Hicks, 20 November 1936, OU). From the available evidence, the discussion at the meeting itself was very intense and highly rated by the participants: the result was seen by some as part of a collective effort to reshape the economic theory of the time. And that explains the priority given to the publication of the papers in *Econometrica*, edited by Frisch – as a comparison, Slutsky's now famous paper took some four years to be published, after the translation was ready.

Frisch engaged in a campaign of letters to try to convince Hicks to include 'an elaborate footnote to be included at the beginning of the paper, explaining what happened in the intensive discussion in Oxford. In particular Lindhal's name should be mentioned. Also perhaps Kalecki and all the English who took an active part. I really think it would be fair to mention these circumstances. It would also be interesting from the new [view?] point of the Econometric Society' (20 November 1936, OU as well as the following). Two months afterwards he insisted 'I wish you would consider the suggestion I made in an earlier letter of adding a rather full footnote referring to the other people who have taken part in the colloquium discussions at Oxford on this topic' (15 January 1937). And still in February Frisch justified his suggestion, which was so important *from the point of view of the Society*: 'With regard to the footnote I thought I remembered your speaking rather enthusiastically in Oxford about the discussion on Keynes. This was the only reason why I suggested something in the way of a footnote. You must of course express exactly what you feel in the matter' (8 February 1937). Hicks had rejected the idea from the beginning, under the initial justification that there were so many persons to acknowledge that it became impossible: 'It [the paper] has had a great deal of rehashing, first as the result of the discussion I have had here. The list of acknowledgements got so long that it had ultimately to be scrapped altogether - which I have no doubt is what all concerned would prefer!' (Hicks to Frisch, 12 November 1936).

This correspondence also makes obvious that the final form of Hicks's paper was deeply reworked:

> With regard to the footnote, I will make some remarks in the proof about a useful discussion at Oxford; but the (problem?) is I can't go very far, because when I came to work it out the things that came out in the discussion didn't lead anywhere, and the version of my paper which was based on those points had to be scrapped. The present version, when it differs from that I read, has been much more influenced by later discussions at Cambridge then by what happened at Oxford. (Hicks to Frisch, 1 February 1937, OU)

If this is correct, then one may conclude that the final form was much more the result of the opinion of Keynes's inner circle than the outcome of the discussion at the *Econometric Society* meeting itself. This is quite plausible, since the driving force for this new simultaneous equations approach was a part of the Keynesian group itself, namely Harrod, (Skidelsky, ibid.: 611): after the Oxford meeting, it was finally Cambridge that dominated, but the result was the frightfully feared accommodation of Keynes's views.[12]

For evidence for this interpretation, one must turn to the other main character in this story, Harrod. Harrod's paper was acknowledged by Keynes in a letter the 30 August 1936 as 'instructive' and 'illuminating' (XIV: 84), and the author, just as Hicks did in his own case, interpreted these words as a 'blessing' (Harrod, 1951: 453 n.). The episode came as a consequence of large efforts put by Harrod to influence the formation of the new theory. Although Kahn and Robinson's cooperation with Keynes were the essential pillar in the preparation of the GT,[13] Harrod took pains to try to influence the development of the new book through a 'heavy bombardment. (...) These comments were composed with fervour (...) but also with a persistent and implacable zeal to convert him on certain points" (ibid.: 452). Namely 'My main endeavour was to mitigate his attack on the 'classical school'. (…) It seemed to me that this was pushing his criticism too far, would make too much dust and would give rise to irrelevant controversies' (ibid.: 453).

The task was undertaken by a redescription of the Keynesian argument in the general equilibrium framework and by the claim that it implied just a 'shift of emphasis' in relation to the traditional theory (Harrod, 1937: 85). Keynes noticed it and, in the same letter to Harrod (30 August 1936), protested against the crucial mistake of ignoring his major contribution: 'You don't mention effective demand (...). To me the most extraordinary thing, regarded historically, is the complete disappearance of the theory of demand and supply for output as a whole, i.e., the theory of employment, *after* it had been the most discussed thing in economics' (XIV: 84). Without effective demand and the employment question, Keynes's general theory became meaningless: the reconciliation implied its misrepresentation and the victory of the classicals.[14]

Equilibrium was being restablished, and so or a long time, as the disciplinary paradigm for the economic science. Indeed, the deep involvement of at least some of the influential Keynesians in the definition of anti-unemployment and anti-cyclical policies cleared the ground for their reconciliation with the equilibrium troops and for the downgrading of the GT to the status of one exception in the framework of 'classical' economics. This movement which was not immediately fought by Keynes, who just emphasized his mains points (QJE, 1937) without apparently understanding the general implications of the disputable interpretation.[15] And, by then, this movement was convergent with that of the econometricians.

2. Cambridge, 1938

Keynes's paper in the *Quarterly Journal of Economics* was not his only reaction against the dangers of encapsulating his (or others') theories in a limited formal model. In fact, the most aggressive and unsuccessful of his polemics in that terrain was engaged just the next year, in spite of the rather poor condition of his health. He was then asked to referee the books Tinbergen was preparing for the League of Nations on the comparison of theories of the business cycles - a crucial question given its policy implications, as Tinbergen was quick to note –,[16] and Keynes ignited a fierce debate on the issue.

2.A. The 'old slippery problems'

Keynes's critique to Tinbergen was his most important contribution to the debate about econometrics. It surpassed by far his early polemics on statistical inference, although recapitulating some of its themes: the critique of the correlation techniques emerged from his 1907 dissertation, the 1910 polemics with Pearson and the preparation of the *Treatise on Probability*. My argument is thus twofold. First, the 1938 critique represented a reaction against the growing formalization of the discipline and the imposition of the mechanical metaphors. Second, Keynes's argument was defeated and subsequently ignored since it was out of phase with his own passivity in relation to the 1936 debate and since his disciples were directly engaged in 'reconciliation'. Furthermore, Keynes's methodological remarks about statistics were not understood, were scarcely discussed and were mostly despised since he was - then as today - seen by many as an outdated economist as far as the fashionable and promising techniques were concerned.

Nevertheless, the fact that Keynes did not follow many of the technical details of the books he was reviewing is quite obvious and was anticipated by many of those who knew him. Some impressive evidence can again be found in the correspondence with Frisch. In 1932, Keynes rejected a paper by Frisch[17] for the *Economic Journal*, on grounds that it could only be read by 'half a dozen readers' (letter to Frisch, 10 February 1932, OU). Two weeks later, he explained that he feared the limits of mathematical formalism, since intuition should lead the research, and not be limited by the restricted set of assumptions of a formal model. Three years later, acknowledging a book sent by Frisch, Keynes indicated his distance in relation to those techniques.[18] He emphatically repeated his distance in relation to the branch of mathematical economics, and claimed his mistrust about its performances and results, when subsequently rejecting another of Frisch's papers:

> But I am unfamiliar with the methods involved and it may be that my impression that nothing emerges at the end which has not been introduced expressly or tacitly at the beginning is quite wrong. (...) It seems to me essential in an article of this sort to put in the fullest and most explicit manner at the beginning the assumptions which are made and the methods by which the price indexes are derived; and then to state at the end what substantially novel conclusions has been arrived at. (...) 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. (...) [This creates] a mass of symbolism which covers up all kinds of unstated special assumptions. (Keynes to Frisch, 28 November 1935, OU)

It is quite obvious that what Keynes mostly feared was the inability of the mathematical language for expressing clear theories and to concentrate on the issues, and therefore the danger of engaging into irresponsible arithmetical mazes.[19] But most of the pedestrian inhabitants of the province of economics were not prepared or attentive to these epistemological quarrels, and were ready to ignore Keynes's advice and concerns.

When he was asked to review the Tinbergen's volumes, Keynes once again did not hide his 'lack of familiarity with the matter'. He even advised his correspondent to look for the *imprimatur* of 'someone more competent in these matters than I am' (letter to Tyler, 23 August 1938, in Keynes, XIV: 285). In spite of that limitation, he maintained his deepest opposition to the general procedure, which he had already argued in a previous letter to Harrod,[20] since 'to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought' (16 July 1938, XIV: 299). But Keynes missed again the exciting news of the econometricians: the emerging methods promised further developments that the simpler techniques he preferred could not even remotely match. Although there is a hunch of that feeling in the letter to Harrod, in which he envisaged simpler alternative methods and concluded that 'However, I may be wrong. I have not studied his work as carefully as you have' (11 August 1938, XIV: 302), Keynes maintained his point of view. It is quite obvious that he saw the whole episode as a remake of the early debate with Pearson, and that Keynes considered his objections still valid.[21] Therefore, when Keynes published the *Economic Journal* critique of Tinbergen's work (September 1939), he included namely the following sentences:

> Thirty years ago I used to be occupied in examining the slippery problem of passing from static descriptions to inductive generalizations in the case of simple correlation; and today in the era of multiple correlation I do not find that in this respect practice is much improved. (XIV: 315)

With this background, one can understand that the econometricians did not care too much about Keynes's critique: it was anticipated and summarized as the mere implication of a 'nasty' - as Marschak had put it - and permanently sceptic attitude against mathematics and, therefore, as part of the old heritage of literary economics that they were struggling to get rid of. The econometricians just felt that Keynes was again 'out of depth', and that was all.[22] On the other hand, his comments can be read as a rejoinder to the Oxford meeting: although Keynes did not openly relate these twin movements of the IS-LM formalism and of the early methods of estimation of the systems of equations as presented by Tinbergen, he could not have missed the issue.

This argument will be now briefly summarized, before considering three pieces of the debate, two which were only recently published (Lange and Marschak's text and Frisch's contribution to the Cambridge conference) and one which remains unpublished (Divisia's review of Frisch's paper).

2.B. Keynes and Tinbergen

Tinbergen's tests of the theories of business cycles earlier reviewed by Haberler were based on a model of 22 equations and 31 variables, computed for the 1923-1935 period for the US (several other series for different countries were used in the first volume). This work constituted the first large applied study with empirical data under the new research program, and many problems of estimation were identified and discussed; therefore, it constituted an impressive performance and progress for econometrics. Tinbergen used multiple regression in order to indicate the strength of the influence of the variables, and correlation to verify a theory as a whole; after estimation, he tested every equation for structural stability in different sub-periods. The author admitted that the specification of the equations was somewhat arbitrary and that it could not encapsulate all possible types of causality. Yet, he argued that the distinction between the impulse and propagation mechanisms was sufficient to provide a good estimation of the structure of the model and therefore to allow for the comparison of the theories of business cycles.

As it is widely known, Keynes's main criticisms to these early econometric methods were based on the complexity, qualitative nature and interdependence of the variables describing real social phenomena, and on the irreducibility of the evolutionary processes to the simple models. Consequently, Keynes suspected these methods using non-experimental and unique sets of data but performing statistical tests primarily designed for social case to which a well accepted probabilistic theory could be applied, and then developed for controlled experiments. In the world of organic systems, the correlationist methods may fail and, since this is the case for most of the relevant economic variables, no general inductive claim is possible from this method, according to the critique.

The main issue was indeed for Keynes the application of the method of multiple regression to non-homogeneous series in real time,[23] and the consequent problem of misspecification: the method and the results are only relevant if the researcher is able to indicate all the possible influences on the endogenous variable, if the theory is previously established and is correct, if there is no change whatsoever in the structure of the modelled system and if enough data is available to establish the correlation — a truly Laplacean set of requisites. Otherwise, misleading results may emerge, and the danger in fact is that the method makes possible any type of conclusion the researcher is looking for, as the Septuagint metaphor emphasized. On the other hand, since the method supposes homogeneity through time, the same structure must account for stable coefficients for the period under inspection, a dozen years in the case of Tinbergen: Keynes argued that this was not conceivable and that there was a trade-off between the length of the series needed for the exercise of multiple correlation and the assumption of the stability of the coefficients, restricted to very short series (XIV: 294). Consequently, the method was criticized in private letters as a 'mess of unintelligible figuring', as some sort of 'black magic' or 'charlatanism', a 'nightmare', a typical product of 'alchemy'[24] (ibid.: 289, 305, 320, 315).

For Keynes, the treatment of time was the *experimentum crucis* for the method - and, indeed, for any inductive statistical method - and he considered that Tinbergen failed to provide any meaningful alternative or even a bit of a progress in relation to the old correlation exercises, just old wine in new bottles. This eventually explains both the cursory reading of Tinbergen and the rudeness of the review, as he explained in a letter to Lange:

Does not every case to which Tinbergen has applied his method assume that the same formula is valid over a long period of years? If this is seldom or never the case, is it worth while to both [bother] about the details of his method? For this is not merely a casual assumption but one which is intrinsic to the whole way of proceeding. (Keynes to Lange, 10 April 1940, Marschak Archive at UCLA)

It is quite possible that, even knowing Keynes's scepticism about the mathematical applications to economics, this rather unfriendly critique surprised Tinbergen.[25] Nevertheless, he acknowledged and accepted some of Keynes's conditions for the use of the method arguing that they could be solved under some restrictions:

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, or

c) that the mathematical form of the relation is given,

certain details on the probability distribution of their "influences" can be given. (Tinbergen, 1940: 141)

Tinbergen was cautious about the misuse of the method, and he accepted that it could not provide a statistical proof for a theory; but still he maintained that empirical data could disprove a theory, what Keynes could not accept either (Keynes, XIV: 307). And, of course, the cursory critique of Keynes of the massive and innovative effort by Tinbergen horrified and rapidly mobilized all the econometricians. Finally, the whole debate turned out to be dangerously close to a waste of arguments, since Keynes misjudged Tinbergen's work and since the early econometricians misunderstood Keynes's critique. His deep reason was indeed the same of twenty-eight years before, and was strengthened by his awareness of the deep complexity of the real economies, not encapsulable by methods designed to analyse fixed conditions and repeated samples.

Since in the social realm one cannot assume the 'principle of limited independent variety', Keynes's argument was that the method fails and cannot be extended to the unpredictable reality of social and economic life. Moreover, correlation proves little if anything about causality, since the *ceteris paribus* conditions — the analogue for the laboratory control in experiments in physics — may easily lead to the fallacy of the *post hoc, ergo propter hoc* argument.[26]

Stone was one of the extreme voices arguing that Keynes both ignored and opposed the progress represented by Tinbergen's book,[27] since he despised the advances in the mathematical formulation of economics. This author also added another explanation for Keynes's attitude: he 'suffered from an irresistible urge to overstate' (Stone, 1978: 12), which was shared by Harrod, 'he certainly had a tendency in general conversation to *épater le bourgeois*' (Harrod, 1951: 468). Even if this may be true, the debate proved that what was at stake was a decisive point dealing with the need for alternative conceptual formulations as the basis of the application of mathematics to the subject matter of economics (O'Donnell, 1997: 132), namely of change, uncertainty and complexity in real time processes.

Hendry and Morgan, who take sides with Tinbergen,[28] recognize that the crucial problems — the completeness of the set of causal factors, the inter-connection between variables, the homogeneity through time and the constancy of parameters — remain a 'greater threat' to the method, although arguing that they are not necessary conditions for the inquiry into 'structural autonomous relations' (Hendry, Morgan, 1995: 55). For this or for another reason, the econometric mainstream, which changed the daily methods of economic inquiry, ignored the crucial criticisms by Keynes.[29]

As a consequence of the whole debate, this crucial epistemological point came to be submerged by the skirmish of harsh critiques and massive counter-attacks. Nevertheless, it was quite clear by that time and, against Keynes's concept of organic unity and evolution, Tinbergen suggested that economic laws could only be intelligible as legitimate statements about structural stability, as measured by the constancy of the parameters. He even emphatically added that such constancy distinguished science from storytelling:

> Even if we assume curvilinearity in our relations and 'coefficients depending on other variables', etc., we come back, in the end, to coefficients that are constant. But that is essential for any theory that really deserves the name. (...) Describing phenomena without any sort of regularity or constancy behind them is no longer theory. An author who does not bind himself to some 'laws' is able to 'prove' anything at any moment he likes. But then he is telling stories, not making theories. (Tinbergen, 1940: 80)

Measurement against literature, exactness against divagation, lawfulness against ignorance and econometrics against metaphysics: wasn't it a very challenging appeal?

2.C. Econometric debates in the 'little League of Nations meeting'

Unfortunately, this crucial point about the constancy of parameters soon became the hidden implication of the discussion - in spite of its centrality to the divergence with Keynes. In fact, the postulate of the constancy of the structure of the equations and of its parameters was not generally accepted at the time. It was not even the opinion of some of Tinbergen's most courageous supporters, Marschak and Lange, who tried to extend the polemics in the pages of the *EJ*. But Keynes refused that,[30] and their paper remained unpublished until 1995 (Lange, Marschak, 1940, in Hendry, Morgan, 1995). It constitutes an important piece, both by the argument and by the authors, who were assisted by nobody less than Haavelmo, Yntema and mostly Mosak.

The authors were convinced of the far-reaching consequences both of the debate and of their own contribution: 'The difference between our article and Tinbergen's concerns not the subjects raised, but the way in which they are treated. Frankly, I think that our treatment is much superior and thorough, and that Tinbergen does not do full justice to his own case' (Lange to Marschak, 3 July 1940, UCLA). As a consequence, Lange and Marschak decided to defend as well as to clarify Tinbergen's program. As Tinbergen, they argued these methods provided the only adequate means to develop the Keynesian program:

> Since we are both in profound agreement with the economic theories of Mr. Keynes, we are anxious to prevent the readers of the *EJ* getting from Mr. Keynes's review the impression that his theories are not capable of empirical and statistical verification. (Lange, Marschak, 1940: 390).

From that point of departure, Lange and Marschak proceeded to rebut the main arguments by Keynes, explaining how could those objections be circumvented. The possibility of refuting theories by statistical tests was defended, and the category of relevant variables was introduced: if the researcher could provide a complete list of these variables, a non-trivial request, he or she was supposed to avoid the argument about the necessity of including all possible causal factors. In order to deal with qualitative variables, they suggested their ordering by rank, allowing for their inclusion in the model. Finally, the assumption of linearity was accepted as a mere 'first approximation'.

But the limited validity of the inference was nevertheless accepted due to the historical nature of the data: at least in that case some important instances of non-constancy of parameters were acknowledged – and that was indeed the crucial point for

Keynes. Here Lange and Marschak touched upon the decisive question: ‘The real difficulty is presented by the case when it appears plausible to expect that the parameters connecting the factors listed (including time) are subject to sudden large changes, either during the period observed, or in the future’ (Lange, Marschak, 1940: 392). Although this was still marginal observation and not part of their central argument, the recognition is decisive.[31]

In the preparation of the paper, Lange and Marschak largely discussed this topic through successive versions of the manuscript. Mosak was at least twice consulted about the matter, and the conclusion was that Keynes had touched upon a decisive question, ‘since the elimination of time from the correlation problem might be interpreted as working with an incomplete list of factors’, and therefore:

> That it is impossible to reconstruct the original equations from a statistical knowledge of their solutions, unless special hypothesis about the shapes of the curves, parameters, etc., are made. It is here that economic theory comes in as a necessary factor in the analysis. I think that on this ground we probably would have to yield to Keynes more than I was inclined to do in my original manuscript. (...) I think this point might be added to the manuscript, and in consequence the results would appear more conciliatory to Keynes than my first draft. (Lange to Marschak, 12 January 1940, UCLA)

Some weeks after this letter, Lange wrote again to his co-worker, on the basis of a second note by Mosak, and argued that Yule’s solution to the treatment of time was not satisfactory, since it implied it to be a purely ‘separate variable’ (5 February 1940, UCLA). Finally, Marschak conceded that ‘As the problem is, to my knowledge, not yet solved, I don’t think we can go any further’ (Marschak to Lange, 11 February 1940, UCLA). Since the problem could not be solved, it should be ignored, at least provisionally. As a consequence, the crucial problem of the nature of time and of change in the historical series was generally avoided in the paper submitted by Marschak and Lange. Indeed, the reference to the question was reduced to a Salomonic solution: provided that the correct functional relation is given, time could be included in the list of variables and the interdependence among observations could eventually be eliminated (Marschak, Lange, 1940: 393). At the same time, the authors recognised that only ‘limited inductive claims’ were possible from that operation, since constancy over time could not be asserted:

> We share Mr. Keynes’s views as to the limited inductive claims which can be made for the results of Professor Tinbergen’s statistical analysis, both on account of the lack of a proof of constancy over time of the statistical relationship investigated,[32] as well as because of the impossibility of evaluating the effects of factors which were not subject to significant changes during the period under discussion. (ibid.: 397)

Although Marschak and Keynes reduced Keynes’s logical and epistemological problem of the nature of time and the importance of historical change in economics to the limited question of measurement and inductive techniques, both their correspondence and their paper accept the primacy of the problem for statistical inquiries. This was also the case of Frisch, and for quite comparable reasons.

Frisch's contribution to the Cambridge meeting also remained unpublished for a long time, and it was not even discussed at the conference since the text arrived some days after its conclusion (Frisch, 17 July 1938 memorandum, OU; included in Hendry, Morgan, 1995). But it was afterwards largely circulated in the econometrician's circles. It was a more sceptical account of Tinbergen's conclusions than that of Lange and Marschak,[33] and furthermore it implied both technical and epistemological requirements that the available methods were unable to meet. Namely, he argued that Tinbergen estimated the parameters from the structural form and consequently ignored the identification and multicollinearity problems, therefore deriving too far-reaching conclusions and, crucially, that the true causal relations could not be demonstrated. In spite of the fact that some of these technical problems were rapidly addressed and solved, the main point was, for Frisch, that the procedure could merely obtain the quantification of coflux equations, and could not achieve the identification and estimation of the autonomous equations that represented the true structural causality in the cycle (Frisch, 1938: 416-7). Once given the economic data, even for homogenous processes through time the real equations could not be recovered. As a consequence, no explanation and no policy conclusions were obtainable from the estimation of the models. Of course, no refutation was possible as well (ibid.: 419) – a conclusion which remarkably rejoined Keynes's own point.

This conclusion was for Frisch in line with his previous shift away from indirect steering mechanisms of Keynesian type. And it had a radical implication: if the true causal relation cannot be estimated, the policy makers cannot base their projections on the traditional and defective tools, since they may just suggest fictions. These were arguments for favouring direct control and extensive planning. According to Frisch, this was also required in order to match the challenge imposed by the pressing social needs – those of the Great Depression and those derived from the 'monstrosity' of the war itself and from the necessary reconstruction of the devastated countries. In that sense, in his first paper to be published in *Econometrica* after being released from a German concentration camp and when returning to his duties as editor of the journal after the war, Frisch included an appeal to econometricians to turn their attention to the fulfilment of social priorities. This implied that the economists should engage in direct policy making, as he wrote later on:

> I have personally always been skeptical of the possibility of making macroeconomic predictions about the development that will follow on the basis of given initial conditions (...). I have believed that the analytical work will give higher yields - now and in the near future - if they become applied in macroeconomic decision models where the line of thought is the following: 'If this or that policy is made, and these conditions are met in the period under consideration, probably a tendency to go in this or that direction is created'. (Frisch, 1958, quoted in Andvig, 1995: 11)

Notice the implicit distance in relation to Tinbergen in the crucial point of the constancy of the parameters and the analytical value of the estimated equations – and the comparability to Keynes's decisive point on the non-homogeneity through time. Of course, Keynes addressed the problem in a rather different way, since he restrained himself to the short term and to the use of known behavioural relations, even if not completely quantified. On the contrary, Frisch tried to solve it with a defined quantitative approach on the basis of decision models. Keynes argued for indirect controls whereas Frisch supported direct controls; nevertheless, both accepted that institutional change alters the equilibrium structure of the economy, and that such a change should be guided in some way.

For some authors, this implication as well as all his future work meant that Frisch abandoned econometrics (Epstein, 1987: 127). In fact, it implies rather the contrary, that econometrics abandoned him, since his own view of the program was clearly defined from the early days in the sense of using the analytical tools to investigate and intervene in the social arena. Frisch wanted to develop the scientific tools to prevent new waves of misery and unemployment – and he never abandoned that concept. That would require planning, economic activism, and whatever means necessary for creating welfare. In other words, economics should always be a ‘moral science’, to put it as Keynes did.

The third piece of evidence on the econometricians’ discussions and elaborations is Divisia’s text, which reflects the discussion at the Cambridge meeting itself. Immediately following that conference, Divisia was asked by League of Nations’ officials to referee Frisch’s review of Tinbergen’s books. He did so in a still unpublished memorandum the 14 November 1938, strongly recommending the publication of the paper and of Tinbergen’s books. The text indicates that there was an intense dispute at the conference about the epistemological and technical implications of the new methods, and that Frisch’s points of view were shared by some of the participants, and indeed were considered as almost trivial:

> En ce qui concerne spécialement son application au travail de J. Tinbergen, il me semble que les observations faites, certainement très importantes, n’ajoutent pourtant pas grand chose à celles qui ont été présentées à ce sujet lors de la réunion de Cambridge. Tout le monde est je crois d’accord (et l’auteur tout le premier) sur l’utilité qu’il y a a formuler nettement des réserves au sujet des résultats à tirer des calculs des correlations. Le memorandum Frisch pousse à préciser et à accentuer d’avantage ces réserves. (Divisia, 14 November 1938 memorandum, OU)

In particular, the representation of cycles, the theme of Tinbergen’s research, was given as the example of the possible lack of meaning of the observed correlation:

> Quand au fond de la question, j’irai encore plus loin que Frisch sur le défaut possible, reconnu par tous, de signification théorique de certaines correlations constatées (...). Je crois, pour ma part, que l’absence possible de signification de correlations constatées est extrêmement générale, particulièrement dans le cas des oscillations. J’expliquerai ma pensée sur un exemple particulier:
>
> Si deux sinusoïdales A et B sont en corrélation parfaite, on trouvera systématiquement une corrélation non moins parfaite entre leurs dérivées d’un ordre quelconque affectées d’un décalage de temps convenable. Or il est évident que la signification d’un mécanisme sera tout différente selon qu’il établit une liaison entre des éléments ou entre leurs dérivées. Cela nous conduit à la vérité bien connue que l’observation statistique ne peut pas fournir à elle seule l’explication des phénomènes. (Autrement dit, l’objet de la statistique est

> beaucoup plus de suggérer que de démontrer) ce qui exige les réserves que tout le monde reconnaît nécessaires. (ibid.)

This implied an argument for a careful attitude in relation to statistical proofs and explanations and, in general, to inductive inference from data. Divisia's remarks witness on that regard a notorious awareness of the epistemic problems of statistics. Nevertheless, he still insisted on the importance of producing mechanical models in order to develop the theoretical insights:

> Pour en revenir au très important et très intéressant travail de Tinbergen, je suis d'autant plus porté à désirer sa publication que, du point de vue même des craintes exprimés par Frisch, il me parait donner des garanties; car il déborde déjà nettement le cadre d'une simple investigation statistique pour s'orienter vers les indispensables explications mécaniques. (...) J'ai d'ailleurs l'impression qu'avec le développement des investigations statistiques nouvelles dans le domaine étudié, la nécessité de telles explications mécaniques pour coordonner les nombreux éléments observés se fera de plus en plus sentir d'elle même; pourvu que les chercheurs soient des théoriciens et non des empiristes, ce qui est précisément le cas ici. (ibid.)

Allea jacta est, the dice were thrown.

3. Conclusions

In this debate in the econometric circles we have, in a nutshell, all the promises and the problems of the early development of the program. Mechanical explanations of reality, although their limits were acknowledged, were supposed to be decisive for the development of statistical information and theoretical understanding. This implies an astonishing balance sheet: although the anticipated critique of Keynes was easily discarded - and I argued that his very public loss of interest in the developments of statistics and mathematics eased that implication – the evidence here considered, and these texts were main pieces in the debate from recognised econometricians, proved they shared in some way some and even crucial points of that critique. This is the case of the central arguments on the non-homogeneity of the 'samples' through time, the non-atomistic character of the economic variables, the role of the institutional change and the inability of the method to detect the true causal relations. These points were accepted just with one eventual exception for Marschak and Lange, as frequent technical constraints to the computation according to Divisia and as permanent obstacles for Frisch. They did not accept, in spite of that, the non-mathematical alternative formulation Keynes was defending, since they deeply shared the conviction that exactness was desirable, possible, attainable, and even indispensable considering the task of economics.

In other words, the decisive difference was epistemological: the early econometric program was built on the solid foundations of the mechanical models whose development required the availability of a well-developed probabilistic theory. That was certainly the case for Tinbergen, for whom the core of the explanation should be the understanding and representation of a mechanism (Boumans, 1992: 74-5). It was the case of Divisia. That was also the case of Marschak and Lange, who ostracized the alternatives, those 'half theories, relying unadmittedly on outside influences, on *dei ex* *machina*' (Marschak, Lange, 1940: 392 n.). And that was surely the case of Frisch, who wrote a manifesto for the program with his seminal paper on the rocking horse, the instrumental impulse and propagation distinction which opened the way for the triumph of the mechanical cum probabilistic approach (Frisch, 1933). Therefore, the scene was set for the spreading of the probabilistic paradigm in economics, which was developed under the auspices of the *Econometric Society* and of the *Cowles Commission* research program, led since the late thirties by Marschak.

But something was lost in the way: the caution, the methodological reflections of the founders of econometrics, and the important elements of their discussion on causality, the constructive role of time and complexity in real societies. This loss is particularly highlighted by the comparison with of the econometricians with Keynes and their debates at the time. They had distinctive appreciations of the role of mathematics in the development of economic theories, and of statistics in their confirmation. They also differed on the epistemological role of the mechanic and organic analogies and yet some prominent econometricians wanted to use Keynes's theories and vision for policy-making.

But, simultaneously with Keynes's harsh critique although not because of it, one of the founders of the *Econometric Society*, not least than Ragnar Frisch, was taking his first steps away from the research program on the estimation of simultaneous equations, as it was defined in the 1930s. The deep reasons were the lasting technical difficulties and, moreover, that he had lost confidence in Keynesian indirect steering actions in order to solve the social problems. Consequently, he shifted from the estimation of structural macro-models and from what inspired the latter mainstream econometrics to decision plans and to direct economic programming. Frisch was followed by Tinbergen himself shortly afterwards and evidence shows that, although inspiring the use (and abuse) of mechanical models, most of these forerunners of econometrics – Frisch, Tinbergen, Roos, Marschak, Lange, Divisia – shared at some point crucial doubts about the implications of the methods that they were fathering.

But mainstream econometrics developed into the fifties in another completely different path, on the basis of the mechanical models and of treasuries of sophistication and expertise, towards the thrilling world of the axiomatic adventures - and naturally ignoring the puzzles of the first great debates. At that time, very few noted that the original pluralism in the emergence of econometrics was fading away and that orthodoxy was being established.

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