The Lucas Critique and Keynes' Response - Considering the History of Macroeconomics

by
Elke Muchlinski[1]

Abstract

Considering the history of macroeconomics it is surprising that Tinbergen's theory of policy is identified with so-called Keynesian economics by Lucas and Lucasians. Keynesian macropolicy is accused of neglecting the role of expectations and the effects of any changes of institutions. Due to textual evidence this paper explains that both the disregard of expectations and the institutional evolutionary process can not be addressed to Keynes's analysis.

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[1] Dr. Elke Muchlinski, Freie Universität Berlin, Department of Economics, Institute for Economic History and Economic Policy. Boltzmannstrasse 20, Germany – 14195 Berlin. Email: elke.muchlinski@wiwiss.fu-berlin.de
Page: http://www.wiwiss.fu-berlin.de/3/w3tomann/muchlinski

1. Introduction

This paper deals with the brief history of the Lucas critique. The Lucas critique was more successful in macroeconomic theory than in macroeconomic econometrics.[2] Sargent's laudatio on Lucas's work, particularly his paper on expectations and the neutrality of money (1972) made it quite plain that Lucas substituted the "flagship of that earlier revolution, Keynes's *General Theory of Employment, Interest, and Money*" (Sargent 1996, 536). In contrast to Keynes's, Samuelson's and Solow's work, Lucas's writings were short and concise, and every reader understands what Lucas was talking about in his work, since he made clear what he *really* wanted to say. Although Lucas's view was not originally created by him since he referred to Friedman, Muth and Irving Fisher, the function of expectations and its impact on the non-effectiveness of policy was evident.

Some authors refer to the Lucas critique in a way that show a certain importance of recognizing that economics is a social science which is not guided by certain laws or by the law of gravity. Any regime change has to be implemented on the basis of changes in expectations and responses by the economic agents, thus leading to institutional changes. Regarding the practical usage of econometrics Mishkin stated: "policy evaluation with Keynesian econometric models involves an assumption that the parameter estimates in the model's equations which have been estimated from past data are invariant to changes in policy. (...) Lucas' challenge to this procedure for evaluating policies is based on a simple principle of rational expectations theory" (Mishkin 1995, 2).

Lucas pointed out an interdependency of the structure of the economy and a model suggesting that agents recognize both. Hoover (1991) argued that the Lucas critique is not itself identical to rational expectations as is often found in the literature.[3] Rational expectation

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[2] See Blinder (1987). Rima (1988) presented a comprehensive work on this issue.
[3] Hoover (1991); see also Hoover (1998, 1997).

as modeled by Lucas can only treat risk, but not uncertainty. Lucas refers to Knight's distinction of risk and uncertainty without accepting it (Lucas 1981a, 125). The aim of rational expectations as defined by Lucas is to build a model that is not substantiated empirically (Lucas 1981a, 125).

Tobin (1993) did not celebrate the Lucas critique: "Jan Tinbergen is a modest man, and he never thought that econometric equations, his or others, would last foreever. I am sure he is now, and was fifty years ago, prepared to believe that clear changes of policy regimes, like other changes in the environment of economic activity, alter structural equations and their coefficients" (1993, 96). To sum up that point: Nowadays the Lucas critique is not used for a criticism of *Keynesian macroeconomics* as it was the case in 1976; it is used to express a *common knowledge*. I now want to turn towards some basic features of the Tinbergen model and the Lucas critique.

2. Lucas's view on Tinbergen

This section deals with some features of both Tinbergen's view on the theory of economic policy he outlined in the 1950s and Lucas's interpretation of it.[4] The Lucas critique focuses on Tinbergen's model, particularly the implication of expectations modeling. From a viewpoint of the history of macroeconomics it is interesting that the Lucas critique is (i) supposed to be identical with rational expectations and (ii) the object of Lucas's critique is supposed to be identical with Keynesian macroeconometrics and macroeconomics.[5]

2.1. A note on Tinbergen's theory of economic policy

Tinbergen's formal approach economic theory and policy and view on the theory of policy was widely accepted among economists and the community of science. His approach

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[4] Tinbergen (1952) See also: Knoester, A./Wellink, A.H.E.M., 1993. (Eds).
[5] See Lawson (1995) for an instructive approach.

to economic theory was normative. He distanced himself from both the Manchester school of 'laissez faire' which denied any effects of economic policy and the 'apriori-belief' of a socialist view which identified the state as a guarantor for social welfare.

Tinbergen emphasized the idea of steering the economy by certain instruments concerning particular economic variables. The object of the theory of economic policy is concerned with the findings of a social welfare function that he called the "collective ophelimity function". Individual preferences are to be seen as given. The deduction of policy targets from the indicator ophelimity function and the choice of appropriate instruments are the primary steps to be done. The determination of the quantitative values of the instrument variables and the formulation of a special proportion between targets and instruments are further steps to create a model of economic policy. These operative steps are dependent on each other: a choice of instruments is not independent of targets and indicators.

Tinbergen outlined many problems of this method, for instance, determining the structure of the economy. Evaluating the targets resulting from bargaining processes between distinct social groups, finding numerical values and identifying the nature of instrument variables is required. A model of the economy links instruments and targets and in principle makes it possible to steer a particular output by the process of maximizing the ophelimity function. With the determination of an optimum of this social welfare function it is also possible to calculate the social costs of a deviation from this optimum (loss function).

Besides the qualitative aspects Tinbergen has listed, he argued more attention should be drawn to quantitative aspects. In his formal theory of economic policy he distinguished between variables defined as policy instruments and other variables defined as objectives of policy makers. Policy instruments are to be interpreted as exogenous since their values are free to choose. Other policy instruments are exogenous as well because of some external conditions policy makers have to accept. Objectives are endogenous, their values are derived from exogenous variables. Tinbergen then differentiated between primary propositions and

secondary propositions. Primary propositions are guided by the inherent logical relation between the chosen variables which are determined by economic behavior or the logic of definition. Therefore a model can be separated into equations of supply, demand, definition, or technical equations which are linearly modeled. The secondary propositions are named side issues which should be understood as a corrector of linearity; they are non-equations. Tinbergen argued that they should mitigate the linearity.

It is hardly reasonable to interpret primary propositions as being determined both by economic behavior and premises, and therefore to confuse observations with premises as a logical starting point of theoretical reasoning. According to this distinction outlined by Tinbergen, the simple case is a compatibility of targets [t] and instruments [i]; but the occurrence of a non-compatibility, say [t < i] or [t > i], is more likely. Furthermore there must be *n-independent* instruments. For instance, two instruments and two targets do not guarantee the final output since fiscal and monetary policy are collinear instruments. The consequences are that the outcome of a particular employment and inflation or aggregate supply and demand is due to a special *policy mix* regardless of whether one accepts the Phillips curve *trade off* or not. Besides that, there are many kinds of monetary sub-instruments which could be of interest for the results as a whole.

2.2. Tinbergen versus Lucas

Lucas's widely known article *Econometric Policy Evaluation: A Critique*, was published in 1976. He discussed the meaning and failures of "the theory of economic policy" based on the traditional meaning, referring to Tinbergen and the long run unemployment-inflation trade-off of the Phillips curve which incorporates wage-price sectors. All of this needed to be revised. As Lucas (1981a) stated:

> "More particularly, I shall argue that the features which lead to success in short-term forecasting are unrelated to quantitative policy evaluation, that the major

> econometric models are (well) designed to perform the former task only, and that simulations using these models can, in principle, provide *no* useful information as to the actual consequences of alternative economic policies" (1981a, 105).

Lucas disclaimed being the first one to have drawn attention to what he will later refer to as the 'hypothesis of rational expectations'.[6]

Box 1 on this page demonstrates the Tinbergen or so-called traditional model. His approach to economic policy is based on an aggregate model; see equations (1) to (4). Lucas' critique on that is described with equations (5) to (6) on page 10. Now let me describe each equation of the model based on the presentation of Lucas.

Box 1: Traditional Model of Economic Policy

(1) $y_{t+1} = f(y_t, x_t, \varepsilon_t)$

(2) $f(y, x, \varepsilon) \equiv F(y, x, \theta, \varepsilon)$

(3) $\sum_{t=0}^{\infty} ß u(y_t, x_t, \varepsilon_t)$

(4) $y_{t+1} = F(y_t, x_t, \theta, \varepsilon_t)$

Equation (1) shows an economy in a particular period *t*; y_t is the vector of endogenous variables; x_t is the vector of exogenous variables and ε_t captures a vector of independent variables known as distributed random shocks; finally f is not directly known and assumed to be fixed. This revealed the problem of policy makers since *f* is a result of an estimation. More precisely this estimation is a crucial factor in every macroeconometric model. For one period *f* can be incorporated in x_t , which leads to equation (2). This equation shows how the view of an estimated *f* could be expressed by a vector θ. Lucas focused on the determination of this unknowable *f* since it relies on past values of x_t. Therefore x_t is "arbitrary" in a mathematical

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[6] Lucas was not the first to outline the invariance problem, as McCallum (1999). Lucas himself referred to Friedman, Muth and Knight (Lucas 1981, 106).

sense (Lucas 1981a, 106). Referring to (2) it is easy for policymakers to initiate policy advisories by a simple reapplication of past x_t data into prospective values, i.e. into F.

How does the process of evaluation work? Given the functions F and θ, a policy maker transforms equation (2) into equation (3). The implication of (3) is that the stochastic behavior of the given (y_t , x_t , ε_t) is specified, defined in this sequence as random variables. This makes it possible to find out its specification by numerical simulation. Equation (4) shows that F and θ are deduced from decision rules of agents which are to be interpreted as suitable for a special environment. Lucas explored that the main problem is an estimation of θ which is compiled from past values and experience. He stressed only that in so far as θ or the observed policy is invariant, the estimated value of θ will be invariant. "There is, as remarked above, no presumption that F, θ will be easy to discover, but it *is* the central assumption of the theory of economic policy that once they *are* (approximately) known, they will remain stable under arbitrary changes in the behavior of the forcing sequence" (Lucas 1981a, 110). He stated:

> "Everything we know about dynamic economic theory indicates that this presumption is unjustified. (...) To assume stability under alternative policy rules is thus to assume that agents' views about the behavior of shocks to the system are invariant under changes in the true behavior of these shocks. Without this extreme assumption, the kinds of policy simulations called for by the theory of economic policy are meaningless" (1981a, 111).

The implication of θ is a model of adaptive expectations which is an obstacle not only to forecasting models but also to economic theory, Lucas argued. The traditional model is regularly used to transform supposed stable parameters into prospective values. Lucas confessed that it is certainly not the case that any economist who works with such a model is neither convinced of a stability of θ in the short run nor in the long run. From the point of

view held by Lucas, the defect of equations (1) to (4) is obvious, since a microeconomic approach is lacking. However microeconomic elements are necessary to demonstrate the supposed impacts of economic policy.

Lucas intended to show the microstructure, which is to be seen as a representation of individual behavior responding to economic policy. Therefore θ moves with policy changes more rapidly than expected in traditional models. His model of the theory of economic policy is expressed in equations (5) and (6). This structure promised to avoid the mistake of arbitrary sequences [x_t] from future shocks. He modeled economic policies ("shocks") as stochastically disturbed functions of the system. Equation (5) shows its parametrical version; *G* is known, λ is a fixed parameter vector, and η_t is a vector of shocks. The economy is then determined by a special relationship between the behavioral parameter θ and parameter λ which is guided by shocks. Any regime change will change λ as well. Any regime change affects λ and the behavior of x_t, and therefore the behavioral parameters $\theta(\lambda)$. The latter concerns the economy as a whole.

Box 2: Lucas's model

(5) $x_t = G(y_y, \lambda, \eta_t)$

(6) $y_{t+1} = F(y_t, x_t, \theta(\lambda), \varepsilon_t)$

Lucas explained:

> "Evidently, the way this latter [($\theta(\lambda)$), EM.] modification can be expected to occur depends crucially on the way the policy change is carried out. (...) If, on the other hand, policy changes occur as fully discussed and understood changes in *rules*, there is some hope that the resulting structural changes can be forecast on the basis of estimation from past data of $\theta(\lambda)$. (...) (According to Muth and Knight, EM) it asserts that agents' response become predictable to outside observers only when there can be some confidence that agents and observers share

> a common view of the nature of the shocks which must be forecast by both" (1981a, 125).

Lucas's view leads to the proposition of a shared *common view or common knowledge* and would make a prophecy possible. As a result, Lucas did not accept it as empirical evidence (see Lucas 1981a, 25-6).

To sum up the Lucas critique, then, the estimation of θ is useful only for the regime for which is was established, but it is useless for alternative regimes and forecasting models. A specification of a workable model requires a modeling of the regime changes in case there will be any. In so far as macroeconomics and macroeconometrics use variables to demonstrate how the economy responds to new targets, it is required to have more justification for this than reference to past driven parameters. This is due to a different view on how private agents respond to a change in economic policy. They are not guided by past events and experience. A microfoundation as proposed by Lucas would abandon the aggregate structure, therefore leading the strong but not convincing assumption of a traditional model behind it. Since private agents anticipate monetary disturbances, any monetary policy is ineffective. Any real effects in the economy occur only through price level surprises, or in other words, differences between anticipated and non-anticipated monetary shocks.

3. Lucasianism

The advent of Lucasianism, known as New Classical Macroeconomics (NCM) in macroeconomic theory, was recognized in the early 1970's.[7] NCM defined itself as a new paradigm opposed to the so-called Keynesian tradition. At this point it is worth to mentioning that this so-called Keynesian tradition was identified with the Tinbergen model as sketched above. NCM and Monetarism particularly objects to the view of an inherent instability in the

[7] An introduction gave Blinder (1997).

economy which needs to be governed by macropolicy. The argument strongly suggests that all instabilities and fluctuations in the economy are due to the erratic behavior of the authorities. The emergence of Lucasianism at this time can be explained by fundamental theoretical upheavals among economists and economic theory. First of all the phenomenon of *stagflation* led to controversies and terminological changes of traditional Keynesian notions.

Considering the history of macroeconomics, the *consequence* of the Lucas critique is more important than the proposition that agents will respond to a change in policy regimes or that parameters do not remain constant to changes in the policy regime. Keynes and Tinbergen would not have denied this proposition. The significant result of the Lucas critique and the 'rational expectations hypothesis' is that all changes in monetary policies, i.e., targets will be anticipated by private agents and therefore do not have any real effects in the economy. More precisely, all nominal magnitudes are useless for the economy since agents adapt their behavior to regime changes. The implication of this view is the hypothesis of a neutrality of money.

3.1. Some Principles of Lucasianism

The Lucasian paradigm rejected the view of adaptive expectations. The main pillars of Lucasianism are: (1) representative agent models and the 'rational expectations hypothesis'. (2) The rejection of Keynesian macroeconomics and macroeconometrics, and (3) the neutrality of money. These main premises then created a vertical Phillips curve which was interpreted as the monument of supra neutrality on macropolicy.

The first pillar was a challenge to both Monetarism and Keynesian since Lucasianism promised to provide a microfoundation for macroeconomics.[8] In this sense it is to be seen as a

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[8] Sargent emphatically described the challenge:"Despite the appearance of its early incarnations like Lucas's 72 JET paper, the canons of rational expectations models - individual maximazation within a consistently understood environment - were evidently wide enough to include Lucas's elegant brand of monetarism" (1996, 545).

continuity of the neoclassical economy. It relies on methodological individualism which entered economic debates through the idea of marginalism represented by Walras, Gossen and Jevons, the founders of economics as a pure science. The microfoundation for macroeconomics was first of all a critique on the aggregate demand models since they could not bridge the gap between individual actions and preferences and the general output.

Methods of microfoundation are grounded on widely accepted instruments in economic theory: game theory, the Walrasian model of perfectly competitive general equilibrium and the model of representative agents. All of these instruments are applied in the field of economic investigation, but they have not replaced lamentable aggregate models. Given the flaws of aggregate models, microfoundation itself caused methodological problems of tautology, or of interference. Kirman, for instance, explored that a microfoundation was actually a claim for an integration of macroeconomics in Walrasian models. Kirman concluded:

> "the problem seems to be embodied in what is an essential feature of a centuries-long tradition in economics, that of treating individuals as acting independently of each other. (...). To argue in this way, however, suggests that once the appropriate signals are given, individuals behave in isolation and the result of their behaviour may simply be added together" (1989, 137).

The microfoundation for macroeconomy should be implemented by a representative agent model. A disaggregation of these models should have provided a method of analyzing individual activities on the basis of a suggested average behavior regarding macropolicy and to eliminate any reasons for a Lucas critique. Macropolicy was not assumed to be invariant for agents, on the contrary, macropolicy was due to an agent's response.

However, the definition and specification of the utility function of the model led to further methodological questions on how economists could seriously know it. Regardless of the size of the group which the representative agent should represent, the representative agent

therefore became an essential part of the aggregate environment which governed his action. This could be the end of the story about NCM and Lucasianism repectively, if there were no further interesting details to tell. Representative agent models eliminate the aspect of coordination which is important in a decentralized economy.

Because dealing with all individually distinct utility functions is not that simple, a representative agent model is reduced to a single function for the average (supposed) response. This reduction should depict how the representative agent will change his decision in light of a new policy. More precisely the picture which arose out of this method was not different from the aggregative model. "The representative agent turned out to be a macroeconomic model in disguise. It did not solve the aggregation problem; it merely hid it. It did not solve the problem of keeping exogenous and endogenous variables straight; it obfuscated it. And ultimately, the final indignity came when the representative agent turned out not to be a microeconomic model at all. (...) The Lucas critique was obviously true but unfortunately unsolvable" (Hartley 1997, 199).

The intention of the representative agent model was highly ambitious. Its methodological starting point failed since given aggregates therefore configure the scope for a decision and action of the agent. The consequence is that the representative agent model which was forced to avoid the untenable and "unrealistic" performance of aggregative economy mutated to a justification for aggregative models. Therefore the representative agent model is not an implementation of the microfoundation for macroeconomy because it represents a certain feature of the microeconomy.

Besides the representative agent as outlined above, the 'hypothesis of rational expectations' (REH) was celebrated as a theoretical refinement applied now to economics by Lucas and Lucasians. All of the academic debates seem to produce further theoretical defense strategy attempts to protect this REH. The rationality of the Lucas critique and Lucasians

occurs only in stable environments in which rational agents, endowed with rational expectations, anticipate the regime choice.[9] Hoover, then resumed:

> "The root of the problem is this: economic theory uses variables to describe economic processes which are not observable; observable variables are, in part, the outcome of interactions among these unobservables, and without further information it is, in general, not possible to infer the behavior of the unobservables from the observables" (1991, 190).

This divergence of observable and unobservable entities is solved, so it is said, by the NCM by an incorporation of the REH.[10] The identification of economic policy with a divergence of observable and unobservable, as maintained by Hoover, is perhaps one way to deal with uncertainty, without being convincing; the hypothesis of rational expectations is not observable at all.

No doubt, the REH is misleading for at least two main reasons. First: The hypothesis of rational expectations implies a convergence of the "true model" which is perceivable and acknowledged by the economic agents and their expectations. This premise is hardly acceptable. Second: Permanent or systematic mistakes by the agents are excluded. Furthermore: REH advocates Knight's definition of risk which has to be distinguished from uncertainty. The latter is not insurable, as Keynes had stated.

The premise of a stable environment excludes explanations on how representative agents learn. The agents in the model of REH are forming expectations on the basis of a 'true model' of the economy which is identical with the model of the NCM itself. Therefore REH is

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[9] Blinder outlined:"I think the weight of the evidence – both from directly observed expectations and from indirect statistical tests of rationality (usually in conjunction with some other hypothesis)I – is overwhelming against the RE hypothesis" (1987, 131).

[10] For a further investigation on questions of the accused incompatibility of models with facts or events, see Muchlinski (1998 2003, 2003a).

addressed only to a special *model of rational expectations building*. It is important to distinguish rational expectations in the manner of rational human behavior and perceive of information from the model of REH. The second pillar can be briefly described by the intention of Lucasians to reject both Keynesian macroeconometric models and the idea of a sound macropolicy. The aim of the NCM was a replacement of Keynesian macroeconomics and macroeconometrics with models avoiding the Lucas critique based on an adherence to REH. This should have led to refinements of equilibrium models and finally to the *real business cycle models*. The latter is not part of my concern here.[11]

According to some authors the Lucas critique led to theoretical improvements, for instance to models of time consistency.[12] The success of macropolicy depends on precommitments to a particular policy regime. "Achieving credibility has thus become an important goal for policymakers" (Mishkin 1995, 22). The theorem of *rules versus discretion* is a further refinement of that idea. It tries to explain the reputation and the desirability of pre-commitment by central banks.[13]

Other authors objected to central assumptions of Lucasianism (Blinder 1999, Goodhart 1994).[14] The assumption of surprise inflation and the willingness of the central bank to fool private agents by abandoning the announced target of monetary policy is hardly acceptable. Moreover, as Cukierman investigated, "the credibility problem of monetary policy is a thing of the past".[15] From the viewpoint of the Bank of England, Vickers outlined: "It should go without saying that the MPC's objectives are given by the Act and by the remit set by the Chancellor. There is a large literature on inflation bias, but it is simply not applicable to the

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[11] See Cooley (1994)
[12] Kydland/Prescott (1977)
[13] As Blinder (1998, 37) comments some implications of basic premises of the NCM:"The harsh but simple fact is that no central bank directly controls inflation, unemployment, or nominal GDP - as an economic theorist would like to pretend otherwise".
[14] Blinder (1999) and Goodhart (1994); Boland also offered an interesting approach (1998).
[15] Cukierman (2002).

MPC. We have no desire to spring inflation surprises to try to bump output above its natural rate (wherever that may be). Quite apart from the obligation to fulfill our statutory duty, we have the strongest professional and reputation incentives, which in my opinion are incapable of being enhanced by financial incentives, to get as close as we can to the inflation target" (Vickers 1998, 6).

Therefore the theoretical assumptions of the KPBG model do not make any sense. There seems to be no dispute about that issue between central banks itself: "If the monetary authority can be clearer about what it is doing now and plans to do – not in the sense of setting future moves in stone, but rather in terms of explaining risks that might influence future policy – then market participants can improve their expectations of future short rates, and possibly reduce the premium for uncertainty" (Ferguson 1999, 2). Disputes in theoretical debates have not been reaching the realm of central bank practice, which focuses on effectiveness of monetary policy as practice and tries to avoid assumptions that are not linked to the contemporary world.

An admitted fact is the asymmetry of time horizons of the different agents in different markets and the central bank. The goal of price stability is a long lasting objective a central bank can only try to achieve by acting in short time horizons, which may conflict with the interests of market participants. Cukierman makes his objection: "The quadratic objective function originally postulated by KPBG carries the rather unintuitive implication that, given inflation, an upward deviation of employment from its desired level is as costly as a downward deviation of the same size. It is hard to see, why policymakers, or social planners for that matter, would object, given inflation, to a positive output gap. As a matter of fact it's quite likely that, in the range of positive output gaps, the quadratic was postulated mainly for analytical convenience rather than for its descriptive realism" (Cukierman 2002, 16).

This model of interaction demonstrates first the supposed homogeneity of private agents and an invariant divergence between both agents and the central bank. Blinder (1998) stated that the alleged temptation of central bankers for an inflation surprise in models of the time consistency literature asserts the capability of a central bank to solve inflation through a rule and that there is an "inflation bias" problem. This postulates that an abandonment from an announced monetary target focuses on the behavior of the central bankers neglecting historical facts and events. Once it happens, private agents will not tolerate it anymore, therefore the central bank loses its credibility. The premise of fooling does not have any link to the *contemporary world* (Keynes) that is the perceived world and therefore the premise should be interpreted as a *dry bone*.[16]

As Blinder explaines "recent history has not been kind to the view that central banks have an inflationary bias" (1998, 40). The reference to a historical argument did not express the attempt of a falsification of a hypothesis since Blinder has already mentioned that models should have a link to reality, otherwise it is not clear what the discussion is about. Concerning solutions for uncertainty in forecasting model selection and parameters, Blinder explained "my intuition tells me that this finding is more general - or at least more wise - in the real world than the mathematics will support" (1998, 12). Although the REH has been initiated to a specification by new classical models, it has not replaced Keynesian macroeconometrics and macroeconomics.[17]

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[16] Keynes (1938, 33).

[17] Blinder (1998, 6) described, that the Tinbergen-Theil framework could be seen as one of many models capturing important aspects on central bank policy. In this model, some variables are endogenous and others are exogenous. The Tinbergen-Theil model does not work on the assumption the central bank could control either the inflation rate or the unemployment rate. Against those criticism who deny any relevance of models since there is no knowledge on the "true" or "right" model at all, Blinder outlines, that "speaking now as a former central banker, I think such know-nothingism is not a very useful attitude. In fact, in my view, we must use the Tinbergen-Theil approach - with as many of the complication as we can handle - even if in a quite informal way".

The problems identified with Keynesian macroeconomics and macroeconometrics interpreted by advocates of Lucasianism is rather due to *their interpretation* of Keynes's view than to Keynes himself (see my next paragraph). This method of *stylized* opinion regarding Keynesian policy was the starting point to demand a shift in the paradigms to Lucasianism. Remarkably, Keynesian macromodels are not out of fashion today. "As the result, the Lucas critique of policy evaluation using Keynesian macroeconometric models does affect the way policymakers make use of these models" (Mishkin 1995, 22). Policymakers have been drawing more attention to expectations.

The third pillar of Lucasianism caused an important controversy within macroeconomics. If expectations are rational, monetary policy cannot produce systematic gaps between the actual and announced inflation. The incorporation of REH implies a neutrality of money. The hypothesis of the neutrality of money was advocated originally by Friedman in 1968.[18] Lucas was devoted to this proposition with more radicalism and rigor. According to the discussion about the Phillips curve, Lucas and Lucasianism objected to the *long termed* trade off between unemployment and inflation as stated by A.W. Phillips and Samuelson/Solow. This opened up never ending controversies within the economy of science as is widely known, so I am sure I can be brief on that point.[19]

The hypotheses of rational expectations and neutrality of money are significant features of the new paradigm by Lucas & Lucasianism.[20] Hahn objected to the *this paradigm* as follows:

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[18] The *presidential address* to the American Economic Association (AEA) by Friedman implemented the concept of NAIRU, the nonaccelerating inflation rate of unemployment. which implies that inflation will increase if unemployment remain below its natural rate.

[19] Further details on the history and implications of the Phillips curve are provided by Staiger/Stock/Watson (1997).

[20] Sargent (1996, 543) objected to the interpretation that there is triade of the hypotheses of 'rational expectations', 'neutrality' and 'policy ineffectiveness' in Lucas's work, since present papers on equilibrium models are concerned with different monetary and fiscal policy arrangements.

> "Money, so the jargon goes is not only neutral, but 'superneutral'. The inflation rate, at least in the long run, is thus irrelevant to the real state of the economy and is simply governed by the rate of increase in the monetary stock" (1982, 71-2).

This new paradigm became important for a few years shortly after Lucas and Sargent's publication of *After Keynesian Macroeconomics* (1979). Hahn stated that if the economy cannot go beyond the rational expectations equilibrium as maintained by Lucas and the NCM, why should inflation be a problem at all? An answer has not yet been given. Hahn concludes:

> "the present neurotic preoccupation with inflation cannot come from agents who live in a Sagent-Wallace world. Recall again that no one is supposed to be permanently fooled. Remember also that there are no distribution effects from inflation in a rational expectations world" (1982, 102).

Hahn resisted following the new paradigm because:

> "Lucas's contribution then is this. He showed how unpredictability or unobservability of monetary policy could interfere with the information-revealing function of prices, and how this could be the source not only of real effects, but of real effects that the macro-literature had noted. (...) Rational expectations equilibria are unique and the economy is always in rational expectations equilibrium. Appearance belie reality" (1982, 45-6).

To sum up this paragraph: The claim for a microfoundation of macroeconomy is not undisputed in the literature. The question remains unanswered of whether microfoundation is necessary to explore economic behavior in a decentralized economy and to understand the process of acquiring knowledge. Therefore the methodological disputes on microfoundation versus macrofoundation still concern contemporary economists (Leijonhufvud 1996).

4. Keynes's critique on Tinbergen

Keynes's critique on econometrics is viewed in the literature as "Keynes's critique on Tinbergen". Its epistemological relevance is emphasized by Brown-Collier/Bausor (1988) and Muchlinski (1996). Its methodological meaning, for instance, has been outlined by Hillard (1992) and Carabelli (1988). Some authors made objections against Keynes's critique arguing that he had not really understood what Tinbergen was talking about (Klant 1985, Phelps 1979). Appropriately, it is said that he did not accept the significance of mathematics and methods of measurement for economic phenomena.

Rima for instance wrote "econometric research has become, for many, the sine qua non of economic science. This development is clearly inconsistent with Keynes's reservations about the usefulness of statistical probability to explain economic outcome that come into being under uncertainty. The vision of the real world to which Keynes first gave expression in *A Treatise on Probability* and which provided the epistemic foundation for the *General Theory of Employment, Interest and Money* is a perspective which is *conceptually* incompatible with the mid-century transformation of economics that has accompanied the formalist revolution" (1988, 19-20).

This interpretation is hardly acceptable since Keynes wrote a treatise on problems of indexation, its methods and application of economic questions in 1908 (C.W., XI, 49-159) and a remarkable treatise concerned with "statistics" (C.W., XI, 174-237).[21] Besides that his *Treatise on Probability*, a book which was published in 1921 but had already been written in its main parts in 1908, is concerned with questions of the theory of knowledge, problems of measurements, statistical methods and explanations, methods of reasoning, and induction. These are important issues for economics as a social science; further contributions are to be

[21] For a critical assessment on econometrics today, see Summers (1991).

found in *The General Theory of Employment, Interest and Money* (1936), and the *Collected Writings* in different volumes. I will comment on this in the next paragraph.

4.1. Divergences

Keynes objected against centrally implicit premises of Tinbergen's work in 1938 and 1940. Particularly, the assumptions of (i) linearity, (ii) homogeneity, (iii) independency, (iv) the choice of variables, (v) the ignored time lags and (vi) reversibility. Keynes was asked by a Mr. Tyler to write a comment on Tinbergen's book. In the very first commentary addressed to Mr. Tylor, Keynes criticized the lack of explanations on the applied methods. Tinbergen has presented a "cryptic method of exposition" (C.W., XIV, 285).[22]

Keynes stated: "There is first of all the central question of methodology, the logic of applying the method of multiple correlation to unanalysed economic material, which we know to be nonhomogeneous through time. If we are dealing with the action of numerically measurable, independent forces, adequately unanalysed so that we knew we were dealing with independent atomic factors and between them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action; (...) In fact we know that every one of these conditions is far from being satisfied by the economic material under investigation. How far does this impair the validity of the method? That seems to me to deserve a most careful preliminary enquiry. The volume which purports to be 'a note on the method' in fact faces none of these difficulties and is in fact mainly occupied, just like the other volume, with elaborate half-explained numerical examples, the method employed in which already begs the question" (C.W., XIV, 285-6). The

[22]The reply by Tinbergen is outlined as a defense of his applied methods (C.W. XIV, 293) in: *The Economic Journal* 1940, No. 50, 141-154.

application of multiple correlation is questionable since the material to which it is applied is not suitable.

The predominance of deductive reasoning is a further inadequacy for economics as a social science. Keynes emphasized the relevance of inductive reasoning in economics. He stated: "It seems to me that economics is a branch of logic, a way of thinking. (...) Progress in economics consists almost entirely in a progressive improvement in the choice of models. (...) Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world" (C.W., XIV, 295-6, Letter to R.F. Harrod, 4. July 1938). Every single choice of a model is due to inductive elements. Whereas model building is the appropriate theoretical approach, deduction as a way of reasoning should be judged with caution. Keynes's methodological reflections are not restricted to the criticism of econometrics. According to contemporary debates on methodological issues in economics, Keynes's contributions are important. Therefore his confession to Mr. Tylor that he has "the utmost difficulty in making heads or tails of" Tinbergen's work, which should be "partly due to my lack of familiarity with the matter" is to be seen as an understatement (C.W., XIV, 285). [23]

Keynes's reply to Tinbergen's book illustrated his theoretical approach to economics. His criticism of econometrics and its implication should be taken as warnings, as claims for reflections and awareness of these problems. Tinbergen’s writings on investment, decision-making and business cycles had amassed descriptions of statistics and statistical methods to demonstrate the validity of central premises such as linearity, homogeneity and interdependency of the chosen variables with each other. This procedure was not acceptable by Keynes. He presented a theory unaccompanied by empirical data that explained for instance, involuntary unemployment through inadequate aggregate demand as pessimistic

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[23] More details on problems of model building is given by Mayer (1996).

expectations about uncertainty. Furthermore, he replaced the non-acceptable premises of classical theory with those premises, which are *relevant to contemporary world*. Uncertainty and precariousness about, for instance, "prospective yields" of an investment (Keynes 1936, 135), and therefore the role of expectations leads to a distinct theoretical approach to economic questions.

Keynes criticized Tinbergen's methods because of misguiding modelling:"The pseudo-analogy with the physical sciences leads directly counter to the habit of mind which is most importance for an economist proper to acquire. (...) One has to be constantly on guard against treating the material as constant and homogeneous. It is as though the fall of the apple to the ground depended on the apple's motives, on whether it is worth while falling to the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth" (1938, C.W., XIV, 299-300).

Keynes did not supply the view that economic variables are to be seen as constant and homogenous through time. He referred to the crucial point in Tinbergen's work of taking past data and statistics to infer future values from them. A precise question addressed to Tinbergen was whether he had drawn any attention to expectations. In the following quotation one can perceive Keynes's sceptical view. The application of statistical methods and measurements is due to theoretical assumptions which Tinbergen did not make explicitly. This is an impediment to achieving validity and credibility in his work. Keynes asked: "How are these coefficients arrived at? Is it by laborious trail-and-error guessing, or by method? How are the time lags arrived at? Is it by common sense guessing or by method? (...) Is it assumed that the factors investigated are comprehensive and that they are not merely a partial selection out of all the factors at work? How much difference does it make to the method if they are not comprehensive? Is it claimed that there is a likelihood that the equations will work approximately *next time*? (...) Is it assumed that the future is a determinate function of *past statistics*? What place is left for expectation and the state of confidence relating to the future?

What place is allowed for non-numerical factors, such as inventions, politics, labour troubles, wars, earthquakes, financial crises? One feels a suspicion that the choice of factors is influenced (...) by what statistics are available, and that many vital factors are ignored because they are statistically intractable or unprocurable. (...). Now I quite agree that it would not be easy to apply the method to these factors. But that seems to me a justification for not using the method in this case rather than for ignoring these matters and telling us what we know alredy with the trimmings of figures which really have no significance" (1938, C.W., XIV, 286-288).

All of these implicit premises need to be explained and made explicit: the assumptions of linearity, reversibility, homogeneity as well as the supposed independencies of the variables. "One would have liked to be told emphatically what is involved in the assumption of linearity. It means that the quantitative effect of any causal factor on the phenomenon under investigation is directly proportional to the factor's own magnitude" (Keynes C.W., XIV, 312). "Is there any ground for the suspicion that the assumption of linearity rules out cyclical factors?" (C.W., XIV, 313). This question was the subject of an interesting debate between economists, e.g. the question on how investments are determined and how profits are to be seen as the crucial element of an investment decision or not. As I have already mentioned, Keynes emphasized the "prospective yields" as significant for the investment decision and not, as argued in the classical theory, the given amount of profit. Tinbergen made no distinction between both.

Furthermore, Keynes cautioned against the hypothesis of reversibility. "Where and how does the element of *reversal* come in? I ask this question without pretending to answer it. But I should like to know the answer" (C.W., XIV, 313). Besides that no explanation is given in Tinbergen's work of how the *trimmings of figures* move during an investigation of such factors determining these *trimmings of figures*. Finally the treatment of time lags deserves more clarity for the reader. Keynes remarked that "no example is given of the process of determining time lags which appear, when they come, ready-made. (...) This he seems to do

by some sort of trial-and-error method. That is to say, he fidgets about until he finds a time lag which does not fit in too badly with the theory he is testing and with the general presuppositions of this methods" (C.W., XIV, 314). Regarding the definition of *trends* open questions remain. The underlying time periods seem to be arbitrary since Tinbergen selected different time periods for the United States and United Kingdom to demonstrate how he dealt with the same question. No justification is given to this divergence of time periods.

In summary, we then come to the next question Keynes addressed to Tinbergen: "How far are these curves and equations meant to be no more than a piece of historical curve-fitting and description, and how far do they make inductive claims with reference to the future as well as the past?" (C.W., XIV, 315). Keynes presented his investigations of inductive arguments and reasoning in the *Treatise on Probability*. The validity of an inductive argument definitely depends on the length of the underlying period or sub-periods since the regression coefficient for each period will change with the choice of the period itself.

Finally Keynes objected to the hypothesis of independence. "Must we push our preliminary analysis to the point at which we are confident that the different factors are substantially independent of one another? This is not discussed. Yet I think it is important. For, if we are using factors which are not wholly independent, we lay ourselves open to the extraordinarily difficult and deceptive complications of 'spurious' correlation" (C.W., XIV, 309). "I infer that he considers independence of no importance. But my mind goes back to the days when Mr. Yule sprang a mine under the contraptions of optimistic statisticians by his discovery of spurious correlation. (...) It becomes like those puzzles for children where you write down your age, multiply, add this and that, substract something else, and eventually end up with the number of the Beast in Revelation. (...) Thus it is sometimes a rate and sometimes an absolute quantity; and when in the final outcome he multiply this hotch-potch, sometimes by a large coefficient and sometimes by a small one, and then substracts from it the rate of interest multiplied (usually) by a small coefficient" (C.W., XIV, 310-311). The analogy of

Tinbergen's method with hotch-potch is an illustration of the missing theory Keynes claimed and leads to a lack of clarity in the applied methods.

4.2. Some generalizations

Having introduced central objections Keynes made against Tinbergen's work, it is quite obvious that expectation is a key notion in Keynes's economic thinking. He came to this view in his fundamental critique on the classical view of Benthamite calculation.[24] In contrast to that, he outlined his metatheoretical or methodological view rejecting natural sciences as an inappropriate approach to economics. Moral science in modern terms could be expressed as a social science.

Surprisingly none of the authors who devotionally follow Lucas and Lucasiansm refer to Keynes's criticism. This could have avoided deep misunderstanding of what Keynes said. The *common sens*e in economics nowadays is to accuse Keynesian macroeconometrics and macroeconomics of having neglected the importance of expectations. The widespread accepted identification of Tinbergen's model of macropolicy with Keynes's view caused a misunderstanding of the core of his criticism of econometrics. His proviso with the presented methods was in no way an expression of model nihilism, since he has already explained significant aspects of model building in economics. One has to be aware that economic material is non-linear, non-reversible, non-homogeneous, not independent of one another and which has to be judged on the basis of time lags. It is necessary to choose those variables which are not only suitable but also important to the purpose in question.

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[24] Classical theory seeks to reduce uncertainty to the same epistemological status as certainty by using mathematical calculus (Keynes, C.W., XIV 213).

Many of Keynes's objections against econometric work can be reread in the current literature on the topic of sound macropolicy and how it works.[25] Blinder for instance considers the monetary policy of the Federal Reserve System (FRS) in the USA:"*Some* kind of a model, however informal, is necessary to do policy, for otherwise how can you even begin to estimate the effects of changes in policy instruments." (...) "Central bank do, too. Or at least they should, for they will surely fail in their stabilization-policy mission if they simply 'put out fires' as they observe them" (Blinder 1998, 7). All models must have a link to the "contemporary world", as Keynes argued. This view by Keynes is known as the Lucas critique which "warn us that some parameters may change when policy does. Yet what are we to do about these problems? Be skeptical? Of course. Use several methods and models instead of just one? Certainly. But abandon all econometric modeling? I think not" (Blinder 1998, 8). This is the contribution Keynes outlined emphasizing that the economy is a way of thinking in terms of models.

Blinder (1998) elaborates in some arguments having contextual importance. Macro-policy relies on models, both macroeconomics and macroeconometrics are concerned with special problems of model building. These are in short:

(1) The unknown "true model". In fact any choice of a model implies uncertainty if the emphasized proposition is a "true" one. There is no way out of uncertainty. Blinder refers to the distinction of uncertainty and risk in Knight's terminology (1921) explaining why uncertainty rather than risk is a problem in model building. "Risk arises when a random variable has a known probability distribution; uncertainty arises when the distribution is unknown" (1998, 77). Since the distribution of uncertainty is persistently and categorically unknown (because there is no such distribution), there is no chance of eliminating uncertainty through model building.

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[25] See for instance Morishima (1991).

(2) Uncertainty in the forecast. This problem can be methodically solved by replacing the unknown future variables with "their expected values" defined as the "certainty equivalence" principle. This operation looks easier than it is because of the great amount of unknown exogenous variables which have to be integrated in the model. Furthermore this method is problematic because of the non-linearity of the economy and serious doubt that the objective function is quadratic. But who knows the objective function? Since no one have ever found a justification of "an objective" function, nor given it, practical economists or central bankers must create such a function. Again, the purpose of model building is due to problems of the *contemporary world* therefore "policymakers almost always will be contemplating changes in policy instruments that can be expected to lead to small changes in macroeconomic variables" (Blinder 1998, 10).

(3) Uncertainty about parameters. Blinder refers to Brainard (1967) to describe that uncertainty about parameters should lead to a more or less *conservative* behavior of the central bankers, i.e. assuming the lowest movement of parameters.

(4) Uncertainty about model selection. This problem is connected with the first three aspects and there is no chance to avoid a choice of models among many others. Relying on a universal model would be the worst case, rather than using a wide variety of different models with a reasonable critique.

(5) Finally, Blinder emphasized the "long and variable lags" macroeconometrics and macroeconomics have to recognize. "It is essential, in my view, for central bankers to realize that, in a dynamic economy with long lags in monetary policy, today's monetary policy decision must be thought of as the first step along a path. The reason is simple: Unless you have thought through your expected future actions, it is impossible to make today's decision rationally "(1998, 14). To summarize these considerations one has to be on guard about relying on simple rules concerning economic questions. The brief reconsideration of both

Keynes and Blinder has shown that the criticism of model building in macroeconomics and macroeconometrics is relevant.

Leeson states that Tinbergen himself vindicated parts of the Keynes's critique a long time after their dispute. Tinbergen, interviewed by Magnus/Morgan (1987), admitted his scepticism. Magnus/Morgan asked: "How do you feel about the way econometrics has developed over the last twenty years or so? In 1952 you feared that techniques could take over from attention to human needs and problems in the field of economics. Do you feel this fear was justified?" Tinbergen replied: "I'm afraid, yes" (1998, 55). Leeson furthermore demonstrates that Friedman, who was acquainted with the use and abuse of statistics and econometrics, "took over much of the Keynes critique and made it his own. Yet the evaluation of econometric evidence became the 'space-time' *arena* of the disputes between Keynesians and monetarists" (1998, 67). Friedman agreed with Keynes in many of his objections, as Leeson enlightens. Take for instance the model selection problem, the methods of regression and correlation or the problem of applying mathematics to the economy, all of this can be reread in Friedman's work (see Leeson 1998, 73-77). "Just as Keynes did, Friedman cautioned against economic theory becoming a species of 'disguised mathematics... a retreat into purely formal or tautological analysis" (Leeson 1998, 76). Like Friedman, Keynes did not identify science or scientific results with mathematical symbols. "Those writers who try to be *strictly* formal generally have no substance" (Keynes, C.W., XXIX, 37-8). Keynes's view on mathematics was not due to a dualism of formal versus real since "reality" is defined as "contemporary world", i.e. the perceived or experienced world, and a model is just a way of thinking about it.

5. Concluding remarks

Considering the history of macroeconomics, the emergence of the Lucas critique and Lucasianism seemed to have caused a terminological revolution in macroeconomics and led

to a replacement of so-called *Keynesian* macroeconometrics and macroeconometrics. The three pillars of Lucasianism, representative agent models, rational expectations hypothesis, and neutrality of money, failed to convince the community of science. As investigated in this paper, the prophecy of a replacement of old-fashioned models was not kept. The exegetical story of Lucas & Lucasianism refers to Tinbergen criticizing his Keynesian view of the theory of economic policy, while Keynes had objected to Tinbergen's model a long time ago. There is no textual evidence that Keynes agreed with Tinbergen while Lucas disagreed with Tinbergen. The Lucas critique advanced the macroeconomic literature leaving the sphere of econometrics behind it. Some authors refer to the Lucas critique in a way that shows the importance of recognizing that economics is a social science which is not guided by certain laws or by the law of gravity or by numbers. Any regime change has to be implemented on the basis of changes in expectations and responses by economic agents. This *common knowledge* once implemented by Keynes, then used by Lucas and Lucasianism to substitute Keynesian macroeconomics, regardless of what Keynes did say, is now applied to aspects of model building.

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