

The Gospel according to Lucas: comments on De Vroey

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1, Introduction

I am glad to accept the invitation of *Economics*, welcome innovative open-access e-journal, to contribute an open assessment of De Vroey's paper "Lucas on the Relationship between Theory and Ideology" published as discussion paper by this Journal (De Vroey, 2010). This is also the occasion for trying a different style of refereeing. Too often, when the referee writes a blind assessment, he is led by the rules of the game to play the role of the public prosecutor that has to discover and corroborate a series of charges, while the author has to defend himself without the help of an attorney. This is not always the most constructive way to proceed as it establishes an asymmetric relation between the author and the referee that may contradict the very rationale of *peer review*. A related problem is that blind referees are chosen by the editors and tend to reflect their biases, while in the open approach everyone may intervene and has to take full responsibility for his assertions. The open assessment allows thus a more equalitarian and constructive style of dialogue with the author, more akin to that entertained with a co-author. In this spirit my comments will be an extension and elaboration of some of the arguments put forward in the paper rather than a mere critical appraisal of it. The points of disagreement will be made explicit but will remain in the realm of reasoned differences of opinion to be settled through a constructive dialogue.

I have to say at the very outset that I found this contribution by De Vroey (henceforth DW) perceptive, illuminating and timely: *perceptive* as it shows an unusually deep understanding of Lucas's methodology, *illuminating* as it cleverly connects the published texts with excerpts from unpublished writings from Duke University Lucas's Archives, *timely* because the ongoing Great recession has seriously questioned the soundness of mainstream macroeconomics whose recent evolution has been greatly influenced by Lucas's methodology.

My comments are articulated in four sections. In the second section I discuss the epistemological and methodological issues while in the third section I analyze their policy implications and related normative issues. In the fourth section I advance a few tentative remarks on the Lucas's methodological standpoint in the light of the preceding comments. Conclusions follow.

To facilitate comparison between my comments and the original text, my comments will broadly follow the order of DW's contribution using as far as possible the same headings.

1. Lucas's conception of macroeconomic theory

DW starts from a summary of Lucas's conception of macroeconomic¹ theory emphasising six crucial points (here summarized through excerpts from DW, p.2-3):

- a) "macroeconomics without microfoundations is unacceptable",
- b) "macroeconomics is part of general equilibrium analysis",
- c) "a macroeconomic theory and a mathematical model are one and the same thing",
- d) "a theory is concerned with imaginary constructions",
- e) "macroeconomic models are of no interest if they fail to reach policy conclusions",
- f) "theory ought to be tested against facts".

This characterization of Lucas's methodology is fairly correct but a few issues have to be further discussed.

a) "macroeconomics without microfoundations is unacceptable"

Keynesian theory was under heavy attack in the late 1960s as its mainstream version ("neoclassical synthesis") found it increasingly difficult to explain and control the growing frequency and gravity of inflation's bouts in industrialized countries. The offensive came principally from two different camps that aimed to revive the classical principles:² the monetarists led by Friedman who attacked mainly the violation of the classical dichotomy and the discretionary power attributed to policy authorities, and the "microfoundationists" led by Phelps who criticized mainly the lack of proper microfoundations. In the late 1960s these two streams of anti-Keynesian criticism directed a convergent attack against the Phillips curve interpreted by mainstream Keynesians as a stable menu of policy options (Phelps, 1967; Friedman, 1968). The defeat of mainstream Keynesian economists on this crucial issue opened a period of transition in search of a constructive alternative. From this point of view the monetarists and the microfoundationists moved in different directions until Lucas succeeded in the early 1970s to suggest a creative synthesis of these approaches working out a new methodology diverging not only from that of Keynes but also from that of Friedman and Phelps. While all these approaches had focused on out-of-equilibrium dynamics, Lucas worked out a pure equilibrium method focusing exclusively on equilibrium positions as described by the Arrow-Debreu general equilibrium model. This assumption, neglected by SW, allowed the systematic adoption in macroeconomic models of Muth's rational expectations hypothesis scrapping the hypothesis of adaptive expectations that had been used, under different assumptions and obtaining

¹ As is well-known Lucas never liked the word macroeconomics and emphasized the unity of economic theory. Not by chance the school originated from his contributions has been called New classical *economics*. I use here the term macroeconomics to narrow the boundaries of our methodological discussion taking account that "the split between micro and macroeconomics has probably grown sharper in recent decades, despite the methodological convergence" (Woodford, 1999, p.31).

² I use the word *classic* in the same sense of Keynes and Lucas.

different results, by Keynesians, monetarists and microfoundedists. Lucas claimed that his suggested methodology endowed macroeconomics with the same rigorous classical microfoundations that underlie Arrow-Debreu general equilibrium model.³

This is questionable for at least four orders of reasons that DW did not discuss in his paper: first, general equilibrium foundations are neither a necessary nor a sufficient condition for classical microfoundations; second, there are reasons to believe that alternative microfoundations could be preferable on many issues; third, theory of complexity and theory of evolution stress the emergence in a socioeconomic system of properties not reducible to those of the underlying micro entities; finally, in a complex evolutionary system the explanation and prediction of individual behaviour requires macroeconomic not less than microeconomic foundations of economic behaviour. The literature on these issues is immense. On the first point we will say something under the next sub-heading. On the second critical argument we just recall the extensive evidence accumulated in the last decades by experimental and behavioural economics on the role of limited rationality, emotions, social and ethical preferences that are at variance with the *Homo economicus* assumptions. On the third point Keynes had started a reflection that was ignored by the mainstream postwar Keynesian economists (neoclassical synthesis) but that was forward looking in the light of a series of successive acquisitions in many sciences including biology and physics. Finally, on the fourth critical argument I maintain that the individual behaviour cannot be seen exclusively in terms of adaptation to a given environment but also as a proactive adaptation of the environment to human needs determining a complex interaction between individual and collective behaviour (Vercelli, 2005). In the light of these insights it is surprising that a critical mind of the depth of Lucas never tried to justify why the issue of microfoundations may be only conceived in the classical way. This fundamental weakness of Lucas's methodology is disturbing also because the necessity of microfoundations has been taken as a dogma by most followers rejecting as non scientific whatever contribution had a different approach on this basic principle.

b) “macroeconomics is part of general equilibrium analysis”

I observe first that this point plays a crucial role in Lucas's methodology but is derived from the assertions under the points a) and e). The deepest and most influential argument in favour of this point is found in the paper, surprisingly neglected by DW, in which Lucas puts forward his influential “critique” (Lucas, 1976). Starting from the normative assertion under the point e), Lucas argues that only proper foundations of a model in terms of general equilibrium theory allows one to understand and predict how the economic agents react to a new environment as modified by a new policy rule. In his opinion, only in this case we can compare the consequences of alternative policy

³ As is well known the classical microfoundations are based on the choices of maximizing self-interested fully rational agents. In their standard version they imply the dichotomy between the real and monetary system.

rules and reach significant policy conclusions. To fully understand the meaning and scope of this point we need two qualifications that are not made explicit in DW's paper. First, what is often reiterated in Lucas is that sound macroeconomics must have explicit foundations in *Arrow-Debreu* general equilibrium analysis. This qualification is necessary since there are various styles of general equilibrium analysis that are characterized by different foundations and have divergent methodological implications. Lucas sticks to the Arrow-Debreu version that is based on axiomatic foundations not only because he considers it is the most advanced version from the technical point of view but also because the previous and alternative versions patently required dynamic foundations that are inconsistent with Lucas's methodology (Ingrao and Israel, 1990). The model-building strategy of Lucas is based on a pure-equilibrium approach that excludes from the formal analysis disequilibrium states and non-equilibrium dynamics as non-intelligible (see Vercelli, 1991, and the quotations from Lucas there reported and commented); therefore the stability of equilibrium is seen in its turn as a non-intelligible issue or requirement.

Lucas has a second line of defence of his systematic neglect of disequilibrium and instability that is developed in an interesting, though often neglected, paper on the relations between psychology and economic theory that is mentioned by DW only en passant (Lucas, 1986). He sets his discussion in terms of an adaptive meta-model of socioeconomic behaviour "I use the term "adaptive" to refer to [...the...] trial-and error process through which our modes of behaviour are determined" (Lucas, 1986, p.217). He then distinguishes the search for the optimal adaptation rules by trial and errors from their empirical testing. According to Lucas, the specification and testing of optimal rules of conduct when the process of adaptation is completed is the natural object of economics that "has tended to focus on situations in which the agent can be expected to "know" or to have learned the consequences of different actions so that his observed choices reveal stable features of his underlying preferences" (ibidem, p.218). The process of adaptation itself is heavily influenced by extraeconomic factors and is the natural object of psychology: "experimental psychology has traditionally focused on the adaptive process by which decision rules are replaced by others" (ibidem, p.217). The logical link between the adaptive approach of psychology and the equilibrium approach of economics is clearly traced: "I think of economics as studying decision rules that are steady states of some adaptive process" (ibidem, p.218). Full employment equilibrium is interpreted as a *rational* position "as the solution to some more complex maximum problem" (ibidem, p.226). Notice that this distinction is reminiscent of that between *context of discovery* and *context of justification* that is one of the basic tenets of positivist philosophy of science and in particular of logical empiricism (see. e.g., Suppe, 1977). This second argument is quite different from the first one. In both cases the stability of equilibrium is not questioned because adaptation is seen as a

process of convergence towards optimal rules. The difference lies in the empirical scope of economic theory. On the basis of the second argument Lucas recognizes that the assumptions of economic theory (in his own version) may work well empirically only if “one assumes that people have long ago hit on decision rules suited to their situations-“rational” rules-and utilizes theories about these rules to predict behaviour“ (Lucas, 1986, p.232). He admits that “this is certainly not true of all problems of interest” (ibidem), but he maintains that the method of economics is powerful only under these assumptions. Notice that this articulated argument on adaptive systems has ontological overtones that are reminiscent of a realist epistemology in potential conflict with his overarching pragmatist epistemology.

Does compliance with point b) imply acceptable microfoundations as required by a) ? Lucas never took great pain to discuss this issue as he seems to consider obvious the positive answer. However, this is not really so. In what follows, for the sake of argument, we give to the requirement of microfoundations the same meaning given by Lucas: “classical” microfoundations in terms of the choices of maximizing self-interested fully rational agents. Within the Lucas’s methodological perspective as expressed in the “Lucas’s critique”, proper microfoundations should be given in terms of decision theory when we consider isolated economic agents, and game theory when we consider their strategic interactions. However, as is well known, we have a host of decision theories under uncertainty and a not smaller variety of game theories; in many cases many of these approaches start from “classical postulates” similar to those assumed by Lucas (Vercelli, 1999b). In the case of decision theory one can provide rigorous foundations to representative agent models but general equilibrium analysis is in fact bypassed; in the case of game theory one can prove the convergence of a model of n agents characterized by strategic interaction towards a general equilibrium model only under stringent conditions (Shapley-Shubik, 1969). The ability to provide rigorous foundations to a model in terms of Arrow-Debreu general equilibrium is thus neither a necessary nor a sufficient condition for proper classical microfoundations. On the first two points we have thus to conclude that there is an unsolved tension in Lucas methodology between the requirement of general equilibrium foundations and that of microfoundations.⁴

c) “a macroeconomic theory and a mathematical model are one and the same thing”. DW maintains that “the hallmark of Lucas’ methodological standpoint is his narrow conception of a theory, which treats a theory and a mathematical model as identical” (DW, p.5). I wish to expand a

⁴ Lucas seems occasionally to recognize this point admitting that his argumentation “is a case for the use of game theory in general, not for the use of competitive theory in particular. The case for the use of competitive theory in modelling business cycles would, if I had to develop it here, be based entirely on convenience, or on the limits imposed on us by available technology for working out the implications of other equilibrium definitions” (Lucas, 1981, p. 293, n.11).

bit on this issue going in some more detail on the presumable roots of this idiosyncratic position and its consequences.

DW succeeds in showing that in order to understand Lucas's methodology we have to take seriously this "minority viewpoint" as is reiterated in many of his published and unpublished writings where it plays a crucial role in the argumentation. It is true that not all his followers showed full agreement with Lucas on this point and that before him it is difficult to find economists who held this position. However, Lucas's assertion does not come out of the blue and may be related to similar approaches in physics such as Mach's empiriocriticism and Bridgman operationalism. Restricting the analysis to the methodology of economics we can understand its genesis and presuppositions by referring to what Lucas has always considered the canon of sound science in economics: Debreu's *Theory of value* (Debreu, 1959). Economic theory is here exposed in two successive stages: the axiomatic system that is avowedly devoid of any empirical implication and its "interpretation", i.e. the attribution of empirical meanings to the symbols of the axiomatic system, that connects the axiomatic system with the empirical evidence. Since the axiomatic system has not an *independent* role in economics, it may seem natural in this context to identify *economic* theory with the mathematical *model* that provides the *general* interpretation of the axiomatic system. We can then specialize this general model in different directions to account for specific (temporal, spatial, thematic) features of the empirical evidence so obtaining a variety of models exhibiting sound foundations in general equilibrium theory. It seems to me that this description represents well the methodological vision of Lucas, or at least its genesis. We wish to emphasize that the Debreu's methodological canon is strictly related to that codified by logical empiricism in the 1950s (see e.g. Hempel, 1952) building on ideas originating from the Vienna and Berlin circles in the 1920s.⁵ According to a well-known metaphor suggested by Hempel, one of the prominent representatives of this school of thought, the axiomatic system is like a network (of axioms and formal relations derived from the axioms) which fluctuates loose over the plane of observation: an empirical science such as economics has to anchor this network to the plane of observation through its empirical interpretation. The paradigmatic bridge between pure theory and empirical evidence is given by an explicit link between an empirical regularity derived from factual evidence and a formal law derived from the theory's axioms. This "anchorage" clarifies whether the regularities

⁵ As Debreu maintains in the Preface of his masterpiece "the theory of value is treated here with the standards of rigor of the contemporary formalist school of mathematics" (Debreu, 1959, p.x). As is well known he had been deeply influenced in his youth in Paris by the exciting achievements of the Bourbaki research programme at the frontier of the formalist school of mathematics. This school deeply influenced logical empiricism that extended to the empirical sciences the dichotomy between the axiomatic system and its interpretation as it was codified by the formalist school of mathematics. As Debreu puts it "allegiance to rigor dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations." (ibidem). This implied in empirical sciences such as economics a similar dichotomy between statements that are true or false according to their logical form and statements whose truth or falsity depend on empirical testing.

detected in the empirical evidence are spurious or have an explanation derivable from the axioms. In the second case we may have some degree of confidence that the empirical regularity under examination may be used for predictive purposes.

Lucas's idiosyncratic view is fairly in tune with the contemporaneous position in philosophy of science, and in particular with the logical empiricist canon of science. If this is true, we could direct against the Lucas's methodological standpoint a wealth of criticisms that philosophy of science and many first-rate scientists have advanced against this view.⁶ We do not pursue here this line of criticism that would require much more than a short paper as this one; however we want to emphasize that Lucas's version of the logical empiricist canon suffers from a further weakness of its own. Debreu's two-stage articulation of theory separates the formal, i.e. logical and mathematical, core of a theory from its empirical interpretation and shows the crucial role of the semantic dimension of any theory. On the contrary, the collapse of theory formation and appraisal within a unique analytical stage (formulation of an empirically significant mathematical model) clouds the crucial role of the dimensions different from the formal ones that Lucas tends to classify under the broad catchall concept of *ideology*. This criticism is not meant to deny that he gives a crucial role to the empirical testing of models. The problem that I want to stress is that the interpretation of a theory cannot be independently tested because the results of the empirical testing of a theory are conditional to the axioms of the theory *and* its interpretation. For example, the systematic interpretation of the solutions of a sound macroeconomic model as an equilibrium position is not implicit in the formal characteristics of the model but has far-reaching implications for empirical testing and policy, as is recognized by Lucas himself (see quotation in DW, p.13). In particular, the continuous maintenance of equilibrium through time implies the exclusion of disequilibrium dynamics that would produce deviations from maximizing behaviour so that the system's dynamics is purely exogenous (further implications of this viewpoint are discussed in Vercelli, 2005). This prejudices the issue of market failures and admissible policy rules (see section 3).

d) "a theory is concerned with imaginary constructions". This is a common view in modern methodology. In particular it implies that the model of general equilibrium does not describe the actual markets in a realistic way. In the general equilibrium tradition this descriptive gap has been seen in two divergent ways that are well synthesized by the quotation from Walras reported by DW in note 13. According to the prevailing view among general equilibrium theorists, as expressed by Arrow, Debreu and Hahn, general equilibrium is a negative benchmark describing an ideal concept of perfect competition that is believed to be very useful to study the real markets (see DW, p.8). In this view the general equilibrium model has to be modified to reduce progressively the gap with real

⁶ A detailed early summary of the rise and decline of logical empiricism may be found in Suppe (1977)

markets. This was the point of view of Pareto, who was a civil engineer, and of many other economists who believe in social engineering. In this spirit the Keynesian economists adhering to the neoclassical synthesis studied the stability of general equilibrium and tried to introduce different kinds of frictions: transaction costs, rigidity of prices, limited rationality and so on. Not surprisingly this approach led to the justification of public intervention and Keynesian policies. In opposition to this “negative theology” Lucas, similarly to Walras, entertains a “positive theology” that aims to realize in practice the utopian state of affairs described by the general equilibrium model through institutional reforms. Therefore he does not endorse fine tuning, counter-cyclical policies, discretionary monetary policies, regulation of markets and other forms of social-engineering measures, but seeks a reduction of the gap between the ideal model of competitive equilibrium through institutional reforms progressively redistributing decision power on economic matters from the state to competitive markets. I see here a potential contradiction that, to the best of my knowledge, neither Lucas nor his followers addressed with the necessary care. The transition from this vale of tears to the heaven of utopia requires a deep knowledge of reality as it is, otherwise it will be impossible to push real markets towards the target, but this requires a knowledge of the real world that cannot rely ultimately on an abstract model of this realized utopia.

f) “theory ought to be tested against facts”; to this end it will use “model economies as analogue systems”. Let’s now discuss the nature of the relationship between models and the empirical evidence. Lucas studied macroeconomics in a period in which the conflict between Keynesian economists and monetarists had a counterpart in a methodological controversy between the Keynesians stressing the requisite of realism for the hypotheses underlying economic models and Friedman rejecting this requisite in name of an instrumentalist standpoint that stressed instead the requisite of their predictive power. The controversy did not have a clear winner as Friedman did not succeed to convince most macroeconomists that a requirement of realism for macroeconomics is useless and misleading. Like Friedman, Lucas did not accept the requisite of axioms’ realism as he believed inescapable the necessity of rooting the analysis in the classical axioms that are in apparent conflict with the empirical evidence; however, he did not reject altogether a requirement of realism different from successful prediction. To this end he shifted the issue in a direction that proved to be much more successful within the community of macroeconomists. He maintained that the requirement of realism, or something reminiscent of it, has to be applied not to the axioms of the model but to their empirical implications: a good model should be able to “mimic” the behaviour of the real economy to which it is applied. This requirement has thus a descriptive dimension that precedes the pragmatic test of prediction and gives some ground to believe in the accuracy of its predictions: “the more dimensions on which the model mimics the answers actual economies give

to simple questions, the more we trust its answers to harder questions. This is the sense in which more “realism” in a model is clearly preferred to less” (Lucas 1981, p.272).

Lucas tries to clarify this delicate point by using the concept of *analogy* that conveys the idea of descriptive variance, but also -at the same time- of partial correspondence, between the time series generated through the model and a few crucial features of the empirical time series. DW documents well the crucial role attributed by Lucas to the concept of analogy in his assessment of the role of models. I do not agree however that “this idea is absent from methodological writings” (DW, p.15). In the last decades much of the methodological literature on the role of models in empirical sciences has stressed that a model is crucially characterized by analogies with the set of phenomena which the model is referred to. This is well documented in the introduction to the book by Morgan and Morrison (1999) cited also by DW. The crucial role of analogy in the models of empirical science has been emphasized long ago by Mary Hesse (1966): “as Hesse puts it, we can think of the relation between the model and the real system as displaying different kinds of analogies. There is a positive analogy ... , a negative analogy, and neutral [analogy] where the similarity relation is not known” (Morgan and Morrison, 1999, p.5). This crucial role of analogy in models may be partly clouded by the use of more or less close synonymous such as similarity (Fraassen, 1980), metaphor (McCloskey (1990), caricature (Gibbard and Varian, 1978). In particular, according to the semantic view of theories, “the relationship between the model and real systems is fleshed out in terms of similarity relations expressed by theoretical hypotheses of the form ‘model M is similar to system S in certain respects and degrees’” (Morgan and Morrison, 1999, p. 4).

What is peculiar in Lucas is thus not his insistence on analogy but on its specific role in the application of the economic models to the empirical evidence:

- a) the analogy is restricted to the *behaviour* of the model as compared with the system to which it is applied, while its role is emphatically denied as far as their *structures* is concerned.
- b) the ability of the model to mimic the behaviour of the empirical system is verbally described in terms of partial correspondence between the description of the two behaviours.

There are passages in which Lucas seems to hint to a realist view of analogy as degree of truth (in the classical sense of correspondence between representation and object represented); in which case the emphasis on analogy would signal a significant departure from the traditional instrumentalism in the Friedman’s version. In the latter case the model is seen as a black box and the only thing that matters is its predictive ability. However, this can be established only ex post while we have to choose ex ante the model whose predictions we believe most reliable. As we have seen above, Lucas seems to suggest that we should rely on the model that succeeded to mimic best the past time series. Using an expression of DW, Lucas seems here, as elsewhere, to walk on a tightrope between

different points of view, in this case instrumentalism, realism, and behaviourism. What is really surprising, however, is that Lucas did not try to translate his remarks on analogy in operational terms, leaving them in the stage, he should say, of sheer ideology. Either analogy is a generic requisite of sound models, or it is a choice criterion of the most reliable option among them. In the first case the choice of a model to clarify a policy issue has nothing to do with its *degree* of analogy. But this view raises two objections. First, if we ignore any sort of measure of analogy, the set of candidate models would be too large as it would be very difficult to demonstrate that a reasonable model does not have any analogy at all with the reference system. Second, if the measure of analogy is irrelevant for choice and what matters is only its predictive accuracy why should a model be analogic? After all a digital barometer may predict the weather better than an analogic one. And why an analogic clock should mimic the time better than a digital clock? So, either the role of analogy is immaterial for the choice of a model and we cannot understand Lucas's insistence on it, or it is relevant for the choice of the most reliable model in which case we need a measure which Lucas never tried to work out.

In the literature we find rigorous formal measures of the analogy between model and empirical systems such as that of isomorphism (see e.g. Suppes, 1967; Fraassen, 1980; Weitzenfeld, 1984). Surprisingly enough, Lucas does not go beyond a vague relation of symmetry that is too generic to be operational. (see DW, p. 7). We have to conclude that Lucas's criteria for the acceptability and choice of models did not go beyond what he would call sheer ideology.

3. Ideology, policy implications and ethical issues

We may turn now to the normative aspects of Lucas's paradigm.

Ideology and the narrowness of Lucas's conception of a theory. DW is right in emphasizing that Lucas's particularly narrow conception of theory implies an unusually broad concept of ideology. In this view, even methodology should be classified as ideology because of its normative character and this should apply also to Lucas's own methodology. I believe on the contrary that each theory is characterized by a suitable methodology that is often developed to support and implement the theory. Newton's theory of gravitation required the development of calculus, while Einstein's new cosmology required the development of tensor theory and non-Euclidean geometry. Analogously Lucas's macroeconomics requires a new interpretation and development of the rational expectations hypothesis introduced by Muth a few years before and of the theory of stochastic processes. One cannot thus separate the intrinsic methodology of a theory from the theory itself by pushing it in the catchall category of ideology. In addition the mathematical models associated with a theory cannot exhaust its cognitive implications. We should introduce at least an intermediate category between

science and ideology that I am inclined to call “vision”, borrowing the term from Schumpeter (1954), since the “analytic effort is of necessity preceded by a preanalytic cognitive act that supplies the raw material for the analytic effort... this preanalytic cognitive act will be called Vision” (ibidem, p. 41). We may observe that the vision, being defined as a cognitive act, is in principle falsifiable. A case in point is the controversy in astronomy between the heliocentric and the geocentric systems. The two competing visions did not translate for centuries in rigorous mathematical models but the controversy between scientists went on in the light of the empirical evidence. The heliocentric vision was rejected by the church for ideological reasons (the alleged contradiction with Scriptures), but was accepted by Galileo on the basis of the empirical observation as is made evident by his famous apocryphal exclamation “and yet it moves!” (“*eppur si muove*”).

It is my conviction that the role of vision does not only precede the analytic effort but has to accompany it. This is partly recognized by Schumpeter himself: “it is interesting to note that vision...not only must precede historically the emergence of analytic effort in any field but also may re-enter the history of any established science each time somebody teaches us to *see* things in a light of which the source is not to be found in the facts, methods, and results of the pre-existing state of the science” (ibidem, p.41). This is the case of scientific revolutions as studied by Kuhn (1970) where the role of vision is particularly evident. But I contend that also in normal science the vision plays a crucial role. This emerges clearly in the second stage of Debreu general equilibrium theory: the interpretation of the axiomatic system is not made by chance but is guided by the vision of the interpreter. Lucas interprets the solution of the axiomatic system as equilibrium, because he believes in the stability of adaptive systems regulated by competitive markets, while Patinkin insisted on the slow convergence towards full employment equilibrium considering it a basic market failure to be corrected by Keynesian policies. In my opinion the role of vision is not fading away as a consequence of scientific progress. On the contrary the growing division of labour enhances the role of a general vision connecting the increasingly smaller and smaller tesserae of analytic research in an overall picture (Vercelli, 1999a). Summing up, the vision underlying a theory (or a family of models) is not devoid of a mechanism of self-correction based on a rational assessment of the empirical evidence. The great impact of Lucas on macroeconomics derived mainly from his new vision that he was able to implement through a new methodology. If this is true, Lucas’s innovation has been mainly the creation, initial implementation and defence of a new macroeconomic vision accompanied by a new methodology capable to support and implement it. In a synthetic way the main contribution of Lucas may thus be interpreted as the proposition of a new *scientific paradigm* in the sense of Kuhn. A scientific paradigm may be defined in the following

way: “(1) a set of shared symbolic generalizations; (2) a common model of reality; (3) shared values as to standards and legitimate procedures; and (4) shared exemplars in the form of concrete problem solutions typical of the approach of the relevant scientific community” (Hutcheon, 1995, p.5). This describes well the impact of Lucas on the profession: on the basis of Debreu’s axiomatic system (point 1) Lucas provided a general model of macroeconomic behaviour (point 2) fixing the legitimate methodological procedures to implement this vision (point 3) and providing the prototype (equilibrium business cycle model) of specific analytic models to solve the policy problems of macroeconomics. If this reconstruction is correct, and we accept the demarcation between theory and ideology as suggested by Lucas, his contribution lies mainly in the realm of ideology since point (1) was pre-existing, point (2) is mainly a matter of interpretation of Debreu’s axiomatic system, point (3) is prescriptive while point (4) did not reach the stage of full-fledged empirical testing.⁷ This is not to minimize the depth and scope of Lucas’s achievements but to confirm DW’s contention that an honest application of Lucas’s overly restricted demarcation between theory and ideology to his own contributions should classify them as a sophisticated ideology.

Point e) “macroeconomic models are of no interest if they fail to reach policy conclusions”.

The policy orientation of macroeconomics is since long the prevailing view in macroeconomics that is shared, at this level of generality, also by Keynesian and heterodox macroeconomics. We find, however, radically different ideas of what policy should accomplish. DW rightly emphasizes Lucas’s strong belief in the utopian role of economic theory, a point that has been overlooked by most previous interpreters. The quotation from Walras is really illuminating as it epitomizes the two opposite attitudes of Walras and Pareto towards the general equilibrium model (DW, n. 13). The attitude (not the ideology) of the young Pareto was analogous to that of exponents of the neoclassical synthesis who aimed to draw Keynesian policy conclusions by injecting more realism in the model: the young Hicks (1939), Modigliani (1944), Samuelson (1947), Patinkin (1956) are proper examples. The translation of this model in policy-oriented econometric models gave a crucial role to macroeconomists as social engineers. The criticism of this role by Lucas, mainly based on the influential “Lucas critique”(Lucas, 1976), was devastating and rapidly swept away this growing and profitable business. Lucas’s scepticism about expertise is motivated by his awareness of the economists’ ignorance about the effective concrete functioning of real markets and about how the economic agents will adapt to a change in economic environment engineered by the policy authorities.

⁷ As Woodford (1999, p. 26) maintains “The “equilibrium business cycle models” of Lucas had really only been parables; they could not be regarded as literal descriptions of an economy, even allowing for the sort of idealization that all models of reality involve”.

The Lucasian theory, being oriented towards policy, is meant to maintain or improve the “health” of the socio-economic system. However, looked from this perspective the new classical economics is anomalous as it relies on a quite sophisticated physiology of the system but lacks a proper pathology. If one looks at the economic system in itself the Lucas’s approach provides only a physiology, while if one look at it within its environment it provides also a rudimentary pathology that is exclusively concerned with exogenous shocks. This basic weakness is strictly connected with the utopian view of general equilibrium theory that Lucas inherited from Walras. We argued elsewhere that this goes a long way in explaining the impotence of mainstream macroeconomics in predicting and controlling the recent crisis and in particular the Great Recession triggered in 2007 by the subprime mortgage crisis in the US (Vercelli, 2010). We believe on the contrary that to keep the socio-economic system in good health, to prevent serious illness and to cure it efficiently we need a full-fledged theory of its pathology that has to consider also its endogenous determinants and how the exogenous factors interact with its endogenous factors.

Lucas on the ‘non-interference’ precept. 20th century philosophy of science, epistemology and methodology have been obsessed by the objective of eliminating the interference of ideology with science.⁸ This is understandable in the light of the disasters produced by ideologies in the past century and before: the precept of “non interference” of ideology on science, as DW aptly calls it, is thus fully justified in its struggle to avoid the ideological contamination of science and to keep intact its power against noxious ideology. Unfortunately all the attempts to isolate science from the interference of ideology have so far failed (Suppe, 1977). Lucas seems to be aware that this precept cannot be fully implemented (see quotation by DW at page 13) and still he adopts it as a guiding rule of his methodology, in particular when he identifies theory with a mathematical model. DW suggests a reasonable solution to this apparent contradiction claiming that this methodological move is meant, if not to sterilize theory from ideology, at least to *tame it*. This could be explained in the following way: the use of a mathematical model allows scientists having a different ideological orientations to agree at least on what they disagree about, by clarifying which are the open or hidden assumptions underlying their different models, modelling styles, or argumentation strategies.

Lucas’s scepticism about expertise and the ‘non-exploitation’ precept. According to Lucas, models must have policy implications but “there is something wrong, and necessarily transient, with this easy translation of a technical contribution to economic theory into a platform for economic policy” (Lucas quoted by DW, p.13). The “minimal” interpretation suggested by DW “that economists should be cautious in extending the policy conclusions of their models into direct advice

⁸ Of course, this tradition has a long pedigree since the scientific revolution of the late Renaissance. An interesting early attempt to identify and sterilize the intrusions of ideology into modern science is the brilliant analysis by Francis Bacon of the distorting factors of science dubbed “idols” (of the tribe, cave, market place and theatre).

to governments” would be subscribed not only by Lucas but also by any honest economist of whatever school. This precept is not superfluous since many economists do not show the necessary cautiousness when endorsing specific policies. However, to understand the specific paradigm of Lucas, I agree that we have to focus on the second, “maximal”, interpretation “that economists should totally refrain from politically exploiting the results of their models” what DW dubs “the non-exploitation precept” (DW, 13-14).

Is there a contradiction in the Lucas’s paradigm between the necessary policy relevance of macroeconomic theory and the precept of non-exploitation of its policy implications? I see a way out from this apparent contradiction that is quite different from that suggested by DW. In Lucas’s opinion the ability of a mathematical model to mimic a real economy and then to predict the outcomes of alternative policy interventions is very limited. Although Lucas does not seem to have fully endorsed the thesis of the “impossibility of economic policy” that emerged early within his entourage (Sargent and Wallace, 1976), he was clearly sympathetic with the main message that underlies also his “critique”: the spontaneous reaction of the economic agents to the new environment, as modified by the policy intervention, will restore a situation similar to the initial one. Taking account of the inevitable transition costs and of the significant risk that the new situation may be worse than the original one, we have to avoid this sort of policy advising. This does not imply, however, that a serious and responsible economist should totally refrain from policy advice but only that he should confine himself to particular categories of policy advice. First he should refrain from encouraging discretionary interventions and fine tuning and restrict advice on policy *rules*. Second, the most significant rules are those that constrain the discretionary power of the subjects than can distort market competition, in particular the public authorities. Third, the advisor should suggest the rules of the game and the institutional setting that may best approach the utopian target. In this interpretation there is no contradiction between the requisite of policy relevance of macroeconomics and the scepticism on its empirical relevance: on the contrary these observations confirm and clarify the utopian perspective of Lucas.

4 Assessing Lucas’s standpoint

I will stay within the boundaries self-imposed by DW restricting my assessment to Lucas’s methodological ideas “and *not* his contributions to the development of macroeconomics” (DW, p.21).

Lucas on methodology, compared to methodologists on methodology

According to some eminent scholars, methodological reflections should never be severed from innovative research (this point of view has been maintained, among others, by one of the greatest

scientist-methodologists of the 20th century, J. Piaget, in many writings). Lucas is no doubt an excellent example of this orientation (as Friedman and Keynes before him). DW rightly emphasizes that “Lucas is a self-taught methodologist who does not bother to relate his thoughts on methodology to the methodological literature” (DW, p. 15) and in his view this may explain the idiosyncratic positions taken on crucial features of his methodology such as the identification of ideology and theory and the emphasis on analogy. I maintained before, however, that also on these two points he is quite in tune with methodologists’ methodology of the second half of 19th century. The almost complete lack of references to the methodological literature even of fellow economists may be explained in terms of common *zeitgeist* or as an excellent tactical choice. Methodology is a very contentious subdiscipline both in philosophy of science and in economics and Lucas was wise enough to avoid a negative feedback between conflicting views on macroeconomics and methodology. Lucas’s methodological reflections are presented as entrenched in substantive discipline-specific issues being instrumental to their clarification and selection. This is one of the reasons that made Lucas’s methodological argumentation a masterpiece in the art of persuasion of peer economists and policy-oriented practitioners.

A political agenda?

DW reconstructs very well the interplay between political and scientific motivations at the root of the new classical revolution as led and catalyzed by Lucas. Starting from the slightly more complex view of the articulation of science here suggested I maintained that he was the original builder of a new macroeconomics paradigm streamlining the vision of classical economics and elaborating a new methodology capable to support and implement an updated version of the classical policy strategy. This paradigm had a great impact on the history of economic ideas and the evolution of policy strategies. As a matter of fact, Lucas’s paradigm as propagated by Lucas’s himself and his followers, was understood as providing a great support to the neoliberal (or neo-conservative) ideology (this time in the broader meaning of policy agenda) that spread since the late 1970s and inspired the political platform of many influential political parties and many governments (starting from that of Mrs Thatcher in 1979). However, we have to distinguish sharply between *ideal* neoliberalism (the position springing from Lucas’s vision) and *real* neoliberalism, i.e. its effective alleged application to the real world. This is not a novelty as we are used to distinguish between the socialism of Marx and “real” socialism; we should also distinguish between the political agenda of Keynes and “real Keynesism” as implemented in the 1960s. Also Lucas, in my opinion, underwent the same fate. In a passage quoted by DW (p.13). Lucas summarizes a policy agenda consistent with his paradigm as aiming to “limited government, budget balance and tight money”. If this is the policy agenda of neoliberalism that could be endorsed by Lucas, we have to say that “real”

neoliberalism did not pursue consistently none of the three targets. Limiting my references to the United States, it is dubious that the markets are today more competitive than they were in the 1970s when the neoliberal revolution began. The privatization policies were occasions to re-enforce crony capitalism and the deregulation policies have strengthened Darwinian competition favouring the most powerful subjects rather than Walrasian competition where no one has a sizeable discretionary power. Balanced budget was far from a priority under Reagan,⁹ Bush senior and junior. As for tight money this precept has been followed by Volker (chairman of the US federal Reserve Board in the period 1979-1987) but not by Greenspan (1987-2006) and Bernanke who preferred to adopt a fairly permissive monetary policy dubbed “Greenspan put” (than followed by the “Bernanke put”).¹⁰ It would be interesting to pursue this analysis in greater detail but this should be sufficient to conclude that also Lucas’s utopia, as many others before him, has been consistently betrayed.

The consistency of Lucas’s conception of theory

According to DW the consistency of Lucas’s methodological vision is doubtful as it “has one foot in Lausanne and the other in Chicago” (DW, p.19), i.e. combines Walrasian elements with Marshallian elements. This assertion by DW seems to me misleading. In my opinion the only important methodological feature of Friedman that is prominent also in Lucas (apart from the obvious common adhesion to classical principles) is his instrumentalism. On the other hand, the points of divergence are multifarious and deep. In particular Friedman’s argumentation focuses on disequilibrium dynamics and its interaction with adaptive expectations. On the contrary Lucas’s approach is a pure equilibrium method characterized by rational expectations. Friedman, as all Marshallian scholars, does not object in principle to a partial equilibrium analysis that, on the contrary, is rejected by Lucas and all Walrasian scholars, and so on. The instrumentalism is not peculiar of Friedman and other Marshallian scholars as it has been very popular in economics since long. The significant differences between the methodological approach of Lucas and that of prominent neo-Walrasian scholars is significant but already Walras, as mentioned above, had brilliantly distinguished, *within* the general equilibrium approach, two different views: Lucas is closer to the Walrasian utopian perspective while the neo-Walrasian scholars mentioned by DW are closer to Pareto’s social engineering approach. I may conclude that Lucas develops the Walrasian ideas in a direction quite different from that pursued by other eminent neo-Walrasian but his self-definition as a “hopeless neo-Walrasian” has to be taken seriously.

⁹ Lucas stigmatized also the attempts by the Reagan administration to fine-tune the economy: “It’s hard to be a conservative with the Reagan administration turning to fine-tuning, which seems insane to me” (Lucas in Klamer, 1984, p.51)

¹⁰ The Greenspan put, followed by the “Bernanke put”, refers to the adoption of a permissive monetary policy whenever the trend of financial assets prices starts to fall.

I agree with DW, however, that Lucas's methodology is eclectic and this may question its inner coherence. As argued above, I see in Lucas's methodology the co-existence of different elements: rationalism (the pure equilibrium method), empiricism even in the extreme behaviourist form (the model mimicking the behaviour of an empirical system), logical empiricism (in the Debreu's version), instrumentalism (similar to that of Friedman but with some grafting of apparently realistic elements) and so on. Lucas's ability to walk on the tightrope between these contrasting methodological traditions renders its position *prima facie* less controversial and may have added to the appeal of his theory.

5. Concluding remarks

I think that the two metaphors suggested by DW likening Lucas as methodologist to a tightrope walker and to a Zen master (DW, pp. 21) capture well the weakness and the appeal of his methodological approach. My comments on the Lucas's paradigm did not conceal my criticisms of his methodological paradigm but they did not mean to question his personal qualities as a deep and honest thinker. It is a pity, however, that -differently from most other intellectual leaders- he did not bother to reply systematically to the growing number of critics. Whatever the reason for Lucas's aloofness, this attitude did not help to discourage the superiority complex of many followers not possessing his critical awareness. Lucas seems at first sight to be a thinker characterized by the systematic Cartesian doubt against dogmatism, and still the prevailing interpretation of his paradigm invites a dogmatic attitude. The crucial point is that Lucas's methodological assertions exclude from the range of sound (scientific) macroeconomics all the existing alternatives, even when they are expressed by state-of-the-art mathematical models. The Keynesian approach is excluded for its lack of (classical) microfoundations and/or the emphasis on limited rationality and disequilibrium dynamics. The monetarist approach (Friedman) is excluded because it lacks rigorous foundations in general equilibrium analysis and its models are not considered state-of-the-art mathematical models. Hayeck's and Coase's contributions are rejected as insufficiently formalized. Finally, questioned about post-Keynesian economists Lucas did not hesitate to give a haughty answer: "I don't know whether to take them seriously [laughter]" (in Klamer, 1984, p. 35). The trouble is not in his outspoken order of preference between alternative economic theories that any researchers has the right, and maybe the duty, to make explicit provided that one accurately motivates his own opinion, but in the fact that all the approaches but one, are excluded as *non-scientific* and ultimately infected by ideology, so that pluralism within economics, and in particular within what we call macroeconomics, has been largely forsaken. Authentic pluralism requires the acknowledgement that there may be different approaches and methodologies to which we

recognize, at least in principle, equality of scientific status. Otherwise a referee feels authorized to reject a priori the paper, or a committee to reject the application, or mass media to neglect systematically the opinion, of economists belonging to different schools of thought. In the mainstream view, a researcher holding an alternative point of view is not seen as a serious competitor to be convinced through rational persuasion but rather as an heterodox scholar if not a crank or an heretic; in any case he is seen as “dangerous”, a vehicle of ideological infection to be prevented and repressed. And this happened more frequently as soon as the Lucasian paradigm became hegemonic. We have recognized that Lucas is aware of the weak points of his paradigm and does not conceal them but most followers did not care of the cautiousness and qualifications of their intellectual leader. Lucas himself betrays on different occasions an attitude hardly consistent with pluralism, as in the following passage: “we [Lucas and Sargent] think we’re using the language of modern economics that, sooner or later, everyone will be using” (Lucas in Klamer, 1984, p.49). In addition he often repeats that he does not see a serious scientific alternative to his own approach; for example at the end of his introduction to Lucas (1981, p, 17) where he asserts that “if there is a single main theme to this introduction it is a sense of having severely limited theoretical options”. Since also Lucas in the end has to recognize that even his own suggested methodology, as logical empiricism, Popper’s fallibilism and many other ambitious attempts before him, is unable to sterilize science from ideology, maybe we should consider an alternative. What is really important is to be aware of the cracks through which ideology may infiltrate science, and to make the public opinion aware that the policy implications of economists models are conditional to the open or hidden ideological assumptions inbuilt in the model and in the procedures of its application. This road is difficult to pursue because the consumers of model-based advice do not like, and often do not understand, this sort of warnings and because the discovery of ideological infiltrations is a never-ending story. However we have to pursue it to comply with the tenets of pluralism and professional honesty. This attitude would also solve the “non exploitation paradox” in the most comprehensive way. A serious and responsible economist should not abstain from policy recommendations, provided that he always clarifies the limits of his models, the conditionality of the results to the assumptions including those that could be interpreted as infiltrated by ideology, the relativity of the recommendations to the kind of model and methodology adopted. In the end, in a democratic society, consensus is obtained through persuasion and as scientists we should hope that persuasion is based as much as possible on the rational assessment of the weight of arguments.

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