Reading excerpts for HOPE Workshop on Friday, April 15, 2016
from “A History of Macroeconomics, from Keynes to Lucas and Beyond,”
by Michel De Vroey
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A Methodological Breach

The aim of this chapter is to substantiate my claim that Lucas's work marked a turning point in the history of macroeconomics. Any scientific revolution always comprises an important methodological component. I will argue that, in the case at hand, three important shifts stand out: a change in the quaeestum of macroeconomics, a change in the conception of the relation between theory and model; and, finally, a change in the notion of equilibrium upon which the analysis is based. The overall picture surfacing from my study is that these differences are linked with Lucas's decision to make macroeconomics neo-Walrasian, while traditional Keynesian macroeconomists followed Keynes by adopting a Marshallian methodology.

Lucas had a long-standing interest in methodological problems; in an interview with Snowdon and Vane, Prescott hailed him as "the master of methodology, as well as defining problems" (Snowdon and Vane 2005: 351). While he never wrote a systematic exposition of his methodological standpoint – his "Methods and Problems in Business Cycle Theory" ([1980] 1981a) comes closest to such an enterprise – it nonetheless worth noticing that about half the essays in his book, *Studies in Business Cycle Theory*, are of a methodological nature. Moreover, these are only the tip of the iceberg. Exploring the Lucas Archives at Duke University's Special Archives division (Lucas, Various) reveals the existence of many drafts and notes by Lucas pertaining to methodological issues. They are invaluable for my inquiry for several reasons: they may complement the published works, they may shed light on the genesis of Lucas's mature vision, and they may reveal Lucas's deep-seated views, what he really believed but preferred not to include in published articles. I will abundantly draw from this little known material.²

I am aware that unpublished pieces found in archives also raise difficulties: they must be interpreted cautiously because the reason they have remained unpublished may well be that the author abandoned the views expressed in them. Be that as it may, the greater the congruency of drafts with published pieces, the higher the likelihood that they are more than tentative ideas committed to paper. This is often true for Lucas, a typical situation being one in which ideas that are expressed succinctly, if not inadvertently, in a published article are based on a fuller treatment in earlier notes which remained unpublished.

² A remnant of the earlier view is Azariadis's statement in an entry in the *New Palgrave Dictionary* that "involuntary unemployment is for many economists the sine qua non of modern macroeconomics" (Azariadis 1987: 734).

³ As will be seen, the unemployment theme made a return in later developments of the DSGE program.
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(b) Macroeconomics is part of general equilibrium analysis. Its concern is with the workings of an entire economy, and it must account for the interactions between the component parts of the economy. It must be dynamic, that is, dealing with the economy over time. Uncertainty is accounted for by ascribing probabilities to future states of the world and to the occurrences of shocks. Thus, the DSGE acronym, which had not yet been invented, fits what Lucas had in mind.

(c) A macroeconomic theory and a mathematical model are two and the same thing. This conception, which can be traced back to Walras, runs counter to another, more widespread, understanding of the relationship between theory and model. According to the latter, they are distinct entities: a theory is a set of propositions about reality while a model, be it mathematical or in prose, is an attempt at rigorously setting out the implications of some part of the theory.

(d) A theory is concerned with imaginary constructions; it is avowedly nonrealistic. Insistence on the ‘realism’ of an economic model subverts its potential usefulness in thinking about reality. On this general view of the nature of economic theory then a ‘theory’ is not a collection of assertions about the behavior of the actual economy but rather an explicit set of instructions for building a parallel or analogue system—a mechanical, imitation economy. A ‘good’ model, from this point of view, will not be exactly more ‘real’ than a poor one, but will provide better imitations. (Lucas [1980] 1981: 271-272)

The central assumptions of macroeconomic models, such as rational expectations, ought thus to be viewed as modeling devices, model-building principles rather than propositions about reality.

One can ask, for example, whether expectations are rational in the Klein-Goldberger model of the United States economy; one cannot ask whether people in the United States have rational expectations (Lucas. Various. Box 23, Barro Folder).

By prescribing these standards, Lucas was treading Walras’s footsteps. However, on two other scores, he adhered to the earlier practice of macroeconomics. First, he held the strong conviction that macroeconomic models are of no interest if they fail to reach policy conclusions.

‘The central question that macroeconomists need to resolve: Which necessarily abstract models can help us to answer which practical questions of economic policy? [Lucas. Various. Box 26, Reflections on contemporary economics folder].

4 “With respect to the qualitative behavior of co-movements among series, business cycles are alike. To theoretically inclined economists, this conclusion should be attractive and challenging, for it suggests the possibility of a unified explanation of business cycles, grounded on the general laws governing market economies, rather than in political or institutional characteristics specific to the particular countries or periods” (Lucas [1977] 1981:218, Lucas’s emphasis).


7 This section is drawn from De Vroey (2013); other studies of Lucas’s epistemology are Vercelli (1991) and Boumans (1999, 2003).

8 Thus, in the traditional view, the model is subservient to the theory. The following quote from Leijonhufvud is a fine depiction of this viewpoint: “I propose to conceive of ‘economic theories’ as a set of beliefs about the economy and how it functions. They refer to the ‘real world’. ‘Models’ are formal but partial representations of theories. A model never encompasses the entire theory to which it refers” (Leijonhufvud 1997). A different approach of the theory-model relationship can be found in Boumans (2003) and Morgan (2012).
Second, he regarded macroeconomics as an applied rather than a purely abstract field. A confrontation between theory and reality needed to be at its heart. What was to be done was to construct "a fully articulate artificial economy which behaves through time so as to imitate closely the time series behavior of actual economies" (Lucas [1977] 1981a, p. 219), what I call "Lucas's FORTRAN injunction."

Our task as I see it is to write a FORTRAN program that will accept specific economic policy rules as "inputs" and will generate as "output" statistics describing the operating characteristics of times series we care about, which are predicted to result from these policies (Lucas [1980a] 1981a: 288).

A theory/model ought to be assessed on its ability to make correct predictions. The better its ability to reproduce past events, the more trustworthy the model is for assessing new policy measures.

Model economies as analogous systems

There is a tension in Lucas's methodological standpoint bearing on how to bring together the disparate sets of requirements I just exposed. Lucas's solution to this riddle was to regard models as analogous systems. Time and again, one finds Lucas writing that economic models should be viewed this way. In Lucas's "Methods and Problems" article alone, I counted seven occurrences of the term 'analogy.' For example:

Progress in economic thinking means getting better and better abstract, analogue economic models, not better verbal observations about the world. (Lucas [1980a] 1981a: 276)

However, in his published papers, Lucas gave no clue as to what he meant exactly. This gap can be filled by searching through his archives. They contain a series of drafts - some handwritten, some typed - in which he expanded at length on the notion of an analogy, on the role of models, and on the relationship between modeling and economic policy. The subsequent discussion is based on one such fragments.

Lucas's argumentation starts from two premises, both of which may come as a surprise. The first is that a model, though a fiction, is nonetheless an observable reality:

We speak of modeling phenomena or models of phenomena, suggesting that observed phenomena are one kind of thing and models of them another thing, but we want to define a model to be itself a phenomenon: something the behavior of which can be observed. Then what is the relationship between a set of phenomena and the second set that we call a model of the first set? I will call this relationship analogy. (Lucas, Various, Box 27, adaptive behavior folder).

This still does not clarify what Lucas meant by the term 'analogy.' An answer is provided later in the same draft, where Lucas wrote that he took an analogy to "mean a symmetric relationship between two things." These things may either be a thing in the usual sense and a theory, which to Lucas is just another thing, or two distinct procedures for generating observations. In another set of notes, Lucas added that "we must make liberal use of analogies: judgments that one situation is similar enough to another to call for the same reaction."

Lucas's second premise was that economic theory (or at least general equilibrium theory) is utopian in nature, a proposition that he drew from a (bold) comparison between economics and anthropology:

Economic theory, like anthropology, 'works' by studying societies which are in some relevant sense simpler or more primitive than our own, in the hope either that relations that are important but hidden in our society will be laid bare in simpler ones, or that concrete evidence can be discovered for possibilities which are open to us which are without precedent in our own history. Unlike anthropologists, however, economists simply invent the primitive societies we study, a practice which frees us from limiting ourselves to societies which can be physically visited as sparing us the discomforts of long stays among savages. This method of society-invention is the source of the utopian character of economics; and of the mix of distrust and envy with which we are viewed by our fellow social scientists. The point of studying wholly fictional, rather than actual societies, is that it is relatively inexpensive to subject them to external forces of various types and observe the way they react. If subjected to forces similar to those acting on actual societies, the artificial society reacts in a similar way, we gain confidence that there are useful connections between the invented society and the one we really care about. (Lucas, Various, Box 13, Directions of macroeconomics 1979 folder).

Surely, anthropologists will fail to recognize themselves in Lucas's account but, leaving that aside, the comparison successfully brings out Lucas's message: models are fictitious economies and by manipulating them, we can learn about the functioning of real economies. Lucas's reason for resorting to the use of analogies followed from his view that macroeconomics should be used to assess the impact of policy measures. Normally, these cannot be assessed experimentally, the stagflation episode of the 1970s being a possible exception. So, second-best solutions need to be found. One of these is to look for analogous real-world experiences - has there been another, not too different, country...
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and Kydland and Prescott's application of it as wrong? (Yes, I think, except for 'dismiss'). An alternative point of view toward things and theory is this: we observe things and events, and we perceive analogies among them. (Lucas. Various. Box 27, adaptive behavior folder).

A NEW EQUILIBRIUM CONCEPT

From Adam Smith onward, equilibrium has played a central role in economic theory, one of the main features differentiating economics from the other social sciences. The traditional view of equilibrium was that it means a state of rest. This was also how it was understood in Keynesian macroeconomics until the Lucasian revolution. The latter introduced a new equilibrium concept in macroeconomics. It could be named 'dynamic equilibrium,' but this label is equivocal since the state of rest view also comprises a dynamic dimension (yet an unsatisfactory one). I will therefore use the 'intertemporal equilibrium' terminology.

Lucas did not invent the new concept; he imported it into macroeconomics from Arrow and Debreu's theory of competitive equilibrium. My aim in this section is to contrast the two concepts.

The traditional understanding of equilibrium: equilibrium as a state of rest

First of all, it must be noticed that traditional equilibrium theory is a set of propositions about reality. It is taken for granted that the functioning of real economies can be interpreted as manifesting that equilibration forces, the so-called 'market forces,' are at work in them. Among many others, here are three quotations, drawn from economists of different allegiances, written in support of the state of rest concept.

Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping. . . . Viewed in this way, the market is like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it. (Walras1954: 380)

The ordinary economic situation is one of disequilibrium moving in the direction of equilibrium rather than of realised equilibrium. [Viner 1931] 1953: 206]

If we are to make empirically interesting statements about disequilibrium and equilibrium, statements that have potential empirical content, we must define these terms so that both are meaningful and both can be observed — in order to say that in fact we do not observe one of them. (Lipsey 2000: 72)

I like to think of theories — economic and psychological, both — as simulatable systems, analogues to the actual system we are trying to study. From this point of view, the Wharton model, say, bears the same kind of logical relationship to the United States economy as France, say, does: it is just a different economy, or system, but one that is similar enough to the U.S. economy that we might hope to learn about the properties of one through the study of the other. If our objective is to learn what the consequences of introducing a value added tax in the U.S. might be, we might study its consequences in France or simulate the Wharton system under such a tax or, better still, do both. (Lucas. Various. Box 27, adaptive behavior folder; Lucas's emphasis)
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\[
\frac{d p_i}{dt} = a_i E_i(p_1, \ldots, p_{m-1})
\]

These equations refer to an exchange economy comprising \( m \) goods, with good \( m \) acting as numéraire; \( p_i \) is the numéraire price of good \( i \); every \( a_i \) is a positive constant expressing the speed of adjustment in the \( i \)-th market; \( E_i \) is the excess demand function for good \( i \).

This traditional conception of equilibrium may well receive some vindication from elementary physics, but the main factor explaining its adoption is that it corresponds to our common-sense understanding of equilibrium. It is thus no surprise that the founding fathers of economic theory adopted it. However, on reflection, its appositeness is far from obvious. The state of rest equilibrium concept captures one aspect of time, namely duration — it takes time for the effects of decisions to be realized. Unfortunately, it cannot come to grips with another of its dimensions, namely that the passage of time is accompanied by incessant irreversible changes, large or small. Therefore, as argued by Donzelli, it turns out to be a-temporal. As a result, any models based on it...

...are structurally incapable of providing the slightest explanation of any economic phenomenon whose occurrence essentially depends on economic activities taking place in time. (Donzelli 1989: 158)

Changes in these data can occur during the period of analysis, but they must be reversible and temporary. Irreversible changes can only enter the picture in the interstices between periods of analysis, which means that they cannot be part of price theory.

A related problem is that the length of the period of analysis cannot be inferred from observation but is decided by economists (this explains why I dislike Marshall's expression 'period of the market'). Yet, they face an impossible dilemma as they need to find a compromise between two opposite criteria: on the one hand, the period of analysis must be long enough to allow adjustment processes to take place, on the other hand, the longer it is, the more contrived the assumption that only reversible shocks can be considered. No satisfying solution to this dilemma can be found.

A last problem is that the speed of the adjustment process is a 'free parameter'; assigning a value to it is a decision left to the economist. If the latter is eager to argue that the economy evolves in disequilibrium, she just has to assign the speed of adjustment a low value, the opposite being true is she likes the idea of a quick return to equilibrium.

Lucas’s equilibrium conception

In their "After Keynesian Macroeconomics" paper, Lucas and Sargent insisted that the new paradigm they advocated was based on a different equilibrium concept, intertemporal equilibrium, arguing as follows:
When Keynes wrote, the terms equilibrium and classical carried certain positive and normative connotations which seemed to rule out either modifier being applied to business cycle theory. The term equilibrium was thought to refer to a system at rest, and some used both equilibrium and classical interchangeably with ideal. Thus an economy in classical equilibrium would be both unchanging and unimprovable by policy interventions. With terms used in this way, it is no wonder that few economists regarded equilibrium theory as a promising starting point to understand business cycles and design policy to mitigate or eliminate them. In recent years, the meaning of the term equilibrium has changed so dramatically that a theorist of the 1930s would not recognize it. An economy following a multivariate stochastic process is now routinely described as being in equilibrium, by which it is meant nothing more than at each point in time, postulates (a) that markets clear, MDV and (b) that agents display optimizing behavior, MDV above are satisfied. This development, which stemmed mainly from work by K. J. Arrow and G. Debreu, implies that simply to look at any economic time series and conclude that it is a disequilibrium phenomenon is a meaningless observation. (Lucas and Sargent [1979a] 1994: 15)

Before discussing Lucas’s equilibrium concept, I have one remark to make about the two postulates Lucas and Sargent mention. These postulates would have been better captured by writing that their approach rests on (a) optimizing planning and (b) a trade technology allowing for a generalized transformation of optimizing planning into optimizing behavior. Since in their framework (b) is taken care of by the auctioneer assumption, these two postulates can be combined into a single one, namely that all agents experience optimal behavior, in the spirit of McKenzie’s observation, mentioned in Chapter 7 that general equilibrium is generalized individual equilibrium.

In Lucas and Sargent’s new approach, time is framed as a succession of points in time which need to be dated. It is assumed that exchanges are confined to some of these. Decisions are made before trade. As a result, the analysis can be considered as an intertemporal planning problem. Equilibrium is defined as a state where all the agents (of the fictitious model) follow an optimizing consumption/leisure intertemporal path. It is no longer equated with ‘being at rest,’ and the impossible task of defining an adequate period of analysis vanishes. Observing that quantities and prices change across time no longer seems to be interpreted as meaning that the economy displays disequilibrium.

It is possible to construct systems in competitive equilibrium in a contingent-claim sense which exhibit a vast variety of dynamic behavior. The idea that an economic system in equilibrium is in any sense ‘at rest’ is simply an anachronism. (Lucas [1980] 1981: 287)

Two traits of the new equilibrium concept must be underlined. The first is that equilibrium is declared to exist as a postulate. This is well encapsulated in Lucas’s ‘equilibrium discipline’ expression. The term ‘discipline’ must be understood as conveying the view that the equilibrium postulate is a rule that economists impose upon themselves when constructing their models. What matters for judging the appositeness of adopting such a stringent postulate is what can be done with models based on it (and what are the drawbacks of not adopting it). The second trait pertains to another important breach from the traditional conception of equilibrium. Supporters of stationary equilibrium take it for granted that equilibrium and disequilibrium are features of reality. Here, Lucas also embraces the opposite viewpoint. His view is that equilibrium ought to be understood as a characteristic of the way in which economists look at reality rather than as a characteristic of reality.

Cleared markets is simply a principle, not verifiable by direct observation, which may or may not be useful in constructing successful hypotheses about the behavior of these series. Alternative principles, such as the postulate of the existence of a third-party auctioneer inducing wage rigidity and cleared markets, are similarly ‘unrealistic,’ in the not especially important sense of not offering a good description of observed market institution. (Lucas and Sargent [1978] 1994: 21)

I think general discussions, especially by non-economists, of whether the system is in equilibrium or not are almost entirely nonsensical. You can’t look out of this window and ask whether New Orleans is in equilibrium. What does that mean? Equilibrium is a property of the way we look at things, not a property of reality. (Lucas’s interview with Snowdon and Vane, 1998: 127)\footnote{In other words, to use Weintraub’s apt formulation, equilibrium is imposed upon the world: “This symposium provided additional examples of such argumetions: the discussions generated by McCallum’s paper, and Grandmont’s, contained various appeals to the ‘Principle’ that the world either was or was not in equilibrium. The commentator in this audience seemed to think that they had a way of discussing the truth of the idea that observed states were equilibria without committing themselves to any particular theory of macroeconomics. This is, of course, an illusion: equilibrium states, or disequilibria are characteristics of our theories, and are thus imposed on the world” (Weintraub 1990: 173).}

This change of equilibrium concept proposed by Lucas amounted to a Copernican revolution. The transformation involved comprises several dimensions: (a) from the state of rest to the intertemporal concept of equilibrium, (b) from an approach wherein equilibrium and disequilibrium are organically linked to an approach using only the equilibrium category, and (c) from an approach wherein the assessment of the existence of equilibrium or disequilibrium is a matter of characterizing reality to an approach where instead the equilibrium concept pertains only to the fictitious model economy. Keynesian economists had a hard time accepting this threefold modification. For example, as will be seen in Chapter 12, time and again they criticized Lucas on the grounds that it was obvious that in reality markets, and in particular the labor market, are in a state of disequilibrium.\footnote{As stated by Farmer: “When economists before Lucas saw unemployment in the labor market, they thought that they were observing a market in disequilibrium” (Farmer 2010b: 70).}

In light of these remarks, it proves difficult to assess whether the new conception of equilibrium, with its exclusion of the disequilibrium notion, amounts to attributing a higher or a lower role to the notion of equilibrium. Getting disequilibrium out of the picture may suggest a higher role for equilibrium. But there is another side to the picture: the fact that equilibrium has
become a postulate and that equilibrium or disequilibrium are no longer claimed to be characteristics of reality amounts to shrinking the pretense of equilibrium theory. The assessment to make about the equilibrium discipline then does not consist in pondering whether it is realistic but whether it is an efficient way of constructing economic theory. Moreover, when every outcome is by construction an equilibrium outcome, the normative connotation that was associated earlier with equilibrium vanishes. Welfare considerations now need to focus on the comparison of alternative equilibrium positions.

According to Lucas, there were at least two reasons, both related to 'theoretical efficiency,' why macroeconomics must shift towards the intertemporal equilibrium approach. The first is that the traditional conception is wanting as explained above. The second reason is that dropping the disequilibrium notion is good riddance. It must be banned because it refers to 'unintelligent behavior' (Lucas [1977] 1981a: 225) or, in other words, it lacks microfoundations. Now that the new concept is available (thanks to him), Lucas stated, not using it would make no sense:

To ask why the monetary theorists of the 1940s did not make use of the contingent-claim view of equilibrium is, it seems to me, like asking why Hannibal did not use tanks against the Romans instead of elephants. ([1980] 1981a: 286)

COMPARING KEYNESIAN AND NEW CLASSICAL MACROECONOMICS

In the above sections, I have dwelled on what are in my eyes the two central programmatic bifurcations made by Lucas. In this last section, I want to integrate these in a broader comparison of Keynesian macroeconomics and DSGE macroeconomics in its first installment, that is, new classical macroeconomics. Table 10.1 summarizes the contrast between them. Several of these benchmarks have already been discussed earlier. Therefore, I will content myself with commenting the remaining ones.

The starting point

Keynes's *General Theory* lay the foundations for Keynesian macroeconomics, while Lucas's "Expectations and the Neutrality of Money" article did so for Lusciian macroeconomics. As noticed by Sargent, it is difficult to imagine two works that are more opposed, a complex kaleidoscopic 384-page book and a 21-page mathematical article:

Many of us regard Lucas's 1972 *Journal of Economic Theory* paper as the flagship of the Revolution; it is different than the flagship of that earlier revolution, Keynes's *General Theory of Employment Interest and Money*, which was ambitious, wide ranging, imprecise and vague enough to induce twenty-five years of controversy about

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what the book really meant. Lucas's paper was a narrow, technical study, ... There was never any confusion about what Lucas's paper meant. (Sargent 1996: 537)

A general equilibrium approach?

Two criteria must be fulfilled for models to qualify as general equilibrium models. First, they must be concerned with the economy in its entirety rather than with particular sections of it. Second, the analysis must encompass the study of the interactions between the composing branches of the economy. A general equilibrium intention was already present in Keynes's *General Theory*. But a full implementation did not follow up. Keynesian econometric models took entire economies as objects of analysis, but dealt with across
equations relations in an offhand way. As for the DSGE program, it is fully
general equilibrium.

Money
Both modeling strategies are concerned with a monetary economy. Contrary to
the other items, there is a similarity here.

Microfoundations
Microfoundations, as understood by Lucas, relate to Walras's method of
starting the analysis by analyzing how agents make optimal choices. With a
few exceptions, Keynesian models usually followed the Marshallian principle
that analysis can start at the level of market supply and demand functions
rather than at the individual decision-making level. It is not that agents are
assumed to behave in a non-optimizing way, but rather that this stage of the
reasoning is skipped. On the contrary Lucas regarded microfoundations à la
Walras as a sine qua non for writing sound theory. To Lucas, microfoundations
were more than the choice-theoretical apparatus. They also involved bringing
people into the picture, a perspective that he claimed was absent from Keyne-
sian macroeconomics.

I think a lot of the work in Keynesian economics has gotten too far away from thinking
about individuals and their decisions at all. Keynesians don't often worry about what
actual individuals are doing. They look at mechanical statistical relationships that have
no connection with what real individuals are actually doing. (Lucas. Various. The
Margin's interview. Box 7, Correspondence 1989 folder).

The driving factor, supply or demand?
According to the Keynesian approach, variations in output and employment result
from changes in aggregate demand. The underlying picture is that labor suppliers
are passive, employment decisions being made unilaterally by firms. Moreover, this
approach tends to consider the supply of labor and the labor force as the same thing,
a fixed magnitude. By contrast, in the DSGE program the driving force of economic
activity is the supply of labor. This change in emphasis, forward-looking behavior

being combined with the idea of intertemporal leisure substitution, is a main ingredi-
ent in Lucas's view of macroeconomics. An implication of this move from demand
to supply is a shift of emphasis from firms' to households' decision-making process.

Real effects of monetary changes
While Hicks favored demand activation under the form of fiscal policy, Modigliani argued in favor of monetary activation. From then on, Keynesians held the conviction that monetary expansion can increase the level of employ-
ment in a sustained way. Friedman and Lucas shared the goal of demonstrating
that Keynesians were wrong.

Empirical method
Keynesian macroeconomics embraced the Cowles Commission's simultaneous
equation method, witnessing to impressive developments. The Lucas critique
brought out its flaws.

Methodological priority
The central methodological principle of Keynesian macroeconomics was exter-
nal consistency. In this view, models are only as good as they are realistic. The
prevailing intellectual mood was pragmatic. That several of the basic notions —
involuntary unemployment, full employment, rigidity, and sluggishness — were
defined in a loose way, or that the analysis focused on the short period with no
attention being given to the linkage between the short and the long period, were
by no means considered harmful methodological practices. Empirical models,
the construction of which was often left to engineers rather than economists,
were more data than theory-constrained. Lucas wanted macroeconomics to
abide by the Walrasian methodological principles. Accordingly, internal con-
sistency became the alpha and the omega of theoretical construction.

Natural policy conclusions
The vision of the economy closest to Keynesian macroeconomics can be
branded 'mitigated liberalism.' It defends the market system as being superior
to a planning system without fully advocating laissez-faire. Keynesian models
tend to support demand activation by the state. The opposite conclusion, a
defense of laissez-faire, emerges from new classical macroeconomics. Fluctua-
tions in employment reflect rational and optimal reactions by economic agents
to changing conditions. If there is no market failure, there is also no need for the
state to intervene.
Intuitiveness of the approach taken and accessibility to the layman

Keynesian macroeconomics theory is simple to understand even by non-economists. Its level of technicality is low. Many of its basic notions have made their way into newspapers and political discourse. Lucasian macroeconomics resorts to new, complex mathematical techniques, such as dynamic programming. Consequently, the technical barrier to entry is much higher than for Keynesian macroeconomics. The Lucasian conception of theory and model is counter-intuitive and of little appeal to the layman.

These are, in my view, the most salient traits on which the two approaches of macroeconomics can be compared. They stand in sharp contrast. However, the breach is not total. They still have a monetary economy as their object of analysis in common. This makes for one precise object of disputation between them, the issue of the real effects of monetary policy.

Three final remarks must be made about this comparison. First, one clue to understand what underpins this contrast and at which I have hinted on several occasions is that the change that occurred can be interpreted as a transition from Marshallian to neo-Walrasian macroeconomics. Second, I need to underline that my comparison only serves a pedagogical purpose. Its main drawback is probably that it is static. This does a disservice to the presentation of Keynesian macroeconomics. It extended over more than two decades, and many improvements took place over this period, so that a snapshot such as mine is necessarily a simplification. My last remark is that the comparison above concerns only the first wave of modeling in the DSGE program. When I will come to the study of the subsequent waves, it will be seen that most of the specificities of the first one remained valid, but that there were significant shifts nonetheless.

Assessing Lucas

In this chapter I assess Lucas’s contribution to macroeconomics. In the first section, I express my disagreement with his judgment about Keynes. In the second, I briefly comment on intertemporal substitution, the cornerstone of DSGE macroeconomics. In the third, I discuss whether Lucas’s attempt of making macroeconomics Walrasian may be regarded a valid move. In the fourth, I probe into Lucas’s methodological standpoint by displaying what I consider to be two ambiguities in his approach. Lucas’s opponents have often argued that his work was ideologically motivated. This is a point that should not be swept under the rug. Therefore, in the penultimate section I consider whether it is a valid contention. Finally, the chapter ends with a few concluding remarks.

Lucas on Keynes

Lucas’s claim, that The General Theory is a minor contribution to economic theory – that “Keynes was not a very good technical economist” (Usabiaga Ibanez 1999: 180) – has the merit of being original. Lucas may have been led to make such a judgment because of his conviction that economic theory ought to be mathematical. Still, I find it difficult to agree with him. By passing such an a-historical judgment, Lucas went against what he wrote in his “Problems and Methods” article, namely, that economists of the past, as clever as they may have been, could not do much better than what the contemporary level of development of the discipline allowed. With respect to the standards of his time, there is no reason to state that Keynes was a bad technical economist. On the contrary, gauged against the writings on unemployment at the time—say, Hicks’s Theory of Wages (1932) and Pigou’s Theory of Unemployment (1933)—Keynes’s work seems to me much superior as far as breadth of inspiration and conceptual innovations are concerned.

Let me give just one example of Keynes’s sharp mind. That the notion of rational expectations could have been conceived of in Keynes’s time is
new classical paradigm, it hardly stopped the momentum of the Lucasian revolution. The reason is that intertemporal substitution is such a crucial ingredient of the whole program that it cannot do without it, while alternative mechanisms are scarce. In Lucas’s words:

I see no way to account for observed employment patterns that does not rest on an understanding of the intertemporal substitutability of labor. The literature contains innumerable examples of possible additional, supplementary considerations, but to my knowledge no alternatives. (Lucas 1981a: 4)

The debate about the empirical validity of the assumption of a high leisure intertemporal substitutability has persisted up to the present (although interesting progress has taken place; more about this in subsequent chapters). For the sake of the argumentation, let us assume that the defenders of low elasticity are right. Would this suffice to condemn the DSGE program? My answer, based on the conclusion of my discussion of monetarism, is ‘No’: demonstrating the empirical invalidity of a theoretical proposition that is a part of a wider theoretical paradigm (however central it may be) is not a sufficient condition for dismissing the whole paradigm.

THE PROS AND CONS OF WALRASIAN MACROECONOMICS

Lucas’s methodological standpoint can be summarized in the statement that macroeconomics should be based on neo-Walrasian principles. He once wrote “I am a hopeless ‘neo-Walrasian’” (letter to Driscoll, dated November 23, 1977 Lucas, Various, Box 30). He also set himself apart from Friedman on the grounds that the latter was Marshallian whereas he was Walrasian.4 When thinking of neo-Walrasian theory, the names which come to mind are those of Kenneth Arrow, Gérard Debreu, Lionel McKenzie, Frank Hahn, David Cass, and Karl Shell, among others. These economists are also those Lucas seemed to refer to in his “Method and Problem in Business Cycle Theory” article. Yet it is unsure whether neo-Walrasians were ready to consider him as one of their own. At least, somebody like Cass was not:

Bob [Lucas] was in the Chicago tradition and was very concerned about empirical testing – whatever the hell that means – something that I have little sympathy for and very little interest in, to be perfectly honest. So there was quite a difference in viewpoints about why you did theory and what the relevance of theory is (Cass’s interview with Spear and Wright 1988: 146).

4 “Snowdon and Vane: You acknowledge that Friedman has had a great influence on you, yet his methodological approach is completely different to your own approach to macroeconomics. Why did his methodological approach not appeal to you? Lucas: I like mathematics and general equilibrium theory. Friedman didn’t.” (Lucas’s interview by Snowdon and Vane 1998: 334).
The problem did not lie in Lucas's "Expectations and the Neutrality of Money" paper in itself. It was acceptable to general equilibrium theorists as it resembled the incursions into neighboring territories in which they sometimes indulged. What matters in Cass's quotation is the last sentence, that is, his perception that neo-Walrasian theory and macroeconomics pursue different purposes and hence must not be mingled. Contemporary neo-Walrasian economists regarded general equilibrium theory as an abstract construction, the strength of which lay in its ability to posit issues in a rigorous way. They were also aware of its limits. In their view, the best it could provide was to be a negative benchmark.

In a nutshell, behind the veil of their mathematical language, neo-Walrasians do political philosophy à la Rawls, while macroeconomists engage in applied work. Lucas may well have wanted macroeconomics to become Walrasian, but he also wanted it to keep its long-standing traits of empirical verification and policy conclusions. This was not to neo-Walrasian economists' liking. To them, testing their models empirically made little sense. As noticed by Roy Weintraub, "empirical work, ideas of fact and falsifications, played no role at all" in Walrasian theory (Weintraub 1983: 37). Or, in Hahn's words:

It is for all these reasons that I have always held the view that the Walrasian theory in all of its manifestations is an important theoretical benchmark but that a vast and unruly terrain had to be traversed before one understood (let alone predicted) the behavior of an actual economy. No economist and certainly no theorist should be ignorant of the Walrasian theory, and no economist and certainly no theorist should pronounce on actual economies and policies on its basis alone (Hahn 1983: 224).

In neo-Walrasian theory agents are price-makers. Hence there must be someone else announcing prices, the auctioneer. The auctioneer hypothesis, though necessary to explain the formation of equilibrium in neo-Walrasian theory, has little to commend it. Indeed, it amounts to pre-empting the main issue that should be addressed, that is, whether market forces are able to bring the economy to a state of efficiency.

As for policy conclusions, the problem is not just that neo-Walrasian theory evolves at a stratospheric level of abstraction. It is also that Keynes's indictment of classical theory because it relied on a 'Panglossian' vision of the economy (Pangloss is Candide's tutor in Voltaire's eponymous book) fully applies to neo-Walrasian theory:

The celebrated optimism of traditional economic theory, which as led to economists being looked upon as Candides, who, having left this world for the cultivation of their gardens, teach that all is for the best in the best of all possible worlds provided we will let well alone ... (Keynes 1936: 53).

Leaving the imaginary, best-of-all-possible worlds behind in order to study the malfunctions likely to arise in real-world economies is certainly not what occurs when taking the Walrasian track. Thus, with respect to Keynes's call to forget the Panglossian vision, Lucasian macroeconomics is clearly a step backward. Economists such as Hahn and Solow, and many others, found this move outrageous:

The irony is that macroeconomics began as the study of large-scale economic pathologies: prolonged depressions, mass unemployment, persistent inflation, etc. This focus was not invented by Keynes (although the depression of the 1930s did not pass without notice). After all, most of Haberler's classic Prosperity and Depression is about ideas that were in circulation before The General Theory. Now, at last, macroeconomic theory has as its central conception a model in which such pathologies are, strictly speaking, unmentionable. There is no legal way to talk about them (Hahn and Solow 1995: 2-5).

Though I can understand Hahn and Solow's viewpoint, I am less categorical than they are. At stake is the internal/external consistency dilemma, the worthiness of engaging in theoretical detours and ... patience – in one of his notes d'honneur (casual annotations scribbled on pieces of paper) Walras wrote:

One must know what one is doing. If one wants to harvest promptly, one should plant carrots and salads; if one has the ambition to plant oak trees, one must be wise enough to say: [posterity] will owe me this shade (Baranzini and Allison (2014): 1).

A possible justification of Lucas's standpoint can be found in a 1989 Journal of Economic Perspectives by Charles Plosser. According to Plosser, the results of Keynesian economists' attempts to theorize malfunctions were disappointing because they were introduced in an ad hoc way, instead of being systematically derived from a theoretical core model. The lesson he drew was that it is wiser to start the analysis with the study of an idealized state of the economy. In his words:

Progress towards understanding this idealized state is essential because it is logically impossible to attribute an important portion of fluctuations to market failure without an understanding of the sorts of fluctuations that would be observed in the absence of the hypothesized market failure. Keynesian models started out asserting market failures (like unexplained and unexploited gains from trade) and thus could offer no such understanding. (Plosser 1989: 53)

Taking such a position is admissible at a condition that I suggest to call the 'non-exploitation principle.' Models à la Lucas may well have policy

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5 In this respect, Lucas was more in the line of the vision held at the Cowles Commission.

6 Solow declared it a 'swindle' in his review of the English translation of Walras's Elements (Solow 1956: 88).

7 Wilhem Butner dubbed his criticism of new classical macroeconomics, "The Macroeconomics of Dr Pangloss" (1980).
conclusions as their normal outlet. Yet, their builders should refrain from recommending these to decision makers. Clearly, this implies a strong dose of stoicism. To his credit, Lucas expressed awareness of this precept as the following passage from the concluding section of his “Understanding Business Cycles” article testifies:

By seeking an equilibrium account of business cycles, one accepts in advance rather severe limitations on the scope of governmental countercyclical policy which might be rationalized by the theory. (Lucas [1977] 1981a: 234, Lucas’s emphasis)

The important element of this quote is the term ‘in advance,’ italicized by Lucas. By stressing this, he admitted that the limitation on countercyclical policy, the policy conclusion of his model, followed from its premises. Again, while this point is only hinted at in Lucas’s published papers, more is to be found in the archives:

One now reads of rational expectations not in Econometrica but in Time and Business Week, where it appears as a ‘school’ or ‘theory’ with apparently sweeping implications for important issues of economic policy. These implications seem primarily of a ‘conservative’ cast, favoring a reducing role for government, balanced budget fiscal policy, and tight and ‘unaccommodating’ monetary policy. Now the idea of limited government, budget balance and tight money are not unimportant to me; they are high on the list of values I carry into the voting booth every year, and for reasons I am willing to defend in some detail. These developments are not, then, ones which I find unwelcome or displeasing, nor do I find the journalistic treatment of rational expectations any less accurate than similar treatments of other developments in economics. ... There can be no simple connection between what appears on the scratch pads of professional economists, however original, and important conclusions about the way our society ought to operate. (Lucas. Various. Box 13, Directions of macroeconomics 1979 folder).

Interpreted in a loose way, this passage states that economists should be cautious when extending the policy conclusions of their models into direct advice to governments. Taken strictly, it means that economists should totally refrain from politically exploiting the results of their models.

**LUCAS’S AMBIGUITIES**

Wavering between monetarism and DSGE reasoning

Like Friedman’s 1968 article, Lucas’s 1972 paper aimed at demonstrating that monetary shocks can have real effects only in as far as they cannot be perfectly unanticipated. When Lucas expanded this into a model of the business cycle, he kept money shocks as the causal factor of fluctuations. However, a few years later, he admitted that RBC modeling, wherefrom money is absent, was the right line to follow. Lucas dated the ‘beginning of the end’ of his viewing the explanation of business fluctuations in terms of monetary shocks to the 1978 Bald Peak Conference where Kydland and Prescott presented an early version of their “Time to Build and Aggregate Fluctuations” paper. Nonetheless, in an October 1982 correspondence with Leijonhufvud, who wrote in a letter to him that he did not understand why he was such a monetarist, Lucas kept claiming his monetarist allegiance.

‘My’ monetarism is simply Friedman’s. I’ve tried over and over again to make this clear, but I don’t think people take it seriously. I suppose this is because Friedman and I have such different styles of doing economics, but really, style has nothing to do with it. (Lucas. Various. Box 3. Letter to Leijonhufvud, October 28, 1982. Correspondence 1982 folder). 8

True to himself, Lucas expressed the same viewpoint three decades later in the Introduction of the volume collecting his papers on monetary theory (Lucas 2013):

Now toward the end of my career as at the beginning, I see myself as a monetarist, a student of Milton Friedman and Allan Meltzer. My contributions to monetary theory have been in incorporating the quantity theory of money into modern, explicitly dynamic modeling. ... It is understandable that in the leading operational macroeconomics models today - real business cycle models and new Keynesian models - money as a measurable magnitude plays no role at all, but I hope we can do better than that in the models of the future. (Lucas 2013: XXVI-XXVII)

These two statements must be related to an intriguing observation made by Sargent in an article commemorating the twenty-fifth anniversary of Lucas’s “Expectations...” article (Sargent 1996). Sargent’s point was that this article, which played such a seminal role, was actually “the first and last paper [Lucas] would write in this line” - a line that Sargent defined as “an unrelentlessly deep approach to modeling monetary and macroeconomic phenomena in terms of explicitly spelled out phenomena” (Sargent 1996: 544), the very program that he set for himself.

The link to monetarism in Lucas’s Journal of Economic Theory paper was incidental to the methodology of the rational expectations program, but integral to the substance of Lucas’s own research program. (Sargent 1996: 544)

The first part of the quotation was confirmed by what happened afterwards, Kydland and Prescott’s transformation of Lucas’s model into RBC modeling from which the monetary dimension was absent, while nonetheless abiding by the DSGE program as set out by Lucas. As for Lucas’s own and distinct research program, Sargent characterized it as mere monetarism, that is, (a)

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8 While nobody could be further from Lucas than Leijonhufvud, the latter nonetheless wrote in the letter to which the excerpt above was Lucas’s answer: “I haven’t read a better book (in economics) for years” referring to Studies in Business Cycle Theory (Lucas 1981a). The moral to be drawn is that economic theory is also a gentleman’s sport.
Assessing Lucas

I personally agree with Lucas’s standpoint. However, there is a flip side to it, which Obstfeld and Rogoff aptly pointed out:

A theory of business cycles that has nothing to say about the Great Depression is like a theory of earthquakes that explains only small tremors. (Obstfeld and Rogoff 1996: 627)

So, Lucas ended up supporting a divide between types of explanations according to the subject dealt with, the very idea that he strongly rejected apropos of the neoclassical synthesis! This is another ambiguity in Lucas’s thinking.

A POLITICAL AGENDA?

In an article entitled “The Fall and Rise of Keynesian Economics,” Alan Blinder, an eminent Keynesian economist from Princeton University who also held important political appointments at the Council of Economic Advisers and the Board of Governors of the Federal Reserve Bank, assessed new classical macroeconomics with the following words:

I argue that the ascendancy of new classicism in academia was instead a triumph of a priori theorizing over empiricism, of intellectual aesthetics over observation and, in some measure, of conservative ideology over liberalism. (Blinder 1988: 2007: 110)\footnote{This dichotomy led Prescott to depart from the Lucasian standpoint, which he had initially endorsed, by claiming that the RBC conceptual apparatus, if slightly changed, can account for great depressions—now without capital letters! Cf. De Vroey and Pensiero (2006).}

In this quotation Blinder made two points. First, he expressed his regret about the ‘Walrasation’ of macroeconomics spearheaded by Lucas; Blinder’s words are close to those which Friedman used to dismiss Walrasian theory. His second point may be interpreted in two ways – either as meaning that the conclusions of new classical models were more conservative than those of Keynesian models, or as meaning that the ascent of new classical models resulted from a political motivation. The first of these statements is obviously right. The second amounts to stating that Lucas pursued a political agenda. In Chapter 4, I concluded that this was the case for Friedman, his declarations to the contrary notwithstanding. Can the same conclusion be reached about Lucas?

When trying to answer this question, the first observation to be made is that Lucas departed from Friedman’s assertion that theory and ideology could be radically split. In Lucas’s mind, macroeconomics is geared towards producing policy conclusions, and such conclusions necessarily support a particular ideological vision, to simplify either the free market solution or the Keynesian one. The two following quotations the first drawn from an interview with The

\footnote{The title of Blinder is not a typo. A few sentences after those I quote, Blinder writes: macroeconomics is already in the midst of another revolution which amounts to a return to Keynesian – but with a much more rigorous theoretical flavor (Blinder 1988: 110).}
Assessing Lucas

Really, there is never going to be such a thing as an uncontroversial way to settle disputes over economic policy, nor do I see why one would hope for such a state of affairs. It seems to me that our job is to try to make controversy useful, by focusing on discussion, analyzable issues. In Taylor's first paper, for example, contract length was selected arbitrarily (hence 'controversially') and was central to the operating characteristics of the model. But labor contracts are something we can? independent evidence on (as John did) or theorize about (as many people are not doing). Work like this is productive not because it settles policy issues in a way that honest people can't disagree over but because it channels controversy onto potentially productive tracks, because it gets us talking and thinking about issues that our equipment may let us make some progress on. (Lucas. Various. Box 5. Letter to Sims, July 17, 1982. Correspondence 1983 folder).15

These different elements point to the conclusion that the criticism I address to Friedman of mixing ideology and theory must not be extended to Lucas. An additional factor in favor of this judgment is the observation made earlier about Lucas's awareness that the policy conclusions of models are embedded in their premises and must not be peddled to policymakers. This is a statement that Friedman would not have uttered.

CONCLUDING REMARKS

Let me begin with a general impression about the present-day perception of Lucas's role in the history of macroeconomics. As mentioned in the Preface, last year I taught a graduate course on the subject matter of this book to third- or fourth-year doctoral students. It made me realize how unfamiliar they were with Lucas's work. They knew his name, they were aware that he had played a pioneering role in macroeconomics, but that was it. I see this as a testimony to how fast one may become passé. Leaving nostalgia aside, I find that a loss in knowledge ensues, which I regret. Of course, this is not the students' fault, but the result of how graduate programs are conceived with too much focus on technique. For their part, economists working at the cutting edge of the profession tread in Lucas's footsteps, but feel no need to refer to his views. They just look forward. The result is that most of what is written about Lucas comes from opponents to his views. On the one hand, there are those who dislike Lucas's work for ideological reasons. They take it for granted that it is part of the weaponry of 'ultra-liberalism,' which they regard as a sufficient condition for dismissing it without delving into its intricacies. On the other hand, there is also a dissension within the macroeconomics profession. It comes from defenders of the neoclassical synthesis viewpoint (meaning a defense of a pluralistic macroeconomics field). The objection here is against the Walrasian hegemony that Lucas wants to impose on the field. My surprise is thus that many of the critical positions taken against Lucas within the macroeconomics profession

15 The work by Taylor to which Lucas refers is staggering contract modeling; it will be studied in Chapter 13.

In a deleted passage from the introduction of his Models of the Business Cycle book, Lucas wrote: "I think it is fair to say that ideology plays an important role in contemporary discussion. There is probably no point in getting angry about this, but I do." (Lucas. Various. Box 13. Models of business cycles 1985-87 folder).

Lucas, quite bluntly, considers that economists such as Coase and Hayek, who broadly speaking share his vision of the economy, are more engaged in ideological rather than theoretical activities. "What I want from economics is a set of principles I can use to evaluate proposed government interventions, case by case, on their individual merits. I agree that explicit modeling can give a spurious sense of omniscience that one has to guard against, ... But if we give up explicit modeling, what have we got left except ideology? I don't think either Hayek or Coase have faced up to this question." (Lucas. Various. Box 8. Letter to K. Matsumura, March 29, 1995. Correspondence 1995 folder; my emphasis).

Admitting that there is inevitably an ideological dimension to economic discussions is one thing, liking it is another, and Lucas surely did not. Thus, since the ideological dimension could not be dispensed with, it had to be tamed. Using the mathematical language, which allowed keeping hermeneutic and ideological discussions at bay, was according to Lucas a first way of doing so. A second way was to draw a sharp separation between those propositions that pertain to the fictitious model economy and those pertaining to the real world. This is supposedly true for the premises of a model, for example, rational expectations, but also for its policy conclusions. Making the methodological standards ruling model construction explicit was a third barrier. Under these conditions, Lucas argued, fruitful conversations can take place even if they will be unable to settle ideological disputes. The following two quotations, the first drawn from a draft fragment, the second from Lucas's correspondence, make the point:

The classical issue of the proper role of government in a democratic society, of 'laws versus men' or 'rules versus authority,' are not going to be settled by technical advances in economics. It follows that no one's position on such basic questions needs to be threatened by such new technologies as may come to be at our disposal [Lucas had in mind here rational expectations]. (Lucas. Various. Box 26. Directions of macroeconomics 1979 folder).

Lucas, quite bluntly, considers that economists such as Coase and Hayek, who broadly speaking share his vision of the economy, are more engaged in ideological rather than theoretical activities. "What I want from economics is a set of principles I can use to evaluate proposed government interventions, case by case, on their individual merits. I agree that explicit modeling can give a spurious sense of omniscience that one has to guard against, ... But if we give up explicit modeling, what have we got left except ideology? I don't think either Hayek or Coase have faced up to this question." (Lucas. Various. Box 8. Letter to K. Matsumura, March 29, 1995. Correspondence 1995 folder; my emphasis).
have to do with the Marshall-Walras divide. I would not be surprised if a high correlation between a disinclination for Walrasian theory and one for Lucas’s approach existed.

My own viewpoint about this divide is relativistic as behooves a historian of economics. Where I depart from the critics of Lucas is that I do not share their view that following the Walrasian paradigm is per se a flaw, while monetarists and Keynesians see eye to eye in this respect. Therefore, I have no objections of principle against Lucas’s methodological canons, which have a Walrasian affiliation (except for what concerns empirical verification). What I want to insist on is the need to acknowledge the limitations of adopting the Walrasian approach. This is often not the case, Lucas himself being an exception here.

My general standpoint is that programs must be judged on their results. More generally, what I find important when assessing theoretical lines is less their value at the time of their rise than their posterity, that is, whether they have led to cumulative developments — in terms of Lejionhufvud’s metaphor, whether a frail branch developed into a sturdy one. Sargent put this nicely in an interview with Klamer:

When we do research, the idea is that you don’t produce a finished produce. You produce an input. You write the paper with the hope that it will be superseded. It’s not a success unless it’s superseded. Research is a living process involving other people. (Klamer 1984: 74)

The subsequent chapters will amply attest that this has been the case for the DSGE program.

I have also shown that Lucas, in spite of his strong desire for consistency, has been unable to avoid ambiguity. This is of course a flaw, but it is an understandable one — after all economics is a social science and the quest for epistemological purity may be fruitless if pushed too far. Lucas’s main ambiguity was his twofold allegiance to neo-Walrasian theory and to empirical verification. Did this amount to wanting to have the best of both worlds, or was it a bold move that would later prove to have been productive? As Lucas himself did not engage in implementing his empirical injunction, again the answer to this question must await my examination of the works of those who set this task out for themselves. Still, I may already ponder on his view of models as analogous systems. I surmise that thereby Lucas was hoping to break the deadlock of the non-exploitation principle. If analogous models can be proven to be empirically robust, they can be used to compare the outcomes of alternative policy measures. As a result, policy recommendations would receive some ‘scientific’

14 Compare Hayek’s papers on knowledge and its role in the functioning of market economies (Hayek 1948) with Walras’s Elements of Pure Economics. The immediate value of the former to understand the economy is incomparably superior to that of Walras’s Elements. Yet, as far as cumulative development is concerned, at the risk of upsetting my Austrian economist friends, I find that the latter has fared much better than the former.