What is (wrong with) economic theory?[1]

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Prologue

Following the greatest economic depression since the 1930s, the grand old man of modern economic growth theory, Nobel laureate Robert Solow, on July 20, 2010, gave a prepared statement on "Building a Science of Economics for the Real World" for a hearing in the U. S. Congress. According to Solow modern macroeconomics has not only failed at solving present economic and financial problems, but is "bound" to fail. Building dynamically stochastic general equilibrium models (DSGE) on "assuming the economy populated by a representative agent" - consisting of "one single combination worker-owner-consumer-everything-else who plans ahead carefully and lives forever" – do not pass "the smell test: does this really make sense?" One cannot but concur in Solow's surmise that a thoughtful person "faced with the thought that economic policy was being pursued on this basis, might reasonably wonder what planet he or she is on."

We will get back to the "representative agent model" below. But although it is one of the main reasons for the deficiencies in modern (macro)economic theory, it is far from the only modeling assumption that does not pass the smell taste. In fact in this essay it will be argued that modern orthodox (neoclassical) economic theory *in general* does not pass the smell test at all.

The recent economic crisis and the fact that orthodox economic theory has had next to nothing to contribute in understanding it, shows that neoclassical economics - in Lakatosian terms - is a degenerative research program in dire need of replacement.

1. Introduction

Tradition has it that theories are carriers of knowledge about real world target systems and that models are of little consequence in this regard. It is no longer so. Especially not in economics (in this essay "economics" should be read as "orthodox, mainstream, neoclassical economics") where "the model is the message" has been the slogan for at least half a century. Today the models are the carriers of knowledge in the realm of "the queen of social sciences". The distinction formerly made within science theory between theories, as a collection of descriptive existential and relational statements about what is in the world, and models as simplified representations of a particular domain of reality, is definitely blurred in contemporary economics. Both theories and models are (partial) representations of certain properties considered important to emphasis for certain aims. In most contexts within a largely quantifiable science that insists on the exclusive use of methods of mathematical deductivist reasoning – as economics – "theory" and "model" are substitutable.

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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 1981:272].

But economic theory has not been especially successful – not even by its own criteria of delivering explanations and understanding of real world economic systems.

Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not for its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigor is everything and practical relevance is nothing [Blaug 1997:3].

So how can it be this mathematical deductivist project of economic theory prevails?

[P]robably the most compelling reason why the emphasis on mathematical deductive reasoning is retained, despite everything, is that it facilitates a second orientation that is conceptually separate. This is a concern with forecasting or *prediction* … The possibility of successful prediction relies on the occurrence of closed systems, those in which event regularities occur. And these, of course, are also precisely the required conditions for mathematical deductive reasoning to be practically useful, conditions therefore effectively presupposed by the (ubiquitous) reliance upon such methods [Bigo 2008:534].

Friedman (1953:15) claimed - rather oddly - that the descriptive realism of a theory has to be judged by its ability to yield “sufficiently accurate predictions”, but as Sen (2008:627) notices, to check whether a prediction actually occurs, “there surely must be some idea of descriptive accuracy … and this has to come before the concept of predictive accuracy can be entertained.” Prediction depends on description, not the other way round.

One of the major problems of economics, even today, is to establish an empirical discipline that connects our theories and models to the actual world we live in. In that perspective I think it’s necessary to replace both the theory and methodology of the predominant neoclassical paradigm. Giving up the neoclassical creed doesn’t mean that we’ll have complete theoretical chaos.

The essence of neoclassical economic theory is its exclusive use of a deductivist Euclidean methodology. A methodology – which Arnsperger & Varoufakis [2006:12] calls the neoclassical meta-axioms of “methodological individualism, methodological instrumentalism and methodological equilibration” – that is more or less imposed as *constituting* economics, and, usually, without a smack of argument. Hopefully this essay will manage to convey the need for an articulate feasible alternative – an alternative grounded on a relevant and realist open-systems ontology and a non-axiomatic methodology where social atomism and closures are treated as far from ubiquitous.

At best unhelpful, if not outright harmful, present day economic theory has come to way’s end [cf. Pålsson Syll 2010:145-48]. We need to shunt the train of economics onto a relevant and realist track. This could be done with the help of some under-labouring by critical realism and the methodological ideas presented in the works of the philosophers and economists such as for example Nancy Cartwright, John Maynard Keynes, Tony Lawson, Peter Lipton and Uskali Mäki.

But before dwelling on that theme, allow me to start by offering some comments on economics and the basic conditions for its feasibility from the perspective of methodology and science theory – in order that I can return later to the future of economics.

I'll argue from a realist perspective for a science directed towards finding deep structural explanations and shed light on why standard economic analysis, founded on unrealistic and reductionist premises, is frequently found to have a rather limited applicability.

There is a tendency in mainstream economics to generalize its findings, as though the theoretical *model* applies to all societies at all times. I would argue that a critical realist perspective can work as a healthy antidote to over-generalized and a-historical economics.

One of the most important tasks of social sciences is to explain the events, processes, and structures that take place and act in society. In a time when scientific relativism (social constructivism, postmodernism, de-constructivism etc) is expanding, it's important to guard against reducing science to a pure discursive level [cf Pålsson Syll 2005]. We have to maintain the Enlightenment tradition of thinking of reality as principally independent of our views of it and of the main task of science as studying the structure of this reality. Perhaps the most important contribution a researcher can make is to reveal what this reality actually looks like. This is after all the object of science.

Science is made possible by the fact that there are structures that are durable and independent of our knowledge or beliefs about them. There exists a reality beyond our theories and concepts of it. It is this independent reality that is in some senses dealt with by our theories. Contrary to positivism, I cannot see that the main task of science is to detect event-regularities between observed facts. Rather, the task must be conceived as identifying the underlying structure and forces that produce the observed events.

The problem with positivist social science is not that it gives the wrong answers, but rather that it does not, in a strict sense, give any answers at all. Its explanatory models presuppose that the social reality is "closed". Since social reality is fundamentally "open," models of that kind cannot explain what happens in such a universe.

In face of the kind of methodological individualism and rational choice theory that dominate positivist social science we have to admit that even if knowledge of the aspirations and intentions of individuals could be considered to be *necessary* prerequisites for providing explanations of social events, this knowledge is far from *sufficient*. Even the most elementary "rational" actions presuppose the existence of social forms that are irreducible to the intentions of individuals.

The overarching flaw with methodological individualism and rational choice theory, in their different guises, is basically that they reduce social explanations to purportedly individual characteristics. However, many of the characteristics and actions of the individual originate in and are only made possible through society and its relations. Even though society is not an individual following his own volition, and the individual is not an entity given outside of society, the actor and the structure have to be kept analytically distinct. They're tied together through the individual's reproduction and transformation of already given social structures.

It is here that I think that some social theorists falter. In economics, the economy is treated as a sphere that can be analyzed as if it were outside the community.

What makes knowledge in social sciences possible is the fact that society consists of social structures and positions that influence the individuals, partly since they create the necessary prerequisites for the actions of individuals, but also because they predispose individuals to act in a certain way.

Even if we have to acknowledge that the world is mind-independent, this doesn't in any way reduce the epistemological fact that we can only know what the world is like from within our languages, theories, or discourses. But that the world is epistemologically *mediated* by theories does not mean that it is the *product* of them.

Our observations and theories are concept-*dependent* without therefore necessarily being concept-*determined*. There is a reality that exists independently of our knowledge and theories. Although we cannot comprehend it without using our concepts and theories, these are not the same as reality itself.

Social science is relational. It studies and uncovers the social structures in which individuals participate and position themselves. It is these relations that have sufficient continuity, autonomy, and causal power to endure in society and provide the real object of knowledge in social science. It is also only in their capacity as social relations and positions that individuals can be given power or resources - or the lack of them. To be a capital-owner or a slave is not an individual property, but can only come about when individuals are integral parts of certain social structures and positions. Just as a check presupposes a banking system and tribe-members presuppose a tribe - social relations and contexts cannot be reduced to individual phenomena.

2. What should we demand of economic models?

Most models in science are representations of something else. Models "stand for" or "depict" specific parts of a "target system" (usually the real world). A model that has neither surface nor deep resemblance to important characteristics of real economies ought to be treated with *prima facie* suspicion. How could we possibly learn about the real world if there are no parts or aspects of the model that have relevant and important counterparts in the real world target system? The burden of proof lays on the theoretical economists thinking they have contributed anything of scientific relevance without even hinting at any bridge enabling us to traverse from model to reality. All theories and models have to use sign vehicles to convey some kind of content that may be used for saying something of the target system. But purpose-built assumptions, like invariance, made solely to secure a way of reaching deductively validated results in mathematical models, are of little value if they cannot be validated outside of the model.

All empirical sciences use simplifying or unrealistic assumptions in their modeling activities. That is (no longer) the issue. Theories are difficult to directly confront with reality. Economists therefore build models of their theories. Those models are *representations* that are *directly* examined and manipulated to *indirectly* say something about the target systems.

The problem is however that the assumptions made in economic theories and models simply are unrealistic in the wrong way and for the wrong reasons.

There are economic methodologists and philosophers that argue for a less demanding view on modeling and theorizing in economics. And to some theoretical economists, as for example Robert Sugden, it is deemed quite enough to consider economics as a mere "conceptual activity" where "the model is not so much an abstraction from reality as a *parallel reality*" [2002:131]. By considering models as such *constructions*, Sugden distances the model from the intended target, for although "the model world is *simpler* than the real world, the one is not a *simplification* of the other" [2002:131]. The models only have to be *credible*, thereby enabling the economist to make inductive inferences to the target systems.

But what gives license to this leap of faith, this "inductive inference"? Within-model inferences in formal-axiomatic models are usually deductive but that does not come with a warrant of reliability for inferring conclusions about specific target systems. Since all models in a strict sense are false (necessarily building in part on false assumptions) deductive validity cannot guarantee epistemic truth about the target system (cf. [Mäki 2008] on the relation between "truth bearer" in the model and "truth maker" in the real world target system). To argue otherwise would surely be an untenable overestimation of the epistemic reach of "surrogate models".

Being able to model a credible world, a world that somehow could be considered real or *similar* to the real world, is not the same as investigating the real world. Even though all theories are false, since they simplify, they may still possibly serve our pursuit of truth. But then they cannot be unrealistic or false in *any* way. The falsehood or unrealisticness has to be qualified (in terms of resemblance, relevance etc). At the very least, the minimalist demand on models in terms of credibility has to give away to a stronger epistemic demand of "*appropriate similarity and plausibility*" [Pålsson Syll 2001:60]. One could of course also ask for a *sensitivity* or *robustness* analysis. But although Kuorikoski/Lehtinen [2009:130] considers "derivational robustness … a way of seeing whether we can derive credible results from a set of incredible worlds", the credible world, even after having tested it for sensitivity and robustness, can still be a far way from reality – and unfortunately often in ways we know are important.

Robustness of claims in a model does not *per se* give a warrant for exporting the claims to real world target systems. The same can be seen in experimental economics and the problem of what Smith [1982:936] calls *parallelism*. Experimental economists attempt to get control over a large variety of variables, and to that aim they have to specify the experimental situation in a specific and narrow ways. The more the experimentalist achieves control over the variables, the less the results they discover are applicable to the real world target systems. One would of course think it most likely that parallelism would hold for e. g. auctions, where we have a naturally demi-closed system in relative isolation and with a transparent and simple internal logic. As Alexandrova [2008:401] however shows, economic theory is unable to account even for this case, which the economists themselves consider to be a paradigm example of model application, the main reason being that "many more factors turned out to be relevant than was thought at first."

And even if "the economic method is very model oriented" and "the ideal of economic theory is to explain as much as possible with a as little as possible" [Torsvik 2006: 60], the simple fact of being in the laboratory or the economic theoretician's model does not necessarily cross any application domains. This (perhaps) sad conclusion reminds of Cartwright's [1999:37] view that if scientific laws "apply only in very special circumstances, then perhaps they are true just where we see them operating so successfully – in the artificial environment of our laboratories, our high-tech firms, or our hospitals."

Anyway, robust theorems are exceedingly rare or non-existent in economics. Explanation, understanding and prediction of real world phenomena, relations and mechanisms therefore cannot be grounded (solely) on robustness analysis. And as Cartwright [1989] forcefully has argued, some of the standard assumptions made in neoclassical economic theory - on rationality, information-handling and types of uncertainty – are not possible to make more realistic by "de-idealization" or "successive approximations" without altering the theory and its models fundamentally.

If we cannot show that the mechanisms or causes we isolate and handle in our models are stable – in the sense that what when we export them from are models to our target systems they do not change – then they only hold under *ceteris paribus* conditions and *a fortiori* are of limited value for our understanding, explanation and prediction of our real world target system. As Keynes [1973(1921):276-468] writes:

> The kind of fundamental assumption about the character of material laws, on which scientists appear commonly to act, seems to me to be [that] the system of the material universe must consist of bodies … such that each of them exercises its own separate, independent, and invariable effect, a change of the total state being compounded of a number of separate changes each of which is solely due to a separate portion of the preceding state … Yet there might well be quite different laws for wholes of different degrees of complexity, and laws of connection between complexes which could not be stated in terms of laws connecting individual parts … If different wholes were subject to different laws *qua* wholes and not simply on account of and in proportion to the differences of their parts, knowledge of a part could not lead, it would seem, even to presumptive or probable knowledge as to its association with other parts … These considerations do not show us a way by which we can justify induction … /427 No one supposes that a good induction can be arrived at merely by counting cases. The business of strengthening the argument chiefly consists in determining whether the alleged association is *stable*, when accompanying conditions are varied … /468 In my judgment, the practical usefulness of those modes of inference … on which the boasted knowledge of modern science depends, can only exist … if the universe of phenomena does in fact present those peculiar characteristics of atomism and limited variety which appears more and more clearly as the ultimate result to which material science is tending.

Haavelmo [1944:28] basically says the same when discussing the stability preconditions for successful application of econometric methods in terms of autonomy:

> If we should make a series of speed tests with an automobile, driving on a flat, dry road, we might be able to establish a very accurate functional relationship between the pressure on the gas throttle … and the corresponding maximum speed of the car … But if a man did not know anything about automobiles, and he wanted to understand how they work, we should not advise him to spend time and effort in measuring a relationship like that. Why? Because (1) such a relation leaves the whole inner mechanism of a car in complete mystery, and (2) such a relation might break down at any time, as soon as there is some disorder or change in any working part of the car … We say that such a relation has very little *autonomy*, because its existence depends upon the simultaneous fulfillment of a great many other relations, some of which are of a transitory nature.

If the world around us is heterogeneous and organic, mechanisms and causes do not follow the general law of composition. The analogy of vector addition in mechanics simply breaks down in typical economics cases. The postulated stability just is not there since there are "interactive effects" between causes.

Uskali Mäki has repeatedly over the years argued for the necessity of "isolating by idealization" by which the theoretical economist can close the system (model) and "control for noise so as to isolate some important fact, dependency relation, causal factor or mechanism" [2009:31]. Sugden's "surrogate systems" view downplays the role of "sealing off" by *isolation* and rather emphasizes the *construction* part of modeling. The obvious ontological shortcoming of this epistemic approach is that "similarity" or "resemblance" *tout court* do not guarantee that the correspondence between model and target is interesting, relevant, revealing or somehow adequate in terms of mechanisms, causal powers, capacities or tendencies. No matter how many convoluted refinements of general equilibrium concepts made in the model, if the model is not similar in the appropriate respects (such as structure, isomorphism etc), the surrogate system becomes a *substitute* system that does not bridge to the world but rather misses its target.

To give up the quest for truth and to merely study the internal logic of credible worlds is not compatible with scientific realism. To argue – as Kuorikoski/Lehtinen [2009:126] – that modeling can be conceived as "extended cognition" that may "legitimately change our beliefs about the world" may possibly be true, but is too modest a goal for science to go for. It is not even enough demanding inference from models to conclusions about the real world. One has to – as Mäki [2009:41] argues – "infer to conclusions about the world that are true or are likely to be true about the world … Justified model-to-world inference requires the model to be a credible surrogate system in being conceivable and perhaps plausible insofar as what it isolates – the mechanism – is concerned."

Modeling may – as argued by [Weisberg 2007:209] - be conceived of as a three stage enterprise. "In the first stage, a theorist constructs a model. In the second, she analyzes, refines, and further articulates the properties and dynamics of the model. Finally, in the third stage, she assesses the relationship between the model and the world if such an assessment is appropriate."

There are however philosophers and theoretical economists, like Gibbard and Varian [1978], who may be considered *outré* constructivist modelers, skipping the third stage and giving up all pretence of their *caricature* models and theories – built on a "deliberate distortion of reality" [671] and for which there is "no standard independent of the accuracy of the conclusions of the applied model for when its assumptions are sufficiently realistic" [671] - representing any *real* target systems. But if so, why should we invest time in studying purely hypothetical imaginary entities? If our theorizing does not consist in "forming explicit hypotheses about situations and testing them," how could it be that the economist "thinks the model will help to explain something about the world" [676]? What is it that caricature models can establish? As noted by, e.g., Rosenberg [1978:683], it is hard to come up with justifiable reasons to treat *fictionalism* a feasible modeling strategy in social science.

Weisberg [2007:224] says that even though "no assessment of the model-world relationship" is made, the insights gained from the analysis "may be useful in understanding real phenomena." That may be, but is – if viewed as an acceptable aspiration-level for scientific activity – too undemanding. And assessing the adequacy of a theory or model *solely* in terms of "the interests of the theorist" [Weisberg 2007:225] or "on purely aesthetic grounds" [Varian 1998: 241] does not seem to be a warranted scientific position. That would be lowering one's standards of fidelity beyond reasonable limits. Theories and models must be justified on *more* grounds than their intended scope or the fact that "most economic theorists admit that they do economics because it is fun" [Varian 1998:241]. Scientific theories and models must have ontological constraints and the most non-negotiable of these is – at least from a realist point of view – that they have to be coherent to the way the worlds is.

Even though we might say that models are devised "to account for stylized facts or data" [Knuutila 2009:75] and that "if conditions of the real world approximate sufficiently well the assumptions … the derivations from these assumptions will be approximately correct [Simon 1963:230] – as Lawson [1997:208] aptly puts it, "a supposed 'stylized fact' is intended to express a partial regularity reformulated as a strict one, in the form of a law." I cannot but concur. Models as "stylized facts" or "stylized pictures" somehow "approximating" reality are rather unimpressive attempts at legitimizing using fictitious idealizations for reasons more to do with model tractability than with a genuine interest of understanding and explaining features of real economies. Many of the model-assumptions standard made by neoclassical economics are *restrictive* rather than *harmless* and could *a fortiori* anyway not in any sensible meaning be considered approximations at all.

Knuuttila [2009:86] notices that most economic models fall short of representing real systems. I agree. Neoclassical economic theory employs very few principles, and among those used, bridge principals are as a rule missing. But instead of criticizing this (as I would) she rather apologetically concludes that “the connections between the models and the data, or what is known about economies more generally, are just looser than what is traditionally assumed” [2009:76]. To my ears this sounds like trying to turn failure into virtue. Why should we be concerned with economic models that are “purely hypothetical constructions” [2009:76]? Even if the constructionist approach should be able to accommodate the way we learn from models, it is of little avail to treat models as some kind “artifacts” or “heuristic devices” that produce claims, if they do not also connect to real world target systems.

Constructing “minimal economic models” may – even though they are without “world-linking conditions” [Grüne-Yanoff 2009:81] – affect our confidence in conjectures about the real world. And being able to explain relations between imaginary entities in “analogue” or “fictitious” models may increase our confidence in “inferential links to other bodies of knowledge” [Knuuttila 2009:77]. But this does not justify the conclusion that “correctly judging models to be credible does neither imply that they are true, nor that they resemble the world in certain ways, nor that they adhere to relevant natural laws” [Grüne-Yanoff 2009:95]. The final court of appeal for economic models is the real world, and as long as no convincing justification is put forward for how the confidence-enhancing takes place or the inferential bridging *de facto* is made, credible counterfactual worlds is little more than “hand waving” that give us rather little warrant for making inductive inferences from models to real world target systems. Inspection of the models shows that they have features that strongly influence the results obtained in them and that will not be shared by the real world target systems. Economics becomes exact but exceedingly narrow since “the very special assumptions do not fit very much of the contemporary economy around us” [Cartwright 1999:149]. Or as Krugman [2000:41] noted on an elaboration of the Mundell-Fleming macro model: “it is driven to an important extent by the details of the model, and can quite easily be undone. The result offers a tremendous clarification of the issues; it’s not at all clear that it offers a comparable insight into what really happens.”

If substantive questions about the real world are being posed, it is the formalistic-mathematical representations utilized to analyze them that have to match reality, not the other way around. “Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because, unlike the natural science, the material to which it is applied is, in too many respects, not homogeneous through time” [Keynes 1971-89 vol XIV: 296].

Taking lessons from models to the real world is demanding. To think that we are “invited to infer the likelihood of similar causes” [Sugden 2009:10] from the similarity of effects is overly optimistic. Abduction is not just inference to a *possible* explanation. To cut ice it has to be an inference to the *best* explanation. “Of course, there is always more than one possible explanation for any phenomenon … so we cannot infer something simply because it is a possible explanation. It must somehow be the best of competing explanations” [Lipton 2004:56].

Sugden’s rather – at least among present-day economists – typical view is far from sufficing. Economists also have to ask questions of how the models and theories contribute to explaining and understanding the real world target system.

The theories and models that economists construct describe imaginary worlds using a combination of formal sign systems such as mathematics and ordinary language. The descriptions made are extremely thin and to a large degree disconnected to the specific contexts of the targeted system than one (usually) wants to (partially) represent. This is not by chance. These closed formalistic-mathematical theories and models are constructed for the purpose of being able to deliver purportedly rigorous deductions that may somehow by be exportable to the target system. By analyzing a few causal factors in their "laboratories" they hope they can perform "thought experiments" and observe how these factors operate on their own and without impediments or confounders.

Unfortunately, this is not so. The reason for this is that economic causes never act in a socio-economic vacuum. Causes have to be set in a contextual structure to be able to operate. This structure has to take some form or other, but instead of incorporating structures that are true to the target system, the settings made in economic models are rather based on formalistic mathematical tractability. In the models they appear as unrealistic assumptions, usually playing a decisive role in getting the deductive machinery deliver "precise" and "rigorous" results. As noted by Frank Hahn [1994:246] – one of the icons of neoclassical mathematical economics – "the assumptions are there to enable certain results to emerge and not because they are to be taken descriptively." This, of course, makes exporting to real world target systems problematic, since these models – as part of a deductivist covering-law tradition in economics – are thought to deliver general and far-reaching conclusions that are externally valid. But how can we be sure the lessons learned in these theories and models have external validity, when based on highly specific unrealistic assumptions? As a rule, the more specific and concrete the structures, the less generalizable the results. Admitting that we *in principle* can move from (partial) falsehoods in theories and models to truth in real world target systems does not take us very far, unless a thorough explication of the relation between theory, model and the real world target system is made. If models *assume* representative actors, rational expectations, market clearing and equilibrium, and we *know* that real people and markets cannot be expected to obey these assumptions, the warrants for supposing that conclusions or hypothesis of causally relevant mechanisms or regularities can be bridged, are obviously non-justifiable. To have a deductive warrant for things happening in a closed model is no guarantee for them being preserved when applied to an open real world target system.

Economic theorists ought to do some ontological reflection and heed Keynes' [1936: 297] warnings on using laboratory thought-models in economics:

> The object of our analysis is, not to provide a machine, or method of blind manipulation, which will furnish an infallible answer, but to provide ourselves with an organized and orderly method of thinking out particular problems; and, after we have reached a provisional conclusion by isolating the complicating factors one by one, we then have to go back on ourselves and allow, as well as we can, for the probable interactions of the factors amongst themselves. This is the nature of economic thinking. Any other way of applying our formal principles of thought (without which, however, we shall be lost in the wood) will lead us into error.

3. Paradigmatic examples

To get a more particularized and precise picture of what neoclassical economic theory is today, it is indispensable to complement the perhaps rather "top-down" approach hitherto used with a more "bottom-up" approach. To that end I will below present – with emphasis on the chosen model-building strategy - three paradigmatic examples to exemplify and diagnose neoclassical economic theory as practiced nowadays.

I. Lucas understanding of business cycles

Economic theory is nowadays, as we have seen, in the story-telling business whereby economic theorists create make-believe analogue models of the target system – usually conceived as the real economic system. This modeling activity is considered useful and essential. Since fully-fledged experiments on a societal scale as a rule are prohibitively expensive, ethically indefensible or unmanageable, economic theorists have to substitute experimenting with something else. To understand and explain relations between different entities in the real economy the predominant strategy is to build models and make things happen in these "analogue-economy models" rather than engineering things happening in real economies.

In business cycles theory these models are constructed with the purpose of showing that changes in the supply of money "have the capacity to induce depressions or booms" [1988:3] not just in these models, but also in real economies. To do so economists are supposed to imagine subjecting their models to some kind of "operational experiment" and "a variety of reactions". "In general, I believe that one who claims to understand the principles of flight can reasonably be expected to be able to make a flying machine, and that understanding business cycles means the ability to make them too, in roughly the same sense" [1981:8]. To Lucas models are the *laboratories* of economic theories, and after having made a simulacrum-depression Lucas hopes we find it "convincing on its own terms – that what I said would happen in the [model] as a result of my manipulation would in fact happen" [1988:4]. The clarity with which the effects are seen is considered "the key advantage of operating in simplified, fictional worlds" [1988:5].

On the flipside lies the fact that "we are not really interested in understanding and preventing depressions in hypothetical [models]. We are interested in our own vastly more complicated society" [1988:5]. But how do we bridge the gulf between model and "target system"? According to Lucas we have to be willing to "argue by analogy from what we know about one situation to what we would like to know about another, quite different situation" [1988:5]. Progress lies in the pursuit of the ambition to "tell better and better stories" [1988:5], simply because that is what economists do.

We are storytellers, operating much of the time in worlds of make believe. We do not find that the realm of imagination and ideas is an alternative to, or retreat from, practical reality. On the contrary, it is the only way we have found to think seriously about reality. In a way, there is nothing more to this method than maintaining the conviction … that imagination and ideas matter … there is no practical alternative" [1988:6].

Lucas has applied this mode of theorizing by constructing "make-believe economic systems" to the age-old question of what causes and constitutes business cycles. According to Lucas the standard for what that means is that one "exhibits understanding of business cycles by constructing a *model* in the most literal sense: a fully articulated artificial economy, which behaves through time so as to imitate closely the time series behavior of actual economies" [1981:219].

To Lucas, business cycles are an inherently systemic phenomenon basically characterized by conditional co-variations of different time series. The vision is "the possibility of a unified explanation of business cycles, grounded in the general laws governing market economies, rather than in political or institutional characteristics specific to particular countries or periods" [1981:218]. To be able to sustain this view and adopt his "equilibrium approach" he has to define the object of study in a very constrained way [cf. Vercelli 1991:11-23]. Lucas asserts, e.g., that if one wants to get numerical answers "one needs an explicit, equilibrium account of the business cycles" [1981:222]. But his arguments for why it necessarily has to be an *equilibrium* is not very convincing, but rather confirms Hausman's view [2001:320] that faced with the problem of explaining adjustments to changes, economists "have become complacent about this inadequacy – they have become willing prisoners of the limitations of their theories." The main restriction is that Lucas only deals with purportedly invariable regularities "common to all decentralized market economies" [1981:218]. Adopting this definition he can treat business cycles as all alike "with respect to the qualitative behavior of the co-movements among series" [1981:218]. As noted by Hoover [1988:187]:

Lucas's point is not that all estimated macroeconomic relations are necessarily not invariant. It is rather that, in order to obtain an invariant relation, one must derive the functional form to be estimated from the underlying choices of individual agents. Lucas supposes that this means that one must derive aggregate relations from individual optimization problems taking only tastes and technology as given.

Postulating invariance paves the way for treating various economic entities as stationary stochastic processes (a standard assumption in most modern probabilistic econometric approaches) and the possible application of "economic equilibrium theory." The result is that Lucas business cycle is a rather watered-down version of what is usually connoted when speaking of business cycles.

Based on the postulates of "self-interest" and "market clearing" Lucas has repeatedly stated that a pure equilibrium method is a necessary intelligibility condition and that disequilibria are somehow "arbitrary" and "unintelligible" [1981:225]. Although this might (arguably) be requirements put on models, these requirements are irrelevant and totally without justification vis-à-vis the real world target system. Why should involuntary unemployment, for example, be considered an unintelligible disequilibrium concept? Given the lack of success of these models when empirically applied (cf. Ball [1999], Estrella & Fuhrer [2002] and Seidman [2005]), what is unintelligible, is rather to pursue in this reinterpretation of the ups and downs in business cycles and labour markets as equilibria. To Keynes involuntary unemployment is not equitable to actors on the labour market becoming irrational non-optimizers. It is basically a reduction in the range of working-options open to workers, regardless of any volitional optimality choices made on their part. Involuntary unemployment is excess supply of labour. That unemployed in Lucas business cycles models only can be conceived of as having chosen leisure over work is not a substantive argument about real world unemployment.

The point at issue [is] whether the concept of involuntary unemployment actually delineates circumstances of economic importance … If the worker's reservation wage is higher than all offer wages, then he is unemployed. This is his preference given his options. For the new classicals, the unemployed have placed and lost a bet. It is sad perhaps, but optimal [Hoover 1988:59].

Sometimes workers are not employed. That is a real phenomenon and not a "theoretical construct ... the task of modern theoretical economics to 'explain'" [Lucas 1981:243].

All economic theories have to somehow deal with the daunting question of uncertainty and risk. It is "absolutely crucial for understanding business cycles" [1981:223]. To be able to practice economics at all, "we need some way … of understanding which decision problem agents are solving" [1981:223]. Lucas – in search of a "technical model-building principle" [1981:1] – adapts the rational expectations view, according to which agents' subjective probabilities are identified "with observed frequencies of the events to be forecast" are coincident with "true" probabilities. This hypothesis: [1981:224]

> will *most* likely be useful in situations in which the probabilities of interest concern a fairly well defined recurrent event, situations of 'risk' [where] behavior may be explainable in terms of economic theory … In cases of uncertainty, economic reasoning will be of no value … Insofar as business cycles can be viewed as repeated instances of essentially similar events, it will be reasonable to treat agents as reacting to cyclical changes as 'risk', or to assume their expectations are *rational*, that they have fairly stable arrangements for collecting and processing information, and that they utilize this information in forecasting the future in a stable way, free of systemic and easily correctable biases.

To me this seems much like putting the cart before the horse. Instead of adapting the model to the object – which from both ontological and epistemological considerations seem the natural thing to do – Lucas proceeds in the opposite way and chooses to define his object and construct a model solely to suit own methodological and theoretical preferences. All those – interesting and important - features of business cycles that have anything to do with model-theoretical openness, and *a fortiori* not possible to squeeze into the closure of the model, are excluded. One might rightly ask what is left of that we in a common sense meaning refer to as business cycles. Einstein's dictum – "everything should be made as simple as possible but not simpler" falls to mind. Lucas – and neoclassical economics at large – does not heed the implied apt warning.

The development of macro-econometrics has according to Lucas supplied economists with "detailed, quantitatively accurate replicas of the actual economy" thereby enabling us to treat policy recommendations "as though they had been experimentally tested" [1981:220]. But if the goal of theory is to be able to make accurate forecasts this "ability of a model to imitate actual behavior" does not give much leverage. What is required is "invariance of the structure of the model under policy variations". Parametric invariance in an economic model cannot be taken for granted, "but it seems reasonable to hope that neither tastes nor technology vary systematically" [1981:220].

The model should enable us to posit contrafactual questions about what would happen if some variable was to change in a specific way. Hence the assumption of structural invariance, that purportedly enables the theoretical economist to do just that. But does it? Lucas appeals to "reasonable hope", a rather weak justification for a modeler to apply such a far-reaching assumption. To warrant it one would expect an argumentation that this assumption – whether we conceive of it as part of a strategy of "isolation", "idealization" or "successive approximation" – really establishes a useful relation that we can export or bridge to the target system, the "actual economy." That argumentation is neither in Lucas, nor – to my knowledge – in the succeeding neoclassical refinements of his "necessarily artificial, abstract, patently 'unreal'" analogue economies [1981:271]. At most we get what Lucas himself calls "inappropriately maligned" casual empiricism in the form of "the method of keeping one's eyes open." That is far from sufficient to warrant any credibility in a model pretending to explain the complex and difficult recurrent phenomena we call business cycles. To provide an empirical "illustration" or a "story" to back up your model do not suffice. There are simply too many competing illustrations and stories that could be exhibited or told.

As Lucas has to admit – complaining about the less than ideal contact between theoretical economics and econometrics – even though the "stories" are (purportedly) getting better and better, “the necessary interaction between theory and fact tends not to take place” [1981:11].

The basic assumption of this “precise and rigorous” model therefore cannot be considered anything else than an unsubstantiated conjecture as long as it is not supported by evidence from outside the theory or model. To my knowledge no in any way decisive empirical evidence have been presented. This is the more tantalizing since Lucas himself stresses that the presumption “seems a sound one to me, but it must be defended on empirical, not logical grounds” [1981:12].

And applying a “Lucas critique” on Lucas own model, it is obvious that it too fails. Changing “policy rules” cannot just be presumed not to influence investment and consumption behavior and *a fortiori* technology, thereby contradicting the invariance assumption. Technology and tastes cannot live up to the status of an economy’s deep and structurally stable Holy Grail. They too are part and parcel of an ever-changing and open economy. Lucas hope of being able to model the economy as “a FORTRAN program” and “gain some confidence that the component parts of the program are in some sense reliable prior to running it” [1981:288] therefore seems – from an ontological point of view – totally misdirected. The failure in the attempt to anchor the analysis in the alleged stable deep parameters “tastes” and “technology” shows that if you neglect ontological considerations pertaining to the target system, ultimately reality kicks back when at last questions of bridging and exportation of model exercises are laid on the table. No matter how precise and rigorous the analysis is, and no matter how hard one tries to cast the argument in “modern mathematical form” [1981:7] they do not push science forwards one millimeter if they do not stand the acid test of relevance to the target. No matter how clear, precise, rigorous or certain the inferences delivered inside these models are, they do not *per se* say anything about external validity.

Formalistic deductive “Glasperlenspiel” can be very impressive and seductive. But in the realm of science it ought to be considered of little or no value to simply make claims about the model and lose sight of the other part of the model-target dyad.

II. Representative-agent models

Without export certificates models and theories should be considered unsold. Unfortunately this understanding has not informed modern economics, as can be seen by the profuse use of so called representative-agent models.

A common feature of economics is to use simple general equilibrium models where representative actors are supposed to have complete knowledge, zero transaction costs and complete markets.

In these models, the actors are all identical. For someone holding the view that “economics is based on a superficial view of individual and social behavior” and thinks “it is exactly this superficiality that gives economics much of the power that it has: its ability to predict human behavior without knowing very much about the makeup and lives of the people whose behavior we are trying to understand ” [Lucas1986:241], it is natural to consider it “helpful” to elaborate his theory with the help of a “representative agent” and build an “abstract model economy” with “N identical individuals” [1981:68] operating in “two markets” that are “structurally identical” and have “no communication between them” [1981:72] within each trading period.

This has far-reaching analytical implications. Situations characterized by asymmetrical information – situations most of us consider to be innumerable – cannot arise in such models. If the aim is to build a macro-analysis from micro-foundations in this manner, the relevance of the procedure is highly questionable. Solow (2010:2) - in the congressional hearing referred to in the prologue – even considers the claims made by protagonists of rational agent models "generally phony".

One obvious critique is that representative-agent models do not incorporate distributional effects - effects that often play a decisive role in macroeconomic contexts. Investigations into the operations of markets and institutions usually find that there are overwhelming problems of coordination. These are difficult, not to say impossible, to analyze with the kind of Robinson Crusoe models that, e. g., real business cycle theorists employ and which exclude precisely those differences between groups of actors that are the driving force in many non-neoclassical analysis.

The choices of different individuals have to be shown to be coordinated and consistent. This is obviously difficult if the economic models don't give room for heterogeneous individuals (this lack of understanding the importance of heterogeneity is perhaps especially problematic for the modeling of real business cycles in dynamic stochastic general equilibrium models, cf. [Hansen & Heckman 1996]). Representative-agent models are certainly more manageable, however, from a realist point of view, they are also less relevant and have a lower explanatory potential.

Both the "Lucas critique" and Keynes' critique of econometrics [cf. Pålsson Syll 2007b:20-25] argued that it was inadmissible to project history on the future. Consequently an economic policy cannot presuppose that what has worked before, will continue to do so in the future. That macroeconom(etr)ic models could get hold of correlations between different "variables" was not enough. If they could not get at the causal structure that generated the data, they were not really "identified". Lucas himself drew the conclusion that the problem with unstable relations was to construct models with clear microfoundations where forward-looking optimizing individuals and robust, deep, behavioural parameters are seen to be stable even to changes in economic policies.

To found macroeconomics on the actions of separate individuals, is an example of methodological reductionism, implying that macro-phenomena can be uniquely inferred from micro-phenomena. Among science-theoreticians this is a contested standpoint. Even though macro-phenomena somehow *presuppose* micro-phenomena, it is far from certain that they can be *reduced to* or *deduced from* them.

In microeconomics we know that aggregation really presupposes homothetic an identical preferences, something that almost never exist in real economies. The results given by these assumptions are therefore not robust and do not capture the underlying mechanisms at work in any real economy. And as if this was not enough, there are obvious problems also with the kind of microeconomic equilibrium that one tries to reduce macroeconomics to. Decisions of consumption and production are described as choices made by a single agent. But then, who sets the prices on the market? And how do we justify the assumption of universal consistency between the choices?

Kevin Hoover [2008:27-28] has argued that the representative-agent models also introduce an improper idealization:

> The representative agent is held to follow the rule of perfect competition, price-taking, which is justified on the idealizing assumptions that n => ∞; yet the representative agent is itself an idealization in which n => 1. The representative agent is –

> inconsistently – simultaneously the whole market and small relative to market. The problem can be summed by the question: with whom does the representative agent trade?

Models that are critically based on particular and odd assumptions – and are neither robust nor congruent to real world economies – are of questionable value.

And is it really possible to describe and analyze all the deliberations and choices made by individuals in an economy? Does not the choice of an individual presuppose knowledge and expectations about choices of other individuals? It probably does, and this presumably helps to explain why representative-agent models have become so popular in modern macroeconomic theory. They help to make the analysis more tractable.

One could justifiably argue that one might just as well accept that it is not possible to coherently reduce macro to micro, and accordingly that it is perhaps necessary to forswear microfoundations and the use of rational-agent models all together. Microeconomic reasoning has to build on macroeconomic presuppositions. Real individuals do not base their choices on operational general equilibrium models, but rather use simpler models. If macroeconomics needs *micro*foundations it is equally necessary that microeconomics needs *macro*foundations.

The philosopher John Searle [1995] has asserted that their might exist something he calls “collective intentionality”. Given the existence of the latter, one might be able to explain to economists the enigmatic behaviour of for example people who vote in political elections. The aggregate outcome is decided by the collective intentions of the citizens, such as “it is your duty as citizen to vote“. To deduce this outcome from a representative actor’s behaviour without taking account of such intentions or institutions, is simply not possible.

The microeconomist Alan Kirman [1992] has maintained that the use of representative-agent models is unwarranted and leads to conclusions that are usually both misleading and false. It’s a fiction basically used by some macroeconomists to justify the use of equilibrium analysis and a kind of pseudo-microfoundations. Microeconomists are well aware that the conditions necessary to make aggregation to representative actors possible, are not met in actual economies. As economic models become increasingly complex, their use also becomes less credible.

Even if economies naturally presuppose individuals, it does not follow that we can *infer* or *explain* macroeconomic phenomena solely from knowledge of these individuals. Macroeconomics is to a large extent *emergent* and cannot be reduced to a simple summation of micro-phenomena. Moreover, even these microfoundations aren’t immutable. Lucas and the new classical economists’ deep parameters – “tastes” and “technology” – are not really the bedrock of constancy that they believe (pretend) them to be.

Now I do not think there is an unbridgeable gulf between micro and macro. We just have to accept that micro-macro relations are so complex and manifold, that the former cannot somehow be *derived* from the latter.

For Marshall [1951:171] economic theory was “an engine for the discovery of concrete truth”. But where Marshall tried to describe the behaviour of a typical business with the concept “representative firm”, his modern heirs don’t at all try to describe how firms interplay with other firms in an economy. The economy is rather described “as if” consisting of one single giant firm - either by inflating the optimization problem of the individual to the scale of a whole economy, or by assuming that it’s possible to aggregate different individuals’ actions by a simple summation, since every type of actor is identical. But do not we just have to face that it is difficult to describe interaction and cooperation when there is essentially only *one* actor?

To economists for whom macroeconomic analysis is largely geared to trying to understand macroeconomic externalities and coordination failures, representative-agent models are particularly ill-suited. In spite of this, these models are frequently used, giving rise to a neglect of the aggregation-problem. This highlights the danger of letting the model, rather than the method, become the message.

III. Econometrics

Economists often hold the view that criticisms of econometrics are the conclusions of sadly misinformed and misguided people who dislike and do not understand much of it. This is really a gross misapprehension. To be careful and cautious is not the same as to dislike. And as any perusal of the mathematical-statistical and philosophical works of people like for example Nancy Cartwright, Chris Chatfield, Kevin Hoover, Hugo Keuzenkamp, John Maynard Keynes, Tony Lawson or Arios Spanos would show, the critique is put forward by respected authorities. I would argue, against "common knowledge", that they do not misunderstand the crucial issues at stake in the development of econometrics. Quite the contrary. They know them all too well - and are not satisfied with the validity and philosophical underpinning of the assumptions made for applying its methods.

Let me try to do justice to the critical arguments on the logic of probabilistic induction and shortly elaborate – mostly from a philosophy of science vantage point - on some insights critical realism gives us on econometrics and its methodological foundations.

The methodological difference between an empiricist and a deductivist approach, on which we have already commented, can also clearly be seen in econometrics. The ordinary deductivist "textbook approach" views the modeling process as foremost an estimation problem, since one (at least implicitly) assumes that the model provided by economic theory is a well-specified and "true" model. The more empiricist, general-to-specific-methodology (often identified as "the LSE approach") on the other hand view models as theoretically and empirically adequate representations (approximations) of a data generating process (DGP). Diagnostics tests (mostly some variant of the F-test) are used to ensure that the models are "true" – or at least "congruent" – representations of the DGP (cf. Chao [2002]). The modeling process is here more seen as a specification problem where poor diagnostics results may indicate a possible misspecification requiring re-specification of the model. The standard objective is to identify models that are structurally stable and valid across a large time-space horizon. The DGP is not seen as something we already know, but rather something we discover in the process of modeling it. Considerable effort is put into testing to what extent the models are structurally stable and generalizable over space and time.

Although I have sympathy for this approach in general, there are still some unsolved "problematics" with its epistemological and ontological presuppositions [cf. Lawson 1989, Keuzenkamp 2000 and Pratten 2005]. There is, e. g., an implicit assumption that the DGP fundamentally has an invariant property and that models that are structurally unstable just have not been able to get hold of that invariance. But, as already Keynes maintained, one cannot just presuppose or take for granted that kind of invariance. It has to be argued and justified. Grounds have to be given for viewing reality as satisfying conditions of model-closure. It is as if the lack of closure that shows up in the form of structurally unstable models somehow could be solved by searching for more autonomous and invariable "atomic uniformity". But if reality is "congruent" to this analytical prerequisite has to be argued for, and not simply taken for granted.

Even granted that closures come in degrees, we should not compromise on ontology. Some methods simply introduce improper closures, closures that make the disjuncture between models and real world target systems inappropriately large. "Garbage in, garbage out."

Underlying the search for these immutable "fundamentals" lays the implicit view of the world as consisting of material entities with their own separate and invariable effects. These entities are thought of as being able to be treated as separate and addible causes, thereby making it possible to infer complex interaction from knowledge of individual constituents with limited independent variety. But, again, if this is a justified analytical procedure cannot be answered without confronting it with the nature of the objects the models are supposed to describe, explain or predict. Keynes himself thought it generally inappropriate to apply the "atomic hypothesis" to such an open and "organic entity" as the real world. As far as I can see these are still appropriate strictures all econometric approaches have to face. Grounds for believing otherwise have to be provided by the econometricians.

Trygve Haavelmo, the "father" of modern probabilistic econometrics, wrote that he and other econometricians could not "build a complete bridge between our models and reality" by logical operations alone, but finally had to make "a non-logical jump" [1943:15]. A part of that jump consisted in that econometricians "like to believe … that the various a priori possible sequences would somehow cluster around some typical time shapes, which if we knew them, could be used for prediction" [1943:16]. But since we do not know the true distribution, one has to look for the mechanisms (processes) that "might rule the data" and that hopefully persist so that predictions may be made. Of possible hypothesis on different time sequences ("samples" in Haavelmo's somewhat idiosyncratic vocabulary)) most had to be ruled out a priori "by economic theory", although "one shall always remain in doubt as to the possibility of some … outside hypothesis being the true one" [1943:18].

To Haavelmo and his modern followers, econometrics is not really in the truth business. The explanations we can give of economic relations and structures based on econometric models are "not hidden truths to be discovered" but rather our own "artificial inventions". Models are consequently perceived not as true representations of DGP, but rather instrumentally conceived "as if"-constructs. Their "intrinsic closure" is realized by searching for parameters showing "a great degree of invariance" or relative autonomy and the "extrinsic closure" by hoping that the "practically decisive" explanatory variables are relatively few, so that one may proceed "as if … natural limitations of the number of relevant factors exist" [Haavelmo 1944:29].

Just like later Lucas, Haavelmo seems to believe that persistence and autonomy can only be found at the level of the individual, since individual agents are seen as the ultimate determinants of the variables in the economic system.

But why the "logically conceivable" really should turn out to be the case is difficult to see. At least if we are not satisfied by sheer hope. As we have already noted Keynes reacted against using unargued for and unjustified assumptions of complex structures in an open system being reducible to those of individuals. In real economies it is unlikely that we find many "autonomous" relations and events. And one could of course, with Keynes and from a critical realist point of view, also raise the objection that to invoke a probabilistic approach to econometrics presupposes, e. g., that we have to be able to describe the world in terms of risk rather than genuine uncertainty.

And that is exactly what Haavelmo [1944:48] does: "To make this a rational problem of statistical inference we have to start out by an axiom, postulating that every set of observable variables has associated with it one particular 'true', but unknown, probability law."

But to use this "trick of our own" and just assign "a certain probability law to a system of observable variables", however, cannot – just as little as hoping – build a firm bridge between model and reality. Treating phenomena *as if* they essentially were stochastic processes is not the same as showing that they essentially *are* stochastic processes. Rigour and elegance in the analysis does not make up for the gap between reality and model. It is the distribution of the phenomena in itself and not its estimation that ought to be at the centre of the stage. A crucial ingredient to any economic theory that wants to use probabilistic models should be a convincing argument for the view that "there can be no harm in considering economic variables as stochastic variables" [Haavelmo 1943:13]. In most cases no such arguments are given.

Hendry acknowledges that there is a difference between the actual DGP and the models we use trying to adequately capture the essentials of that real world DGP. He also criticizes forecasting procedures based on the assumption that the DGP is constant. That kind of closure just is not there in the world as we know it. When "we don't know what we don't know" it is preposterous to build models assuming an ergodic DGP. It's like assuming that there does exist a "correct" model and that this is the actual DGP whose constant parameters we just have to estimate. That is hard to take seriously. If such invariant parameters and concomitant regularities exist, has to be assessed *ex post* and not be assumed as an axiom in model-construction. This has to be an empirical question. The proof of the pudding is in the eating.

Like Haavelmo, Hendry assumes that what we observe are random variables which we can treat *as if* produced in accordance with a complex joint probability distribution. If we are performing a fully-controlled experiment or a Monte Carlo simulation this is of course true, since we control the characteristics of the DGP ourselves. But in the time series we work with in applied econometrics, is that really a tenable position? Can we really come to identify, know and access the DGP outside experiment-like situations? Hendry would insist that even if the answer to these questions is no, constructing useful models and theories of econometrics is still possible. From an instrumentalist point of view you may have good reasons for wanting to design a useful model that bridges the gap between "theory and empirical evidence" [Hendry 1995:359]. You may even persist in the hope that there exist "invariant features of reality" since otherwise "neither theories nor econometric models would be of much practical value" [Hendry 2000:474]. But it's a slippery slope. Hendry and other econometricians sometimes have a tendency to conflate the DGP as a hypothesis and as an actual reality. This placing model on a par with reality is an example of what Marx called reification and is from a methodological and scientific-theoretic point of view an untenable equivocation. But where some methodologists of econometrics, like Hugo Keuzenkamp [2000:154], wants to get rid of the ambiguity by dropping the idea of the DGP as an actual process and treat it solely as an invention of our mind, one could rather argue that we have to drop the idea that we in our models ever can be sure that we have got hold of the Hoy Grail of econometrics – the DGP.

Of course you are entitled – like Haavelmo and his modern probabilistic followers – to express a hope "at a metaphysical level" that there are invariant features of reality to uncover and that also show up at the empirical level of observations as some kind of regularities.

But is it a *justifiable* hope? I have serious doubts. The kind of regularities you may hope to find in society is not to be found in the domain of surface phenomena, but rather at the level of causal mechanisms, powers and capacities. Persistence and generality has to be looked out for at an underlying deep level. Most econometricians don't want to visit that playground. They are content with setting up theoretical models that give us correlations and eventually "mimic" existing causal properties. The focus is on measurable data, and one even goes so far as defining science as "a public approach to the measurement and analysis of observable phenomena" [Hendry 1997:167]. Econometrics is basically made for *modeling* the DGP, and not to account for unobservable aspects of the real world target system (DGP).

We have to accept that reality has no "correct" representation in an economic or econometric model. There is no such thing as a "true" model that can capture an open, complex and contextual system in a set of equations with parameters stable over space and time, and exhibiting invariant regularities. To just "believe", "hope" or "assume" that such a model *possibly* could exist is not enough. It has to be justified in relation to the ontological conditions of social reality. And as Toulmin [2003:34] so neatly puts it:

In order for a suggestion to be a 'possibility' in any context … it must 'have what it takes' in order to be entitled to genuine consideration *in that context*. To say, in any field, 'Such-and-such is a possible answer to our question', is to say that, bearing in mind the nature of the problem concerned, such-and-such an answer deserves to be considered. This much of the meaning of the term 'possible' is field-invariant. The criteria of possibility, on the other hand, are field-dependent, like the criteria of impossibility or goodness. The things we must point to in showing that something is possible will depend entirely on whether we are concerned with a problem in pure mathematics, a problem of team-selection, a problem in aesthetics, or what; and features which make something a possibility from one standpoint will be totally irrelevant from another.

In contrast to those who want to give up on (fallible, transient and transformable) "truth" as a relation between theory and reality and content themselves with "truth" as a relation between a model and a probability distribution, I think it is better to really scrutinize if this latter attitude is feasible. To just say "all models are wrong … some, however, are useful" [Keuzenkamp 2000:116] is to defeatist. That is to confuse social engineering with science. To abandon the quest for truth and replace it with sheer positivism would indeed be a sad fate of econometrics. It is more rewarding to stick to truth as a regulatory ideal and keep on searching for theories and models that in relevant and adequate ways express those parts of reality we want to describe and explain.

Econometrics may be an informative tool for research. But if its practitioners do not investigate and make an effort of providing a justification for the credibility of the assumptions on which they erect their building, it will not fulfill its tasks. There is a gap between its aspirations and its accomplishments, and without more supportive evidence to substantiate its claims, critics will continue to consider its ultimate argument as a mixture of rather unhelpful metaphors and metaphysics. Maintaining that economics is a science in the "true knowledge" business, I remain a skeptic of the pretences and aspirations of econometrics. So far, I cannot really see that it has yielded very much in terms of relevant, interesting economic knowledge.

The marginal return on its ever higher technical sophistication in no way makes up for the lack of serious under-labouring of its deeper philosophical and methodological foundations that already Keynes complained about. The rather one-sided emphasis of usefulness and its concomitant instrumentalist justification cannot hide that neither Haavelmo [cf. 1944:10] nor Hendry [cf. 2000:276] give supportive evidence for their considering it "fruitful to believe" in the possibility of treating unique economic data as the observable results of random drawings from an imaginary sampling of an imaginary population. After having analyzed some of its ontological and epistemological foundations, I cannot but conclude that econometrics on the whole has not delivered "truth". And I doubt if it has ever been the intention of its main protagonists.

Our admiration for technical virtuosity should not blind us to the fact that we have to have a more cautious attitude towards probabilistic inference of causality in economic contexts. Science should help us penetrate to "the true process of causation lying behind current events" and disclose "the causal forces behind the apparent facts" [Keynes 1971-89 vol XVII:427]. We *should* look out for causal relations, but econometrics can never be more than a starting point in that endeavour, since econometric (statistical) explanations are not explanations in terms of mechanisms, powers, capacities or causes [cf Sayer 2000:22]. Firmly stuck in an empiricist tradition, econometrics is only concerned with the *measurable* aspects of reality, But there is always the possibility that there are other variables – of vital importance and although perhaps unobservable and non-additive not necessarily epistemologically inaccessible - that were not considered for the model. Those who *were* can hence never be *guaranteed* to be more than potential causes, and not real causes. As science-philosopher Mario Bunge [1979:53] once stated – "the reduction of causation to regular association … amounts to mistaking causation for one of its tests."

A rigorous application of econometric methods in economics really presupposes that the phenomena of our real world economies are ruled by stable causal relations between variables. Contrary to allegations of both Hoover [2002:156] and Granger [2004:105] I would say that a perusal of the leading econom(etr)ic journals shows that most econometricians still concentrate on fixed parameter models and that parameter-values estimated in specific spatio-temporal contexts are *presupposed* to be exportable to totally different contexts. To warrant this assumption one, however, has to convincingly establish that the targeted acting causes are stable and invariant so that they maintain their parametric status after the bridging. The endemic lack of predictive success of the econometric project indicates that this hope of finding fixed parameters is a hope for which there really is no other ground than hope itself.

This is a more fundamental and radical problem than the celebrated "Lucas critique" have suggested. This is not the question if deep parameters, absent on the macro-level, exist in "tastes" and "technology" on the micro-level. It goes deeper. Real world social systems are not governed by stable causal mechanisms or capacities. It is the criticism that Keynes [1951(1926): 232-33] first launched against econometrics and inferential statistics already in the 1920s:

The atomic hypothesis which has worked so splendidly in Physics breaks down in Psychics. We are faced at every turn with the problems of Organic Unity, of Discreteness, of Discontinuity – the whole is not equal to the sum of the parts, comparisons of quantity fails us, small changes produce large effects, the assumptions of a uniform and homogeneous continuum are not satisfied. Thus the results of Mathematical Psychics turn out to be derivative, not fundamental, indexes, not measurements, first approximations at the best; and fallible indexes, dubious approximations at that, with much doubt added as to what, if anything, they are indexes or approximations of.

The kinds of laws and relations that econom(etr)ics has established, are laws and relations about entities in models that presuppose causal mechanisms being atomistic and additive (for an argumentation that this is also the case for experimental economics, cf. Siakantaris [2000:270]). When causal mechanisms operate in real world social target systems they only do it in ever-changing and unstable combinations where whole is more than a mechanical sum of parts. If economic regularities obtain they do it (as a rule) only because we engineered them for that purpose. Outside man-made “nomological machines” they are rare, or even non-existant. Unfortunately that also makes most of the achievements of econometrics – as most of contemporary endeavours of economic theoretical modeling – rather useless.

4. Why neoclassical economic theory is a dead end

The failures of mainstream macroeconomics are largely attributable to its use of deductivist theory and method. Its foundations are not as strong as Lucas and other neoclassical economists assume them to be. There’s a huge gap between the purported ideal of building economics from the behaviour of individual actors and the fact that what one accomplishes has very little to do with the behaviour of real individuals. As Toulmin [2003:236] notes:

> If we ask about the validity, necessity, rigour or impossibility of arguments or conclusions, we must ask these questions within the limits of a given field, and avoid, as it were, condemning an ape for not being a man or a pig for not being a porcupine.

A realist and relevant economic theory has to do better. Even though there may be no royal road to success, I would contend neoclassical economics has definitely come to the end of the road.

Let me just give some hints of the kind of ontological and methodological building stones that are missing in neoclassical economics and what a viable alternative for economic theory would be.

I. Relevance, realism and the search for deep causal explanations

Instead of taking for granted that we are in possession of the one “correct” model, we have to have a more humble attitude. We know certain things and to know more we dig. We don’t content ourselves with surface appearances and correlations between observable variables. We dig deep. Correlations between observables are clues and form the starting points in our search for deeper causal structures in economy and society. But they aren’t invariant parameters *à la* “tastes” and “technology” in Lucas analysis of business cycles. As a famous philosopher once put it - “all that is solid melts into air”. That goes for the alleged “deep parameters” too.

Economics can’t be a “Euclidean” science. It reduces it to a logical axiomatic system in applied mathematics, with little bearing on real economies. As Keynes stated, we should use a more “Babylonian“ approach and aim for less universal theories and accept that there will always be binding spatio-temporal restrictions to the validity of our theories. The real economy is – to use the words of Cartwright [1999] - no “nomological machine”, but rather a “dappled” world.

As Wesley Salmon [1971:34] famously noted, one can *deduce* that a male person who takes birth-control pills will not get pregnant, but that surely does not *explain* why that person does not get pregnant. Economics should definitely be in the explanation business, and deductions, though not useless, is less helpful than citing relevant causes.

Paul Samuelson [1964:737] once wrote that to describe “how” was to explain, and that “economists as scientists shouldn’t waste their time with unfruitful questions of “why?” To pose questions regarding underlying causes was considered metaphysical.” As a critical realist I would rather say that a social science that doesn't pose "why-questions" can hardly count as a science at all.

Explanation and prediction are not the same. To explain something is to uncover the generative mechanisms behind an event, while prediction only concerns actual events and does not have anything to say about the underlying causes of the events in question. The barometer may be used for predicting today's weather changes. But these predictions are not explanatory, since they say nothing of the underlying causes.

Every social phenomenon is determined by a host of both necessary and contingent relations. It's also for this reason that we can never confidently predict them. As Maxine Singer [1997:39] has put it: "Because of the things we don't know that we don't know, the future is largely unpredictable."

If we want the knowledge we produce to have practical relevance, our knowledge-aspirations and methods have to adapt to our object of study. In social sciences – such as economics – we will never reach *complete* explanations. Instead we have to aim for *satisfactory* and *adequate* explanations.

As is well known, there is no unequivocal criterion for what should be considered a *satisfactory* explanation. All explanations (with the possible exception of those in mathematics and logic) are fragmentary and incomplete; self-evident relations and conditions are often left out so that one can concentrate on the nodal points. Explanations must, however, be real in the sense that they are "congruent" to reality and are capable of being used.

The *relevance* of an explanation can be judged only by reference to a given *aspect* of a problem. An explanation is then relevant if, for example, it can point out the generative mechanisms that rule a phenomenon or if it can illuminate the aspect one is concerned with. To be relevant from the explanatory viewpoint, the adduced theory has to provide a good basis for believing that the phenomenon to be explained really does or did take place. One has to be able to say: "That's right! That explains it. Now I understand why it happened."

While deductivist approaches try to develop a general *a priori* criterion for evaluation of scientific explanations, it would be better to realize that all we can expect to establish are *adequate* explanations, which it is not possible to disconnect from the specific, contingent circumstances that are always incident to what is to be explained.

Here I think that neoclassical economists go wrong in that they – at least implicitly - think their general models and theories are applicable to all kinds of societies and economies. But the insistence that all known economies have had to deal with scarcity in some form or other does not take us very far. I think we have to be more modest and acknowledge that our models and theories are time-space relative.

Besides being an aspect of the situation in which the event takes place, an explanatory factor ought also to be causally *effective* - that is, one has to consider whether the event would have taken place even if the factor did not exist. And it also has to be causally *deep*. If event *e* would have happened without factor *f*, then this factor is not deep enough. Triggering factors, for instance, often do not have this depth. And by contrasting different factors with each other we may find that some are irrelevant (without causal depth).

Without the requirement of depth, explanations most often do not have practical significance. This requirement leads us to the nodal point against which we have to take measures to obtain changes. If we search for and find fundamental structural causes for unemployment, we can hopefully also take effective measures to remedy it.

Relevant scientific theories do more than just describe (purported) event-regularities. They also analyze and describe the mechanisms, structures, and processes that exist. They try to establish what relations exist between these different phenomena and the systematic forces that operate within the different realms of reality.

Explanations are important within science, since the choice between different theories hinges in large part on their explanatory powers. The most reasonable explanation for one theory's having greater explanatory power than others is that the mechanisms, causal forces, structures, and processes it talks of, really do exist.

When studying the relation between different factors, a neoclassical economist is usually prepared to admit the existence of a reciprocal interdependence between them. One is seldom prepared, on the other hand, to investigate whether this interdependence might follow from the existence of an underlying causal structure. This is really strange. The actual configurations of a river, for instance, depend of course on many factors. But one cannot escape the fact that it flows downhill and that this fundamental fact influences and regulates the other causal factors. Not to come to grips with the underlying causal power that the direction of the current constitutes can only be misleading and confusing.

All explanations of a phenomenon have preconditions that limit the number of alternative explanations. These preconditions significantly influence the ability of the different potential explanations to really explain anything. If we have a system where underlying structural factors control the functional relations between the parts of the system, a satisfactory explanation can never disregard this precondition. Explanations that take the micro-parts as their point of departure may well *describe* how and through which mechanisms something takes place, but without the macro-structure we cannot *explain* why it happens.

But could one not just say that different explanations – such as individual (micro) and structural (macro) – are different, without a need to grade them as better or worse? I think not. That would be too relativistic. For although we are dealing with two different kinds of explanations that answer totally different questions, I would say that it is the structural explanation that most often answers the more relevant questions. In social sciences we often search for explanations because we want to be able to avoid or change certain outcomes. Giving individualistic explanations does not make this possible, since they only state sufficient but not necessary conditions. Without knowing the latter we cannot prevent or avoid these undesirable social phenomena.

All kinds of explanations in empirical sciences have a pragmatic dimension. We cannot just say that one type is *false* and another is *true*. Explanations have a function to fulfill, and some are *better* and others *worse* at this. Even if individual explanations can show the existence of a pattern, the pattern as such does not constitute an explanation. We want to be able to explain the pattern *per se*, and for that we usually require a structural explanation. By studying statistics of the labor market, for example, we may establish the fact that everyone who is at the disposal of the labor market does not have a job. We might even notice a pattern, that people in rural areas, old people, and women are often jobless. But we cannot explain with these data why this is a fact and that it may even be that a certain amount of unemployment is a functional requisite for the market economy. The individualistic frame of explanation gives a false picture of what kind of causal relations are at hand, and *a fortiori* a false picture of what needs to be done to enable a change. For that, a structural explanation is required.

II. Taking complexity seriously

With increasing complexity comes a greater probability of systemic instability. Real economies are complex systems and they have to be analyzed with an eye to instability. Macroeconomics has to be founded on analyses of the behaviour of agents in disequilibrium. Stability considerations have to be made. Otherwise we are shadow-boxing. Just as increasing returns to scale, dynamic instability can no longer be ruled out just because it doesn't fit some preferred theoretical preconceptions or models. In moving equilibrium systems, the interesting things usually take place in-between, in the transitional phases.

A fallacy often made in neoclassical economics is the (implicit) assumption made, that the structure of the real system of which the model is supposed to be a (partial) representation of, is invariant. Structural changes, breaks, regime-switches and innovations are continually taking place and we cannot simply *assume* that the system is dynamically stable. It has to be justified and not just treated as "infinitely improbable".

With increasing complexity comes a greater probability of systemic instability. Real economies are complex systems and they have to be analyzed with an eye to instability. Macroeconomics has to be founded on analysis of behaviour of agents in disequilibrium. Stability considerations have to be made. Just as increasing returns to scale, dynamic instability can no longer be ruled out just because they do not fit some preferred theoretical preconceptions or models. Even though not sufficient in itself, sensibility analysis ought to be self-evident, since eventual equilibria without robustness are uninteresting coincidences in dynamically open systems. In continually moving equilibrium systems the interesting things take place in between, in the transitional phases.

The methodological implications of the awareness of these considerations are far-reaching. If the plausibility of analyzing the economy as a structurally stable system (partly) hinges on its degree of complexity, it is of cause of the outmost importance to use models and theories that are open to and able to reflect an ontologically complex economic system. Simply assuming structural stability without justification is unacceptable. It has to be convincingly argued that the real counterparts of our macroeconomic models and theories are in line with these assumptions. At least if the aim of our scientific endeavours is more than predictive, also aspiring to explain the deeper mechanisms at work in the economy and having instruments to affect it.)

Rational expectations are used in new classical economics to analyze macroeconomic equilibria, and it does not really bother to really found it in actors dynamic behaviour out-of-equilibrium. Lucas and other neoclassical economists just *assume* that the distribution of the possibilities of economic actors coincide with the distribution holding for the "real" data generating process. This implies the well-known description of actors as not committing systematic errors when predicting the future.

This kind of model presupposes - if it is to be applicable – that the stochastic economic processes are stationary. This in its turn means that the equilibrium is permanent and that the future is perfectly predictable. This kind of *ergodicity* is impossible to reconcile with history, irreversible time and actors learning by doing. How do you justify such a far-reaching assumption? Is it a self-evident axiom, a reasonable assumption describing real actors, empirically corroborated, an as-if assumption in the spirit of Friedmanian instrumentalism, or is it the only hypothesis of expectations formation that happens to be compatible with neoclassical axiomatic deductivist general equilibrium theory? I would take my bet on the last. The problem with this is that it is rather unenlightening from a realist viewpoint. What has to be argued is that actors that realize *ex post* that they have misjudged the situation and formed inadequate expectations, do learn from this and swiftly adapt their expectations so to instantly move towards a new (possibly permanent) equilibrium.

All *ad hoc* arguments for this view cannot do away with the obvious fact that once you allow for instability you also have to accept a certain degree of indeterminacy and the non-existence of event regularities. This is the only tenable way out of the model-conundrums that the hypothesis of rational expectations gets us into. If reality is to a large extent indeterminate, uncertain and instable, our model-assumptions have to reflect these ontological facts. There are regularities in the economy, but they are typically contextual, conditional and partial.

If we follow that path we, of course, have to give up the Euclidean hope of analyzing the economy as an axiomatic, deductively closed system. This is necessary. It is better to admit there are "things we don't know we don't know" and that therefore the future is uncertain in ways we don't know. Some economic factors are inherently unpredictable (as e. g. stock-market prices, foreign exchange rates etc) and give rise to structural breaks, shifts and non-linearities and genuinely unanticipated events that disrupts any eventual equilibrium.

When the relation between map and reality is poor, we have to redraw the map. An economic model is only relevant to the economy if it somehow *resembles* it. Real economies are evolving over time and are intermittently subject to large and unanticipated shocks. They are non-stationary and over time they sometimes show great changes in all the moments of the distribution of its constituent variables.

Models based on the hypothesis of rational expectations are, to say the least, far from ideal representations of macroeconomic behaviour in such systems. If economists want to say something relevant of real economies and not only of "thought-of-economies" they have to develop other models and methods.

III. The need for methodological pluralism and abduction

Criticizing neoclassical economics is no license for a post-modern and social constructivist attitude of "anything goes". Far from it. There *are* limits to feasible methods and we *do* have criteria for choosing between them. As a critical realist, I'm acutely aware of the danger of sliding down the slippery slope of relativism. On the other hand, however, I think there's need for a large amount of open-mindedness when it comes to the choice of relevant methods [cf. Danermark et al. 2002:150-176]. As long as those choices reflect an argued and justified position vis-a-vis ontology we have to admit that different contexts may call for more than one method. Contrary to the beliefs of deductivist-axiomatic theorists - one size doesn't fit all.

Keynes [1936:297] maintained that "the object of our analysis is not to provide a machine, or method of blind manipulation, which will furnish an infallible answer." Strictly deductive argumentation is possible only in logic. "In … science, and in conduct, most of the arguments, upon which we habitually base our rational beliefs, are admitted to be inconclusive in a greater or less degree" [Keynes 1973(1921):3]. In economics you can't "convict your opponent of error" but only "convince him of it". Hence, the aim of economic reasoning can only be to "persuade a rational interlocutor" [Keynes 1971-89 vol XIII :470]. Economics is an *argumentative* science. Since you can't really prove things, you have to argue and justify. And if one does use deductive arguments, one has to be aware of the limits of their validity and justify their use.

If this is the case, what kind of inferences should we aim for in economics? Arguably the most promising method is abduction - or inference to the best explanation as it is also called.

In abduction one infers "from the fact that a certain hypothesis would explain the evidence, to the truth of that hypothesis" [Harman 1965:89]. Or more schematically:

e is a collection of evidence

H would, if true, explain e

No other hypothesis can *explain* e as well as H does

Therefore, H is (probably) true

In contradistinction to deduction and induction it's neither logically necessary, nor an empirical generalization. It's rather reminiscent of Sherlock Holmes. Different frames of interpretation are tentatively deliberated, the problem is re-contextualized and with a little help from creativity and imagination, new connections and meanings are discovered, helping to solve the puzzle or explain the event or process. We don't know for sure that the new connections and meanings constitute true knowledge, but it's possible that they constitute better or deeper knowledge.

The scientific method should preferably be both *ampliative* – increase our knowledge – and also increase our *epistemic warrant* in the results it gives us. The best balance between these goals is given by abduction.

That the scientific method should extend our knowledge is a self-evident starting-point for a scientific realist. But it is not always easy to combine ampliation and epistemic warrant. What is it that gives warrant to one hypothesis rather than others when we go beyond our sensory impressions? A purely deductive method would ensure us that conclusions were as probative as the premises on which they build. But deduction is totally unampliative. Its output is in its truth-transmitting input. If we are to use content-increasing methods we therefore have to accept that they can't be of a deductive caliber. Our data never guarantees that only *one* hypothesis is valid. But on the other hand it doesn't follow that they possess *the same degree* of validity. All cats aren't necessarily grey. If a standpoint is tenable can't be decided solely on formal-logic considerations but has to take into account consideration of what the world is and how it is structured. That a method isn't the best in all possible worlds doesn't preclude it being the best in the world in which we happen to live. To hold the view that abduction is not an inference "can be upheld only if one entertains the implausible views that to infer is to deduce and that to infer is to have 'an automatic warrant' for the inference" [Psillos 2002:619].

What we infer with ampliative methods will always be more or less defeasible. In contrast to the either/or of Kierkegaard and deductivism, the inferences of an ampliative method can always be changed, modified or rejected as a result of more and new information or by having conducted better analysis.

The problem of induction is that its ampliation is narrow and builds on going from "some" instances to "all" via generalization. This "more of the same" method enhances our knowledge in a purely horizontal manner. No new entities, relations or structures emerge. In that regard, induction signifies a minimal ampliation of knowledge, based on an underlying assumption of the world as ruled by event-regularities. Its short-comings are obvious. What we gain in epistemic warrant we lose in strength of the ampliation. It's to restrictive to give us hypotheses or explanations of the causes behind observed phenomena.

In science, the hypothetic-deductive method makes possible a forceful ampliation through confirmation of posited hypothesis and opens up for using unobservable causes. As the Duhem-Quine problem exemplifies, it however, does not help us in discriminating which of the assumptions or hypothesis that is wrong when the theory can't be confirmed. If both hypotheses A and B may explain X, the hypothetic-deductive method doesn't give us any means to discriminate between them. What we gain in ampliation, we lose in epistemic warrant. The hypothetic-deductive method simply is too permitting, since it doesn't enable us to discriminate between different hypotheses that are compatible with the evidence. A method that can't rank hypotheses such as "contemporary Swedish unemployment is a result of Swedish workers being lazy" or "contemporary unemployment is a result of globalization, technological development and economic policy" simply isn't an adequate method.

Abduction, on the other hand, can rank competing hypothesis and tackles the Duhem-Quine problem, since it urges us to look beyond the properties and implications of single hypotheses and also judges and ranks their explanatory power. Abduction is both a logic of justification and a logic of discovery.

The trade-off between ampliation and epistemic warrant results from a kind of risk present in all ampliation, and the more risk we are willing to take the less epistemic warrant we have to live with. We get to know more, but are less sure of that which we know. If we want to have a larger degree of confidence in our knowledge we are usually forced to forgo new knowledge and its accompanying risks.

Then, having argued for abduction as striking the best balance between ampliation and epistemic warrant, what does a good abduction look like? A natural demand for a critical realist to posit is that it should establish a causal relation between explanandum and explanans. To say that H is the best explanation of X is simultaneously to say that of the hypothesis we are comparing, the causal story H paints is in best concordance with our background knowledge. The *contrastive* character of explanation [cf. Garfinkel 1981] is thereby emphasized since it is not possible to decide which explanation - out of many potential explanations - is the best, without taking account of relevant background knowledge.

Of course there are other criteria that are mentioned when one tries to describe explanatory merit: consilience, depth, simplicity, precision. But even if these criteria often are desirable, they are not self-evident or even decisive for our evaluation of potential explanations. To a large extent they are pragmatic virtues and domain-specific in character.

If explanatory power in the shape of simplicity, unification, coherence etc, has to do with truth is a matter you have to argue for. They *may* be criteria for theory-choice, but they *need* not be. These criteria chiefly express the more or less idiosyncratic preferences of different scientists. *Ceteris paribus* it is as a rule preferable to have a more unified, simpler or coherent theory. This you can defend from purely thought- and cognition-economic or esthetic considerations. But you can't *a priori* maintain that they have to be better, more probable or truer than their rivals.

IV. Why it is better to be vaguely right than precisely wrong

When applying deductivist thinking to economics, the neoclassical economist usually sets up an "as if"-model based on a set of tight axiomatic assumptions from which consistent and precise inferences are made. The beauty of this procedure is, of course, that if the axiomatic premises are true, the conclusions necessarily follow. The snag is that if the models are to be relevant, we also have to argue that their precision and rigour still holds when they are applied to real-world situations. They often don't. When addressing real economies, the idealizations necessary for the deductivist machinery to work, simply don't hold.

So how should we evaluate the search for ever greater precision and the concomitant arsenal of mathematical and formalist models? To a large extent, the answer hinges on what we want our models to perform and how we basically understand the world.

For Keynes the world in which we live is inherently uncertain and quantifiable probabilities are the exception rather than the rule. To every statement about it is attached a "weight of argument" that makes it impossible to reduce our beliefs and expectations to a one-dimensional stochastic probability distribution. If "God does not play dice" as Einstein maintained, Keynes would add "nor do people". The world as we know it, has limited scope for certainty and perfect knowledge. Its intrinsic and almost unlimited complexity and the interrelatedness of its organic parts prevent the possibility of treating it as constituted by "legal atoms" with discretely distinct, separable and stable causal relations. Our knowledge accordingly has to be of a rather fallible kind.

To search for precision and rigour in such a world is self-defeating, at least if precision and rigour are supposed to assure external validity. The only way to defend such an endeavour is to take a blind eye to ontology and restrict oneself to prove things in closed model-worlds. Why we should care about these and not ask questions of relevance is hard to see. We have to at least justify our disregard for the gap between the nature of the real world and the theories and models of it.

Keynes [1971-89 vol XIV:296] once wrote that economics "is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world." Now, if the real world is fuzzy, vague and indeterminate, then why should our models build upon a desire to describe it as precise and predictable? Even if there always has to be a trade-off between theory-internal validity and external validity, we have to ask ourselves if our models are relevant.

Models preferably ought to somehow reflect/express/partially represent/resemble reality. The answers are not self-evident, but at least one has to do some philosophical under-labouring to rest one's case. Too often that is wanting in modern economics, just as it was when Keynes in the 1930s complained about the econometricians' lack of justifications of the chosen models and methods.

"Human logic" has to supplant the classical, formal, logic of deductivism if we want to have anything of interest to say of the real world we inhabit. Logic is a marvelous tool in mathematics and axiomatic-deductivist systems, but a poor guide for action in real-world systems, in which concepts and entities are without clear boundaries and continually interact and overlap. In this world I would say we are better served with a methodology that takes into account that "the more we know the more we know we don't know".

The models and methods we choose to work with have to be in conjunction with the economy as it is situated and structured. Epistemology has to be founded on ontology. Deductivist closed-system theories, as neoclassical economic theory, could perhaps adequately represent an economy showing closed-system characteristics. But since the economy clearly has more in common with an open-system ontology we ought to look out for other theories - theories who are rigorous and precise in the meaning that they can be deployed for enabling us to detect important causal mechanisms, capacities and tendencies pertaining to deep layers of the real world.

Rigour, coherence and consistency have to be defined relative to the entities for which they are supposed to apply. Too often they have been restricted to questions internal to the theory or model. Even if "the main role of deductive approaches is to guarantee consistency" [Moses & Knutsen 2007:282], clearly the nodal point has to concern external questions, such as how our theories and models relate to real-world structures and relations. Applicability rather than internal validity ought to be the arbiter of taste. There is no need to abolish economic theory altogether. But as Hicks (1984:?) noted, it needs to be carried on in a different way, "less abstract, more history-friendly, less technical, more concerned with real economic phenomena, less reductionist and more open to taking advantage of the contributions coming from other social and moral Sciences."

V. Open systems, equilibrium, expectations and uncertainty

Expectations have to be treated in a context of real, *historical* time. Real individuals don't settle their accounts at the end of periods in general equilibrium *mechanical* time. Actors have to make decisions, plans and act in the absence of equilibrium. Most importantly, firms have to plan their investments in the light of a more or less uncertain future, where there may even not yet exist a market for their products and where the present economic outlook offers few guidelines. Output and employment – ruled by expectations – are largely indeterminate and the structure of the economy changes continually and in complex ways, making it extremely difficult to predict or model.

Since the alternative non-neoclassical framework is not restricted to *individuals*, there is an open possibility for investigating expectations-formation in different *groups*. Mores, conventions and norms differ between consumers, firms and governments. If they are strong, there might be a possibility to detect certain kinds of *demi-regularities* in their formation [cf. Lawson 1997:204-231].

It's also a fact that different groups have to tackle different *kinds* of uncertainty. For macroeconomics, the expectations of investors are as a rule the most important. Unfortunately these are strongly influenced by Keynes "animal spirits" which are extremely tricky to handle in analysis. Shocks and surprises repeatedly make it impossible to predict the shifting moods in spirit. No matter what the interest rates, animal spirits can suddenly shift and affect plans to invest. This increases the uncertainty in the sense of Keynes "weight of argument" view – confidence in our predictions fall.

This applies to both long-run predictions of the price of gold five years hence and to short-term predictions of exactly on which day and minute the asset markets turn and we need to cash in on our position.

This is also one of the main reasons why *money* plays such an important role in real economies. Money makes it possible to postpone investments and not commit ourselves until we are more confident in our expectations and predictions.

All this confirms the basic "problem" – the economy is an open system. This has to be reflected by our analytical aspirations. Anything else will only lead to continual frustration. Markets are not usually totally chaotic. However, when it comes to expectations and the future, Keynes *dictum* still stands – often "we simply don't know".

Individuals in neoclassical economics are usually assumed to be in a behavioural equilibrium and to have rational expectations. This assumption presupposes - if it's to be applicable – that the stochastic economic processes are stationary. This in turn means that the equilibrium is permanent and that the future is perfectly predictable. From a critical realist point of view, this is dubious. This kind of ergodicity is impossible to reconcile with history, irreversible time and actors learning by doing.

Once you allow for instability you also have to accept a certain degree of indeterminacy and the non-existence of event regularities. This is the only tenable way out of the model-conundrums that the hypothesis of rational expectations gets us into. If reality is indeterminate, uncertain and instable, our model-assumptions have to reflect these facts. There *are* regularities in the economy, but they are typically contextual, conditional and partial.

If we follow this path we have to give up the Euclidean hope of analyzing the economy as an axiomatic, deductively closed system. In my view this is essential.

Economic theory cannot just provide an economic model that *mimics* the economy. Theory is important but we can't start to question data when there is a discrepancy. This would presuppose an almost religious faith in the validity of the preferred theory. When the relation between map and reality is poor, we have to redraw the *map*.

When it comes to equilibrium a tenable non-neoclassical economic theory has to reject the mechanical time equilibrium used by mainstream macroeconomics since it is not possible to apply it to real world situations. Real-world phenomena such as creative destruction, new technologies and innovations are not really compatible with general equilibrium. Institutions, endogenous technology, increasing returns to scale, irreversible time, non-ergodicity and uncertainty are not – as has been repeatedly shown in history - easily incorporated within the neoclassical framework.

From an explanatory point of view, it is more feasible to use partial analysis and to try to give explanations in terms of what are deemed to be the most causally important variables in specific contexts, instead of trying to encapsulate everything in one single timeless interdependent general equilibrium model.

5. Epilogue

Let me round off with some remarks on where the great divide in economics is currently situated.

In the history of economics there have existed many different schools of economic thought. Some of them – especially neoclassical economics - we have touched upon here. They are usually contrasted in terms of the theories and models they use. However, the fundamental divide is really methodological. How we categorize these schools should basically refer to their underlying ontological and methodological preconceptions, and not, for example, to their policy implications, use of mathematics and the like.

Much analytical-philosophical efforts has lately been invested in untangling terminological a conceptual analysis of models and theories. I think this necessary and good. But it is certainly not sufficient. The use and misuse of different theoretical and modeling strategies also have to be evaluated and criticized.

To develop economics along critical realist lines it is necessary to give up the ill founded use of closed representative-agent models, since these eliminate the basic problem of uncertainty and coordination between individual actors and groups, and make conventional behaviour totally unintelligible.

Henry Louis Mencken [1917] once wrote that "[t]here is always an easy solution to every human problem – neat, plausible and wrong." And neoclassical economics has indeed been wrong. Its main result, so far, has been to demonstrate the futility of trying to build a satisfactory bridge between formalistic-axiomatic deductivist models and real world target systems. Assuming, for example, perfect knowledge, instant market clearing and approximating aggregate behaviour with unrealistically heroic assumptions of representative actors, just will not do. The assumptions made, surreptitiously eliminate the very phenomena we want to study: uncertainty, disequilibrium, structural instability and problems of aggregation and coordination between different individuals and groups.

The punch line of this is that most of the problems that neoclassical economics is wrestling with, issues from its attempts at formalistic modeling *per se* of social phenomena. Reducing microeconomics to refinements of hyper-rational Bayesian deductivist models is not a viable way forward. It will only sentence to irrelevance the most interesting real world economic problems. And as someone has so wisely remarked, murder is unfortunately the only way to reduce biology to chemistry - reducing macroeconomics to Walrasian general equilibrium microeconomics basically means committing the same crime.

If scientific progress in economics – as Lucas and other latter days neoclassical economists seem to think – lies in our ability to tell "better and better stories" *without* considering the realm of imagination and ideas a retreat from real world target systems reality, one would of course think our economics journal being filled with articles supporting the stories with empirical evidence. However, the journals show a striking and embarrassing paucity of empirical studies that (try to) substantiate these theoretical claims. Equally amazing is how little one has to say about the relationship between the model and real world target systems. It is as though thinking explicit discussion, argumentation and justification on the subject not required. Economic theory is obviously navigating in dire straits.

Recent events in the financial markets have, as rightly noticed by Paul Krugman [2009], "pretty decisively refuted the idea that recessions are an optimal response to fluctuations in the rate of technological progress" and that "unemployment is a deliberate decision by workers to take time off". According to Krugman what went wrong was basically that "the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth." This is certainly true as far as it goes. But it is not deep enough. Mathematics is just a means towards the goal – modeling the economy as a closed deductivist system.

If the ultimate criteria of success of a deductivist system is to what extent it predicts and coheres with (parts of) reality, modern neoclassical economics seems to be a hopeless misallocation of scientific resources. To focus scientific endeavours on proving things in models, is a gross misapprehension of what an economic theory ought to be about. Deductivist models and methods disconnected from reality are not relevant to predict, explain or understand real world economic target systems. These systems do not conform to the restricted closed-system structure the neoclassical modeling strategy presupposes. If we do not just want to accept that "in the social sciences what is treated as important is often that which happens to be accessible to measurable magnitudes" [Hayek 1974], critical realism can help make it possible to reorient our endeavours in more constructive directions (in macroeconomics, e. g. Jespersen [2009] is a valuable contribution) and build a relevant and realist economics that can provide advances in scientific understanding of real world economies.

In this essay an attempt has been made to give an up-to-date coverage of recent research and debate on the highly contentious topic of the status and relevance of economic theory. It shows that what is wrong with economics is not that it employs models, but that it employs poor models. They are poor because they do not bridge to the real world target system in which we live. Economic theory today consists mainly in investigating economic models.

Neoclassical economics has since long given up on the real world and contents itself with proving things about thought up worlds. Empirical evidence only plays a minor role in economic theory (cf. Hausman [1997]), where models largely functions as a substitute for empirical evidence. But "facts kick", as Gunnar Myrdal used to say. Hopefully humbled by the manifest failure of its theoretical pretences, the one-sided, almost religious, insistence on mathematical deductivist modeling as the only scientific activity worthy of pursuing in economics will give way to methodological pluralism based on ontological considerations rather than formalistic tractability.

If not, we will have to keep on wondering - with Robert Solow and other thoughtful persons - what planet the economic theoretician is on.

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