

Economic methodology and the crisis: a critical realist perspective^{*}

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Texto submetido às Seções Ordinárias do XVI Encontro Nacional de Economia Política

Área 1. Metodologia e História do Pensamento Econômico

Sub-área 1.1. Metodologia e Caminhos da Ciência

Resumo

O texto discute alguns problemas metodológicos do formalismo em economia, tendo por motivação e contexto a recente crise financeira de 2008. Após a introdução, na qual o problema é exposto por referência a uma série de autores (inclusive não heterodoxos), apresentamos uma visão simplificada da concepção realista crítica de ciência e explicação. A seguir, discutimos a questão do papel e do uso de modelos formais em economia. No contexto desta discussão encontramos rastros do ensaio metodológico de Friedman, como uma permissão para lançar mão de modelos irrealísticos. Após examinar as condições e objetivos do uso de modelos formais em economia, defendemos uma abordagem mais realística e ontologicamente ciente dos fenômenos econômicos como uma condição para a realização de trabalhos relevantes (do ponto de vista prático) e para aumentar a percepção das instituições e relações sociais potencialmente criadoras de situações de crise.

Abstract

In this paper we deal with some problems of the economics profession, regarding its formalist methodology with the recent financial ('subprime') crisis of 2008 as a backdrop. After exposing the problem drawing on various (including non heterodox) authors, we briefly outline the critical realist account of science and explanation. Then we turn to the role and use of formal models in economics. Here we find some unsuspected fingerprints of Friedman's essay on positive economics as a license to lay hold of unrealistic models. After examine the conditions for, and aims of, using formal models in economics, we argue for a more realistic, ontologically concerned discussion of economic phenomena as a condition for relevant work and eventually to make economists aware of social relations and institutions potentially conducting to economic turmoil.

^{*} This paper is part of a wider work on the theoretical and methodological causes of the recent financial crisis. We are grateful for many helpful comments by Peter Kriesler and other (anonymous) economists to a preliminary draft. However, we are the only responsible for remaining errors or misconceptions, as this is yet a work in progress. Comments are welcome.

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Theorising in economics, I have argued, is an attempt at understanding and I now add that bad theorising is a premature claim to understand. (Hahn, 1985, p. 15)

1. Considering methodological issues

In the following pages we outline a conception of what are the fundamental, metatheoretical failures involved in, and explaining, the theoretical problems of economics. In our view, the main problem (formalism) allows the use of very inappropriate models to understand economic reality. Moreover, since formalism presupposes an ontology of ‘closed systems’ it is unable to avoid economic disasters caused by phenomena of ‘open systems’, like uncertainty, bounded rationality, herd psychology, etc. Our interest is not primarily in debates within economic methodology, but in using methodological critiques of economic theory in order to see if we can learn from this episode how can we profit from paying attention to the real world when designing models. We do so in three steps: outlining the critical realist approach to economic methodology, which draws attention to the ontology of economic world; discussing formalism and the problems concerning design and use of overly unrealistic models; and finally proposing some ways to proceed.

2. The critical realist conception of scientific explanation

Critical realism is a comparatively new and expanding approach to the methodology of economics. Proposed by the British philosopher of science Roy Bhaskar (1975, 1979), it was introduced in economics by a group of economists and other social scientists mostly associated with the University of Cambridge. It has made an appeal to philosophically-oriented economists and schools, like Post Keynesians and Austrians. The best known name of critical realist economic methodology is Tony Lawson (1997, 2003, and several papers), formerly editor of *Cambridge Journal of Economics*. Closely associated are Sheila Dow (2002, 2003), who writes extensively on macroeconomic theory and economic methodology, and the (old) institutionalist and evolutionary economic theorist Geoffrey Hodgson (2004, 2006). The pivotal theme within critical realism is the nature of scientific activity and explanation.

According to that approach, traditional issues in economic methodology (logical empiricism and Popperian falsificationism) mistake the nature of scientific activity and so

propose a misleading aim to (natural as well as social) working scientist. Let us briefly elaborate. In the natural sciences, theories are law-like statements from which implications are deduced and that ‘explains’ the object of interest. Thus, for example, an explanation of falling bodies is a deduction from Galileo’s Law plus a series of auxiliary or attending or simplifying/idealizing statements (e.g. perfect vacuum, flat surface of earth, etc.) According to the traditional methodology, explanation is subsuming a case of falling body into at least one general law. Prediction, on the other side, is to expect that from the same cause (a general law) the same effect will always (deterministically or probabilistically) ensue. Explanation and deduction are symmetrical (the famous Hempel-Oppenheim’s ‘symmetry thesis’). The empirical test of theories is at the same time condition for its acceptance, and a sign of growth of knowledge¹. To prescribe this methodology for economics implies that (i) there is only one valid method of inquiry all over the sciences (‘methodological monism’); and (ii) the search for regularities or constant conjunctions of events as the only possible mean of attaining knowledge (‘epistemic fallacy’).

Taking the latter implication first, for critical realists constant conjunctions of events are neither necessary nor sufficient condition to claiming scientific knowledge. Scientific law-like statements are formulated in experimental (i.e. controlled) settings (‘closed systems), where constant conjunction of events obtains because one causal factor of interest is sealed off from any other countervailing factors that bear on the phenomenon of interest, such that we can always say ‘whenever (event type) X, then (event type) Y’. If valid, these statements will be successfully applied also in the nature (an ‘open system’). How is that possible? Traditional methodologists have a problem here: if stable conjunction of events are sought after, they are rather rarely spontaneously founded (astronomical laws, one of Lawson’s favourite example of spontaneous regularity, is indeed obtained under conditions of closure, see Mäki, 1992b, p. 186); if, on the other hand, the subject matter of scientific investigation is explained by the scientist’s intervention, then they are bound to admit that there is no genuine laws out there in the nature. Critical realists solve this problem by claiming that the aim of experimental activity is to isolate a putative causal mechanism from all others bearing on the phenomenon of interest. Whenever a theory thus obtained is successfully applied in open

¹ We will not delve into the details and the historical crumbling of ‘received’ (i.e., logical empiricist) and Popperian views of methodology, but see Hands (2001, chap. 3). But it does not prevent Blaug (1997, p. xxiii), an important supporter of Popperian ideas in economic methodology, of saying that ‘the Methodology which best supports the economist’s striving for substantive knowledge of economic relationships is the philosophy of science associated with the names of Karl Popper and Imre Lakatos. To full attain the ideal of falsifiability is, I still believe, the prime desideratum in economics.’

systems it is so because scientists have identified correctly the causal (i.e. dominant) mechanism. This picture has important implications for a critical realist account of science.

Firstly, science should not be seen as the search for constant conjunction of events. The prime interest of critical realists is in ontology. Ontology is the study of the nature of world and what there is in it (its ‘ontic furniture’). Critical realists advance a series of ontological propositions. Reality is structured in layers, each of them more encompassing and deep from top-down. The first layer is the *empirical* domain (our sensory perception of events and state of affairs), the second one is the *actual* domain (things ‘as they really are’, irrespective to our knowing of or feeling them) and the third one is the *real* or *deep* domain, populated by structures, mechanisms, powers and tendencies that shape and condition the events of actual domain. Structures are the properties of an object of inquiry, its mode of being. Mechanisms are the way an object operates, due to its structure. Powers are capacities of the object, what it can cause when its mechanisms are triggered. Yet these mechanisms do not operate in isolation, but in open systems, such that many other mechanisms (enhancing or countervailing) might typically be at work simultaneously – thus concealing the mechanism we are interested in. That is why critical realists claim that mechanisms operate as tendencies, i.e. when triggered a mechanism will necessarily operate, no matter what events ensue. In this ontological commitment, reality is stratified (in layers) and structured (any layer may be out of phase from each other), but an explanation is the move from the empirical domain into even deeper layers of reality, searching for the causal mechanisms of what exists in the actual domain and which we perceive in the empirical domain².

Secondly, due to this account of explanation, it does not require strict regularities. A unique event can be explained if we have sufficient information on its structure, and antecedent knowledge from where to start the research. Constant conjunctions of events are insufficient for explanations, too. For explaining a phenomenon is studying its structure looking for plausible mechanisms causally responsible for its occurrence, rather than simply recording correlations between empirical events. In fact critical realists charge positivists of all stripes of what they call ‘epistemic fallacy’ – mistakenly conflate ontological questions into epistemic questions. For example, the restless search for models that better ‘fit’ facts to a theory is a case in point, insofar as it reduces all phenomena to some measurable and all-compassing analytical categories referring only to empirical events.

At last, thirdly, social scientific research can be done along lines broadly similar to

² That is only a sketchy picture of critical realist account of scientific practices. See a lengthy and sophisticated discussion of these matters in Lawson (1997, chap. 3).

natural science. The structures, mechanisms, etc. are obviously different, but the aim is equal: unearth causal mechanisms, powers and tendencies of structured objects of knowledge. From this point Lawson (1997, chap. 14 to 16 and 2003, chap. 2) elaborates the nature of social reality at length. It is characterized by internal (constitutive) and external (contingent) social relations, mediated by positions (hierarchies) and rules (norms, mores, conventions, etc.) Society is thus an unbroken net of relations, dependent of individual action but irreducible to it, with mechanisms and powers of its own. It constrains the alternative courses of action for individual decision, but does not determine the action actually chosen. Moreover, at any time individual action is simultaneously reproducing and transforming society. Critical realists like Archer (1995, chap. 5) and Fleetwood (1995, pp. 86-90) call this process ‘the transformational model of social activity’: in society our actions are always based on structures inherited from the past and always transforming or reproducing this same structure for the future. That is why Lawson (2009, p. 764) claims that social processes are ‘a totality in motion’.

One last question is: how can one obtain knowledge of these hidden structures? Critical realism is not a disguised form of outdated essentialism? That is where critical realists claim their position as fallibilism – there is no guarantee for putative mechanisms besides its power to illuminate some reality (natural or social). The problem of discriminate among alternative theories (the old problem of ‘identification’) is to be solved by the degree in which each theory can explain more (and/or in a better way) events than its competitors. Of course, this is a very hotly debated issue in the philosophy of science, opening the doors for relativism. Critical realists call in their help two notions: knowledge, as a social product is itself a ‘produced mean of production’ of knowledge, such that in the start of any research we have at least one theory to proceed. Moreover, the ontological commitment (‘how reality is’) traces a divide between knowledge of reality and its object. This make possible to be fallibilist, not relativist: reality is the contrastive backdrop onto all scientific claims can be evaluated and our prior (scientific) beliefs revised. Thus, critical realists can (and relativists cannot) differentiate changes in the world from changes in knowledge. When we perceive some event (supposedly) disjunctive to our existing knowledge, we can abducting a mechanism (i.e., propose a cause for that effect) and investigate its occurrence. We shall deal with the question of how identify a causal mechanism shortly. Before that, we deal with the problem of formalism in economics and its (supposed) culpability for the crisis.

3. Formalism and economic models

In the aftermath of global financial crisis of 2008 is oftentimes found opinions to the effect that economic theory was made irrelevant due to its formalism. In the simply and plain words of Blaug: ‘what characterizes ‘formalism’ is that technicalities are prized as ends in themselves, such that theories which do not lend themselves to technical treatment are set aside and with them the problems they address. Formalism is the *worship* of technique and that is what is wrong with it.’ (Blaug 2002, p. 36) In fact, both critics and supporters of mainstream economics have made pronouncements on this regard. Of course, not all mainstream economists recognize a problem in the way of doing economics. They typically blame some factor exogenous to the discipline (regulatory shortcomings, excessively lax monetary policy, a swiftly turn of events, and so on) for the financial crisis.³ Others think that is just business as usual. Note, for example, LSE professor Albert Marcet:

What do economists do? We think economics as a science... [That means] if you don’t have a model, data is a mess... We need models just to see where to look. I should teach to our MSc students and undergraduate students theories (with internal consistency) that the research community has thoroughly and very strongly tested in empirical terms, because that’s our job. Necessarily, economic models are oversimplified [there follows the Galileo’s Law as an example] and thus, in order a model to be a model, is the easiest thing in the world to make fun of economic theories... But, unless I find better models, isn’t fair to make fun. (Marcet, 2010; our transcription)

Yet Alan Blinder, when praised the progress of economics in the twentieth century, has recognized the problem:

Economics was off to the mathematical races. Intellectual giants like Samuelson and Arrow led the way, sweeping away the old, more literary tradition in economics and attracting a small army of scholars with a more scientific bent. [And Blinder adds:] But somewhere along the way the warm embrace of mathematics developed first into a infatuation, and then into a obsession. And that, I am afraid, is where economics lost at least some of its scientific moorings – moorings we have yet to regain... [Mathematics] is, of course, both a high and exceedingly difficult form of thought and an indispensable tool for every science... But mathematics seems entirely too self-referential, too deductive, one might almost say too pure to be considered a science. Let me dwell on these three words – self-referential, deductive, and pure – for they describe where economics has gone wrong, in my view. (Blinder, 1999, pp. 146-7)

³ For a sad report on how the failures of efficient markets and DSGE models are being received by their supporters, see Cassidy (2010) and Cohen (2009). Some mainstream economists are simply loosing their temper. In a reply to Krugman (2009), Cochrane (2009) defends his own stance as follows: “Imagine this weren’t economics for a moment. Imagine this were a respected scientist turned popular writer, who says, most basically, that everything everyone has done in his field since the mid 1960s is a complete waste of time. Everything that fills its academic journals, is taught in its PhD programs, presented at its conferences, summarized in its graduate textbooks, and rewarded with the accolades a profession can bestow, including multiple Nobel prizes, is totally wrong. Instead, he calls for a return to the eternal verities of a rather convoluted book written in the 1930s, as taught to our author in his undergraduate introductory courses. If a scientist, he might be a global-warming skeptic, an AIDS-HIV disbeliever, a stalwart that maybe continents don’t move after all, or that smoking isn’t that bad for you really.”

In our view, the theoretical shortcomings that have brought us into the crisis are closely linked to methodological and ontological assumptions mostly held by mainstream economists. The problem concerns to something that Dow (1990) calls ‘Cartesian mode of thought’ and Lawson (1997, pp. 17-18) calls ‘deductivism’. In short, this mode of thought sees theories only as logically derived series of propositions⁴. Moreover, those propositions are interpreted as entities apt for formalization. The next short step is supposing that, since logical structures have intersubjectively demonstrable truth value, they are the only valid and sound theorizing in any science, economics included. And since formal structures are contentless⁵, this presupposition also amounts to (even unwillingly) sacrifice relevance for rigor, elegance and precision for practical implications. But, if there is a problem with formalism in economics, what exactly is the problem? How could we come to such a state? Can we do any better?

To begin with, formalism is a complex term, interwoven with mathematization, axiomatization and model-building. Following Chick (1998, p. 1860) – who in turn follows Woo (1986, p. 20, n. 1) – our focus will be on axiomatization and model-building as forms – syntactical and semantical, respectively – of formalism. Chick, once again, helps to understand each of them:

The axiomatic approach and less rigorous mathematical models have a certain symmetry. In the first, one starts with ‘self-evident’ axioms, applies the deductive method using agreed rules of logic and, providing one’s logic is correct, arrives at demonstrable truths. Mathematical modelling is more relaxed and less ambitious: assumptions need not be ‘self-evident’; thus there is some scope for the theorist’s judgment, and that judgment may be questioned (the ‘realism of assumptions’ debate). In both cases, transformations are then made following agreed rules, and the conclusions follow as long as the rules have been obeyed. This procedural homology allows one to order one’s thoughts into points about the issue of the appropriate starting point of analysis, precision, and the biases inherent in conventional models. (Chick, 1998, p. 1861)

This passage has many points worth consideration. Axiomatics and model-building both require an appropriate translation of empirical objects of interest into their formal counterparts. This is made by the modeller ‘judgment’⁶. Models apparently also meet their

⁴ This characterization is more apt in Dow’s case than Lawson’s, as it is doubtful whether mainstream economic methodology is empiricist or axiomatic (see Viskovatoff, 1998). Lawson’s account of deductivism is in terms of Popper-Hempel hypothetical-deductive model of explanation which is (while mainstream is not) empiricist.

⁵ ‘While there are different formalist programmes, the unifying principle is self-contained rule-following, by which to construct formal languages and deductive systems that are independent of content.’ (Chick, 1998, p. 1859).

⁶ The mathematician Christian Henning is in full accordance: ‘Mathematical modelling always requires the interpretation of elements of the formal mathematical domain in terms of (personal or social, non-mathematical) reality. There is no formal way to check whether such interpretations are ‘true’, and the mathematical truth of theorems applied to such models does not warrant claims of ‘objective truth’ concerning the modelled reality.’ (Henning, 2010, p. 46)

user's anxiety for 'precision' and 'certainty', giving logical consistency to reasoning based on them. They are used also to promote agreement on a given issue, by assuming that it is correctly described in the model. But note that benefit is gained only at the syntactical level; models are also inherently interpretative, semantical, such that their elements are debatable, and their assumptions can be questioned. So, the problem with formalism, in economics or elsewhere, is misunderstanding that precision and consistency are completely different from validity, let alone practical implications – and giving to the formers ultimate value.

This is enough to point out that models are valuable and important, but must be carefully used. Dow (2008) gives examples of how the 'framing' of a question in a formal model can hinder further understanding of, or even worse, distorting the object of inquiry. Models of asset-pricing supposing equilibrium 'as the end-state of market processes' (p. 17) are a case in point; another is 'new' behavioural economics: 'While there is reference in behavioural economics to social framing, as in the conditioning of choice by social norms, there is little exploration of how it arises, although sociology might well have provided insights. Because of the axiomatic focus on atomic individuals, the influence of society is limited to the introduction of social norms as exogenous constraints on rational individual behaviour, without explanation for the emergence of these norms or the reasons that rational individuals accept them.' (p. 19) In a similar vein, Blaug (2002, p. 35) asserts that, despite using higher techniques, 'it is difficult to see how the new economic geography illuminates the locational aspects of economic activity any better than the old economic geography.'⁷

Several commentators find Milton Friedman's 1953 essay on 'The Methodology of Positive Economics' the prime source of formalism in economics, nevertheless statements in contrary in this very piece (Friedman 1953, pp. 10, 11-12), in other places (Friedman, 1999, p. 137), and the most pronounced influence of others, like von Newman, Morgenstern, Arrow and Debreu (Blaug 2002, p. 27; 2003).⁸

Take the following, rightly considered the most important (and controversial) methodological statement of the essay:

In so far as a theory can be said to have "assumptions" at all, and in so far as their "realism" can be judged independently of the validity of predictions, the relation between the significance of a theory and the "realism" of its "assumptions" is almost the opposite of that suggested by the view under criticism.

⁷ Reference to new economic geography is not material for the argument. Other examples (such as industrial organization and games, cf. Fisher, 1989) could be used as well.

⁸ See Chick (1998, p. 1865), Blaug (2002, p. 30), Lawson (2009, p. 766), Hodgson (2009, p. 1216), Dow (2008, p. 17), and especially Hands (2009). Backhouse and Medema (2009, p. 486) find also an influence of Lionel Robbins on the path towards formalization, once his definition of economics as allocation of scarce resources was seen by mathematical economists (mostly associated to the Cowles Commission) as easier (than Marshall's?) to formalize.

Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it “explains” much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained.’ A footnote attached to this passage reads: ‘The converse of the proposition does not of course hold: assumptions that are unrealistic (in this sense) do not guarantee a significant theory’. (Friedman, 1953, pp. 14-15, our italics)

Why Friedman is so important to the formalization of economics? Based on this only passage, almost every writer finds Friedman ‘licensing’ the free use of unrealistic assumptions while constructing economic models. In the context of global financial crisis, his fingerprints are found in sanctioning models which contain assumptions of substantive rationality, efficient markets, dynamic stochastic general equilibrium, and so on⁹. No matter what *did* Friedman think on economic theory and practice; his essay cries louder. A supporter of Friedman methodological statements evaluated its far-reaching consequences this way: ‘working economists look for heuristics that orient them in the fruitful direction *and also make them feel that their work is scientific*. When seeking fruitful heuristics, coherence and philosophical sophistication are not necessarily the dominant considerations. Crude, intuitive notions may be perfectly adequate to point an economist in the right direction.’ (Mayer, 1993, p. 214; our italics) Interestingly, in spite of so much ink spent with his essay, Friedman never disavowed any comment, whether supporting or attacking it¹⁰. Thus, the way was freed for economists to pursue any kind of assumption, no matter how ‘wildly inaccurate’ it may be.¹¹

⁹ Parenthetically, one should note that Lucas (1998, p. 132) counts himself as a ‘Fridmanite’ in methodology.

¹⁰ A conference celebrating the fiftieth anniversary of Friedman’s essay was organized by the Erasmus Institute of Philosophy and Economics in 2003. In the published book of the conference, Milton Friedman was invited to write the ‘Final Word’ in 2004. Here is Mäki’s (2009, p. xviii) comment: ‘To my knowledge, this is the first time that he has publicly spelled out his views about what others have written about his essay, but unsurprisingly perhaps, he keeps his statement very general and polite (while in private correspondence and conversations, he has been active in reacting to various criticisms and suggestions in more substantive ways). He had decided to stick to his old private rule according to which he will let the essay live its own life. It remains a challenge to the rest of us to live our academic lives together with the methodological essay that he left behind.’

¹¹ Moreover, when Friedman (1953, p. 8) delimitates the domain of validity of theories according to his methodology, any theory/hypothesis is made unassailable: ‘Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to ‘explain’.’ Lawson (1992, pp. 154-156, 158 note 6) takes ‘class of phenomena’ to mean posited conditions of closure, thereby stable conjunction of events can be obtained. In a similar vein, Mongin (1987, p. 86, n. 16) asserts that being so vaguely stated, this domain of applicability “corresponds, in a circular reasoning, to simply exclude the known falsifiers of that theory.” It would be a short step to treating economics as a kind of intellectual game played for its own sake. All in all, we find that much of this explain the alluring of Friedman’s essay for working economists – a rhetorical success gained at the expense of methodological coherence. Limitations of space prevents a detailed methodological treatment of these issues, but see Nagel (1963); Brunner (1969); Musgrave (1981); Caldwell (1982, chap. 8) Mäki (1986, 1992b); and Lawson (1992) for specialized critiques of Friedman.

At this point is important to note the Hands (2009, p. 158) in fact *denies* any responsibility of Friedman-the-man for formalism in economics. And he points to Friedman's warnings against excessively 'tautological' methods of analysis. However economists, when pressed to make their methodological allegiances explicit, take comfort in Friedman's essay and feel themselves good scientists.

Of course, methodological strictures do not run in a vacuum. Hodgson (2009, pp. 1215-1216), for example, provides a range of socio-cultural factors accounting for the winning of formalism, like the changing system of university teaching and research – towards more and more specialization and quantification, the downplaying of 'big questions' concerning society and the ultimate aims of scientific endeavour, and the 'publish-or-perish' pressure. Outside universities, market individualism and the cult of (quantifiable) performance has had been in line with these developments.

But one should not imply, from the critique of formalism sketched above, an utter rejection of mathematical methods in economics. A more tempered stance was recommended by Dow (1995, pp. 723-724) drawing on Keynes's remarks on the use of mathematics in economics. The key point is Keynes's turning the focus away from the dual use/do not use, to situations in which mathematical modelling is appropriate. These conditions can be briefly stated: (i) when the assumption of constant structure is reasonable for the subject at hand; (ii) when the object of theorising does not include significant non-quantifiable elements; (iii) when variables are commensurable. There is also conditions for using formal reasoning, independent of quantification: (iv) that the structure being analysed can reasonably be represented as constant, such that the variables can be represented as independent, or, if not constant, that interdependence can be expressed deterministically; (v) that all relevant factors can in practice be expressed formally. The danger with giving priority to mathematization is that the range of relevance is limited to those factors which can, given current capabilities, be expressed formally; and (vi) that the internal logic of the mathematical model is sufficient for persuasion. That is, the words employed in presenting mathematical argument themselves carry moral authority. Summing up, the more constant the structure of interest and the more it can be expressed formally, the more confident can one be of properly using formal models.

However, this does not exhaust the possible uses of formal models¹². Henning (2010, pp. 44-45) gives a lists the following ones: (1) to improve mutual understanding; (2) to

¹² There is an increasing literature on models, their relation to reality and their construction. Here we can only redirect the interested reader to it. See the papers included in the Part III of Mäki (2002), in Morgan and Morrison (1999) and in a rather recent issue of *Erkenntnis* (January 2009).

support agreement; (3) to reduce complexity; (4) for prediction; (5) to support decision; (6) to explore different (quantifiable) scenarios; (7) to explore the implications of the model; (8) to guide observations and support learning; (9) to lend beauty and elegance to theories. It is apparent that Keynes's concerns regard the purposes (4)-(6), whereas the method of idealization (Mäki, 1992a; Nowak, 1989) regards purpose (3), and 'conceptual exploration' (Hausman, 1992, p. 221) regards purposes (7)-(9). Purposes (1) and (2) are uncontroversial¹³.

Sugden (2002) offers a different view of models. Analyzing Schelling's segregation model and Akerlof's 'market for lemons', he notes that these models do not fit in any of above conditions or uses. Akerlof's model, for example, does not predict the price for used cars. Nor Schelling's model predicts any behaviour of racial discrimination in industrial cities. Thus, they are not concerned with prediction or control. Sugden assents that this model can be interpreted as 'conceptual exploration', but that is not all about them. They are constructed as counter-examples, counterfactuals, to shed light in some unperceived stretch of reality, to be likely to explain real world phenomena. Models do this job caricaturing, exaggerating, deforming some feature, isolating some putative causal factor, but keeping correspondence with reality (pp. 114-117).

This interpretation accepts Mäki's (1992a, p. 335; 2005) vision of models as (idealized) thought experiments but, in Sugden's (2002, p. 121) words:

[I]f a thought experiment is to tell us anything about the real world (rather than merely about the structure of our own thoughts), our reasoning must in some way replicate the workings of the world. For example, think how a structural engineer might use a theoretical model to test the strength of a new design. This kind of modelling is possible in engineering because the theory which describes the general properties of the relevant class of structures is already known, even though its implications for the new structure are not. Provided the predictions of the general theory are true, the engineer's thought experiment replicates a physical experiment that could have been carried out. On this interpretation, then, a model explains reality by virtue of the truth of the assumptions that it makes about the causal factors it has isolated.

Therefore, models are devices to think about real world phenomena; its validity depends on what we know about the real world and if the workings of causal factors cohere with it. Models are deductive devices and we fill the gap between model world and real world by making inductive inferences from the world of model to the real world.

If a model is genuinely to tell us something, however limited, about the real world, it cannot be *just* a description of a self-contained imaginary world. And yet theoretical models in economics often *are* descriptions of self-contained and imaginary worlds. These worlds have not been formed merely by abstracting key features from the real world; in important respects, they have been *constructed* by their authors. (Sugden, 2002, p. 133)

¹³ Suppes (1968) argues for the use of formalism in science, but considers only purposes (1)-(4) and (7).

In sum, it seems that there are good reasons to require realism of models. Although no model is, perfectly realistic ('whole-the-truth'), our acceptance of them depends on their realistically picture the workings of some isolated causal factor ('nothing but the truth'). That is, they must correspond to what we *do* know about the real world. Would be a leap of faith to suppose that models which are unrealistic in both senses could, nevertheless, illuminate phenomena of the world we live in. However, they could function as heuristic devices or might be serviceable for 'conceptual explorations'. This point leads us back to the issue of identifying real causal factors in a hidden, intransitive layer of reality – which we deal by discussing the possible ways to proceed.

4. Proposing alternative modes of thought in the aftermath of the crisis

Since formalism is a process which exhibit, according to Hodgson (2009), path-dependency and positive feedbacks, would be naïve to expect its instant abandoning. Yet, while some authors, like Colander *et al.* (2009), Keen (2009), Kirman (2009), Colander (2010) or even Blaug (2002) propose a way out through looking for better, more empirically-driven models (e.g. complexity theory, experimental and behavioural economics), Lawson considers any adjust in the economist's toolkit unhelpful.

[I]t is clear that the recent crisis situation (like almost any social situation) is something that needs to be understood rather than modelled... [I]t seems overly heroic to suppose that in order to capture the sorts of developments that occurred, all that is required of modern academic economics is a different type of mathematics, or internal 'theoretical' adjustments like the treating of a model's still isolated atoms as heterogeneous or as forming independent expectations; or focusing on the possibility of multiplicity and evolution of equilibria; or hoping that cointegrated vector autoregression (VAR) models will uncover robust structures within a set of data, and so forth... [I]t is apparent that the legitimate and feasible goal of economic analysis is not to attempt to mathematically model and perhaps thereby predict crises and such like, but to understand the ever emerging relational structures and mechanisms that render them more or less feasible or likely. Amongst other things, this requires an account of the background conditions against which ongoing developments are taking place. In the current context, this includes understanding how the credit expansion triggered by liberalised financial markets set the conditions for the current situation, and the assortment of developments and mechanisms by which it has come about. (Lawson, 2009, pp. 774-775)

From the previous two sections it is easy to see why Lawson takes such a stark stance. Mathematical modelling amounts to suppose an ontology of closed systems. Reality, as we see it is, in contrary, an open system. Therefore, formalism is *ex definitione* inappropriate for studying processes in the real world. As others (e.g., Hodgson, 2006; Chick and Dow, 2005; Mearman, 2002) have pointed out, Lawson runs in difficult here. Recall that we leave an open question above: how to identify causal factors hidden in the deep layer of reality? Lawson's (1997, chap. 15) answer is: by examining contrastive pattern of events or demi-regularities

(equivalent to Nicholas Kaldor's stylized facts). Such events are 'rough and ready', not strict, semi regularities, etc. Thus, an example from Lawson himself will help to understand the point and its problem. Take the pattern of productivity growth in the British manufacturing sector in the twentieth century. It is inferior to (otherwise similar) advanced countries. That is a contrastive demi-regularity. We can abduct a cause to it in a non-empirical domain (e.g. the British system of labour relations), and we can corroborate or revise it with further research, always giving prime concern to the ontology of the object.

This is a research conducted in an open system? No, because it isolated as negligible, or temporarily 'out of focus', many other facts as worth of being considered 'causes' as the isolated one. Thus, demi-regularities are partial closures, and for two reasons: (i) it is impossible to take all the relevant facts at once; theorizing is necessarily to discriminate and therefore to exclude some aspects of reality from our model world; and (ii) as Chick and Dow (2005) argue at length, the distinction between open and closed systems is not just on/off, as Lawson has lead us to belief, but is more nuanced. They identify eight conditions for a system to be open and other eight conditions to be closed. It requires satisfying any one of the former to be open, but all of the latter to be closed. Moreover, 'complete openness is incompatible with a system remaining recognizable as a system.' (p. 367) So it is important to be in mind that when Lawson insist on the pointlessness of modelling work, we would assuming he is reflecting Keynes's qualms on mathematical modelling, which concern mostly with stability of, and prediction upon (quantifiable) data. Closeness is often partial and this feature provides scope to discuss meanings, aims, and assumptions of that work, including its ontological commitments. The problem, as we see it, is not the use of models *per se*, but what are the elements, the method and the judgment made in its design.

But it does not mean that we are enthusiast of the new assortment of modelling techniques, such as complexity theory, behavioural economics, evolutionary game theory, and so on. Along similar lines of Lawson's critique (see the previous quotation) of Colander *et al.* (2009), also Hodgson (2009) and Dow (2008) cast doubts on these new techniques. And for a fundamental reason: it is a mirage, a Sisyphean task, to look for models that 'fit' better the data of recent financial turmoil. Mathematical modelling is inherently unhelpful to deal with stuff that makes for strong uncertainty, such as innovation, inexistent information and animal spirits. New modellers seem to let this uncomfortable feature pass in oblivion. Yet, we have already paid a high price for placing prediction above understanding.

If our assessment is valid, there is strengths and weakness in both positions. So, could we do better? Our answer would be in line with two pluralist statements¹⁴. Twenty-five years ago, the sociologist Etzioni (1985, p. 390) proposed “a medical model” for economics, which consists in making use of ‘findings from a variety of basic sciences’, including sociology, political science, environmental science, psychology, etc. aiming at transcend the rational economic man, but ‘without reverting to a much less analytical science, to the way [nineteenth century] political economy was’. Similarly, Chick (1998, p. 1868) says: ‘I hope that I have argued persuasively that the role of formalism is to be precise and rigorous where that is possible, and that other modes of analysis exist as valid and valuable complements. Formalism is fine, but it must know its place’. This way, we hope, it will be possible transforming economics into a more realistic and useful science.

5. Concluding Remarks

The recent financial crisis gives an opportunity for reflection on the foundations of economic theory and the practices resting upon it. Despite some factors which could have been avoided – such as excessive reliance on the self-correction properties of markets, on rating agencies, and on the self-regulation capacity of market participants, on excessive freedom, even the cult, of market, seen as guardian of growth and entrepreneurship, and the damaging effects of believing in normalcy of self-seeking behaviour – we think this episode brings with it deeper lessons.

At the theoretical level, this paper echoes a host of non-orthodox economists who urge for a change in the foundations of economic theory (*e.g.* Dymski, 2010). The dominance of the New Keynesian thought, and its twin conceptions of (systemic) equilibrium and (representative agent) substantive rationality (alas, conceptions ‘imported’ from New Classical economics), are dangerously fragile and even damaging in episodes of crisis (Colander, 2010). How can one explain the volatility of asset prices, once one assumes that markets are in continuous equilibrium through time, in a random process? Moreover, how can one sustain that this macro equilibrium emerges from optimizing decisions of agents with perfect knowledge, not only about economic fundamentals, but even about the dynamics of markets, such that they do not commit systematic errors? In other words, these perplexities clearly point out that this model is overly unrealistic in the sense defined in this paper,

¹⁴ In a more recent reflection on the crisis, Colander (2010, p. 420) argue for a shift in the institutional incentives of economics profession towards a plurality of models (*viz.*, the models proposed in Colander *et al.*, 2009, p. 259). Nowhere in the paper, though, has he discussed non formal modes of thought.

namely, that a model validity depends on what we know about real economic systems, rather than on dogmas of competitive (and thus efficient) markets.

Orthodox economists certainly would explain the crisis by failures in models of evaluation of risks and in predictions provided by them. Some would blame governments for their ubiquitous failures. Some would also complain that bailouts can jeopardize public belief in market systems (or even in ‘free societies’) by hindering market discipline (i.e. bankruptcy). They will keep on seeking more sophisticated models to provide previsions “fitting” better the data. And they will keep on preaching about the virtues of markets and the sinfulness of regulators (Acemoglu, 2009). From these quarters one should have low expectations of transforming economics because of what Keen (2009) calls ‘inertia of the immovable object of the economic belief’. Thus, the orthodox lessons from the crisis oscillate between recitation of old sermons and marketing of new techniques. We shall not discuss – we not even dare – how changes in the scientific community’s beliefs take place. But economic methodology can be helpful to assess arguments for change the economist toolkit.

The economists from who we have drawn upon in this paper hold converging views that failures of orthodox economic theories can be tracked down to methodological misunderstandings, though methodology is seldom explicitly discussed by those theories. And that is why the influence of Friedman’s essay plays such an important role in our argument. Despite the perception of Friedman as a foe by formalist revolutionaries, or Friedman’s admonitions on the importance of empirical testing of theories, ‘once the assumption do not matter, the cat was out of the methodological bag, the profession was free to go speeding down the formalist road’ (Hands 2009, pp. 150-1). Assumptions of DSGE, efficient markets, representative agents, etc. simply do not matter, only its empirical predictive implications. They are badly unhelpful to analyse even the possibility of systemic crises, as its own supporters admit (see Laider, 2010, p. 41, 46). But during the booms reality seems to authorize this kind of assumption. Moreover, ‘it is all very well to have economic theory dominated by a school of thought with an innate faith in the stability of markets when those markets are forever gaining – whether by growth in the physical economy, or via rising prices in the asset markets. In those circumstances, [heterodox] academic economists can rail about the logical inconsistencies in mainstream economics all they want: they will be, and were, ignored by government, the business community, and most of the public, because their concerns don’t appear to matter.’ (Keen, 2009, p. 2)

The methodological approach endorsed here, that of critical realism, puts forthright emphasis on the importance of considering the ontology of objects under scientific economic

investigation. It argues for considering the nature of objects of interest for economists, like households, firms, markets, production, distribution, trade, money, etc., as they really are in the world we live in, rather than as they could be in an idealized world model. Mäki (1992a) could make an objection to that claim, since by defending realisticness we are, in fact, restraining our view to ‘common-sense realism’ (as opposite to ‘scientific realism’ which contains non-observable entities). However, as we have seen, economists of different persuasions would reply that a model credibility is not divorced from what we know about the real world.

This approach is sceptical about the capability of new formal models to solve the theoretical problems we are faced, even though their ontological compromises are richer than the orthodox ones. And that is so because: (i) a theory have to be translated into a formal language to be a model. In such a translation problems are “stripped out” of most of its non-formalisable aspects; and (ii) creativity and surprise cannot be modelled. It is clear enough that computer simulations, for example, depend on the instructions on how to ascribe/change probability distributions over results, according to rules defined by the programmer. Thus, although they are important and superior to overly simplified worlds of neoclassical models, those models hardly can improve our knowledge of social and economic reality where decision-taking under uncertainty is part of the ontology. Yet those methods need not be abandoned. They can provide heuristic frames for better theories, function as pedagogical devices and in some cases give insights on counterfactuals (Sugden, 2002). But they really must be very carefully handled. And they are very limited tools for prediction, as Keynes said long ago. It is past time to shake off the old prejudice of Lord Kelvin, and embrace less formalism in doing economics. If this path is not chosen, the dismal science may lose by persisting in their ‘physics envy’ and cyclical recantations, just when some ‘past masters’ are rescued from the dustbin. That is to say, by not doing that a large part of economics may, in due course, be doomed to irrelevance.

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