

Criticizing the critique. Some methodological insights into the debate on the state of economic theory in the face of the post 2008 crisis

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Submitted: 26 January 2010. Accepted: 7 July 2010.

Abstract

The aim of this paper is to investigate the current debate on the state of economics from a methodological perspective. We claim that the majority of contributions criticizing modern economics are not based on clear methodological principles and thus many of them are not correct. We show this with respect to such issues as the problem of realisticness of models and their assumptions, the role of mathematics in economics, the way we conceptualize the relation between economics (theory) and economy (empiria), as well as the general problem of comparing theories. In doing so we use the research apparatus taken from the philosophy of science and also we benefit a lot from recent developments in the philosophy of economics. Finally, we show one of the paradoxes of that debate, namely that many critics of economics accuse economists of using the wrong language (mathematics) while they do not use proper language themselves while criticizing economics, namely the apparatus taken from the philosophy of science.

Keywords: methodology of economics, mathematization, assumptions, theories of explanation, global economic crisis

JEL: A11, A20, B40, B41, G01

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1. Introduction

Studies on the interplay between language and reality became increasingly popular in the methodology of social sciences in the 20th century. However, this was not the case in economics. By shifting into a mathematical science in the 2nd half of the 20th century, economics became resilient to those types of criticism which essentially transformed other social sciences in the latter decades of the 20th century, which in turn were stimulated mainly by the emergence of questions regarding the relationship between the narration (language) and the empiria (reality) described in modern philosophy (e.g. in the Wittgensteinian tradition). Since mathematics became the language of economics, neither a linguistic nor a sociological turn could essentially affect the methods of conducting economic research, or the status of the theory of economics itself (Backhouse et al. 1993). Therefore, methodological studies became far less popular in economics than in other social sciences (Mäki 2008). At the time when the problem of relating language to reality was absorbed by sociologists, psychologists, historians, political scientists and others, the general conviction among economists was that these “philosophical” problems do not apply to their discipline and that the research structure of economics successfully describes increasingly larger areas of reality. That, in effect, led to a unique imperialism and expansionism of economics (Lazear 2000).¹

A methodological reflection on the state of economics is important, because it was the language of economics that was mainly responsible for the dramatical transformation of economic theory after World War II. The rapid dissemination of mathematical methods, especially the axiomatic method, led to the emergence of mainstream economics (ME, henceforth) based on general equilibrium theory (GET, henceforth) and the marginalisation of other schools of economic thought which used non-mathematical languages for describing the economy. Thus, the language of mainstream economics, which is frequently extremely abstract and which does not shun highly unrealistic assumptions, became the demarcation line separating not only the “scientific” from “non-scientific” language, but also the “good” theories from “bad theories”. That was possible because language in economics is not merely a conventional code of signs which economists use to communicate with each other, but also part of the paradigm in the Kuhnian sense. Simply put, the language of economics determines, even if implicitly, the range of problems worth pursuing and the analytical toolbox for doing so. Thus, in this paper, the language of economics is treated as indistinguishable from the theoretical framework of economics, even though a precise description of interactions between the two may be an interesting problem on its own account.

Over the past 30 years, changes in economic theory, e.g. the popularization of the game theoretical framework, led to its greater differentiation, although pluralism in economics is still limited.² Research approaches based on the concept of limited rationality, the behavioural approach, chaos theory, complexity, as well as experimental models spread in economics. These approaches use various research methodologies, however, the majority of them use a formal

¹ The starting point of this paper is hence pretty much the same as in our another recent contribution (Hardt 2009a), however, here the focus is precisely on language and the methodological reflection presented here has a deeper philosophical background. Nevertheless, readers interested in the historical background of developments in economics presented here are encouraged to read the above mentioned paper.

² Here we distinguish between plurality and pluralism. The former simply means that we have many various theories (theoretical plurality), while the later denotes the situation in which the practitioners of the discipline treat plurality as something “good”, e.g. due to the fact that the diversity of theories enhances our explanatory capacity.

mathematical language and treat the economic system as being in equilibrium. Furthermore, modelling in terms of general equilibrium still plays a leading role in economics and most of the assumptions of mainstream economics are still not realistic (Hardt 2009a; Hardt 2010a). However, unrealistic assumptions do not always lead to unrealistic models. Consequently, the problem of the language(s) of economics and its inapplicability to reality has become more urgent in recent years.

This problem has additionally gained in importance in the face of the post 2008 global crisis, which not only came unpredicted by mainstream economics, but could also have been to some extent caused by it (Colander et al. 2009). This, in turn, raises the question whether it will be able to propose appropriate remedies. This situation was not only noticed by a group of participants in the public debate, but also by economists themselves, many of whom have recently published important articles on the analysis of the state of economics and the reasons for its crisis. Economists such as P. Krugman, J. Stiglitz, V. Smith, D. Acemoglu and others spoke out in this debate. Although they present different – sometimes even contradictory – opinions on the causes of the crisis and the possible ways in which it affects economic theory, the common element is their conviction that the language of economics is facing an unprecedented challenge, which could significantly affect the shape of economics in the near future. A good example of how different the views are on the direction in which economics should evolve is the dispute between Krugman (2009) and Cochrane (2009). In a somewhat overcritical opinion of the mainstream of economics, Krugman suggested that – both in the area of pure theory and economic policy – the only reasonable direction economics should take is, paradoxically, a return to Keynesianism. Keynes' economic language, as Krugman argues, is not only the only language which allows to understand the current crisis, but is also free of mathematical formalism, behind which economic pseudoknowledge has been hiding until now. However, Cochrane maintains that, if – and here, he agrees with his adversary – inadequate consideration of various frictions, institutional conditions of markets and meanders of human behaviour are the reasons for the failure of economics, then the extent of the use of mathematics should be increased in the future rather than reduced. He argues that economics preferred to occupy itself with market “perfection” rather than “imperfection” because the former was simply easier to model and, therefore, the only true reform should involve the improvement of mathematical tools. The divergence of views regarding the current economic crisis and the role in it of the language of mainstream economics goes far beyond that. For instance, in a letter to Queen Elizabeth, British economists, Besley and Hennessy (2009) apologize for not being able to predict the oncoming crisis and blame their erroneous theory. In contrast, Saint-Paul (2009) believes that the task of economists is not, and should not be, to predict “rare systemic phenomena”, just like the task of good physicians is not to predict the diseases from which humanity will suffer in the future. Finally, according to Eichengreen (2009), the problem is not so much economics, which, as a science, gives many useful tools for analysing the causes of the crisis, but rather economists themselves, who experienced a unique cognitive blur. Therefore, it is the institutional environment and the incentives that economists and financiers face that should be reformed rather than the language in which the theory of economics is practiced. The disputes mentioned above and the differences in opinions confirm that studies on categorising reality through the language of economics in which economic theories are formulated, are not only important, but also highly topical and relevant.

What lacks in the abovementioned contributions on the state of economics is a clear methodological agenda for analyzing the relation between theory and reality. Moreover, some authors commit very basic mistakes while investigating the interplay between language and the realm of empirical phenomena. The most common one is the belief that unrealistic assumptions make economic models unrealistic. However, in the philosophy of science no such a general claim is made. Here we assert that in many situations unrealistic assumptions make the very modelling possible. Moreover, some models based on unrealistic assumptions offer good descriptions of reality. Take for instance the Newtonian physics where we do not find any variables for friction. Does anyone blame Newton for not incorporating them? Or, does anyone accuse Newton of building unrealistic models due to unrealistic assumptions? Rather not, and there is a consensus in the philosophy of science that building models with perfectly realistic assumptions is not a methodological virtue. Moreover, a model with perfectly realistic assumptions would have the same practical value as a map with a scale of 1:1. One of the aims of this paper is, therefore, to shed some light on the issue of assumptions in economic models.

The paper is organized as follows. Section 2 focuses on the current debate on the state of economics in the face of the global crisis. Section 3 starts a methodological analysis of that debate and is concentrated on the issue of assumptions. In section 4 we raise the question of how one can compare different economic theories and decide on their relative merit. Finally, in section 5, we comment on the very subject of criticism, namely the vision of contemporary economic theory its critics have. Section 6 offers some preliminary reflections on the need for a more philosophically oriented debate on the state of modern economics. Conclusions follow.

2. Crisis in economy, crisis in economics?

The current global economic crises has provoked many questions on the validity of contemporary economic theory. Many economists are accusing economics of being a false mathematical science as for instance P. Krugman did while stating that “the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth” (in his article *How Did Economists Get It So Wrong?* published in *The New York Times*). The debate on economics is very intensive and in many respects even emotional and to prove that it only suffices to cite the titles of the most important contributions on that subject, e.g. *The Failure of the Economy & the Economists* (B.M. Friedman in *New York Review of Books*), *The Financial Crisis and the Systematic Failure of the Economics Profession* (D. Colander et al. in *Critical Review*), *The Great Crash of 2008 and the Reform of Economics* (G. Hodgson in *Cambridge Journal of Economics*), *The Crisis of 2008: Structural Lessons for and from Economics* (D. Acemoglu in *Critical Review*), *What Went Wrong with Economics* (a debate in *The Economist*), *Economics Crises* (T. Lux in *Nature Physics*), and many more. In Poland the above debate has also been started recently with some articles in general journals (including the contributions by Hardt, Hockuba, Wiśniewski and Żylicz in *Rzeczpospolita*) as well as academic papers in the special issue of *Studia Ekonomiczne* on the state of economics in face of the global crisis (with contributions by Gradzewicz et al., Hardt, Jasiński, Kwaśnicki, Machaj, Pysz, and Żylicz). Not all of the texts mentioned above have the same importance. The one with high impact is for sure Krugman’s contribution which provoked a vivid

debate and a lot of polemics (we made a brief survey of that debate in the introduction). The main message of his paper is that economics prefers beauty (i.e. formal models) rather than truth. Below we cite the most important parts of his contribution and we underline the most interesting (and controversial) points he makes, i.e.:

(K1) “the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth”

(K2) “the central cause of the profession’s failure was the desire for an all-encompassing, intellectually elegant approach that also gave economists a chance to show off their mathematical prowess”

(K3) “this romanticized and sanitized vision of the economy led most economists to ignore all the things that can go wrong. They turned a blind eye to the limitations of human rationality that often lead to bubbles and busts; to the problems of institutions that run amok; to the imperfections of markets – especially financial markets – that can cause the economy’s operating system to undergo sudden, unpredictable crashes; and to the dangers created when regulators don’t believe in regulation”.

(K4) “That is, they will have to acknowledge the importance of irrational and often unpredictable behaviour, face up to the often idiosyncratic imperfections of markets and accept that an elegant economic ‘theory of everything’ is a long way off”.

The above quotations from Krugman clearly show what we have noticed just earlier, namely that there was (maybe still is) a desire to unify economics on the basis of GET (“an all-encompassing, intellectually elegant approach” in K2). Also that economics has become a mathematical science (K1). Moreover, that having become a mathematical science economics somehow lost contact with reality. Next, he underlines the problem of unrealistic assumptions and claims that due to that fact economics is poorly equipped to analyse markets (K3). In the same vein is the K4 quotation where he focuses on the limited possibility of constructing an economic “theory of everything”, something obvious in the philosophy of science where the term “theory of everything” is just an oxymoron, especially if we are in the Hempel’s (1948) tradition. However, many Krugman’s claims are unclear. Take for instance his opinion reflected in quotations K3 and K4 where we find an implicit claim that unrealistic assumptions are (at least partially) responsible for unrealisticness of economic theories. However, he did not explain why would models with more realistic assumptions have a higher explanatory power. His statements about the role of mathematics in economics are even more unclear. The first message is that mathematics is *per se* something “bad” and responsible for the current crisis in economics. So, the more mathematics we have in economics, the more unrealistic models we have. But again, why does he claim so? Maybe, the truth is that while mathematics (as a whole) is not a problem, there is a problem with mathematics being badly used or with our use of inappropriate mathematical apparatus, as recently stated by Hodgson (2009, p. 1210): “the problem is not necessarily mathematics *per se*, but the obsession with technique over substance”. There is not such a reflection in Krugman’s paper. Paradoxically, one could claim that the mathematics we have in economics is too simple to offer analytical tools for investigating various imperfections of the markets. Consequently, in order to be more realistic we would probably have to incorporate a more advanced mathematical apparatus. We will come back to these issues later in section 5.

Another interesting paper on the crises in economics is the one by Collander et al. (2009). That paper is of a more scientific character than Krugman’s contribution and it contains more

methodologically grounded analysis of the current state of economic theory. Collander's paper is widely cited and the majority of authors writing on the impact of the crisis on economics make direct references to Collander's work.³ The main message of his contribution is that we have a systematic failure of the economics profession. That failure is due to the inability of current models to describe the reality, namely to answer "why?" questions rather than simply the "what?" question. In other words, economics does not offer good explanations – "In our hour of greatest need, societies around the world are left to grope in the dark without a theory. That, to us, is a *systemic failure of the economics profession*" (Colander et. al 2009, 250). We give more quotations below to present Collander's insights into the current state of economics (we underline the most important parts), i.e.:

(C1) "The failure of economists to anticipate and model the financial crisis has deep methodological roots" (p. 251).

(C2) "Researchers have an ethical responsibility to point out to the public when the tools that they developed are misused" (p. 252).

(C3) "[...] analysis of these issues [of how market coordination is achieved] would require a different type of mathematics than that which is generally used in most prominent economic models" (p. 252).

(C4) "The representative-agent assumption in many current models in macroeconomics (including macro finance) means that modellers subscribe to the most extreme form of conceptual reductionism [...]: By assumption, all concepts applicable to the macro sphere (i.e. the economy or its financial system) are fully reduced to concepts and knowledge in the lower-level domain of the individual agent" (p. 257).

(C5) "Once one acknowledges the importance of empirically based behavioural micro foundations and the heterogeneity of actors, a rich spectrum of new models becomes available" (p. 259).

(C6) "This occurred because such research was incompatible with the premise of the rational representative agent, which had come to be thought the only allowable model. That belief made the economics profession blind to the role of interactions and connections between actors" (p. 263).

Colander's starting point is slightly different than Krugman's, since Colander opens his paper with a clear C1 statement. Thus, methodology matters. Moreover, not only the state of economics is investigated but also the role of economists in developing modern economic theories (and policy recommendation) is analyzed (C2). As far as today's issue is mainly with macroeconomics, Colander focuses on the relation between micro and macro. In his opinion a big mistake of modern macroeconomics is its conceptual reductionism, namely the assumption that the macro world is built of micro phenomena, and that there is a clear, deterministic relation between them. That is contrary to the long lasting antireductionist tradition in neoclassical economics built partially on the work of Marshall who in his *Principles* states the following: "As a cathedral is something more than the stones of which it is made, as a person is something more than a series of thoughts and feelings, so the life of society is something more than the sum of the lives of its individual members" (Book I, Charter I, Paragraph VI). Despite the fact that we had a strong movement in economics in favour of reductionism (the so-called microfoundations for macrotheory) in the 70's, due partially to the implications of Sonnenschein-Mantel-Debreu theorem, economists once again are becoming more cautious when it comes to the possibility of reducing macro to micro. However,

³ "Access statistics" for that work are significant (see, e.g. <http://econpapers.repec.org/paper/kudkuiedp/0903.htm>).

due to a strong GET unificationist project macroeconomics has subscribed to the conceptual reductionism described in C4. Colander's point made in C5 requires also a comment. Here an implicit claim is that more realistic assumptions would result in new (and probably better) models. However, in the entire paper Colander does not define precisely what "new" (or better) models mean. There is only a very general conviction that better models (and economics) should be "closer" to reality. Finally, in C6 Colander claims that systemic failure of economics is the result of unifying economics on the basis of rational representative agent. Although Colander's paper has a greater methodological depth than Krugman's, still some methodological issues are neglected. They are disregarded not only in the two above described papers, but also in other studies mentioned at the beginning. We list them below and in the subsequent sections we offer a deeper analysis.

First, there is the issue of assumptions. Both papers share the view that models with more realistic assumptions are simply better models (see K3 and C5). Second, there is an implicit belief that "new economics", based on more realistic assumptions, would be better than the current theory. However, both authors do not define what "better" means. Third, in both papers there are clear statements that the current state of economics results mainly from theoretical unification based on the GET approach (see K2 and C6), also in both papers there is a desire to start the process of ontological unification in economics, namely a unification based on ontologically important concepts (e.g. C5)⁴. Last, but not least, the issue of mathematization⁵ of economics is raised and should be analyzed jointly with the topic of theoretical unification and unrealistic assumptions, since the mathematization is a by-product of the two.

3. Realistic models with unrealistic assumptions?

What is often raised in the ongoing debate on the state of economics is the opinion that models we have are unrealistic due to unrealistic assumptions and hence the way to improve economic modelling is by making our assumptions more realistic. That is contrary, for instance, to the main message from Friedman's (1953) (F53, henceforth) influential paper where he clearly stated that "economic theories should not be judged by their assumptions but by their predictive implications [...], the unrealisticness of the assumptions of a theory is no reason for complaint or worry about the theory". It is worth defining more precisely here what we mean by the unrealisticness of the assumptions of a theory. A good starting point here is to use the deductive-nomological model of explanation where unrealisticness of the assumptions is a characteristic of the explanans.⁶ In other words, the more unrealistic our theories are, the more sentences are

⁴ Theoretical unification is understood here as "a matter of unifying previously separate theories by means of one that possesses all (or most of) their explanatory content" (Marchionni 2009, p. 13). Here we treat ontological unification as "[...] a matter of discovering a real unity in the world by way of redescribing apparently independent and diverse phenomena as manifestations (outcomes, phases, forms, aspects) of one and the same small number of entities, causes, mechanisms, processes" (Marchionni, Mäki 2009, p. 190).

⁵ The term "mathematization of economics" has a narrower scope than "formalization of economics", since formalization has three meanings, i.e. axiomatisation, mathematization and a third category that can be termed methodological formalization (Backhouse 1998, p. 1848). In this paper we refer only to mathematization due to the general opinion that formalization equals mathematization, however, we should have in mind the above Backhouse's definition.

⁶ The explanans is defined as the set of sentences (explaining items) responsible for answering the questions present in the explanandum (the set of sentences describing the phenomena to be explained) (Hempel et al. 1948, pp. 136–137).

excluded from the explanans. Consequently, we use a restricted set of sentences for explaining the phenomena described in the explanandum. However, the importance of particular sentences in the explanans is not the same. Some sentences explain more, some less. Therefore, we may have two theories both with five elements in the explanans but with different levels of realisticness, e.g. theory 1 (T1) with 3 ontologically relevant sentences and 2 irrelevant is more realistic (in terms of assumptions) than theory 2 (T2) with 2 ontologically relevant sentences and 3 irrelevant. Therefore, the above claim – the more unrealistic our theories are, the more sentences are excluded from the explanans, holds for theories that include sentences in the explanans with the same level of ontological importance. That is coherent with the very meaning of realisticness, namely the compatibility of theories (assumptions) and reality. The above is nicely explained by Friedman who claims that “Most phenomena are driven by a very few central forces. What a good theory does is to simplify, it pulls the central forces and gets rid of the rest” (Snowdon, Vane 1997, p. 196). The simplification means isolation, namely we isolate our models from the processes we treat as unimportant.⁷ That is the case of Newtonian mechanics where the very basic model is isolated from the influence of frictions. For that reason the question we ask should not be – why we neglect so many issues in our models, but we should ask whether we exclude unimportant issues and include the substantial ones. In other words, the issue is not the method of isolation per se but the question what we are abstracting from. Models should be simple, with many isolations (i.e. unrealistic assumption) but with many crucial forces, and as it was observed by R. Solow “the very complexity of real life ... [is what] makes simple models so necessary” (Solow 2001, p. 111). Paraphrasing Solow one could say that we need unrealistic assumptions in order to cope with the complexity.

The reader of this article should not have an impression that the more simple (unrealistic) a given model is, the better explanations it offers. That claim is often described as a strong version of F53 thesis and it must be mistaken (see, e.g. Mäki 2009a, p. 95). The unrealisticness should not be treated as a methodological virtue despite the fact that sometimes violating the (whole) truth may be desirable, e.g. in assuming that all firms are maximizing profits. If we would agree with a strong version of F53 thesis, then, for instance, instead of choosing theories with firms maximizing their profits we should prefer theories with firms maximizing the loses (the more unrealistic assumption than the one of profit maximization, since firms generally try to increase profits). Therefore, that assumption of loses maximization does not have any epistemic value and hence the strong version of F53 thesis should be questioned.

Summing-up the above discussion, we subscribe to the following statement by Mäki: “There is no general problem with unrealistic models with unrealistic assumptions or the method of isolation by idealization. This means that the locus of appropriate criticism of any chunk of economics does not mostly lie at the level of general philosophical description of method, but rather at the level of how the method is used and how its use is constrained and what results it produces” (Mäki 2009b, p. 93). Therefore, one cannot claim that economic models poorly describe reality due to unrealistic assumptions. The issue is more complex and requires more methodological insights as the ones offered above. In that sense, some authors claiming that economics went wrong due (mainly) to unrealistic assumptions are simply not right.

⁷ For the simplification of our arguments we treat isolation as idealization.

4. Comparing the incomparable. Problems in appraising economic theories

The methodological issues described above become even more complex if we focus on the problem of comparing different theories. We do it constantly and we often say that a given theory is better than another one, e.g. Krugman and others cited above claim that more realistic theories are better than the unrealistic ones. However, there is no universal measure of the “goodness” (or “power”) of theories, e.g. in Hempel’s (1948) approach “[...] the systemic power of a theory T will be reflected in the ratio of the amount of information derivable by means of T to the amount of initial information required for that derivation” (164). Therefore, the more powerful a given theory is, the more kinds of events it explains while trying to restrict the size of its explanans. The virtue of explaining much by little emerges and that forms the methodological foundation for the idea of explanatory unification, including the GET based unification in economics. In other words, explanations reduce to the fundamental core and hence in writing the casual histories of events we reach the basic cause or mechanism, e.g. the principle of economizing on transaction costs in transaction cost economics. In this view unification simply equals explanation, while many others claim that unification and explanation are separate issues. For instance, Morrison (2000) claims that there is a trade-off between unification and precision (of explanations), i.e. the more a given theory is unified, the less precise explanations it offers. What is rather clear in the modern philosophy of science is that the issue of unification and explanation is rather complex and the two should be studied separately. Moreover, the explanatory power can have many dimensions, e.g. the two (precision and scope) mentioned above. Consequently, there is no objective and “aggregated” measure of explanatory power. What dimension of explanatory power is more important is often the sole decision of the researcher who is driven by pragmatic reasons, e.g. if he is interested in precision, he will choose theories offering more precise descriptions of the explanans. As a result, we do not have a good tool for comparing different theories. In that context, comparisons of different economic theories that appear in many papers on the state of economics are not methodologically grounded since comparisons should be made in precisely defined dimensions (e.g. precision, or the familiarity of the explanans) rather than in general terms. General comparisons are simply methodologically wrong.

5. Criticizing economics, but which economics?

Last, but not least, the issue of the growing popularity of mathematics in economics is worth commenting on. According to Krugman the mathematization is *per se* a problem and the reason for mistaking beauty for truth. The above discussion proves that such a strong criticism is not methodologically grounded and that the problem is not mathematics as such but the way we use mathematical tools when isolating the explanans and the explanandum. The reason for the emergence of non-realistic models is not the formal mathematical language alone. The problem is not the complexity of mathematics but the wrong use of the method of idealization and the exclusion of ontologically important elements from the explanans. Also the problem is that we often use wrong mathematical methods as it is clearly stated in C3. However, the problem with

mathematics is that its popularity makes economists more likely to include in the explanans the elements that can be mathematically modelled rather than the elements impossible to operationalize. In that sense mathematics is partially responsible for excluding ontologically important concepts from economic theories. The above is claimed for instance by T. Lawson who states the following: “Social reality, in other words, is of nature that is significantly at variance with the closed systems of isolated atoms that would guarantee the conditions of mathematical deductivist modelling” (2009, p. 765). However, what is disputable in the above quotation is a strong claim that mathematical modelling is of a deductivist nature and that a closed systems of isolated atoms is a necessary condition for this kind of modelling. If you are to take the mathematics from the original work of Arrow-Debreu, then Lawson is probably right, but if you take the game theory or the mathematical theory of chaos, then mathematical modelling is not necessarily deductivist and it is not required for the system to be a closed one. Consequently, the issue of mathematization is rather complex but for sure it is not true that the mathematics alone “led the economics astray”.

What should be added here is the issue of the kind of economics that is under critique. While reading Krugman’s contribution one can have the impression that his vision of economics is somehow one-dimensional, as he does not mention such branches of modern mainstream economics as behavioural economics, transaction cost economics, new institutional economics, or experimental economics. The same holds for Colander’s paper, but to a lesser extent. Krugman often equates economics to neoclassical economics, however, the modern mainstream economics is no longer the neoclassical one only. Contemporary mainstream is more diversified with approaches containing realistic assumptions (e.g. behavioral economics), approaches with solid empirical studies (e.g. new institutional economics), or branches based on the grounds of ontological unification (e.g. transaction costs economics). The growing diversification of economics led Colander to declare in 2000 that “the term neoclassical economics [is] dead” (Colander 2000, p. 127). Consequently, the critique of economics in the face of post-2008 crisis should be treated as directed at the neoclassical economics and not on the modern mainstream as a whole. Moreover, some authors also have a text book picture of neoclassicism (e.g. Keen 2002). Take here for instance the claims that economics does not offer insightful comments on the issue of managers realizing their own interests rather than the ones of the owners. However, in many works from the 60’s we can find models using neoclassical apparatus explaining the divergence of behavior between managers and owners (e.g. Alchian 1965).

Last, but not least, the issue of ontological unification should be raised. At the background of Krugman’s K1 statement there is an assumption that economics prefers beauty (here: formalism) than truth, i.e. theories with ontologically important explaining items but with limited ability of being “translated” into formal, mathematical language. However, in contemporary economics we find many examples of theories driven by the desire to “open the black box, and displaying the secret, internal machinery that governs the social event that is the object of our enquiry” (Rios 2004, p. 52). Take here the example of transaction cost economics where the secret is the fundamental message of TCE, i.e. that the emerging organizational forms result from the existence of transaction costs. In other words, trading parties choose the contracting schema that is characterised by the lowest possible level of transaction costs. Moreover, the concept of TCs is,

according to Williamson, a one with high ontic importance,⁸ i.e. transaction costs are crucial to the functioning of the market system. If you exclude transaction costs, you cannot see; and “the element [here: transaction costs] to be added is not just casually influential, it is necessary for the functioning of the system” (Mäki 2001, p. 381). This is coherent with the Coasean statement that “there were costs of making transactions in a market economy and that it was necessary to incorporate them in the analysis” (Coase 1993, p. 46). The above is in line with Coase’s essentialist spirit, namely his claim that the aim of theorizing is “to get to the essence of what [is] going on in the economic system” (Coase 1988a, 68). Coase and Williamson do not claim, however, that the ontic importance of transaction costs means that the whole economic theory should now be built around that concept. They accept plurality, namely that we may have many projects of ontological unification that try to identify important explaining items: “No doubt other factors should be also added” (Coase 1988b, p. 30). In modern economics we can find some examples of such projects of unification, e.g. Richardson’s approach of incorporating considerations of information into neoclassical economics and his treatment of information as “one of the essential elements” (Richardson 1960, p. 23), and even more than that, as can be seen in his assertion that “by neglecting the whole problem of information, the perfect competition model condemns itself not only to unrealism but to inadequacy even as a hypothetical system” (Richardson 1960, p. 69). The above discussion of TCE and Richardson’s approach should be treated just as an illustration of ontological unifications and not as the claim that these two approaches have played a decisive role in the recent changes in economic theory.⁹

The above shows us that economics is searching not only for beauty, but also for truth. Mechanistic explanations are important in many branches of contemporary economics. The role of that kind of explanations is underlined in the causal mechanical model of explanations where “Casual processes, casual interactions, and casual laws provide the mechanisms by which the world works; to understand “why” certain things happen, we need to see “how” they are produced by these mechanisms” (Salmon 1984, p. 132). More and more social philosophers agree that mechanical explanations should be widely used in social sciences, including economics (cf. Woodward 2009). So again, the critics of modern economics underestimate the role casual mechanical models play in economics. Moreover, they somehow criticize only the language of economics (mathematics) and not the essence of a given model which is formulated using mathematical terms. Finally, one may even conclude that many critics of economics do not offer an adequate critique of mainstream economics but rather a critique of their own vision of economics which might have been true in the 60’s or the 70’s but which is incorrect nowadays.

⁸ It is important to define precisely what we are conceptualizing here under the notion of ontic importance. First, ontic differs from ontological. Ontic is related to the particular characteristics of a given thing that are essential for its existence. Also, we may talk about ontic elements of a particular scientific theory, e.g. in physics atoms are ontic. Therefore, when we say that atoms are objects with high ontic importance in physics we just claim that they are essential in that particular area of scientific investigation. Even more, they are not only essential but also ontically indispensable. Thus, claiming that atoms are ontic elements of physics does not mean that we are asking (or should ask) more fundamental questions about their existence, precisely ontological questions. In the case of TCE the fact that transaction costs are ontically important results from the fact that the central unit of analysis is the transaction and the cost of transacting is an important characteristic of the later.

⁹ Readers interested in the role TCE played in the developments of modern economics are encouraged to read, e.g. Hardt (2009b; 2010b) or Klaes (2000a; 2000b).

6. A plea for more methodological studies in economics

The above discussion clearly shows that what is lacking in the debate on the state of modern economics is a well defined methodological apparatus for investigating such issues as explanatory power, realisticness of assumptions, and the role of idealizations we make in building economic models. That is why the debate on the state of economics is so fragmented and lacking in conclusive outcome. Moreover, quite often authors with extensive experience in empirical research have very little competence in investigating the language in which they describe reality. It turns out that investigating the descriptions of reality is often more difficult than analyzing the reality *per se*, however, looking at economics from the perspective of the philosophy of science is unavoidable if we want to understand what is really going on.

Furthermore, not only the language should be studied but economic theories in their relation to reality as well. This is particularly important nowadays when many postmodernists would argue that there is not any connection between the real world and descriptions thereof. In that sense they attack the whole philosophical tradition of constructing explanations based on the logical relation between the explanans and the explanandum (e.g. D. McCloskey's rhetorical analysis of economics). The research perspective we are taking is different – it is realistic. Thus we claim that language can describe the true and objective reality, and hence that Hempel's model is generally right. In claiming that we subscribe to the Wittgensteinian thesis that “the questions that we raise and our doubts depend on the fact that some propositions are exempt from doubt” (Wittgenstein 1969, §341). The propositions that are exempt from doubt are “these central elements [...] taken as presuppositions, as relatively absolute absolutes, and, as such, they become, themselves, the constraints (the constitution) within which the scientific discourse is conducted” (Buchanan 1991, pp. 13–14). The search for the absolute of absolutes should define the future research practice of economics. Consequently, more studies in economic ontology are needed. Ontology is defined here as “the study of being as being, of the most general characteristics of all things, whether actual, possible, or even impossible, whether physical, mental, or social” (Mäki 2001, p. 7). The current debate on the state of economics should inspire us to a more in-depth reflection about the most general and fundamental mechanisms governing the markets. It could be that a modern economics having become so diversified somehow lost the ability to pose fundamental questions. It is to be hoped that the debate on the state of economics would make economists more sensitive to methodological questions.

7. Conclusions

The aim of this paper was to investigate the current debate on the state of economics in the face of the 2008–2009 global crisis. We have shown that the debate itself is very fragmented and not firmly rooted in methodological ground. Thus the conclusions participants are arriving at are often wrong, e.g. the one that unrealistic assumptions alone make economic models unrealistic. Moreover, and paradoxically, although the critics of economics accuse the queen of social sciences of using too much mathematics and hence of using the wrong language, the critics themselves apply an inadequate language while writing on economics since the majority of them simply do not employ the approach of the philosophy of science. Writing conclusively on the state of economics without

knowing the recent developments in the philosophy of science is nearly impossible. Consequently, probably due to the above-described lack of interest in the philosophy of science some papers are overly critical in their descriptions of economics as merely a science fallen astray. The criticism presented in methodologically richer papers, e.g. Colander's contribution, is more nuanced and focused on particular characteristics of modern economics. However, economists still have a lot of problems while debating the issues of assumptions, explanations, explanatory power, realisticness of models, and more generally the relation between economics (theory) and economy (empiria). More in-depth studies on these issues are advisable as much for their own sake as in order to equip economists with appropriate tools to deal with criticism that economics is facing.

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Acknowledgements

Some parts of this work were written while I was a visitor at the Trends and Tensions in Intellectual Integration (TINT) project at the University of Helsinki. I am grateful for their hospitality and stimulating discussions; particularly I would like to thank Uskali Mäki for his insightful comments and encouragements. Also I would like to thank Juliusz Jabłęcki for his remarks on the debate on the financial crisis. I would like to thank two anonymous referees for their comments which significantly helped to improve the paper. This research was partially financed by a research grant from the Ministry of Science and Higher Education (grant no. N N112 127936).

