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**SOME METHODOLOGICAL ASPECTS OF THE CONTROVERSY
BETWEEN MAINSTREAM ECONOMICS AND
INSTITUTIONALISM**

BY

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SOME METHODOLOGICAL ASPECTS OF THE CONTROVERSY BETWEEN MAINSTREAM ECONOMICS AND INSTITUTIONALISM

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Mainstream economics has been running the gauntlet of adverse criticism for decades. These critiques claim as a message of central importance that mainstream economics has lost its relevance as for understanding reality. By making a brief comparison between the methodological strategies of the main stream and institutional economics I suggest that the firm demarcation between the streams stems from the difference between their methodologies. Its peculiar interest directed mainstream economics to take a unique methodological path and consequently the adherents have not been able to be on the lookout for certain facets of socio-economic reality. However, the chosen path, the axiomatic-deductive strategy proved to be an appropriate method for identifying economic laws. This claim is justified even by some recent efforts of new institutional economics. In order to support the conversation between the schools I highlight some causes that currently make it impossible to start a rational discourse.

Keywords: mainstream economics, institutional economics, methodology of economics, isolation, homo oeconomicus

JEL codes: B13, B15, B41, C12

It is one of the triumphs of the human that he can know a thing and still not believe it.

(John Steinbeck: *East of Eden*)

The problem: a “différance” at the methodological level

Mainstream economics has been running the gauntlet of adverse criticism for decades. These critiques claim as a message of central importance that mainstream economics has lost its relevance as for understanding reality. But can this claim be true? It is a commonplace that the

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ultimate purpose of science is to understand and explain reality. Rejecting its connections with reality is the same as rejecting the scientific status of mainstream economics. Is it possible that its often-criticised excessive formalism has derailed mainstream economics and modern macroeconomics as its subset from the intention of understanding reality?

The case of physics is not in support of this idea. Physics is commonly believed to have a need for mathematics. It is well-known that economics regarded physics as its ideal when having defined its purposes and methods. For 19th century physics realist purposes were evident, since the famous Einstein–Bohr debate that later ended up in the triumph of instrumentalism did not even occur then. It is only the problems around the interpretation of quantum mechanics as a description of reality that led to an inevitable abandoning of the realist purposes. For economics these developments were of external character. Even though economics also became affected by Friedman’s positivist paper and ambitions, these influences only emerged in a period when the worldview was complete. However, even in the case of an explicit espousal of instrumentalism such as F53 it is still debated whether a discipline in its entirety can permanently be distracted from the purpose of understanding socio-economic reality. All the more because even Friedman himself was not consistent in his adherence to instrumentalism, as it is highlighted by some recent studies (Mäki, 2009).

Simultaneously, institutional economics having shown up as an opposite of the main stream chose a much more evident way of understanding reality. No matter how strong interests the proponents of institutionalism have shown in methodology, this curiosity is directed towards finding the answers to their own methodological problems. This development can hardly strengthen the communication between the currents. Even though the approach applied by the arch-rival is thoroughly analysed, these investigations are always built on the methodological foundations the institutionalists prefer. Admittedly, the founding masters of institutional economics paid a profound attention to methodological considerations, but a reconciliation between the schools is still hindered by the institutionalists’ lack of any willingness to tolerate the epistemological strategy underlying mainstream economics.¹

¹ In this respect, John Dewey’s methodological recommendations are most interesting. Dewey (1941, p. 179) rejected the whole body of correspondence theory of truth for its inherent subjective–objective distinction of fundamental importance. As a physical heritage, correspondence theory serves as the very fundament of mainstream economics. According to his critique, mainstream theorists neglect the fact that even the object of the analysis (i.e. society itself) also changes objectively (Dewey, 1938, p. 161). Instead, he set out his own methodological pole (conjugate correlation), according to which both sides of the relation take part in experience (Bush, 1993, p. 65–68). However, it is not justified to claim that the recognition of this peculiarity is completely missed by mainstream economics. Being put in a formalized way and on individualistic methodological grounds, the point of the Lucas-critique is to underline the ability of macro-economies to change and adapt to economic

By making a bird's eye comparison between the methodological strategies of mainstream economics and institutionalism I suggest that the firm demarcation between the currents stems from the difference between their methodologies. Its peculiar interest directed mainstream economics to take a unique methodological path and consequently adherents have not been able to be on the lookout for certain facets of socio-economic reality. However, the chosen path, the axiomatic-deductive strategy proved to be an appropriate method for identifying economic laws. This claim is justified even by some recent efforts of new institutional economics. In order to support the conversation between the schools I highlight some causes that currently make it impossible to start a rational discourse.

The institutional strategy: the methodology of understanding unique constellations

The German historical school led by Gustav Schmoller exerted the strongest influence on institutional economics. The paths of mainstream economics and institutionalism diverged as early as in the 19th century as an immediate result of the *Methodenstreit*. It is exactly the time when the conflict between the currents got detached from the methodological considerations supposed to be sterile (Caldwell, 1990, p. 65). Consequently, it was extremely difficult for modern economic methodology to justify its existence as a sub-discipline, often giving the impression as if activity itself was the evidence of success. In this debate, Menger defended the stance of the later neoclassical orthodoxy as the right method of economic theorizing. Soon afterwards, Max Weber (1949) joined him, who clarified for the social sciences the exact scope of the formalized models of optimization built on individual rationality better than anyone had ever done before. However, the opposition of the camps soon became antagonistic, despite the fact that Austrian economics represented by Menger showed in his time an interest in institutions and evolutionary logic far more intense than the later neoclassical orthodoxy did (Jackson, 2009, p. 54–55). Contrary to formal neoclassical theorizing underpinned by idealisation-based concepts, Schmoller suggested comparisons between certain unique constellations and emphasized the evolutionary character of both the development of institutions and economic processes. Generally speaking, Schmoller underlined the importance of the historic and cultural aspects of economic research (Richter, 1996).

policy. An endless process of change may make the search for timeless economic laws unreasonable, of course. Perhaps it is this idea that characterizes the stance of institutionalists most clearly and that gives them a ground for rejecting mainstream economics.

Early institutional economics openly admitted this relationship by eulogizing Schmoller for his strong interest in particular data and minutiae through which some refined generalizations much more sensitive to contextual details can be achieved (Veblen, 1901, p. 79–81). From the German historical school, institutionalists inherited the special worldview, according to which every society has its own characteristic institutions. It is strongly believed that taking the evolution of such institutions into account, that is, studying economies in their own social and historical contexts is the only research strategy that can result in genuine achievements in economics. Through these efforts, institutional economics defines itself as the discipline of reality² (Parada, 2001, p. 48). At the same time, mainstream economics was deprived of all its relevance. Its rationality postulate was supposed to be dubious and strange and its abstract worldview that is built on perfect competition whilst neglecting any historical-cultural determinants was completely rejected.

Institutional methodology is a description of the preferred way of looking at socio-economic life. Here an economist is given a lot of flexible principles that can be put into practice in various ways. The evolution of the schools raised fundamentally different methodological problems. Mainstream economics shows great concern about both the way of connecting with reality and the importance of this connection. The issue of relevance arose in a special way. As time was passing by, justifying the claim that general economic laws can convey some relevant messages about any particular macro-economies became a special challenge. At the same time, for institutional approach showing a more intense interest in the fine details of socio-economic reality, the problem has been to find the generalizable content in them that can be referred to as theory at all. Abstraction is also a major concern for institutionalism. It is necessary to find the appropriate level of isolation that can help us to separate the important from the unimportant in order that the models could preserve a considerable level of descriptive quality. This constraint caused a lot of problems especially for old institutional economics. Nowadays new institutional economics, as it is highlighted below, is at peace with a number of epistemological achievements provided by mainstream economics.

Today the dialogue between the main stream and institutionalism is an exception rather than a rule. Any exchange of ideas is further hindered by the fact that for institutionalists questioning the scientific status of mainstream economics seems to be a compulsory act. A thunderous

² In goes without saying that not only has institutional economics made efforts to unveil a multitude of mechanisms of the complex causal structure, but it has also been engaged more deeply in analysing and supporting operative economic policy (Yonay, 1998, p. 63).

proclamation to announce in order to give proof of one's affiliation.³ Manoeuvring at a lower level of generalization, old institutional economics did not establish a coherent and well-formalized theoretical system, which has been a standard for neoclassical orthodoxy from the very beginning. This is not a failure, of course, only a peculiarity that comes from a unique interest and some methodological decisions. It is undoubted that institutionalism applies more de-idealized concepts— notions that are created at a lower level of abstraction, so they can have a higher degree of direct descriptive relevance. However, the difference is more fundamental. Upon its birth, institutional economics did not show any interest in abstract deductive systems. Instead, institutionalists tried to capture the historical trends experienced in real societies and the country-specific social phenomena emerging under the influence of complex causal structures (Veblen, 1898). It is much more than a simple interest in social determinants⁴ (i.e. the institutions) that are well beyond the scope of mainstream models.⁵ Members of the old institutionalist camp positioned their interests to another area. For them, individual and social behaviour was not controlled by timeless economic laws, so they did not need formalism which proved to be very effective in discovering the consequences of premises and axioms. Even though these effects can hardly be approached through less formalized methods, institutionalists categorically rejected formalism (Rutherford, 1994, p. 9). On the basis of their concepts, they made efforts to understand the particularities rather than the general features. The ultimate purpose was to understand historically and socially determined constellations and evolutionary processes tied to unique places and periods. Such an ambition evidently requires a methodology radically different from mainstream formalism.

Whilst mainstream economics has evolved under the influence of physics, old institutional economics has shaped by Darwinian evolutionary logic (Hodgson, 1998, p. 168). Even though Darwinian evolution does not provide a full explanation of macro-social phenomena, it can still serve as an ontology and as a general theoretical framework on the basis of which the emergence and evolution of social institutions can be described. On this basis, it became possible to break

³ At the same time, mainstream economics tends to apply passive resistance. In his famous *Economics*, Paul Samuelson did not even devote a single word to institutionalist achievements (Tsuru, 1993, p. 59). As it is highlighted below, we can easily find examples for an open attitude towards institutionalism.

⁴ Habits, norms, rules and their evolution, and the like. These factors jointly control human behaviour, the working and the evolution of real societies and economic sub-systems. By today, new institutional economics has arranged these social institutions into a complex, multi-level hierarchy (Williamson, 2000).

⁵ Mainstream economics put these factors under the care of sociology and psychology (Keizer, 2007, p. 10) or even social psychology. Consequently, interdisciplinarity has different meanings for both of the schools. Mainstream economics expects related branches of knowledge to succeed in exploring their own territories, so answering certain questions is transferred to these disciplines. The institutionalist purpose is an active utilisation of the achievements of the related fields (Brousseau – Glachant, 2008, p. 5).

up with the worldview of sciences put on a Newtonian footing, where world has been conceived as of static nature (Atkinson, 1998, p. 33–34). Hodgson (2004, p. 143–153) highlights that Darwinian evolutionary logic was essential for Veblen for the possibility of sequential causal analysis or cumulative causal sequence. The idea of Darwinian evolution is a framework that can serve as a background without forcing theorists to apply excessive reductionism and abstraction (Chavance, 2009, p. 72). The evolution of a particular organisation (either a biological or a social organisation or institution) is governed by general laws, whilst this evolutionary process is closely embedded in a context that can be uniquely characterized. Thus for old institutional economics both general and specific laws and law-like tendencies are necessary in order that the preferred path of understanding could be taken. Whilst in mainstream economics the description of price dynamics is put on the well-known physicalist footing, i.e. the formalisation of the interplay between opposing market forces, price in institutionalism is conceived as determined by social institutions. Price is a convention that can reflect even sectoral differences and its dynamics is affected by routine as well as by the act of valuation. Collecting and processing information and communication of market agents also have effects to be taken into account. Similar effects are assigned to the forms and norms of the concentration of power inside and between economic units; to the legal rules of market interaction or even to political arrangements that establish both the source of discretion over market processes in the social hierarchy and the set of privileged social groups (Tool, 2003). Price itself and expectations are not the outcomes of impersonal market forces, but rather the results of a calculation process influenced by innumerable social institutions. Any explanation of pricing cannot be complete unless institutions affecting prices are all taken into consideration.

Therefore, the key idea of institutionalism is leaving abstractness for the sake of concreteness. According to this worldview, general theories have only limited descriptive capacities for leaving out a large number of determinants underlying particular macro-social constellations. That is, generality is a major concern. It is only the understanding of unique constellations that can catapult us towards a theory that describes the working of a particular institution or class of institutions. The ultimate purpose is to understand the evolution of societies and economies, which is still specific and hence lacks any general validity independent of time and space.⁶ The

⁶ In order to explain unique phenomena, institutional economics has recently started paying attention to Grounded Theory elaborated in sociology (Yefimov, 2004, p. 1–2). Grounded Theory is conceived as an appropriate mix of description and theorizing. In this framework, empirical research is not emphasized for the sake of testing the hypotheses outlined through abductive reasoning; rather, data are processed in order to *ground* the act of creating

desire to understand unique conditions naturally deviated institutionalism from the mainstream worldview of disregarding particular (social and individual) features.

Some methodological compromises in institutionalism

For institutionalists, a completely different way of connecting with reality became the standard. Trying to answer a different set of questions, they could not approve of the extremely abstract mainstream theories claiming any form of realism or relevance (Vromen, 1995, p. 13–15). It is also true that institutionalists could only turn their attention to unique constellations at the cost of giving up any efforts towards universality. It is exactly this particularity that can help us to realize: mainstream economics and institutionalism have never been rivals. The camps have different interests; and look for answers to different questions that require different approaches. Both traditions entertain different ideas about science and hence believe in different ways of approaching reality (Mirowski, 1987).

However, some modern efforts in institutionalism have had a clear tendency to adopt certain elements of mainstream methodology (Yefimov, 2004), whilst leaving the constrained approach of neoclassical orthodoxy behind is still of primary importance (Brousseau – Glachant, 2008, p. 4). Simultaneously, some institutionalist achievements have considerably entered into the mainstream worldview (Keizer, 2007, p. 5). Conceiving macro-systems as deprived of social institutions is no longer accepted even in the main stream.⁷ Moreover, institutions are apprehended as a structure that can enhance efficiency⁸ (the same idea naturally occurs in new institutional economics; see Brousseau – Glachant, 2008, p. 5 and Furubotn – Richter, 2005, p. 20–21).

concepts, theories and hypotheses. So, Grounded Theory as a methodology refers to data-based theorizing. This way of hypothesizing contrasted to the mainstream theorizing strategy is supposed to be deductive and rather speculative—even if this contrast seems to be unreasonably sharp (Glaser – Straus, 1967; Martin – Turner, 1986).⁷ It is far more interesting that Lucas (1981, p. 9) designates institutionalist Wesley Mitchell as one of those who inspired his theory. The recurrent character of business cycles; or the characteristic that how economic agents try to react to the nominal signs of real changes; and the errors of this adaptation are the factors that trigger off business cycles—these are the elements that Lucas found in the vast pre-Keynesian literature. Lucas revised this unformalized framework and put it under the label of “signal processing”.

⁸ One can hardly neglect the theory of political business cycles (Nordhaus, 1975) when summarizing both the shared interests of neoclassical orthodoxy and institutional economics on one hand and the institutionalist influences on mainstream economics on the other hand. It is one of the most important conclusions of the argumentation presented here that the traditions under scrutiny do not primarily differ in terms of questions but it is rather in methodology, epistemological strategy and ontology that fundamental discrepancies can be found. The questions that are of central importance for institutionalists are neglected in mainstream economics, since on account of the preferred methodology it is more palpable not to pay attention to them. If so, it has been carried out within the boundaries of their own methodology. It is erroneous to infer from mainstream methodology that theorists deny the existence of the causal mechanisms they neglect.

The heterogeneity of research directions naturally affects methodology in the new institutional case (Ménard, 2001). There are some efforts in new institutional economics that echo the formalism of neoclassical orthodoxy (e.g. game theory or agency theory). However, there are attempts that strongly insist on verbalism. In order to achieve more explicit, more formalised theories, new institutional economics occasionally returned to neoclassical methodology. This is exactly the central issue in old institutionalist critique against new institutional economics.

The debate on the desired level of formalism also crumbles new institutional economics. Game theory or some agency models are highly formalised. In these cases, new institutional economics directly returns to neoclassical orthodoxy. Consequently, self-interested rationality and the exaggeration of the cognitive capabilities of the individual as assumptions; the high level of abstraction and formalism; the explicit micro-foundations; and the axiomatic-deductive scheme have shown up again (Rutherford, 1994, p. 21–22). Levinthal (1988) explicitly refers to these agency models as neoclassical, calling attention to the fact that formalism circumscribes validity and applicability. In his view, all these achievements can burgeon outside mathematical economics only if the empirical connections can be found.

Mirowski (1986, p. 252–254) draws similar inferences as for new institutional game theory. On account of the *constancy* assumptions, these models are clearly incapable of describing institutional *changes*. The constancy of humans and human nature, rules, objectives and the environment must be stipulated for the sake of formalism. However, such assumptions recall the well-defined models of the neoclassical orthodoxy—and abstracting the peculiarities of agents directly leads us back to the idea of the representative neoclassical agent.⁹ On these grounds, one must realize that focusing on particular puzzles and setting up the research instruments accordingly have also deprived institutional economics of the possibility of analysing certain problems. This was true not only of old institutional economics that hardly made any efforts towards universality but also of formalised new institutional economics that by aspiring a higher level of generality has drifted back to neoclassical orthodoxy. The currents within new institutional economics that succeed in abandoning neoclassical postulates and in providing comprehensive descriptions of institutional environment and evolution are the least formalised attempts (see e.g. Nelson – Winter, 1982, p. 22). However, it must also be noted that rigor or the application of the formal deductive method of seeking and analysing laws have

⁹ I am not cognizant of a debate from the history of natural science on the appropriateness or efficiency of the microscope or the telescope as instruments. Perhaps it is related to the relatively young age of economics that until now only a few have taken notice of the absurdity of our analogous methodological debates.

made even institutionalism blind to some particularities of the institutional environment. Admitting the shortcomings of our methods as for scrutinizing some puzzles is hardly a failure. Or if it is so, it is also a failure if the critics are also bound to admit the same thing and to make dismal compromises. Strange as it is, the compromises made by institutionalism justify the directions of mainstream economics.

From the end of the 1980s and simultaneously with the rise of the new institutionalist movement, old institutional economics developed under the influence of Veblen have got some fresh intellectual injections thanks to Geoffrey Hodgson¹⁰ (Chavance, 2009, p. 71–72). In the case of new institutional axiomatic-deductive models, the mainstream influences primarily consist in the intention of analysing the emergence of some institutions, where such institutions are conceived as the outcomes of the optimizing behaviour of rational individuals (Keizer, 2007, p. 4–6). So, in new institutional economics the emergence and the evolution of institutions are described in the framework of non-Darwinian evolutionary processes, where selection is based on rational decisions, as self-interested members of society also consider the expected consequences of their decisions (Ménard – Shirley, 2005, p. 21–22). As the methodology of mainstream economics has been changing along its 150-year-long history, similarly, one could only talk about a homogenous institutional methodology at the cost of serious distortions. It must also be kept in mind that micro-oriented new institutional economics looking for the laws of the working of societies has drifted far from the macro-oriented old institutional approach. In old institutional economics having shown interest in large-scale historical processes and evolution, institutions were not supposed to be the outcomes of optimizing behaviour but rather as culturally embedded pillars that reflect norms and values. They are not created by rational individuals. Instead, they are the fundamentals that underpin societies in an unintended way.

Deep drillings in high theory

It is a commonplace that economics inherited the extensive application of mathematics from natural science. Certain problems cannot be analysed with formalised theories—or only inefficiently at best. However, the appropriate problems can be scrutinized with high precision

¹⁰ Rutherford (1994, p. 1–4) calls attention to the fact that demarcating old and new institutional economics is as oversimplifying as referring to these movements as homogenous systems. Old institutional economics consisted of two major directions at least (Veblen–Ayres tradition vs. the Commons-led way). New institutional economics is far more heterogeneous. By the way, there are similar disagreements between old and new institutional economics about some atheoretical efforts and the holistic approach, excessive abstraction and formalism, and stealing back the rationality postulate.

and in high depth.¹¹ Undoubtedly, we need to disregard the contingencies in order to focus on the law-like tendencies of universal validity. Based on its own methodology, institutional economics could not have been able to analyse the problems sorted under mainstream scrutiny successfully. Mainstream economics is blind to some problems, so it can always be justly criticised for not paying attention to them. However, it definitely cannot be accused of not picking the right methods for analysing its chosen puzzles.

Mainstream economics cannot be blamed for scrutinizing its problems on the basis of an inappropriate methodology. It is hard to believe that through a loose and anti-formalist institutional reasoning the real effects of changes in the money supply or, say, the nature of the business cycles triggered by information deficiencies could have been analysed with precision and in the depth of mainstream achievements. The formalism of modern macroeconomics is never a superfluous ornament or a characteristic we drag along out of sheer habit because of the inertia of our scientific community. It is rather an instrument that makes it possible to answer certain questions in higher depth and with higher consistency than in any other approaches.¹² By all appearances, any dissatisfaction with mainstream economics is actually a dissatisfaction with the problems it scrutinizes. The critics try to eliminate their displeasure by suggesting their own methodology and worldview. However, at the same time, they neglect the fact that they have no answers to the problems mainstream economics has been investigating successfully. Moreover, they are not even aware of the problems at all.

Formalism has its price, of course. This is high level of abstraction, to which scrutinizing institutions and the historical context falls victim. However, these puzzles can be analysed along other methods—methods, by which one can enter the territory of mainstream economics only clumsily. As long as the complementarity of approaches is not recognized and as long as we keep having debates on the primacy of certain necessarily incomplete approaches, any synthesizing achievements can hardly be expected to occur in our discipline. Any specific problem requires a specific methodology the shortcomings of which we should be aware of all the time. If methodology has a message for all the schools of economics at all, then it is exactly

¹¹ Just for the sake of good order, *depth* definitely does not refer to the comprehensive exploration of a complex causal structure underlying a phenomenon (I label it as the *comprehensiveness* of a model or a theory). Rather, to an exhaustive and consistent analysis of one of the mechanisms in this causal structure in order to pay attention to a wide range of consequences.

¹² Admittedly, deductive reasoning does not necessarily require formalism. Rutherford (1994, p. 8) mentions Austrian economics as an example, where deductive reasoning has been performed not on grounds of symbolic logic or mathematics. This example, however, is rather in line with the details mentioned above. Even though careful deductive reasoning can be carried out in verbal form, the optimal rate of the growth of money or the slope of the Phillips-curve can hardly be calculated without any numerical data and formalism.

this idea. Salvation does not come with a unique methodology that has only advantages and that can be applied universally.

All the methodological difficulties of mainstream economics come from the intention of applying celestial physics to terrestrial social conditions. When it comes to studying the history of physics, we can easily have the impression that the victory of the heliocentric system or of the application of elliptical orbits was only the question of time. In our Solar system, the motions of planets are quite simple; orbits are only disturbed by a minimal amount of external effects. Once the observation data were available with the desired level of accuracy, solving the Solar system became also possible (Weinberg, 2015, Chapter 12). In order to apply this celestial physics to our highly complex social world, it is necessary to disregard all the (social, institutional and cultural) conditions that interfere with the fundamental economic laws.¹³ However, it goes with the fact that one can only infer the working of a law underlying social phenomena from dubious numerical evidences at best. Keeping this in mind, the development of econometrics seems to be a major achievement in applying abstract ideal-typical mainstream economics directly to reality.

The scheme Ernan McMullin (1984) labels as T-science is a constituent of the heritage of mainstream economics from physics. For economics Frank Hahn (1973, p. 323–324) clearly underlines these methodological analogies. One of the achievements of 18th century natural science was the hypothetical or consequential (ex suppositione) reasoning, in which it is from the consequences or effects experienced at the level of phenomena that we try to infer the causes. The point of the new method shaped by Descartes and Bacon is to form a particular¹⁴ causal hypothesis on the basis of the effects (i.e. a phenomena); then to test it through experience. In other words, it is comparing the empirical statements generated from the hypothesis and the underlying axioms on one hand and reality on the other hand (Hempel, 1965, p. 3; Hempel – Oppenheim, 1948, p. 138).¹⁵ A phenomenon under scrutiny obtains an

¹³ Perhaps it is the most important message of Popper's (1962, p. 97) commentaries that in celestial physics realist purposes result in simpler and more accurate models, whilst in economics in more complex representations. The more realist a model is (i.e. the more mechanisms of the real causal structures are preserved), the more complex it is. One of the consequences is the doubt often cast on the realism of abstract realist mainstream models. This is exactly the crucial point in the institutionalist critique—the belief that only something complex can be realist. Judging by this ungrounded belief has not even been eased by the fact that mainstream economics often highlights the real causal analyses that lie behind the fragmentary character of their models (e.g. Phelps, 2006, p. 7). In the realist main stream, the confirmation of a theory has also been interpreted as the sign of an approximately true ontology (cf. Fine, 1984, p. 87).

¹⁴ This particularity is also a physical heritage. Accordingly, our theories can always be applied only to certain sets of phenomena (cf. Weinberg, 2015, Chapter 14).

¹⁵ Here it is only of secondary importance that to identify the efforts towards better empirical performance as instrumentalism is a sign of misunderstanding this method. As some hidden layers of reality can only be accessed

explanation by getting sorted under a general law or tendency. If our efforts are successful, it is confirmed by the accordance of the model outcomes with the data. This method is fraught with difficulties, since we need to give account of experienced phenomena on the basis of unexperienced entities and mechanisms. It is only by forming and testing hypotheses that this invisible world (of which consumers' preference systems or Friedmanian permanent income are all constituents) can approximately be approached.¹⁶ The immediate result of inferring unexperienceable causes from experienced effects is a *theory*. In such a theory, we hypostatize a *plausible* causal mechanism that seems to have real existence (cf. Van Fraassen, 1980, p. 36). On this ground, we deduce the consequences from the premises in order to compare them to reality. These steps give the key for applying an axiomatic–deductive science to reality.

But how does it happen? Is it really necessary for us to intuitively admit the realism of our assumptions, as it is required in institutionalism? The answer is negative. Moreover, the lack of intuitive admission does not lead to efforts towards inconsistency with reality or to admitting such an inconsistency (both cases can be sorted under the label of instrumentalism). The arguments against modern macroeconomics and, eventually, mainstream economics as such miss direct plausibility. According to them, it is totally absurd to analyse an *evidently* disequilibrium situation through an equilibrium approach. The problem hidden in this stance is that general equilibrium environment, rational expectations or utility maximization postulate belong to the economic calculus *as axioms*, and not to the premises. They are not data. Premises support the testing process. They are inputs to a theory; the data from which we can draw inferences in order to confirm a theory. General equilibrium or the rationality of expectations do *not* mean inputting directly experienced reality into our models.

The lack of direct evidence does not imply either any inherent contradiction to reality or instrumentalism. The purpose is to highlight a plausible mechanism underlying the causal structure. If following the principles of T-science we *cannot* generate from the data (premises) some outcomes that are in line with the facts more or less, then we say *our theory is not*

through hypotheses, and as we need to confirm our hypotheses, in this procedure explanation and prediction are two inseparable sides of the same coin. The only rare exception is the case of *rational plausibility* (Weber, 1978, p. 11), when no empirical confirmation is required, since one can evidently assume the working of the causal mechanism highlighted in the hypothesis. Only the doubts cast on understanding in the 20th century led modern natural science back to the minimalist purpose of mere prediction.

¹⁶ This difficulty has put the realist scientific ideal into the crossfire of recurrent attacks. Putnam (1975) interpreted the confirmation of a causal mechanism as an evidence of the real existence of the entities and mechanisms postulated therein. However, Hacking (1983) and others called attention to the fact that for a realist, good empirical performance would make even the models of a sceptical instrumentalist true. However, an instrumentalist would never infer truth from good empirical performance. This debate is referred to as the indeterminacy of choosing theories with identical empirical consequences.

appropriate. We must conclude that the causal mechanism we hypothesized is not working in reality—and not that our premises were not appropriate (cf. Carnap, 1939, p. 57–61). Critique rejects general equilibrium as an axiom—and no other sensible option is available. Moreover, a theory built on an alternative system of axioms cannot be regarded as a refutation. Affine geometry and absolute geometry both extractable from Euclid’s *Elements* constitute two systems of the same rank (Coxeter, 1969, p. 175). It is exactly this relationship that characterizes the whole equilibrium–disequilibrium battle.

By the way, in the disequilibrium approach, disequilibrium has the same status (an axiom). One of the major issues in the conflict between mainstream economics and anti-mainstream institutionalist efforts consists in the fact that in the latter direct evidence is advanced to be the criterion for selecting the axioms. This is the reason why it became the most often voiced institutionalist argument against neoclassical orthodoxy that the assumptions applied in the models cannot be found in reality. On this ground, institutionalists still believe mainstream models to be refutable.¹⁷ However, we should not forget about the fact that it is exactly the privileged position of equilibrium in equilibrium economics that exempts it from direct comparison to reality and rejection. Any theory impinges on experience only at some parts of it, so direct comparison to experience (while neglecting the purpose of applying general equilibrium or rationality as assumptions) in the case of its non-descriptive parts (that is, which are *not* required to save the phenomena) is not a relevant regard. Those parts of a theory that impinge on experience can always be modified so that the assumptions remote from the experiential periphery can be kept intact (Quine, 1951, p. 421). The situation in everyday scientific practice is a lot more complicated. As mainstream theories due to isolation and the abundantly applied *ceteris paribus* clauses are much simpler than the complex social phenomena to be explained, empirical failures rarely lead to rejecting plausible and fruitful hypotheses (Hausman, 1992, p. 221–223). Axioms are even more protected, since together with other elements of the methodology they define the research program itself (cf. Boland, 1992, p. 18). On account of scrutinizing the violations of the circumstances set with the *ceteris paribus* clauses, or of exploring the causal structure simplified with isolation, institutional economics

¹⁷ In the theory of scientific representation the precise description of how we can draw *valid* and *sound* inferences from representations that distort our reality is still a vivid and exciting topic. Shech (2015) picks political caricature as an example. Such caricatures deliberately misrepresenting reality serve the purpose of drawing sound inferences. Only guides to ontology can be expected to have similar capabilities—these guides reflect the real ontic structure without distortions. However, these guides only seem to be theoretical possibilities and not real options. For economics they are definitely not available. In our discipline, the real question is not the application of isolation but the extent of isolation.

interested in these directions gives a comprehensive complement to mainstream economics. As long as the intention of unveiling the causal structure is present, as long as hypotheses seem to be plausible in advance, any direct comparison to reality in order to judge the assumptions and the methodology is completely futile.

Axioms and hypotheses make up a two-layer system, in which the axioms are situated in the core, surrounded by the testable hypotheses (theories) as a protective belt. When comparing our theories to reality, it is the hypotheses that we test, not the axioms. In case we cannot find a considerable concordance between the outcomes we generate and reality, we can only reject the hypotheses, not the axioms. Axioms cannot be refuted within the system. Whilst it is true that axioms cannot be traced back to anything in the system, they are not based on anything inside, they still have a source outside the theory. Axioms do not turn up by *deus ex machina*, but they directly rise from the interest of scientists. Axioms can only be analysed on the basis of the purposes underlying a theory. In other words, the only question that makes sense is whether the axioms are appropriate as for achieving the purposes. All this calls attention to the fact that the ultimate grounds of any theory lie concealed in the scientist himself. It is us who create the theory.

Axioms make up the inner protected core not because of our having chosen them for this role. The reason is that testing axioms directly, i.e. without any theory is unattainable. Through a theory that is directly confronted with reality an axiom that bears indirect relation through the theory itself to reality cannot be tested. Naturally, it does not rule out the possibility of treating an axiom of a theory as a testable hypothesis in another system. It was exactly the case with rational expectations hypothesis suggested by Muth (1961). As the econometric tests had considerably confirmed the hypothesis, Lucas (1973) could apply that as an axiom. However, as soon as a hypothesis is put amongst the axioms, it loses its testability. This is in complete consonance with the results of Carnap (1939, p. 59). It is the same process as what was described by Lakatos (1968, p. 168–176) analysing the evolution of modern natural science. On account of the positive heuristics and negative heuristics, the testable protective belt built around the hard core prevents the most characteristic and most stable system of theories and assumptions from the critique and from any modifying efforts. The neutrality of mainstream economics against the institutionalist critique is not only the result of an instinctively followed scientific strategy. We should not forget about the fact that modern macroeconomics after

Keynes has never made efforts to set up general theories.¹⁸ It is exceptionally true of Friedman who worked along an eclectic methodology and was not even worried about the occasional inconsistency between his different theories. However, it is also true of the subsequent Lucas–RBC-line that has always been directed at analysing only certain individual mechanisms of the complex causal structure.

Anytime when testing a theory, it is at stake whether the postulated mechanisms can be detected as the constituents of actual social conditions—and not whether these mechanisms make up the totality of the causal mechanism. These tests have mostly been successful, so the institutional rejection is not grounded by a doubt cast on the numerical results, but by the emphasis put on the fact that these mechanisms cannot be expected to comprehend the causal structure in its totality—whilst it has never been a purpose.

Assessment: a critique of the critique

The critique should demonstrate the failure of mainstream economics. This could be carried out in two ways. First, critics need to shed light on the inefficiency of mainstream economics in applying its preferred methods. In other words, the effects of some factors consistently neglected by mainstream economics could be still analysed *in a formalised way* and in the mainstream depth simultaneously with and in addition to the traditional problems (i.e. in the same model/theory). In this case, it would be clear that mainstream economics gratuitously curtailed its territory. The most part of institutional economics, however, categorically rejected this formalism. Moreover, as we have just seen, institutionalism emerged as a direct opposition to formalism, since institutionalists regarded depth provided by formalism as needless in terms of understanding.

Institutional critique against the main stream is confined to playing down the advantages of formalism as for understanding societies. Authors do their best to replace a methodology conceived as abortive with the approach they favour. As institutionalism does not often admit the possible advantages of formalism, their own, less formal methods seem to have only benefits. At the same time, non-formal efforts of institutional economics have voluntarily given up the gains from the development of mathematical methods.¹⁹ In this respect, the formalised

¹⁸ Keynes was far from the level of complexity that is the ideal for institutional economics. De Vroey (2016, p. 74) quotes a passage where Friedman writes that his esteem to Keynes was raised by the fact that Keynes could focus his attention on small number of key variables.

¹⁹ The fact that in mainstream economics some traditional neoclassical postulates can be gradually (albeit slowly) outdone is due to the development of mathematical and empirical methods.

efforts of new institutional economics are to be evaluated as abandoning the initial purposes. Those directions, however, that ambitioned to give up the standard neoclassical assumptions tried to cut down on formalism at the same time. These analyse man not as the *homo oeconomicus*, but as an actual social being, and instead of preserving depth, they make efforts to provide some more comprehensive descriptions of the causal structure. Unfortunately, abandoning formalism is not possible with insisting on depth simultaneously (cf. Williamson, 1985, p. 386), and it is exactly the aspect which institutionalism tends not to pay attention to.

Here it must be noted that preferring comprehensiveness to depth is only a question of taste. A researcher always follows his own interest when deciding on how large a slice of reality he would like to scrutinize. Actually, it is the only decision he can make, since there are methods belonging to the chosen problem that make efficient analyses possible. This constraint cannot be eliminated, that is, efforts towards a formalised comprehensive social theory would turn out to be unreasonable for the time being. Formalism that limits interest but can provide high depth; and institutionalism that is comprehensive and non-formalist but, hence, cannot claim to have considerable depth are the endpoints of a continuum. Here, the intermediate positions are available in infinite number. Not only does this recognition make the debate unreasonable, but it also restrains it from being a debate. After all, in the mainstream–institutionalist controversy, two *attitudes* are opposed to one another, and neither of them can take priority over the other on logical grounds. Any choice is grounded by preferences and taste.

Second, institutional (or any other) approach needs to demonstrate that by abandoning formalism, the problems mainstream economics analyses could be scrutinized more thoroughly and by doing so, some new insights could be revealed. It would be an increase in depth. However, no such instances have occurred until now, let alone the efforts. So, the critique would be efficient only if the range of the comprehended causal mechanism was increased *ceteris paribus*, that is, while formalism is also preserved; or formalism itself proved to be insufficient in increasing the depth of analysis. As we have seen, increasing the number of factors treated within a model necessarily leads to a drop in the level of formalism, the price of which is an involuntarily act of giving up depth. The circumstance that the critique succeeds in scrutinizing some problems mainstream economics neglects with methods the main stream does not apply, does not meet any of these requirements. What is more, some problems interesting for institutionalism can also be effectively approached in a formal way (and in separate models, which is of crucial importance), but this rather stands in support of the mainstream approach. De Vroey and Pensieroso (2016, p. 17–18) explicitly ascribe the convergence of some

institutionalist researches towards mainstream economics to the efficiency and the attraction of mainstream methodology. It is about time to talk about complementarity rather than insist on the critical attitude.

Conclusions: arguments for a methodology of economics

I have been arguing that the rivalling approaches have neither clarified nor accepted the methodological foundations of their counterparts. Instead, both try to absolutize their own methodological standards. This attitude, however, can hardly result in a consensus and in recognizing complementarity. This process, albeit unfavourable, is not surprising at all. Rather, it inherently belongs to the mechanism how science works as a social institution. Rivalling ideas do not look for dialogues, but driven by the belief in the appropriateness of their own worldviews they commit themselves to their own visions of reality—which naturally separates the camps. Becoming an adherent of a research tradition or a research program, we devote ourselves to scrutinizing certain problems, which in the tradition some conventional methods and some underlying rules as for the techniques and the assumptions to be applied belong to (Leijonhufvud, 1994, p. 144). Systems are stable, since any of them can explain the facts relevant for them and can justify why other facts are neglected. This is the reason why it seems as if external complements are not needed at all. For their insiders, any group can satisfactorily give accounts of the things that are found relevant from the set of phenomena to be explained.

A worldview, the way of turning to reality and of posing questions amount to a methodology that defines a research tradition. Such conceptual schemes are segregated from any other alternative systems that are based on some different approaches to reality. Theoretical systems become completely different languages of describing reality. Being adherents of any of them, we expect other languages not to be able to describe efficiently that slice of reality that we are interested in. Judging the relevance of the rivalling systems does not take place, since it would require a self-modifying act. A scientist would be led to conclusions which he now abhors. A formal reasoning is void, since exhorting the rivalling group to become converted (or at least to accept the relevance of the alternative approach) requires us by speaking their language to make them repudiate the system they cherish now. However, it never happens, since we cannot find anybody making an attempt.²⁰ There remain the efforts to make the rivalling approach appear unreasonable. Such efforts may still trigger conversions and can justify why we refuse to enter

²⁰ It is hardly accidental that De Vroey (2016, p. 141–142) appraising Lucas' performance emphasized such conversions. Impressed by Lucas' high impact papers, at the beginning of the 1970's, a number of economists abandoned their previous researches in order to join him as followers.

(Polanyi, 1958, p. 158–161). So, such debates are likely to bear resemblance to theological controversies, in which mutual excommunications are heard instead of some tolerant manifestations conveying mutual acceptance (Silvestri, 2017, p. 6–11). It is exactly the drives towards conversion and the risk of the subsequent confusion that are ruled out by protecting the hard core. Backhouse (1992a, p. 78) analyses the Hahn–Kaldor debate as one of the most memorable episodes of the mainstream–instrumentalist controversy in a way from which one can easily feel out both this indeterminacy and the locking away in the distinctive traditions. The Kornai–Hahn debate can similarly be considered. Kornai (1971) did not pen a critique against the main stream; rather, withdraw its scientific status. In his reply, Hahn (1973) did the same thing to Kornai, highlighting those epistemological and methodological foundations that can demonstrate the scientific status for the mainstream camp. On these grounds, however, an outsider standing on different principles cannot be convinced. Like Kornai, he is likely to ignore these maxims. This is the reason why it is true that both Hahn and Kornai won the debate according to their camps—and this is the reason why by highlighting some epistemological considerations mainstream economists cannot convince the institutionalist camp of even the basic level of relevance. The same rejection forced John Dewey to make attempts to work out his own philosophy of science. Debates between the major currents are paralysed by the lack of a common language.

Thus, we can hardly expect either emphasizing the methodological aspects of the debate or the methodological analysis of the details to necessarily divert the conflict upon more rational grounds. By realizing this fact in advance, we can at least prevent ourselves from referring to methodology as a higher order meta-science on economics. Cherishing the idea of universal meta-sciences has always fallen through. Neither general systems theory nor logical positivism could live up to expectations. Good economic methodology should not be directed at revealing some ultimate truths at all. We do not formulate our statements regarding our discipline from a position outside economics. Rather, we ponder over some arguments the research traditions tend to ignore. In this case, the purpose is to clarify the methodological principles along which the camps keep making progress. Such maxims, as it is highlighted above, do not take priority over one another. Their importance lies only in the fact that the different schools define and organise themselves following them as guidelines (Backhouse, 1992a, p. 73–74). Exploring and clarifying these lines are instrumental in understanding the theoretical contents produced by the different schools. Considering and assessing the content of some theories is only possible when methodologies are also taken into account. This is an undertaking radically different from

working out methodological principles for one's own use. If convergence is possible at all, that is to be placed on methodological grounds. Simultaneously, it should also be taken into consideration that methodological analyses evidently have new results to share with the historiography of economics (c.f. Spanos – Mayo, 2015, p. 3544).

All this can be included in the fundamental purpose of methodology that it aspires as an instrument of self-reflection to be the descriptive and normative discipline of economic theorizing (Caldwell, 1990, p. 65). It is quite on the cards that the controversy between mainstream economics and institutionalism will be reproduced at the level of methodological analyses, even though it is here that we have the highest hopes for revealing the hidden methodological principles.²¹ The axiomatic–deductive scheme mainstream economics follows makes it possible for us to assess the statements made in the system by the criterion of formal truth. However, since models are linked to reality, we cannot skip assessing material truth either (Schlick, 1934, p. 215). The most serious methodological problem of mainstream economics is how its formal truths can be made material truths as well. The answer is to be found in the assumptions, the hypotheses, and the way we hypostatize universal laws—the laws that are set as axioms, but have only hypothetical status. These are the links through which we can tie our models to reality. This is the reason why we can say that the realism of assumptions is a question far more important than Friedman (1953) thought—and this is the reason why methodology is essential for economics. We can understand the details a theory asserts about reality only by analysing the relationship between model and reality. However, this analysis is of methodological nature. For this reason, even the economic policy applications of theories raise methodological questions (Hoover, 1995, p. 717). Assessing material truth always means comparisons to reality. However, the methodological grounds (e.g. which facet of reality is actually addressed) and the relationship both with the related branches of knowledge and the alternative research programs are not of secondary importance. These are the fundamentals standing on which any theory makes its empirical statements.

Modern methodology, a line of research specialized in discussing methodological issues can mostly support the discourse by making these often-hidden maxims explicit in order that they can be exposed to calm evaluations. What is more, it is not only the methodological principles, but also the criteria of their careful assessment that can be judged (Backhouse, 1992b, p. 61).

²¹ As a special mixture of irony and conceit, here I can drop the hint that the present paper is of methodological nature and urges the sharing of ideas between the main stream and institutionalism. On this ground, we are free to draw the inference that methodology is good and beneficial.

Recognizing the methodological fundamentals of our debates may not give us brand new viewpoints, but at least some new arguments to consider—and we might turn our controversies towards a consensus we have been waiting for so long.

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