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# Crisis and Methodology: Some Heterodox Misunderstandings

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## Abstract

Whether justified by the concrete circumstances or not, an economic crisis is, by simple association, taken as an implicit refutation of the invisible hand vision and the underlying theory. The fundamental heterodox critique locates the source of apparent theoretical difficulties at the level of methodology. Although acceptable in principle, this belief involves some actual misunderstandings with regard to the respective roles of deterministic laws and deductive reasoning. In order to clarify these, the present paper revisits some key episodes in the history of economic methodology.

**JEL** B10, B20, B41

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One positive consequence of the ongoing economic crisis is that the intellectual malaise of the modern academic discipline of economics is becoming ever more widely recognised. ... It is ... not at all surprising that mainstream contributions are found continually to be so unrealistic and explanatorily limited. *The (mathematical) method, or rather the emphasis placed upon it in the modern economics academy, is the overriding problem.* (Lawson, 2012, p. 3), original emphasis

Those who have lost their job, their money, or their home in the latest financial crisis and its aftermath will be surprised to learn that their misfortune is ultimately caused by methodological problems of academic economics. This attribution seems bizarre. Yet, on second thought, it resonates well with Keynes's closing sentence in the *General Theory* about the power of ideas: '... it is ideas, not vested interests, which are dangerous for good or evil' (1973, p. 384). If it is ideas that ultimately rule the world this must hold with extra force for the methodological ideas that inform scientific research.

When economists daydream they fancy that practical men are the intellectual 'slaves of some defunct economist' (Keynes, 1973, p. 383). Let us face the facts.

Late in life, moreover, he [Napoleon] claimed that he had always believed that if an empire were made of granite the ideas of economists, if listened to, would suffice to reduce it to dust. (Viner, 1963, p. 1)

The more intelligent part of practical men has an instrumental relation to ideas and takes whatever suits best in the given circumstances to advance their cause. This should not come as a surprise. A natural preference for *doxa* instead of *episteme* is implied in Adam Smith's vision of the self-interested agent. If they cared for one, practical men have – given the dubious methodological status of economics – always found the congenial academic underpinning for their agenda.

However, these practical contributions of economists for the most part did not require any great scientific apparatus. The argument that "the market works" has been known for many centuries, even before Adam Smith. It often amounts to little more than saying that a money system with prices for goods and services will outperform a barter system as an arrangement for their exchange – something recognized by all kinds of societies and stated in many times and places before modern "technical" economics. (Nelson, 2006, pp. 330-331)

*A priori*, an alliance of economics and politics does not prove anything for or against a specific approach and may sometimes even come as a surprise to the theoreticians themselves.

I have always regarded Competitive General Equilibrium analysis as akin to the mock-up an aircraft engineer might build. My amazement in recent years has accordingly been very great to find that many

economists are passing the mock-up off as an airworthy plane, and that politicians, bankers, and commentators are scrambling to get seats. This at a time when theorists all over the world have become aware that anything based on this mock-up is unlikely to fly, since it neglects some crucial aspects of the world, the recognition of which will force some drastic re-designing. (Hahn, 1981, p. 1036)

According to Hahn's testimonial the real world application of the apex of mathematical economics and the core of standard economics is, if anything, a proof for the power of misapprehension. The fact that existent theoretical economics is sometimes taken seriously by politicians, central bankers, business people and economists themselves is not a proof of its validity but rather of a widespread naïveté (cf. Stiglitz, 2011).

Lawson's nexus between economic crisis and methodology boils down to a bidirectional causal chain: the latest economic crisis is attributed to orthodox theory; this theory is the achievement of a wrong methodology. In reverse causality this suggests that the abandonment of the mathematical method, the hallmark of orthodoxy, will lead to a better theoretical understanding of how the economy works and eventually to the prevention of crises. Although not illogical, this belief involves subtle misunderstandings. In order to clarify these it is illuminating to revisit some key episodes in the checkered history of economic methodology.

## 1 The elements of economic methodology

In the era of political economy, economists actively participated in the grand project of the advance of knowledge.

I am inclined to say even more: from Plato to Descartes, Leibniz, Kant, Duhem and Poincaré; and from Bacon, Hobbes, and Locke, to Hume, Mill, and Russel, the theory of knowledge was inspired by the hope that it would enable us not only to know more about knowledge, but also to contribute to the advance of knowledge – of scientific knowledge, that is. (Popper, 1980, p. 19)

It was rather commonplace that a physicist, e.g. Mach, or a mathematician, e.g. Poincaré, quoted J. S. Mill on matters of methodology.<sup>1</sup> Mill expressly advocated borrowing from physics. This, though, involved two essentially different elements. First, the idea of deterministic causal laws.

The backward state of the Moral Sciences can only be remedied by applying to them the methods of Physical Science, duly extended and generalized. (Mill, 2006b, p. 833)

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<sup>1</sup> "One text he [Dirac] took out of the library was John Stuart Mill's *A System of Logic*, which the young Einstein had studied some fifteen years before. ... He [Mill] influenced Dirac, and many others, more than they knew." (Farmelo, 2009, p. 43)

Second, the deductive method.

In the definition which we have attempted to frame of the science of Political Economy, we have characterized it as essentially an *abstract* science, and its method as the method *à priori*. Such is undoubtedly its character as it has been understood and taught by all its most distinguished teachers. It reasons, and, as we contend, must necessarily reason, from assumptions, not from facts. It is built upon hypotheses, strictly analogous to those which, under the name of definitions, are the foundations of other abstract sciences. (Mill, 2004, p. 110), original emphasis

Mill was explicit about the subsidiary role of the deductive method.

The ground of confidence in any concrete deductive science is not the *à priori* reasoning itself, but the accordance between its results and those of observation *à posteriori*. (Mill, 2006b, p. 896-897)

It was rather obvious to Mill that deterministic causal laws and human behavior do not match.

The phenomena with which this science [of human nature] is conversant being the thoughts, feelings, and actions of human beings, it would have attained the ideal perfection of a science if it enabled us to foretell how an individual would think, feel, or act, throughout life, with the same certainty with which astronomy enables us to predict the places and occultations of the heavenly bodies. It needs scarcely be stated that nothing approaching to this can be done. (Mill, 2006b, p. 846)

To get economics off the ground as a science and to demarcate it from psychology and sociology made it imperative to say something general about human behavior in the economic realm. Mill put it thus:

Just in the same manner [as geometry] does Political Economy presuppose an arbitrary definition of man, as a being who invariably does that by which he may obtain the greatest amount of necessaries, conveniences, and luxuries, with the smallest quantity of labour and physical self-denial with which they can be obtained in the existing state of knowledge. (Mill, 2004, p. 110)

Mill regarded this proposition as an empirical law which resembles, but has to be carefully distinguished from, universal deterministic physical laws. Empirical laws are neither deterministic nor universal, they express merely a local and temporary tendency.

In political economy for instance, empirical laws of human nature are tacitly assumed by English thinkers, which are calculated only for Great Britain and the United States. (Mill, 2006b, p. 906)

Mill identified deterministic laws and deductive reasoning as the two crucial elements of the scientific method and adapted them to economics. With regard to a sufficiently articulated theory the first element implies the criterion of material consistency, the second of logical consistency. A theory must satisfy both criteria, that is to say, it can be rejected either on empirical or on logical grounds alone.

Mill realized that human behavior is not subject to deterministic laws but, if at all, to the weaker form of empirical laws. A detailed analysis of human behavior, though, was not the topmost issue for the classicals. It was the laws of price formation, growth and distribution they were really interested in.

## 2 Law and logic

The conception of a deterministic causal law had been made abundantly clear by Newton. His approach became paradigmatic among economists (Redman, 1997, pp. 208-218) and gave rise to an inflation of laws beginning with the laws of demand and supply and ending with the laws of motion of the society as a whole.

But it was a second and more important quality that struck readers of the *Principia*. At the head of Book I stand the famous *Axioms, or the Laws of motion*: ... For readers of that day, it was this deductive, mathematical aspect that was the great achievement. (Truesdell, quoted in Schmiechen, 2009, p. 213)

While J. S. Mill derived his behavioral tendencies inductively, Jevons chose a systematic approach to impose order upon the arbitrary multitude of laws.

The science of Economics, however, is in some degree peculiar, owing to the fact ... that its ultimate laws are known to us immediately by intuition, or, at any rate, they are furnished to us ready made by other mental or physical sciences. That every person will choose the greater apparent good; that human wants are more or less quickly satiated; that prolonged labor becomes more and more painful; are a few of the simple inductions on which we can proceed to reason deductively with great confidence. From these axioms we can deduce the laws of supply and demand, the laws of that difficult conception, value, and all the intricate results of commerce, so far as data are available. (Jevons, 1911, p. 18)

Just like Mill, Jevons applied the deductive method. But there is a subtle shift of meaning. When Jevons uses the term law he has the deterministic laws of Newton at the back of his mind and not the empirical laws of Mill. Jevons accorded his fundamental behavioral law the logical status of an axiom.

... the theory here given may be described as the mechanics of utility and self-interest. Oversights may have been committed in tracing out

its details, but in its main features this theory must be the true one. Its method is as sure and demonstrative as that of kinematics or statics, nay, almost as self-evident as are the elements of Euclid, when the real meaning of the formulæ is fully seized. (Jevons, 1911, p. 21)

His program was from outer appearances the same as Mill's but Jevons went one decisive step further.

An explicit maximization hypothesis has been the hallmark of neoclassical economic since the end of the nineteenth century and might easily be seen to be the one major departure that distinguishes neoclassical from classical economics. (Boland, 2003, p. 49)

This established marginalism as *the* explanatory device from consumer choice to production and distribution.

These economists were implicitly treating microeconomics as a pure axiomatic system, whose terms may or may not be instantiated in the real world, but which is of great interest, like Euclidean geometry, whether or not its objects actually exist. (Rosenberg, 1994, p. 229)

The crucial point of Jevons's approach, which culminated in the existence proof of general equilibrium (Weintraub, 1985), are the premises. To recall, Newton's set of axioms contained the deterministic laws of motion. Because these have a counterpart in the real world the deductive method provides conclusions that are potentially in agreement with observation. This cannot happen if the terms in the axioms have no real world interpretation. In this case, the criterion of logical consistency is satisfied but that of material consistency is inapplicable and therefore undecidable. Keynes clearly identified the salient methodological point.

For if orthodox economics is at fault, the error is to be found not in the superstructure, which has been erected with great care for logical consistency, but in a lack of clearness and of generality in the premises. (Keynes, 1973, p. xxi)

### **3 The return of Common Sense**

To make the world we live in understandable to ourselves we have not only myth and science but also common sense – uneasily sitting between the two. J. S. Mill had no friendly word for common sense.

People fancied they *saw* the sun rise and set, the stars revolve in circles round the pole. We now know that they saw no such thing; what they really saw was a set of appearances, equally reconcileable with the theory they held and with a totally different one. It seems strange that such an instance as this, . . . , should not have opened the eyes of

the bigots of common sense, and inspired them with a more modest distrust of the competency of mere ignorance to judge the conclusions of cultivated thought. (Mill, 2006b, p. 783), original emphasis

Apart from being presumptuous, common sense is simply not up to the task.

But, as beings of limited experience, we must always and necessarily have limited conceptive powers; while it does not by any means follow that the same limitations obtain in the possibilities of nature, nor even in her actual manifestations. (Mill, 2006b, p. 753)

Keynes thought otherwise. He was acquainted with Quine's argument that theoretical simplification is achieved through formalization but held that this did not apply to the social realm.

Between the alternatives of metaphorical *jouissance* and an austere canonical notation there is a middle route, and its viability has been argued for, and displayed by Keynes. (Coates, 2007, p. 87)

Before starting work on the *General Theory*, Keynes had made up his mind.

In the early thirties he confessed to Roy Harrod that he was "returning to an age-long tradition of common sense." (Coates, 2007, p. 11), see also (Skidelsky, 2009, p. 82)

Now, economics deals not only with individuals and social relations but the economic system as a whole and Keynes had to come to grips with 'definitions and ideas'. In fact, he did not. He spent an immense amount of his time on Book II 'and still left his successors in confusion' (Moggridge, 1976, p. 33).

By choosing definitions on the ground that they correspond with actual usage Keynes was formulating an ordinary language social science, one that bears a resemblance to those argued for by philosophers of hermeneutics. (Coates, 2007, p. 90)

To recall, Newton first defined his basic concepts mass and force by giving them a precise meaning that was quite different from the woolly everyday usage. In marked contrast, Keynes related his definition of income expressly to 'the practices of the Income Tax Commissioners'. He was in grave doubt whether "it might be better to employ the term *windfalls* for what I call *profits*". But he was quite sure that "saving and investment are, necessarily and by definition, equal – which after all, is in full harmony with common sense and the common usage of the world." (Keynes, quoted in Coates, 2007, pp. 93, 91, original emphasis)

Keynes had no clear idea of the fundamental economic concepts income and profit, and he knew it.

His *Collected Writings* show that he wrestled to solve the Profit Puzzle up till the semi-final versions of his *GT* but in the end he gave up and discarded the draft chapter dealing with it. (Tómasson and Bezemer, 2010, pp. 12-13, 16)



In the discussions following the publication of the *General Theory* Keynes had ‘no desire’ that the particular forms of his ‘comparatively simple fundamental ideas ... should be crystallized at the present state of the debate’ (cited in Rotheim, 1981, p. 571). Keynes kept the discussion within the compass of common sense, where ‘nothing is clear and everything is possible’ (Keynes, 1973, p. 292).

With his middle route Keynes followed the philosophically well-established Cambridge tradition of loose verbal reasoning.

Another danger is that you may ‘precise everything away’ and be left with only a comparative poverty of meaning. ... Such a problem was avoided, said Keynes, by Marshall who used loose definitions but allowed the reader to infer his meaning from “the richness of context.” (Coates, 2007, p. 87)

But, again, common sense, legitimized by its descent from the Scottish School of common sense and euphemized as vigilant observation and intuition, was not up to the task.

Looking back over the last 70 years it is an inescapable fact that the theoretical arm of the Keynesian Revolution never got off the ground. (Rogers, 2010, p. 152)

The Cambridge tradition, continual frustration notwithstanding, still has its epistemological adherents.

For Keynes as for Post Keynesians the guiding motto is "it is better to be roughly right than precisely wrong!" (Davidson, 1984, p. 574)

If we define the ambition of science as to get it *precisely right* then the guiding motto of Post Keynesianism amounts to an invitation of ‘Babylonian incoherent babble’ (cf. Dow, 2005, p. 385) and leads, predictably, to a loss of theoretical coherence (King, 2002, pp. 203-208). Confronted with the phony alternative relevance vs. rigor or truth vs. precision (Mayer, 1993) the non-Keynesians opted for rigor.

Mathematical economics, it seems, had the great virtue of demonstrable irrelevance, which was morally preferable to spurious relevance. (Porter, 1994, p. 155)

#### **4 Weirdness, realism, formalization**

Since Keynes’s days common sense came steadily more under pressure with the escalation of weirdness in the natural sciences and mathematics.

... the *fundamental* problem in philosophy of science – making sense of and determining how science has arrived in a justified way at its present, extremely *weird*, beliefs about how the world is. ... Thales

and Aristotele could not have arrived at quantum theory; no naive examination of experience could have suggested such a view of the world. (Suppe, 1977, p. 684), original emphasis

This opened a welcome chance to defend all kinds of weird concepts with fresh panache. Had not Newton introduced the occult force of gravitation, and had not Galileo assumed a nonexistent vacuum? This became the first line of defense against the critique of unrealism in economics.

The most important methodological issue in economics has been and persists to be over what is called the ‘realism’ of theories and their ‘assumptions’. Profit maximization, perfect information, transitive preferences, diminishing returns, rational expectations, perfectly competitive markets, givenness of tastes, technology and institutional framework, non-gendered agents – these and many other ideas have been assumed by some economists and questioned by others. (Mäki, 1994, p. 236)

There are, though, two kinds of weirdness: justified and unjustified. The first thing to notice is that physical weirdness occurs on very small or very large scales (Feynman, 1992, p. 127). Second, Newton could not, in the strict sense, explain gravitation but he could express it in a neat formula. The calculations that were performed with it proved to be quite accurately in correspondence with facts.

The second line of defense appeals rhetorically to common sense.

But can the model be true? Can any model be true? I do not think so. Any model, whether in physics or in the social sciences, must be an over-simplification. (Popper, 1994, p. 172)

Indeed, who could ever deny this truism? The map is not the landscape. The point is that Newton knew how to properly over-simplify (Cohen, 1999, pp. 148-155) and thereby to gain real insights while his imitators in the social sciences did not.

In economics the conceptual primitives are humans, middle-sized objects, measurable variables like prices, and in most cases trivial events like buying and selling which involve rather down-to-earth human faculties. That is, the economic realm is coextensive with the physical realm that has been satisfactorily explained by classical mechanics. Physics has to be taken seriously as a boundary condition.

Political Economy, therefore, presupposes all the physical sciences; it takes for granted all such of the truths of those sciences as are concerned in the production of the objects demanded by the wants of mankind; ... (Mill, 2004, p. 102)

Yet classical mechanics is not weird at all. It is alone economic theory that is weird, as Walras learned to his chagrin.

Walras approached Poincaré for his approval. ... But Poincaré was devoutly committed to applied mathematics and did not fail to notice

that utility is a nonmeasurable magnitude. . . . He also wondered about the premises of Walras's mathematics: It might be reasonable, as a first approximation, to regard men as completely self-interested, but the assumption of perfect foreknowledge "perhaps requires a certain reserve." (Porter, 1994, p. 154)

By the same token is Keynes's uncertainty argument perfectly justified.

The sense in which I am using the term [uncertainty] is that in which the prospect of a European war is uncertain, or the price of copper and the rate of interest twenty years hence, or the obsolescence of a new invention . . . About these matters there is no scientific basis on which to form any calculable probability whatever. We simply do not know. (Keynes, 1937, p. 214)

Compared to the weirdness of assumptions like foreknowledge Keynes's return to justified common sense must therefore be counted as theoretical progress, notwithstanding the fact that it brings us only back to from where Socrates started, i.e. to 'I know that I know nothing.'

The problem is . . . that the assumptions made in economic theories and models simply are unrealistic in the wrong way and for the wrong reasons. (Pålsson Syll, 2010, p. 26)

Physicists do not reject unrealistic abstractions and idealizations as long as they do not distort the object of inquiry beyond recognition, yet there is perfect unanimity that, for example, an ideal construct like a perpetual motion machine is impossible in principle and not merely infeasible in practice. What Keynes called the 'classical' theory is the economic counterpart of a perpetual motion machine. To spell this out made the *General Theory* a conversation-stopper. And it still is. Keynes's scientific stance is consensus among methodologists.

A scientific theory cannot require the facts to conform to its own assumptions. (Keynes, 1973, p. 276)

This is in full accordance with the classical stance.

Such thinkers do not reflect that the idea, being a result of abstraction, ought to conform to the facts, and cannot make the facts conform to it. (Mill, 2006b, p. 751)

Realism led Keynes to the conclusion that the 'classics', i.e. the British neoclassical school, stood on the wrong side of the line that demarcates science from nonscience but he could not offer an in all respects superior alternative. With regard to weird behavioral assumptions common sense points the way to the right side of the demarcation line. To follow it, however, is beyond common sense.

... Keynes, too, sometimes gave the impression of not having fully grasped the logic of his own system. (Laidler, 1999, p. 281)

Keynes famously announced his revolution with a reference to Euclid.

Yet, in truth, there is no remedy except to throw over the axiom of parallels and to work out a non-Euclidean geometry. Something similar is required to-day in economics. (Keynes, 1973, p. 16)

This in turn would have required some sort of non-Euclidean axioms, that is, a bit more formalization than Keynes was prepared to do himself.

I mean by this that formalization eliminates provincial and inessential features of the way in which a scientific theory has been thought about. ... Formalization is a way of setting off from the forest of implicit assumptions and the surrounding thickness of confusion, the ground that is required for the theory being considered. ... In areas of science where great controversy exists about even the most elementary concepts, the value of such formalization can be substantial. (Suppes, 1968, pp. 654-655)

## 5 Heterodox disarray

The main 'culprit', I shall argue is a mode of explanation that can be referred to as *deductivist*, or, more particularly, it is the conception of 'laws' (or 'significant results' or 'theoretical formulations') upon which deductivist explanation ultimately depends. (Lawson, 1997, p. 16), original emphasis

Is deductivist the same thing as deductive, i.e. 'the process of reasoning from one or more general statements ... to reach a logically certain conclusion' (Wikipedia: Deductive reasoning)? Obviously not.

By deductivism I simply mean the collection of theories ... that is erected upon the event regularity conception of laws .... (Lawson, 1997, p. 17)

Now, the conception of a law implies a deterministic event regularity in the causal form 'if event X then event Y.' This, though, is quite different from the deductive form which states 'if antecedent X then consequent Y.' This form has nothing to do with deterministic causal laws.

... deductive chains of reasoning cannot on their own establish the existence of causal processes in the real world. (Hodgson, 2001, p. 76)

Yet the two are closely interrelated in physics.

To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*. (Popper, 1980, p. 59), original emphasis

The key point is that universal laws are taken as premises. Deduction is applied in physics too, but physics's hallmark are deterministic laws while mathematics is purely deductive.

It is a well-known jest that 'a mathematician is a scientist who knows neither what he is talking about nor whether whatever he is talking about exists or not'. (Cartan, quoted in Ronan, 2006, p. 70)

Nobody has ever criticized mathematicians for being "deductivist". Quite the contrary, the plain fact that products of pure deductive reasoning correspond in numerous cases admirably to the objects and processes of reality has puzzled physicists, philosophers, and the mathematicians themselves since the ancient Greeks.

It is the idea of an event regularity in the form of a law that has been identified by Lawson as main culprit. Hence "determinist" instead of "deductivist" would have been a less ambiguous characteristics. The deductive method does not necessarily imply deterministic laws that enable prediction in the social realm. This is known since J. S. Mill.

It is evident, in the first place, that Sociology, considered as a system of deductions *à priori*, cannot be a science of positive predictions, but only of tendencies. (Mill, 2006b, p. 898)

Positive prediction would only be possible if the premises were universal deterministic laws.

If the conditions of the theory are satisfied, the events that it predicts will necessarily take place. This inevitability of the analysis accords it a considerable prognostic significance, according to Robbins. Seldom has a simple view of a matter found so much support as the apriorism that he professed, which John Stuart Mill . . . developed for the first time under the name 'concrete deduction' as a variant of the hypothetico-deductive model of physics. (Klant, 1994, p. 25)

The salient point is easy to see. Robbins presupposed the existence of universal deterministic behavioral laws. This, evidently, has nothing to do with the deductive method. What Lawson criticizes under the label "deductivist" is Robbins's misapplication. At first it seems that Lawson got the point.

Certainly, any application of the retroductive . . . form of reasoning requires an explicit prior statement of the premises which are used to initiate the analysis. Nor, of course, is deduction *per se* ruled out in the latter, or in *any* other general approach to reasoning. (Lawson, 1997, p. 112)

But in the next sentence he equates “deductivist” with what in fact is “determinist”.

The employment of deductive logic, where it is appropriate, is not accepting the deductivist form of analysis (whereby the object always is to deduce specific claims about actualities from accepted ‘laws’ and initial conditions, possibly including its axioms and assumptions). (Lawson, 1997, p. 112)

This means in more concrete terms.

The essence of neoclassical economic theory is its exclusive use of a deductivist Euclidean methodology. A methodology – which Arnsperger & Varoufakis calls the neoclassical meta-axioms of “methodological individualism, methodological instrumentalism and methodological equilibration” – that is more or less imposed as constituting economics, and, usually, without a smack of argument. (Pålsson Syll, 2010, p. 24)

We are no longer occupied with the deductive method pure and simple as conceived by Mill. So this is what is at issue: (a) the deductive method is mistaken, or (b), there is nothing wrong with the method but the neoclassical meta-axioms and deterministic behavioral laws are beside the point.

And here is where the flimsy logic of the critics of the neoclassical approach comes in. From the widely accepted fact that neoclassical economics is unsatisfactory and the correct observation that it applies the deductive method and produces an abundance of vacuous mathematical models the conclusion is drawn that the method is wrong. The simple fact is – as already noticed by Poincaré – that the foundational assumptions of neoclassical economics are inadmissible. Hence the correct conclusion is to reject the meta-axioms and to keep hold of the deductive method because it is neutral with regard to premises. With false premises it yields the false conclusion and vice versa with true premises. It is as straightforward as ‘garbage in, garbage out’. What is needed are true premises.

Each theory (heterodox approaches are no exception) starts from ‘hypotheses or axioms or postulates or assumptions or even principles’ (Schumpeter, 1994, p. 15). Therefore, the crucial question is:

What are the propositions which may reasonably be received without proof? That there must be some such propositions all are agreed, since there cannot be an infinite series of proof, a chain suspended from nothing. (Mill, 2006a, p. 746)

No theory whatever can dodge this question. Emphasizing that neoclassical economics is unconvincing is neither new nor helpful. Mathematics as pure deduction is not the problem either. It allows us to express the wrong idea that the planets move in circles or the right idea that they move in ellipses. By the same token it allows us to express the wrong idea that the economy is a deterministic equilibrium system and the right idea that it is a nondeterministic open system. Now, take the mathematics away and what is left?

To Plato's question, "Granted that there are means of reasoning from premises to conclusions, who has the privilege of choosing the premises?" the correct answer, I presume, is that anyone has this privilege who wishes to exercise it, but that everyone else has the privilege of deciding for himself what significance to attach to the conclusions, and that somewhere there lies the responsibility, through the choice of the appropriate premises, to see to it that judgment, information, and perhaps even faith, hope and charity, wield their due influence on the nature of economic thought. (Viner, 1963, p. 12)

This is a fair appraisal of the deductive method. What could be the objections against it? No methodologist ever maintained that it automatically produces 'true' theories. This may appear as a serious drawback, but neither exaggerated claims nor disappointed expectations provide a valid argument against the method.

The gist of the whole matter is: by rightly sticking to the deductive method yet applying indefensible premises neoclassical economics discredited the method in the eyes of critics. This would be a minor casualty were it not for the fact that by rejecting the method heterodoxy deprives itself of one of the most elementary scientific tools to build up a serious theoretical alternative.

... we may say that the long-lasting success of our categories and the omnipresence of a certain point of view is not a sign of excellence or an indication that the truth or part of the truth has at last been found. It is, rather, the indication of a *failure of reason* to find suitable alternatives which might be used to transcend an accidental intermediate stage of our knowledge. (Feyerabend, 2004, p. 72), original emphasis

## 6 Deduction vs. intuition: a phony trade-off

A purely deductive method would ensure us that conclusions were as probative as the premises on which they build. But deduction is totally unampliative. Its output is in its truth-transmitting input. If we are to use content-increasing methods we therefore have to accept that they can't be of a deductive caliber. (Pålsson Syll, 2010, p. 48)

Indeed, but this is the very strength of the method and not a lamentable weakness. Two points are essential: to state the premises explicitly and then to develop the logical implications without tacitly changing the premises on the way and without introducing additional premises. If there is truth in the premises it is conserved, nothing is added and nothing is lost. The method ensures formal consistency, not more, not less.

Research is in fact a continuous discussion of the consistency of theories: formal consistency insofar as the discussion relates to the logical

cohesion of what is asserted in joint theories; material consistency insofar as the agreement of observations with theories is concerned. (Klant, 1994, p. 31)

Formal consistency, of course, is not all but it is a necessary condition ‘for he who contradicts himself proves nothing’ (Klant, 1988, pp. 112-113).

By its very nature the deductive method must not be content-increasing. The content resides in the premises. Hence the choice of premises is decisive. This choice, though, is antecedent to the application of the deductive method. This is long known from the history of science.

Popper demonstrates that “logic, whether deductive or inductive, cannot possibly make the step from these theories [of Galileo and Kepler] to Newton’s dynamics. It is only ingenuity which can make this step.” (Cohen, 1977, p. 335)

In a similar way Einstein speaks of the ‘search for those highly universal laws . . . from which a picture of the world can be obtained by pure deduction. There is no logical path’, he says, ‘leading to these . . . laws. They can only be reached by intuition, based upon something like an intellectual love (‘Einführung’) of the objects of experience. (Popper, 1980, p. 32)

And yet, by three incorrect steps . . . , Kepler stumbled on the correct law. It is perhaps the most amazing sleepwalking performance in the history of science . . . (Koestler, 1979, p. 333)

The pivot of any scientific inquiry is – once more:

What are the propositions which may reasonably be received without proof? That there must be some such propositions all are agreed, since there cannot be an infinite series of proof, a chain suspended from nothing. But to determine what these propositions are, is the *opus magnum* of the more recondite mental philosophy. (Mill, 2006a, p. 746), original emphasis

Deduction does not prevent intuition, it rather presupposes the *opus magnum* of intuition.

## **7 Refocusing the domain**

In fact, the history of every science, including that of economics, teaches us that the elementary is the hotbed of the errors that count most. (Georgescu-Roegen, 1970, p. 9)

This brings us to the very question of what the elementary in the infinite multitude of economic phenomena is.



Thus, economics is apparently the study of the economy, the study of the coordination process, the study of the effects of scarcity, the science of choice, and the study of human behavior. One possible conclusion to draw from this lack of agreement is that the definition of economics does not really matter. (Backhouse and Medema, 2009, p. 221)

The task of theoretical economics is to create a mental map of the whole economy without firsthand experience.

And in the social sciences it is even more obvious than in the natural sciences that we cannot see and observe our objects before we have thought about them. For most of the objects of social science, if not all of them, are abstract objects; they are *theoretical* constructions. (Popper, 1960, p. 135), original emphasis

That is, one has to leap from commonplace economics which trades in easy to grasp phenomena on a small scale to an extremely abstract set of foundational propositions about the economy as a whole.

Since, therefore, it is vain to hope that truth can be arrived at, either in Political Economy or in any other department of the social science, while we look at the facts in the concrete, clothed in all the complexity with which nature has surrounded them, and endeavor to elicit a general law by a process of induction from a comparison of details; there remains no other method than the *à priori* one, or that of “abstract speculation.” (Mill, 2004, p. 113-114)

The set of basic propositions has to reduce the vast complexity of the real thing to almost nothing. From this almost-nothingness the real world complexity then has to be logically reconstructed. The first task is to clarify the domain of the inquiry which is neither well-defined nor arbitrary.

Scientific domains are characterized as a number of *items of information* (putative facts, including, perhaps, accepted laws and theories) which come to be associated together as a *body of information* having the following characteristics: the association is based on some well-grounded, significant, relationship between the items of information which are suggestive of deeper unities among the items; ... (Suppe, 1977, p. 686), original emphasis

The clarifying of the domain involves a tentative decision of what to take in and what to leave out. For example: the trajectories of a feather and a canon ball both belong to the physical realm. Being too complex the physicists ignored the flying feather and focused on the falling canon ball. In this manner most real world phenomena drop out of the domain – at least for the time being. One has no guarantee that this abstraction from supposedly insignificant phenomena will work or whether one gets hold of the significant relationships. Here is where intuition and skill come in.

The more complicated the model and the greater the number of the variables involved, the further it moves beyond our mental control, which in social sciences is the only possible control. ... A “simple-minded” model may after all be the more enlightening representation of the economic process provided that the economist has developed his skill to the point of being able to pick up a few but significant elements from the multitude of cluttering facts. The choice of relevant facts is the main problem of any science, as Poincaré and Bridgman insisted. (Georgescu-Roegen, 1971, pp. 340-341)

For the purposes of theoretical economics real human beings have been reduced to homo oeconomicus.

No science has been criticized by its own servants as openly and constantly as economics. The motives of dissatisfaction are many, but the most important pertains to the fiction of *homo oeconomicus*. (Georgescu-Roegen, 1971, p. 1)

Since homo oeconomicus is patently alien there was an almost instinctive call for more realism. Commonsensical as it is, this conclusion jumps too short. The fact that human beings belong to the economic realm does not automatically imply that they belong to the domain of economics or that they have to occupy a larger part of it. In classical economics the main issues were accumulation, innovation, competition, productivity, distribution of income and wealth etcetera. Homo oeconomicus was, if anything, a side-show. The real humans belonged to the domains of psychology, anthropology, sociology and biology.

It cannot be the intent of an economist who is on his way to understand how the economy works to get lost in these domains. Insofar, the reduction to homo oeconomicus is justified. What is more, the prospects of rendering economics more realistic by making homo oeconomicus more realistic are rather unpromising.

The human or personal factor will remain *the* irrational element in most, or all, institutional social theories. (Popper, 1960, p. 157), original emphasis

The quest for the laws of human behavior begins and ends either with a diffuse psychological account that is hardly ever distinguishable from a projection or with a patently weird idealization. Therefore it was, in the first place, not such a good idea to put theoretical economics on so weak a foundation.

The abstract idea of wealth or value in exchange ... must be carefully distinguished from accessory ideas of utility, scarcity and suitability to the needs and enjoyment of mankind .... These ideas are variable, and by nature indeterminate and consequently ill suited for the foundation of a scientific theory .... (Cournot, quoted in Mirowski, 1995, p. 208)

Let us call this Cournot's Unfitness Proposition. It asserts that behavioral assumptions are incapable of supporting a sophisticated theoretical superstructure that corresponds reasonably well with real world phenomena.<sup>2</sup> And from this follows for the route to be taken:

The purpose . . . is to criticise the notion that economics is a science of behaviour or that a science of behaviour is fundamental to economics. This plausible and, as I believe, mistaken idea has sometimes been called (methodological) psychologism, . . . . In opposition to psychologism I put forward the notion of economics as a study of spontaneous order independent of any behavioural science. . . . If it is correct, then all the attempts to derive an adequate model of economic behaviour (as practised, for example, by the representatives of 'behavioural' or 'psychological economics') are misconceived. (Hudík, 2011, p. 147)

The critics of the neoclassical approach correctly spotted that the whole edifice rests on a set of behavioral axioms. Yet with the attempt to make the formal representation of choice more realistic the critics actually confirm its implicit assumption which reads: in order to explain the economy it is necessary to explain human behavior first.

If we ask, 'What is the most adequate model of behaviour for economics?' we implicitly assume that economics actually needs a model of behaviour; hence, we already assume psychologism of a kind. (Hudík, 2011, p. 147)

Therefore, one has to go one step further and to move human behavior from the center of the domain to the periphery. Put simply, it is advisable to change the definition

Economics is the science which studies human behavior as a relationship between ends and scarce means which have alternative uses. (Robbins, 1935, p. 16)

to

Economics is the science which studies how the economic system works.

What is demanded, then, is the reconstruction of a coherent theoretical superstructure on a *nonbehavioral* foundation. This, of course, is not an entirely novel idea.

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<sup>2</sup> Some classics grasped this intuitively: "Macaulay pointed out that asserting restrictive, unrealistic assumptions about human nature and then deducing the whole science of politics was ridiculous." (Redman, 1997, p. 322). See also (Hudson, 2010, pp. 14-16). Modern physicists are perfectly aware of the decisive methodological point: "By having a vague theory it is possible to get either result. . . . It is usually said when this is pointed out, 'When you are dealing with psychological matters things can't be defined so precisely'. Yes, but then you cannot claim to know anything about it." (Feynman, 1992, p. 159)

The highest ambition an economist can entertain who believes in the scientific character of economics would be fulfilled as soon as he succeeded in constructing a simple model displaying all the essential features of the economic process by means of a reasonably small number of equations connecting a reasonably small number of variables. Work on this line is laying the foundations of the economics of the future . . . (Schumpeter, 1946, p. 3)

The mathematical method as such is not the cause of the ongoing economic crisis. This is not to say that the method has been applied correctly. Thus far the heterodox critique is justified. Yet:

. . . it is important to understand that what is put in question by recent destructive results is not formalization in general but rather the particular formalization generally employed in economic theory. That a paradigm should be shown to be deficient does not imply that one should cease to search for a paradigm. (Kirman, 1997, p. 97)

Neither common sense nor plain realism nor psychologism is a promising replacement. As it stands at the moment, heterodox methodology is part of the malaise rather than part of the solution.

## 8 Conclusion

The main result from the appraisal of economic methodology has been that behavioral assumptions, rational or otherwise, are not solid enough to be eligible as first principles of theoretical economics. Hence all – orthodox and heterodox – endeavors to lay the formal foundation on a new site and at a deeper level need no further vindication.

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