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Egmont Kakarot-Handtke

University of Stuttgart, Institute of Economics and Law

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The Rhetoric of Failure: A Hyper-Dialog About Method in Economics and How to Get Things Going

Egmont Kakarot-Handtke*

Abstract

All are agreed that orthodox economics is unsatisfactory but there is wide disagreement, especially among heterodox critics, whether the problems lie at the level of substantive theory or at the level of methodology. This paper gives first an overview of the methodological questions at issue. The frame of reference includes J. S. Mill, Jevons, Popper, Keynes, and Lawson. Drawing on the conclusions, the domain of economics is subsequently refocused. Human behavior is moved from the center to the periphery. From elementary systemic properties the relation of income and profit is then consistently derived. This solves the profit conundrum.

JEL B10, B20, B30, E10

Keywords new framework of concepts; structure-centric; axiom set; income; profit; Mill's Impossibility Proposition; Physicist's Nonentity Proposition; Cournot's Unfitness Proposition; Hudfk's Independence Proposition

*Affiliation: University of Stuttgart, Institute of Economics and Law, Keplerstrasse 17, D-70174 Stuttgart. Correspondence address: AXEC, Egmont Kakarot-Handtke, Hohenzollernstraße 11, D-80801 München, Germany, e-mail: handtke@axec.de

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. (Lawson, 2012, p. 3), see also (Colander et al., 2009), (Buiter, 2009), (Solow, 2010), (Stiglitz, 2011)

Except for the casualties, an economic crisis is good for many things. Defensive heroism is the natural stance of the orthodoxy. After all, a crisis is the opportunity to learn from mistakes.

... economists are in fact reenacting yet another round in what has become by now an old ritual in the profession: past failures of the most analytically sophisticated methods to gain much economic insight (as is now the verdict on neoclassical analysis) are candidly acknowledged, but then encouragement is offered for the economics profession to press forward to develop still more analytically advanced methods ... (Nelson, 2006, p. 227)

For the heterodoxy it is an opportunity to make their point with more force and convenient empirical evidences. Joan Robinson did so in *The Second Crisis of Economic Theory*:

I am talking about the evident bankruptcy of economic theory which for the second time has nothing to say on the questions that, to everyone except economists, appear to be most in need of an answer. (Robinson, 1972, p. 9)

The first crisis, of course, was the Great Depression. Among Keynesians it was taken as a self-evident refutation of orthodox economic theory. However, Keynesians learned to their chagrin that this stratagem works both ways. The stagflation of the 1970s was taken as self-evident refutation of Keynesianism. The pattern is quite clear: an economic crisis is at the same time a crisis of the ruling paradigm.

While certainly not denying the rhetorical usefulness of this kind of argumentation it nevertheless cannot be admitted in the dispute of competing paradigms. The interrelation between economic theory and the actual performance of the economy is too subtle as to allow for a straightforward causality. Moreover, critics are rarely poised to accept the logically symmetrical argument that the relatively good performance of the economy between crises is due to the ruling paradigm. And finally it is worth remembering that

... there is no theory that is not beset with problems. (Popper, 1994, p. 157)

That there must be something better than orthodox economics, all are agreed (including the orthodox), but this consensus is accompanied by a bookshelves-filling disagreement about diagnosis and remedy. Regrettably the better theory is not available when the next crisis hits. The rhetoric of failure is not of much help.

The moral of the story is simply this: it takes a new theory, and not just the destructive exposure of assumptions or the collection of new facts, to beat an old theory. (Blaug, 1998, p. 703)

1 All are agreed – but on what?

As will become evident, there is more agreement on the defects of orthodox theory than there is on what theory is to replace it: but all agreed that the point of the criticism is to clear the ground for construction. (Nell, 1980, p. 1)

Logical inconsistency:

... Keynes was simply arguing that microeconomic theory is *false*! Presumably, it is false because it is not logically consistent with all macrophenomena – such as persistent disequilibria – and thus, by *modus tollens*, at least one of the assumptions of microtheory is false and hence microtheory as a whole is false. (Boland, 2003, p. 143), original emphasis

Shoddy argumentation:

The currently prevailing pattern of economic theorizing exhibits the following three characteristics: (1) a syncopated style of argument fluctuating back and forth between literary and symbolic modes of expression, (2) naive translation, or the loose paraphrasing of formulae into sentences, and (3) loose verbal reasoning for certain aspects of theoretical argumentation where explicit symbolic formulation is lacking. (Dennis, 1982, p. 698)

Inappropriate copying of physics:

Thus many are inclined to blame inappropriate copying of physics for the willingness of neoclassicals to tolerate bizarrely unrealistic assumptions and to place everything historical, cultural, institutional, and even psychological outside the framework of economic analysis. (Porter, 1994, p. 128)

Borrowing the wrong concepts from mathematics:

The discipline of economics has so far successfully resisted all efforts to alter its character as an exercise in how to reason deductively from axiomatic principles. That is, it has insisted on remaining the Euclidean geometry of the social sciences. (Eichner, 1979, p. 172)

Overemphasis of the mathematical method:

It is thus 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

Misapplication of mathematics:

A second class of economists contain those who have abundantly employed mathematical apparatus, but, misunderstanding its true use, or being otherwise diverted from a true theory, have built upon the sand. (Jevons, 1911, p. xxv)

Mock precision:

Much economic theorising to-day suffers, I think, because it attempts to apply highly precise and mathematical methods to material which is itself much too vague to support such treatment. (Keynes, quoted in Chick, 1998, p. 1864)

Lack of facts:

Next, the empirical background of economic science is definitively inadequate. Our knowledge of the relevant facts of economics is incomparably smaller than that commanded in physics at the time when the mathematization of that subject was achieved. ... It is due to the combination of the above mentioned circumstances that mathematical economics has not achieved very much. (von Neumann and Morgenstern, 2007, p. 4)

Inconclusiveness of facts:

Even econometricians using identical, or almost identical, data sets are regularly found to produce quite contrasting conclusions, usually with little attempt at explanation. The systematic result here, as the econometrician Edward Leamer observes, is that: "hardly anyone takes anyone else's data analysis seriously". (Lawson, 2012, p. 9)

Denial of empirical refutation:

... suppose they [the economists] did reject all theories that were empirically falsified ... *Nothing would be left standing*; there would be no economics. (Hands, 2001, p. 404), original emphasis

Misleading principles:

Now the rationality principle, which in the social sciences plays a role somewhat analogous to the universal laws of the natural sciences, is false, and if in addition the situational models are also false, then both the constituent elements of social theory are false. (Popper, 1994, p. 173)

Mistaken beliefs:

The notion that microeconomics is a branch of applied mathematics does economists more credit than several possible alternative explanations for its empirical weakness. ... It isolates the limitations of the theory in a factual supposition about the determinants of human behavior, one that economists share with all of us. But the supposition we all share is false, and so economics rests on a purely contingent, though nevertheless central, mistaken belief (Rosenberg, 1992, p. 247)

Lack of realism:

Upon leaving office, each new president of the American Economic Association gives the expected speech showing that he knows full well it is all just a game, and chastises his colleagues for not being more realistic. (Hudson, 2010, p. 9)

Assumptionism:

Cunningham in 1891 remarked that in the choice of premises “it is not always easy to tell when a professor of the dismal science is making a joke” ... (Viner, 1963, p. 12)

Ignorance of time and history dependence:

The notion of time is so primitive and basic an element in man’s experience that its neglect by much economic theory constitutes an incredible puzzle. This puzzle is attributable, perhaps, to the almost irresistible lure of formalism – particularly one that cannot adequately handle time. (Rizzo, 1979, p. 1)

History dependence stares us in the face . . . , but it is not the stuff of pure theory. (Hahn, 1991, p. 48)

Ignorance of institutions:

In the earlier part of the century there were major conflicts between institutional economists, who saw the particular arrangements by which particular economies conducted their economic affairs as essential, and neoclassical economists, who sought to see through these inessential details to the underlying fundamental forces – the forces of demand and supply. (Stiglitz, 1991, p. 136)

Counterfactual equilibrium and zero profits:

The Walrasian prices correspond to the Marshallian long-run equilibrium prices where every producer is making zero excess profits. . . . But, from the macro perspective of of Walrasian general equilibrium,

the *total* profits in this case cannot be other than zero (otherwise, we would need a Santa Claus to provide the aggregated positive profit) . . . (Boland, 2003, p. 150), original emphasis

Questionable foundational assumptions and unjustifiable policy advice:

Much of economic theory is based on three questionable assumptions: (1) the world is deterministic; (2) decision makers act as if they know the values of all relevant parameters; and (3) consumers and firms respectively, act as if they were maximizing utility and profit. . . . In order to tell the politicians and practitioners something about causes and best means, the economist needs the true theory or else he has not much more to offer than educated common sense or his personal opinion. Economists derive theorems, make predictions, and (!) suggest economic policies using arguments which explicitly or implicitly postulate the validity of the first two assumptions mentioned above. Such endeavors are unjustifiable unless it can be demonstrated that the most important doctrines of economics are insensitive to a relaxation of these two assumptions. (Stigum, 1991, pp. 29-30)

Wrong methodology:

The essence of contemporary mainstream economics does not lie at the level of substantive theory as most of its critics suggest, but at the level of methodology. (Lawson, 1997, p. 282)

Evasion of reality:

The modern history of economic theory is a tale of evasion of reality. Faced with the challenge of these vast changes and vital problems for over a century, it reacted by denying their existence in order to be able to produce a scientific system, a smoothly-functioning self-balancing model. (Balogh, 1982, p. 32)

No predictive capacity:

Economic theory seems permanently stuck at the level of generic predictions – predictions that some change will happen some time and some place, without ever telling us when and where and how much of a change will occur. (Rosenberg, 1994, p. 217)

An unfinished task:

It is good to have [the technically best study of equilibria], but perhaps the time has now come to see whether it can serve in an analysis of how economies behave. The most intellectually exciting question of our subject remains: is it true that the pursuit of private interest produces not chaos but coherence, and if so, how is it done? (?, p. 102)

Lack of imagination:

In other words, the main developments in economics of the twentieth century . . . had been more a matter of form than of substance. . . . Little new of any great significance has been learned about the workings of markets since Adam Smith and . . . Smith added much less to the discussion than most economists have commonly supposed. (Nelson, 2006, p. 298)

Lack of convincing alternatives:

If one calls those individuals working in the field of microeconomic foundations of Keynesian economics Keynesian-economic theorist, then, as Hahn has said, these Keynesians were not much better. (Morishima, 1984, p. 57)

No future:

There seems to be a quiet confidence in the profession that we are moving, if only slowly, towards a more scientific basis for economics. . . . Paradoxically many of those who have contributed much to the development of general equilibrium theory are less complacent. (Kirman, 1989, p. 126)

. . . anything based on this mock-up is unlikely to fly. (Hahn, 1981, p. 1036)

Ideological bias:

Evidently, the tools are not strong enough to discriminate among fundamentally different hypotheses, or at least not strong enough to overcome differences in prior beliefs, beliefs which are often influenced by ideological concerns. (Stiglitz, 1991, p. 134)

Political bias:

Broadly speaking, policies fall into two categories: laissez faire or interventionist public regulation. Each set of advocates has its own preferred mode of mathematical treatment, choosing the approach that best bolsters their own conclusions. (Hudson, 2010, p. 6)

Lack of public assurance:

. . . it is clear that the public's lack of faith in the scientific nature of economic knowledge is a fact, past and present. (Benetti and Cartelier, 1997, pp. 211-212)

Pseudo-science:

Suffice it to say that, in my opinion, what we presently possess by way of so-called pure economic theory is objectively indistinguishable from what the physicist Richard Feynman, in an unflattering sketch of nonsense "science," called "cargo cult science". (Clower, 1994, p. 809)

Pre-science:

The position I now favor is that economics is a pre-science, rather like astronomy before Copernicus, Brahe and Galileo. I still hold out hope of better behavior in the future, but given the travesties of logic and anti-empiricism that have been committed in its name, it would be an insult to the other sciences to give economics even a tentative membership of that field. (Keen, 2011, p. 158)

Counterproductive organization:

It could be argued, moreover, that the present organization of the profession is itself a negative factor to the extent that the requirement of rapidly obtained results discourages researchers from entering the domains that are most uncertain (even if they are essential). (Benetti and Cartelier, 1997, p. 215)

Reluctance to scrap obsolete intellectual capital . . .

Gary Becker has suggested that a substantial resistance to the acceptance of new ideas by scientists can be explained by two familiar economic concepts. One is the concept of specific human capital: the established scholar possesses a valuable capital asset in his command over a particular body of knowledge. That capital would be reduced if his knowledge were made obsolete by the general acceptance of a new theory. Hence, established scholars should, in their own self-interest, attack new theories, possibly even more than they do in the absence of joint action. The second concept is risk aversion, which leads young scholars to prefer mastery of established theories to seeking radically different theories. Scientific innovators, like adventurers in general, are probably not averse to risk, but for the mass of scholars in a discipline, risk aversion is a strong basis for scientific conservatism. (Stigler, 1983, p. 538), see also (Sy, 2012, pp. 71-73)

... or just waiting for the heterodox to deliver?

There is no evidence to suggest that economists abandon degenerating programs in the absence of a progressive alternative. We do not, in the face of falsified theories in the belt of a program, abandon that program until there is an alternative program with theories that are themselves corroborated. (Weintraub, 1985, p. 148)

... if you think you can do better with a non-neoclassical model ..., then you are quite welcome to try. (Boland, 1992, p. 19)

2 Demarcation: The birth of science out of the spirit of the myth

. . . since we are all humans everything reduces to psychology (Boland, 2003, p. 107)

Myth, well told, is still the most convincing way to explain how the world and humankind came to be in their present form. To recall, Zeus oversaw the universe, assigned the various gods their roles, and was known for his erotic escapades. Zeus had a lot of trouble. At Prometheus, for example, he was angry for being tricked on sacrifices, stealing fire for man, and for refusing to tell him which of his children would dethrone him. To handle their controversies the Olympian agents regularly fell back to chicanery and violence. Purified from all religious connotations myth is the tried and tested raw material of art, history, soap-opera and small talk. Psychologism is the most congenial mode of explanation. The ancient Greeks regarded myths as ‘true stories’ and distinguished them from fables as ‘false stories’. Xenophanes made his contemporaries aware that their true stories were what is now called a projection (Popper, 1994, p. 39).

With this, the problem of demarcation arose for the first time. And it was easily solved. The Pre-Socratics rejected the mythological explanations of the world because they saw that everything could be explained by the human-like actions of gods which meant on closer inspection: nothing. This methodological insight set science on its track. Rejecting gods as valid explanation, however, caused an obvious problem. If myth is not truth, what then is truth? And to this question the Pre-Socratics could not offer an immediate answer but only a vague research program. They sought the material principle of things and they came up with different answers.¹ This was not an entirely satisfactory outcome because, as a matter of principle, only one of the answers could be true. The demarcation problem appeared in a new form *within* the compass of the infant science. All finer points of methodology are derivatives of the primordial demarcation.

The finer points arose quite naturally. It is easy to keep Zeus out of the discussion. But what about notions like the Absolute, the One, Logos; and what about the invisible atom or the somewhat fantastic harmony of numbers, music, and celestial spheres? Demarcation became more subtle. Basically however, it remained a purely methodological question.

The problem of finding a criterion which would enable us to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as ‘metaphysical’ systems on the other, I call the *problem of demarcation*. This problem was known to Hume who attempted to solve it. With Kant it became the central problem in the theory of knowledge. (Popper, 1980, p. 34), original emphasis

¹ For short overviews see Wikipedia under Mythology and Pre-Socratic Philosophy For a pertinent account of the Pythagorean synthesis see (Koestler, 1979, pp. 27-42)

Being humans, ‘a muddled horde of erring mortals, always in two minds about things’ (Parmenides), it was inevitable that demarcation eventually became a social issue. The early philosopher-scientists organized themselves in schools (cf. Fleck, 1980) and this brought with it the necessity to define their relations towards their social and political surroundings. This they did. The Pythagoreans preferred to keep their thoughts and findings for themselves. Others spoke on the agora to their fellow-citizens. Demarcation was a real issue.

In our day McCloskey solved the demarcation problem by simple deconstruction, that is, by muddling the epistemic and the social. After all, the pursuit of scientific truth, whatever that may be, is undeniably a social activity and, as things are, ultimately boils down to the usual social status quarrels.

In practice methodology serves chiefly to demarcate Us from Them, demarcating science from nonscience. Once the modernist have found a Bantustan for nonscience such as astrology, psychoanalysis, acupuncture, nutritional medicine, Marxist economics, spoonbending, or anything else they do not wish to discuss, they can get on with the business at hand with a clear head. Methodology and its corollary, the Demarcation Problem (What is Science? How is It to be distinguished from nonscience?), are ways at stopping conversation by limiting conversation to people on our side of the demarcation line. (McCloskey, 1998, p. 161)

Popper, for one, was as clear on the difference between epistemic demarcation and social discrimination as anyone could wish. And he was quite explicit that he was not much interested in the latter.

And my doubts increase when I remember that what is to be called a ‘science’ and who is to be called a ‘scientist’ must always remain a matter of convention or decision. (Popper, 1980, p. 52)

Within McCloskey’s framework of rhetoric the demarcation problem is correctly perceived as a conversation-stopper. Rhetoric as a discipline becomes pointless if conversation dries up, hence it has to keep out, not gods, but conversation-stoppers. Rhetoric itself faces the demarcation problem, albeit in a different guise. What is more, conversation *per se* is the ideal of the talk-show, not of science. Here, the regulative idea is quite different.

A critical discussion is well-conducted if it is entirely devoted to one aim: to find a flaw in the claim that a certain theory presents a solution to a certain problem. . . . Thus critical discussion is essentially a comparison of the merits and demerits of two or more theories . . . The chief demerit is inconsistency, including inconsistency with the results of experiments that a competing theory can explain. (Popper, 1994, pp. 160-161)

The demarcation problem cannot be muddled away and it cannot be solved once and for all.

... we have seen that a characteristic feature of science is that it develops canons of rationality for evaluating knowledge claims and deciding what to admit into the domain of knowledge. ... these canons of rationality change, evolve, and become more sophisticated as science develops. (Suppe, 1977, p. 724)

To date the demarcation problem has not been answered satisfactorily for economics.

Economics is a perplexing subject. Though I have spent the better part of my academic career thinking about its aims and methods, I have never been confident that I or anyone else for that matter really understand its cognitive status. ... Without assurance about the cognitive status of the theory; there is no basis of confidence in it. (Rosenberg, 1994, pp. 216-217), see also (Kirman, 1997, pp. 98-99)

3 J. S. Mill: The denial of methodological individualism before its invention

As we shall see, general economic equilibrium theory originated and developed in the context of a project put forward in varying forms by different scholars to repeat Newton's titanic achievement ... (Ingrao and Israel, 1990, pp. 33-34), see also (Redman, 1993, pp. 98, 108)

The ancient Greeks had excluded psychologism from the study of nature and mathematics but it came back with double force in their inquiries of man and society.

... the entire topic had become a scandal in the eighteenth century. If scientific method could institute some degree of order in chemistry, in physics, in astrophysics, in astronomy and so forth, why did we have to be plunged into this dreadful chaos of conflicting opinions, with not a thread to guide us? ... so that nobody is able to institute the kind of order which Newton established in the great realm of nature? Naturally enough, men's wishes began to move towards the delineation of some simple single principle which would guarantee just such order and yield truths of just such an objective, general, lucid, irrefutable kind as had so successfully been obtained concerning the external world. (Berlin, 2002, p. 9)

J. S. Mill tackled the problem head-on.

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)

And he concluded that methodological individualism is impossible even under ideal conditions.

Hence, even if our science of human nature were theoretically perfect, that is, if we could calculate any character as we can calculate the orbit of any planet, *from given data*; still, as the data are never all given, nor ever precisely alike in different cases, we could neither make positive predictions, nor lay down universal propositions. (Mill, 2006b, p. 847), original emphasis

Let us refer to these two arguments as Mill's Impossibility Proposition (cf. Rizvi, 1997, pp. 281-283). There seems, though, to be a way around this difficulty.

Inasmuch, however, as many of those effects which it is of most importance to render amendable to human foresight and control are determined, like the tides, in an incomparably greater degree by general causes, than by all partial causes taken together; depending in the main on those circumstances and qualities which are common to all mankind, or at least to large bodies of them, and only in a small degree on the idiosyncrasies of organization or the peculiar history of individuals; it is evidently possible with regard to all such effects, to make predictions which will *almost* always be verified, and general propositions which are almost always true. (Mill, 2006b, p. 847), original emphasis

With this, J. S. Mill defined a niche for economics as 'inexact and separate science' (Hausman, 1992), (Hands, 2001, pp. 14-25). That is to say, he shifted the demarcation line somewhat in favor of economics. It took some time until standard economics rediscovered Mill's insights with Arrow's Richard T. Ely lecture about the problems of methodological individualism in 1994 (Arrow, 1994).

4 Jevons: Here are the behavioral 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)

The priority was to elaborate the theory. The data were expected to come in due course.

I do not hesitate to say, too, that Economics might be gradually erected into an exact science, if only commercial statistics were far more complete and accurate than they are at present, so that the formulæ could be endowed with exact meaning by the aid of numerical data. (Jevons, 1911, p. 21)

And from this clearly follows:

I contend that all economic writers must be mathematical so far as they are scientific at all, because they treat of economic quantities, ... (Jevons, 1911, p. xxi)

Jevons developed the calculus of pleasure and pain. That is, circumventing psychologism as well as numerical data, he cut introspection and mathematics short.

The axioms that are used to define “rationality” are based on the introspection of economists and not on the observed behaviour of individuals. Economists from Pareto through Hicks to Koopmans have long made this point. Thus we have wound up in the weird position of developing models that unjustifiably claim to be scientific because they are based on the idea that the economy behaves like a rational individual, when behavioural economics provides a wealth of evidence showing that the rationality in question has little or nothing to do with how people behave. (Kirman, 2009, p. 81)

5 The art of borrowing

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 (Mach, 1898, p. 230), (Poincaré, 2007, p. 151).

Mill expressly advocated borrowing from the exact sciences. This, however, involved two essentially different elements. First, the idea of 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)

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. (? , p. 110), original emphasis

Mill was quite 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), original emphasis

And without undue delay he stated the fundamental behavioral principle.

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 necessities, 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. (? , p. 110)

With this Mill got rid of psychologism, yet he never claimed that he had formulated a universal behavioral law.

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)

Jevons, too, borrowed from physics.

The Theory of Economy thus treated presents a close analogy to the Science of Statistical Mechanics ... (Jevons, 1911, p. vii)

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.

By the time a sophisticated logical system is build up, it is no longer easy to relax [the] originally *instrumental* but now *ontological* assumptions on a one-by-one basis, because the relaxed assumptions (even those of a more realist kind), still have to be blended into the rest of the system in order to derive new theorems. This, the system can hardly escape being contaminated by the unreal ontology unwittingly formed in the rest of the system, an ontology that no longer has any conceivable counterpart in reality (Woo, 1992, p. 37), original emphasis

While J. S. Mill was on equal terms with the physicists and mathematicians of his day, Jevons did not enjoy the same acceptance.

None of of these scientist-critics aimed to deny the legitimacy of theory, not even in political economy. Nor did they commonly denounce a premature use of mathematics. They objected, rather, to "loose" theorizing. (Porter, 1994, p. 130)

Generally there is nothing wrong with borrowing, only with look-alike copying.

Successful imitation is anything but straightforward. The most indiscriminate copying will not suffice to create a perfect correspondence. What begins as imitation, if it succeeds, must inevitable take a life of its own. (Porter, 1994, pp. 128-129)

Sometimes, however, the look-alike looks more authentic than the original.

... science's evaluatory procedures for assessing knowledge claims ... although generally reliable, are fallible. Sometimes false claims are admitted. (Suppe, 1977, p. 725)

6 Popper: The defense of the indefensible

Profound truths are not to be expected of methodology. (Popper, 1980, p. 54)

Popper declared situational analysis to be a generalization of the method of economic analysis, i.e. the concept of rational choice as inaugurated by Jevons (Blaug, 1994, p. 112).

In my view, the idea of a *social situation* is the fundamental category of the methodology of the social sciences. (Popper, 1994, p. 166), original emphasis

This entailed a demarcation against psychologism.

... I propose to treat both ... aims and knowledge not as psychological facts, to be ascertained by psychological methods, but as *elements of the objective social situation*. (Popper, 1994, p. 167), original emphasis

So first of all, the whole bunch of soap-opera explanations had to be replaced by one principle.

... it is the central point of situational analysis that we need, in order to 'animate' it, no more than the assumption that the various persons or agents involved act adequately, or appropriately – that is to say, in accordance with the situation. ... Thus there is only one animating law involved ... It is known in the literature under the name of 'rationality principle', ... (Popper, 1994, p. 169)

Unfortunately, as Popper did not fail to notice, this principle does not hold water.

But a principle that is not universally true is false. Thus the rationality principle is false. I think there is no way out of this. ... Now if the rationality principle is false, then an explanation that consists of the conjunction of this principle and a model must also be false, even if the particular model is true. (Popper, 1994, pp. 172)

It seems that we have nothing gained with this move.

If we wish nevertheless to uphold the method of situational analysis as *the* proper method of the social sciences, as I certainly do, and if we wish to uphold the view that science searches for truth, are we not in a hopelessly difficult position? (Popper, 1994, p. 173), original emphasis

Yes, indeed, if one contradicts one's most important criterion of demarcation.

... if one maintains the fundamentally individualistic approach to constructing economic models no amount of attention to the walls will prevent the citadel from being empty. Empty in the sense that one cannot expect it to house the elements of a scientific theory, one capable of producing empirically falsifiable propositions. (Kirman, 1989, p. 126)

7 Keynes: The return of common sense

Life is short. Nature is niggardly. Our fellows have other objectives.
(Robbins, 1935, p. 13)

There has always been a third kind of truth. 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 (Coates, 2007, pp. 8-11). 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), original emphasis

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)

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). And there it remained, with a whole generation of economists guessing ‘what Keynes really meant.’

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), see also (Hoover, 1998, p. 243), (Colander, 1995, p. 283), (Harcourt, 1995, pp. 67-69, 207)

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* (or else to keep a low profile), 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)

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), see also (Hoover, 1995, p. 40)

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), see also (Davidson, 2012)

Economists seem to be irresistibly attracted by insolvable puzzles. The solutions then consist regularly of a *petitio principii* (Mill, 2006b, p. 820), that is, the result, e.g. simultaneous equilibrium, is prefigured in the premises, e.g. perfect competition, diminishing returns etc., and then logically derived. Since simultaneous equilibrium is a nonentity the premises cannot be other than weird.

Mankind in all ages have had a strong propensity to conclude that wherever there is a name, there must be a distinguishable separate entity corresponding to the name; ... (Mill, 2006b, p. 756)

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, which means, no entity is ‘corresponding to the name.’ Let us call this the Physicist’s Nonentity Proposition. It is, clearly, a conversation-stopper. 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)

Most important, this stance marked the demarcation line between the modern scientist and the ‘stuffed shirts of the peripatetic school’ (Koestler, 1979, p. 363).

These savants, as Galileo put it, first decided how the world should function in accordance with their preconceived principles. . . . He openly criticized scientist and philosophers who accepted laws which conformed to their preconceived ideas as to how nature must behave. Nature did not first make men’s brains, he said, and then arrange the world so that it would be acceptable to human intellects. (Kline, 1982, p. 48)

A rather obvious example for the peripatetic stance of the orthodoxy is behavioral marginalism. A profit maximum exists only with decreasing returns. Whether returns decrease or increase is an empirical question and we may well find out that decreasing, constant, and increasing returns are randomly distributed among the firms in an economy. However, if one is determined to apply calculus one is forced to ‘arrange the world’ and to take refuge in a weird well-behaved production function, blithely ignoring the fact that it is a nonentity (Hudson, 2010, p. 11).

Realism led Keynes to the conclusion that the ‘classics’, i.e. the British neoclassical school, stood on the wrong side of the demarcation line but in contradistinction to Galileo he could not offer an in all respects superior alternative. Ultimately, a common sense statement is a hypothesis that may be true or false or undecidable like any scientific hypothesis. Therefore common sense needs justification and cannot claim to be an independent and direct source of knowledge. Nonetheless, the appeal to common sense makes a good argument in the political arena. Hence ‘telling a plausible story’ (Harcourt, 1995, p. 207) was Keynes’s uppermost priority.

I consider that Keynes had no real grasp of formal economic theorizing (and also disliked it), and that he consequently left many gaping holes in his theory. I none the less hold that his insights were several orders more profound and realistic than those of his recent critics. (Hahn, 1982, pp. x-xi)

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)

8 Lawson: 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), see also (Westfall, 2008, pp. 422, 645)

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 (Wigner, 1979), (Kline, 1982, pp. 328-354), (Feynman, 1992, p. 171), (Velupillai, 2005), (Penrose, 2007, pp. 613-614).

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)

Yet Mill’s idea of the deductive method was quite different.

The simplest supposition which accords with any of the most obvious facts, is the best to begin with; because its consequences are the most easily traced. This rude hypothesis is then rudely corrected, and the operation repeated; and the comparison of the consequences deducible from the corrected hypothesis, with the observed facts, suggests still further correction, until the deductive results are at last made to tally with the phenomena. (Mill, 2006b, pp. 496-497)

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 premise 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. That the economy evolves is a heterodox tenet at least since Veblen (1961). But since 1898 the proponents of open systems methodology (Dow, 2006, pp. 139-143) could not produce anything that resembles a theory.

The idea of the economy as an open, adapting and evolving system has always been present, but the failure of this point of view to generate any firm propositions about what one might expect to observe, and at the same time its failure to construct any sort of axiomatic theoretical framework led to its marginalization. (Kirman, 1997, p. 103)

This is not a question of formalization but of theoretical ingenuity. The first important point is to get around the peripatetic fallacy and to start with the right foot.

The mathematical language used to formulate a theory is usually taken for granted. However, it should be recognized that most of mathematics used in physics was developed to meet the theoretical needs of physics. *The moral is that the symbolic calculus employed by a scientific theory should be tailored to the theory, not the other way round.* (Wittgenstein, quoted in Schmiechen, 2009, p. 368), original emphasis

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. Science is a trial-and-error process and the deductive method does not provide a free lunch.

Methodology has commonly been seen as providing us with a rule book for rational procedure to follow if it is to discover the way the world is, ... (Coates, 2007, p. 9)

Methodology is nothing of the sort; 'science is not so simple that following a set of ready-made rules will guarantee instant success' (Redman, 1993, p. 86).

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

9 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 (Whewell, 1989, p. 319).

10 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.” (? , 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.

In Mach’s view, facts, however complicated, are reconstructed by a synthetic combination of idealizations. (Klant, 1994, p. 119)

The first task, then, 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)

In the social realm we are confronted with the problem of reflexivity, that is, with humans trying to make sense with human brains of human behavior and this may prove to be a ‘strange loop’ (Hofstadter, 1979, p. 21), see also (Soros, 2010, pp. 10-11).

... several other sciences have emerged ... at a comparatively recent date, none now remain ... except those which relate to man himself, the most complex and most difficult subject of study on which the human mind can be engaged. (Mill, 2006b, p. 833)

For the purposes of economics the multi-dimensional Olympian personnel has been reduced to the one-dimensional 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), original emphasis

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 was 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, as the ancient Greeks already realized, with a psychological account that is hardly ever distinguishable from a projection. 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), see also (Ingrao and Israel, 1990, pp. 38, 41, 47, 81)

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.²

Now, at any rate, we have an explanation for why the assumptions of economic theory about individual action have not been improved, corrected, sharpened, specified, or conditioned in ways that would improve the predictive power of the theory. None of these things have been done by economists because they cannot be done. (Rosenberg, 1992, p. 149)

And from this follows for the route to be taken:

² 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 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)

Let us call this Hudík’s Independence Proposition.

The critics of the neoclassical approach correctly spotted that the whole edifice rests on a set of behavioral axioms (Hands, 2001, pp. 291-294).

There is in economics, or at least among the overwhelming majority of its disciples, broad agreement as to what represents the corpus of their subject. This corpus revolves around the concept of maximizing behaviour, whether it be by the individual, firm or institution. (Blaug, 1990, p. 209)

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)

It is rather obvious that one cannot understand the behavior of the economy by speculating about the behavior of individuals. Therefore, one has to go one step further and to move human behavior from the center of the domain to the periphery. Put simply, we 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.

This involves a shifting of the analytical starting point from the subjective to the objective.

... 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)

11 The real pros and the apparent cons of axiomatization

I believe that it is quite possible to axiomatize economics. (Morgenstern, quoted in Ingrao and Israel, 1990, p. 193)

Axioms are anathema among the critics of orthodoxy, unfortunately for the wrong reasons.

Then there are *axioms* (everyone maximizes his profits; resource allocation is the only economic problem): these are not known in other sciences. An axiom ... is only a premise one is not allowed to question, dressed up as something grand. But it is precisely the scientists duty to question everything! Our crime is not that we use *a priori* reasoning, for often we can use nothing else, but that we push the *a priori* all the way up to the axiom. "Axiom" is, of course, a polite but impressive-sounding word for a "sacred proposition." The concept gives us the impression that it is worthwhile to erect vast superstructures of deduction on virtually no fact, and this has now become a deep-rooted tradition. ... These, then (abstractions, parsimony, axioms, economic determinacy) surely are the "Ricardian Vices" to which we are all heirs; it is these that divert and corrupt our energies. (Wiles, 1979, pp. 163-164), original emphasis

This critique does not apply generally because, as we have seen, Cournot's Unfitness Proposition excludes *a priori* the assumption that everyone maximizes his profits from becoming an axiom. In point of fact, it excludes any behavioral assumption. The critique has been led astray by neoclassical practice. Ignoring this detour, the rationale for an axiomatic approach is rather straightforward.

A formal approach gives the opportunity to make assumptions explicit and to prove results which may previously have been perceived intuitively, or even to deduce conclusions from self-evident truths. These results can then be communicated unambiguously, because they are obtained within a system of agreed rules. There can only be disagreement about the assumptions or the chosen method itself. (Chick, 1998, p. 1867)

This is the proper understanding of axiomatization since the classics.

To Senior belongs the signal honor of having been the first to make the attempt to state, consciously and explicitly, the postulates that are necessary and sufficient in order to build up ... that little analytic apparatus commonly known as economic theory, or to put it differently, to provide for it an axiomatic basis. (Schumpeter, 1994, p. 575), see also (Stigum, 1991, p. 4), (Rosenberg, 1992, pp. 239-248)

Axiomatization has become, again, a serious issue in economics since Wald, von Neumann, McKenzie, Debreu, Arrow and Hahn (Mirowski, 2002, pp. 104-113, 303, 408-409), (Weintraub, 1998), (Leonhard, 1995, pp. 755-756). It is supposed to guarantee the coherence and consistency of all parts of a theoretical edifice. Axiomatization *per se*, however, leads merely to 'rigorous rubbish' (Clower, 1994, p. 409) if the domain is not accurately identified. Hence axiomatization is a necessary but not a sufficient condition for any theoretical approach concerned with the complex real and nominal interdependencies of the evolving economic system and their eventual outcomes. Keynes, we know, was not enthusiastic of anything more formal than ordinary language.

Formalization of even a part of what goes into our common sense understanding of society would be, as he [Keynes] said "prolix and complicated to the point of obscurity." (Coates, 2007, p. 8)

This may even be correct with regard to the understanding of society but does not apply to the description of the economic system. Keynes echoes the perennial critique of formalism and reinforces a widespread misunderstanding.

... 'formalism' is not the same thing as 'formalization' or 'mathematization' because it is possible to express a theory mathematically and even axiomatically without necessarily degenerating into 'formalism', which simply means giving top priority to the formal structure of modelling irrespective of its content; ... (Blaug, 1994, p. 131)

The formalism-formalization debate has been fought among the physicists ...

Mathematicians are only dealing with the structure of reasoning, and they do not really care what they are talking about. ... You state the axioms, such-and-such is so, and such-and-such is so. What then? The logic can be carried out without knowing what such-and-such words mean. ... But the physicist has meaning to all his phrases. That is a very important thing that people who come to physics by way of mathematics do not appreciate. Physics is not mathematics, and mathematics is not physics. One helps the other. But in physics you have to have an understanding of the connection of words with the real world. It is necessary at the end to translate what you have figured out into English, into the world. (Feynman, quoted in Clower, 1995, p. 308-309), see also (McCloskey, 1991)

... and among the mathematicians themselves. Hilbert's program of the axiomatization of mathematics did not go unchallenged. As a matter of fact, the mathematicians had a fierce *Methodenstreit* of their own.

Brower blasted away the formalists. Of course, he said, axiomatic, formalistic treatment may avoid contradictions, but nothing of mathematical value will be obtained in this way. "An incorrect theory, even if it cannot be rejected by any contradiction that would refute it, is nevertheless incorrect, just as a criminal act in nonetheless criminal whether or not any court could prevent it." ... "To the question, where shall mathematical rigor be found, the two parties give different answers. The intuitionist says, in the human intellect; the formalist says, on paper." (Kline, 1982, pp. 252-253)

Hilbert's formalism consisted in cutting the semantic connection between mathematical concepts and real world interpretations (Kline, 1982, p. 193). Formalism is entirely self-contained and some mathematicians were not prepared to go this far.

... Poincaré, the most outstanding mathematician at the turn of the twentieth century, while acknowledging the value of axiomatic systems, rejected Hilbert's formalism. Similarly Frege, the leading logician of the period, while rejecting Hilbert's formalism, extensively used the AA [Axiomatic Approach]. (Boylan and O'Gorman, 2007, p. 432)

The AA-variant of formalization maintains the reference to distinct real world phenomena. It is therefore not liable to the charge of formalism. In marked contrast, Debreu expressly committed himself to the 'contemporary formalist school of mathematics' (Debreu, 1959, p. x). This difference is of some importance.

Formal axiomatic systems must be interpreted in some domain ... to become an empirical science. (Boylan and O'Gorman, 1995, p. 198), see also (Koopmans, 1957, p. 173)

By consequence, when we talk of formalization the AA-variant is meant.

... Suppes sees formalization as having the following pay-offs: (1) formalizing a connected family of concepts is one way to bring out their meaning in an explicit fashion; (2) formalization results in the standardization of terminology and the methods of conceptual analysis for various branches of science; (3) the generality provided by formalization enables us to determine the essential features of theories; (4) formalization provides a degree of objectivity which is impossible without formalization; (5) formalization makes clear exactly what is being assumed, and thus is a safeguard against ad hoc and post hoc verbalizations; (6) formalization enables one to determine what the minimal assumptions are which a theory requires. (Cohen, 1977, p. 111-112)

What is demanded in economics according to Hudík's Independence Proposition is the reconstruction of a coherent theoretical superstructure on a minimalist *nonbehavioral* foundation. When the market economy is conceived as a complex mesh of structural and behavioral interdependencies then the probability of eventually finding some strong regularity is greater on the structural than on the behavioral side (Boylan and O'Gorman, 1995, p. 203). Behavior can initially be taken as random until something more specific is known. There is a sizeable *a priori* probability that the beneficial operations of the invisible hand will have the character of structural interdependencies, which of course are not apparent to the naked eye but have to be abstracted from readily accessible phenomena on the surface. This analytical venture requires a unique coordinate system of primitive concepts to start with. This framework of concepts has to be formalized by a set of axioms.

The adequate response to inadequate economic theories is not to abandon the attempt to be rigorous. (Backhouse, 2010, p. 186)

The longstanding quarrel about excessive formalization in economics therefore boils down to the 'trite injunction' (Baumol) that powerful tools should be applied with a sense of proportion.

As a personal matter, I have long believed that in dealing with M[odel]-Worlds, axiomatization is useful as well as safe ... and I have not changed my mind, even though my faith in formalization has been sorely tested My opinion continues to be that axiomatics, like every other tool of science, is no better than its user, and not all users are skilled. (Clower, 1995, p. 308)

12 Axioms: The stuff theoretical economics is made of

Keynes always believed that 'a little clear thinking' or 'more lucidity' could solve almost any problem. (Moggridge, 1976, p. 39)

During five years, Humphrey saw Newton laugh only once. He had loaned an acquaintance a copy of Euclid. The acquaintance asked what use its study would be to him. "Upon which Sir Isaac was very merry." (Westfall, 2008, p. 192)

Keynes, as we have seen, relied on the ordinary language concepts of income and profit, with the effect that their relation remained ill-understood.

This [profit] is a subject to which economists have addressed themselves for at least two hundred years, but without much success. For there is at the moment no general theory of profits which commands anything approaching universal acceptance either among academic economists or among men of affairs. (Wood, 1975, p. i)

Since profit is the pivotal phenomenon of the market system it should be evident that a perfectly clear understanding of income and profit is the precondition for the analysis of how the real world economy works.

The first three structural axioms relate to income, production, and expenditures in a period of arbitrary length. Here the period length is conveniently assumed to be the calendar year. Simplicity demands that we have at first one world economy, one firm, and one product (for the geometrical exposition of the axiom set see 2012a).

Total income of the household sector Y in period t is the sum of wage income, i.e. the product of wage rate W and working hours L , and distributed profit, i.e. the product of dividend D and the number of shares N .

$$Y = WL + DN \quad |t \quad (1)$$

Output of the business sector O is the product of productivity R and working hours.

$$O = RL \quad |t \quad (2)$$

The productivity R depends on an underlying production function. The 2nd axiom should therefore not be misinterpreted as a linear production function.

Consumption expenditures C of the household sector is the product of price P and quantity bought X .

$$C = PX \quad |t \quad (3)$$

The axioms represent the pure consumption economy, that is, no investment expenditures, no foreign trade, and no taxes or any other state activity. Albeit quite obvious, it is worth to emphasize that all axiomatic variables are measurable in principle. No nonentities like equilibrium or perfect competition are put into the premises, neither are behavioral assumptions like rationality or utility maximization.

A set of axioms cannot be assessed *ex ante* because the full range of implications is not immediately evident, yet:

The contents can be disclosed completely by deduction. It then brings nothing to light which had not already been contended in the composite of axioms. (Klant, 1984, p. 25)

Definitions are supplemented by connecting variables on the right-hand side of the identity sign that have already been introduced by the axioms (Boylan and O’Gorman, 2007, p. 431). With (4) wage income Y_W and distributed profit income Y_D is defined:

$$Y_W \equiv WL \quad Y_D \equiv DN \quad |t. \quad (4)$$

Definitions add no new content to the set of axioms but determine the logical context of concepts. New variables are introduced with new axioms.

The economic meaning is rather obvious for the set of structural axioms. What deserves mention is that total income in (1) is the sum of wage income and *distributed profit* and not of wage income and profit. Profit and distributed profit are quite different things that have to be thoroughly kept apart.

To exclude human behavior from the set of structural axioms implies just this: human behavior does not yield to the axiomatic method. This is the main conclusion of our appraisal of orthodox and heterodox methodology. Therefrom does not follow that behavior is totally excluded from the analysis. According to Hudík's Independence Proposition human behavior moves from the center to the periphery of the domain. It can, in a second step, be consistently connected with the structural axiom set via an own formal interface (for details see 2011b).

13 Profit: The stuff that makes the economy going

A satisfactory theory of profits is still elusive. (Desai, 2008, p. 10)

The business sector's financial profit in period t is defined with (5) as the difference between the sales revenues – for the economy as a whole identical with consumption expenditures C – and costs – here identical with wage income Y_W :³

$$Q_{fi} \equiv C - Y_W \quad |t. \quad (5)$$

In explicit form, after the substitution of (3) and (4), this definition is identical with that of the theory of the firm:

$$Q_{fi} \equiv PX - WL \quad |t. \quad (6)$$

Using the first axiom (1) and the definitions (4) one gets:

$$Q_{fi} \equiv C - Y + Y_D \quad |t. \quad (7)$$

The three definitions are formally equivalent. Profit can be seen from different perspectives. Taken together, the three perspectives make a comprehensive view.

If distributed profit Y_D is set to zero, then profit or loss of the business sector is determined solely by expenditures and wage income. For the business sector as a whole to make a profit consumption expenditures C have in the simplest case to be greater than wage income Y_W . So that profit comes into existence for the first time in the pure consumption economy the household sector must run a deficit at least in one period.

This is rather obvious. At the moment we have nothing more than the household and the business sector. As long as the households spend their wage incomes fully the business sector will not make a loss but it will not see any profits either. Only

³ Profits from changes in the value of nonfinancial assets are neglected here, i.e. the condition of market clearing $O = X$ holds throughout. For details about changes of inventory see (2011c, p. 5). Nonfinancial profit is treated at length in (2011a).

if consumption expenditures are greater than wage income the business sector as a whole will make a profit. Since wage costs are the flip side of wage income and revenues are the flip side of expenditures there is a straightforward connection between all magnitudes. This connection is established through the set of axioms which is the shortest possible formal description of the economic system. The axioms ensure that we do not get lost in the multitude of economic appearances.

The *logical* explanation of profit therefore consists in: revenues can only be greater than costs if, in the simplest of all possible cases, consumption expenditures are greater than wage income.

The household sector's initial deficit in turn makes the inclusion of the financial sector mandatory. A theory that does not include at least one bank that supports the concomitant credit expansion cannot capture the essential features of the market economy.

Once profit has come into existence for the first time (that is: logically – a historical account is a quite different matter) the business sector has the option to distribute or to retain it. This in turn has an effect of profit. This effect is captured by (7) but it is invisible in (5). Both equations, though, are formally equivalent.

If nothing is distributed, then profit adds entirely to the financial wealth of the firm. Retained profit Q_{re} is defined for the business sector as a whole as the difference between profit and distributed profit in period t :

$$Q_{re} \equiv Q_{fi} - Y_D \Rightarrow Q_{re} \equiv C - Y \quad |t. \quad (8)$$

Retained profit is equal to the difference between consumption expenditures and total income. It is easily seen that retained profit is the complementary of saving–dissaving.

14 Dissolving the muddle

... one of the most convoluted and muddled areas in economic theory:
the theory of profit. (Mirowski, 1986, p. 234)

The determinants of profit look essentially different depending on the perspective. For the firm price P , quantity X , wage rate W , and employment L in (6) appear to be all important; under the broader perspective of (7) these variables play no role at all. The profit definition provokes a cognitive dissonance between the micro and the macro view. It is therefore worthwhile to realize that equations (5) to (7) are not only equivalent but indeed *indispensable* for a consistent view of profit.

It is of utmost importance that profit Q_{fi} and distributed profit Y_D is clearly distinguished. The latter is a flow of income from the business to the household sector analogous to wage income. By contrast, profit is the difference of flows within the business sector. Profit is not connected to a factor input. So far, we have labor input as the sole factor of production and wage income as the corresponding factor remuneration. Since the factor capital is nonexistent in the pure consumption

economy, profit cannot be assigned to it in functional terms. And since profit cannot be counted as factor income (cf. Knight, 2006, pp. 308-309, Schumpeter, 2008, p. 153), there is no place for it in the theory of income distribution. To treat profit as factor income is a category mistake in orthodox as well as in heterodox economics.

The individual firm is blind to the structural relationship given by (7). On the firm's level profit is therefore subjectively interpreted as a reward for innovation or superior management skills or higher efficiency or toughness on wages or for risk taking or capitalizing on market imperfections or as the result of monopolistic practices. These factors play a role when it comes to the *distribution* of profits between firms and these phenomena become visible when similar firms of an industry are compared. Business does not 'make' profit, it redistributes profit. The case is perfectly clear when there is only one firm. It is a matter of indifference whether the firm's management thinks that it needs profit to cover risks or to finance growth or whether it realizes the profit maximum or not. If the consumption expenditures are equal to wage income and distributed profits are zero, profit will invariably be zero. The existence and magnitude of total profit is not explicable by the marginal principle.

Because of this, it is not wise to take the considerations of the individual firm's management as analytical starting-point and then to generalize. The microeconomic approach is inherently prone to the fallacy of composition (cf. Eucken, 1989, p. 143). The profit definition entails a cognitive dissonance between micro and macro but no logical contradiction. *Ab origine* total profit is a factor-independent residual (Ellerman, 1986, pp. 61-65). This distinction is crucial.

We know from the history of science that entrenched classificatory schemes and misleading descriptive vocabularies have impeded scientific advance as much or more than the complexities and observational inaccessibility of the subject matter. (Rosenberg, 1980, p. 114)

Models that are based on the collapsed definition $\text{total income} \equiv \text{wages} + \text{profits}$ are erroneous because profit and distributed profit is not the same thing.

Under the condition $C = Y$ profit Q_{fi} is according to (7) equal to distributed profit Y_D . The fundamental difference between the two variables is not an issue in this *limiting* case. The equality of profit and distributed profit is an implicit feature of equilibrium models (Godley and Shaikh, 2002, p. 425), (Patinkin, 1989, p. 329), (Buiter, 1980, pp. 3, 7). These have *no* counterpart in reality. In the real world profit and distributed profit are never equal.

The barter-economic notion of surplus stands in no relation to profit as determined with definition (5). Neither is the neoclassical equilibrium condition, profit rate = marginal productivity of capital, applicable in the pure consumption economy because we have profit but no capital. And, since profit and capital must not be treated like Siamese Twins, as they have since the classics, the tendency of the profit rate to fall is perhaps in need of revision. This, in turn, must be of consequence for the theory of crises.

The question of whether in equilibrium profit is zero or not – Walras’s ‘ni bénéfice ni perte’ – is of no concern within the structural axiomatic framework because the notion of simultaneous equilibrium is *no* constituent part of it. In the general case, profit or loss depends on consumer spending and profit distribution. If in the limiting case distributed profit is zero in (7), then we have three logical alternatives: $C < Y$, $C = Y$, $C > Y$. The first alternative entails a loss for the business sector as a whole, the second means zero profit, and only the third leads to profit which in turn is the indispensable condition for a reproducible economy. Hence the real question is not about the existence of a zero-profit equilibrium, but how the market economy can, and in fact does, avoid this predicament over a longer time span.

The definition of profit (5) has another important implication. There is no real residual that corresponds to the nominal residual profit. Real (O, X) and nominal (Y, C) flows are to some degree independent. Profit belongs *entirely to the nominal sphere*, in a real model it cannot exist (for details see 2011d). This is the defining characteristic of what Keynes termed the entrepreneur economy (Rotheim, 1981, pp. 575, 577, 579).

Distributed profit, in contrast, can have a real counterpart. If the product market is cleared the whole output is shared between wage earners and receivers of distributed profit according to their expenditure ratios. Since distributed profit has no relation whatsoever to the production function the marginal productivity theory of distribution cannot be true (for details see 2012b).

This much follows immediately from the first three structural axioms. It is sufficient to logically and empirically refute orthodox and heterodox profit theories. Without the correct profit theory as core any explanation of how the real world economy works must fail. The correct profit theory in turn effects modifications in all other parts of the theoretical edifice. To start from objective structural axioms instead of subjective behavioral axioms involves the most profound change of economic methodology.

15 Conclusion

The main results from the appraisal of methodology in economics have been summarized in Mill’s Impossibility Proposition, the Physicist’s Nonentity Proposition, Cournot’s Unfitness Proposition and Hudík’s Independence Proposition. From these follows: 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. The present paper suggests three nonbehavioral axioms that comprise ten measurable variables as groundwork for the formal reconstruction of the monetary economy. This makes it possible, first of all, to clarify the concepts of income and profit and to resolve the perennial profit conundrum.

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