Mainstream Aversion to Economic Methodology and the Scientific Ideal of Physics

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Abstract

There is a persistent aversion towards methodological discourse by most mainstream economists. Frank Hahn (1992) exemplified this attitude and provoked a number of reactions concerning the role and the reasons for methodological aversion. After offering a categorization of the main explanations for methodological aversion, the paper suggests an explanation that is based on the role of the physics scientific ideal. It argues that the strive to achieve the high scientific status of physics by following the methods of physics, contributed to the negative mainstream attitude towards economic methodology. This can be reinforced by examining the writings of extremely influential mainstream economists such as Irwin Fisher and Milton Friedman. These works clearly imply that the hard science status of economics renders methodological discussions and especially methodological criticism, rather pointless. Given that the existing prescriptions for making economic methodology more attractive do not give much thought to this important aspect of mainstream economics, the paper also argues for a more systematic discussion of this issue.

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I. Introduction

Methodological discussions concerning the discipline of economics were never very popular among many mainstream theorists. On the contrary, one can discern a certain aversion (often hostility) towards methodological matters. This aversion to economic methodology was exemplified by the well-know Frank Hahn's (1992a,b) arguments against the pursuit of methodological discourse. This negative attitude towards economic methodology is still the case, given that nowadays papers on economic method are rarely published in established high ranking mainstream journals. The prevailing view (still mostly based on Hahn's line of thinking) is that questions concerning the method of economics are not worthy. The core of Hahn's views was that these questions do not make much difference as how economic research is pursued. Moreover, the selection process of economic foundations ensures the prevalence of sound methodological foundations, and that in any case, economists are amateurs to deal with these issues (Hahn, 1992a; see also Hargreaves Heap, 2000).

Hahn's position provoked a number of responses mainly by specialists in economic methodology. The most prominent of these responses included Backhouse (1992), Lawson (1992, 1994), Caldwell (1993), and Hoover (1995). These authors elaborated various lines of arguments in their attempt to refute Hahn's anti-methodology stance. This discussion had also a very important repercussion: it opened the ground for the investigation for the main reasons for the observed methodological aversion of mainstream economics. Although the literature on this issue remains rather undeveloped, Lawson summarized the following reasons: ideological concerns, psychological

motives, merely defensive responses through fear, or dislike, of criticism, the lack of any philosophical training, and sheer ignorance (Lawson, 1994, p.107). Furthermore, other reasons that have been suggested have to do with the internal structure of mainstream economics as well as reasons related to the philosophy of the discipline (e.g. Caldwell, 1990; Frey, 2001).

However, another possible reason which has not received attention in the relevant literature can be attributed to the continuous dominance of the physics scientific ideal in economics. In particular, the orthodox perception is that the scientific prestige of physics-based methodology with a high degree of formalism, makes methodological discussion and critique obsolete. This tendency can be observed in the development of the influence of physics in economics and the resulting growing mathematization of the discipline especially after WWII. The relevant writings of Irwin Fisher and Milton Friedman provide the prime examples of this trend. Fisher (1892; 1932) was the first major theorist to dismiss methodological discussion by appealing to physics methods. Furthermore, Friedman's (1953) essay provided a methodological outline which effectively dismisses any discourse concerning the role of assumptions in economics. To a large extent, Friedman employed examples from physics in order to support his methodological arguments.

The paper will start with a discussion of issue of mainstream methodological aversion. It will proceed to a presentation of the main explanations that have been offered in the literature. Consequently, it will examine the connection between the physics methodological ideal and methodological aversion focusing on the writings of Fisher and Friedman. With the above in mind, it will also argue that the physics ideal is also relevant

in explaining the general hostility towards the study of economic methodology. The implications of this argument for methodological discourse will also be considered.

II. Methodological Aversion

Methodological discussion concerning the nature of economics as a field of study is not new. As Hausman writes "There have been reflections on economic methodology for as long as there have been reflections on economics itself" (Hausman, 2001, p.65). The examples of specialist works by such figures as J.S. Mill, J.N. Keynes and L. Robbins are indicative (for a history of major methodological contributions, see Blaug, 1980; Hands, 2001a). However, the field of economic methodology as a separate discipline was established in the early 1980's. In the words of Lawrence Boland: "Since 1982 there has been the establishment of a small, non-mainstream group of would-be methodologists…" (Boland 2003, p. 4). Nowadays, economic methodology has the characteristics of a distinguishable subfield (see also Hands, 2001b; Davis, 2007; Düppe, 2011).

A number of authors have identified the established methodological aversion among mainstream economists. Even since 1980's the period where there was the emergence of economic methodology as a separated discipline, most mainstream economists still paid no heed to this rising trend (Boland 1982, pp. 1-2). A decade later, Bruce Caldwell reaches the same conclusion: "Lest there be any doubt, it should be stated at the outset that, at least in the US., most economists are indifferent towards methodology, and

many of the rest are openly hostile to it" (Caldwell, 1990, p.64). A similar observation is made by Tony Lawson a few years later when he writes:

"It is not, I think, contentious to observe that explicit methodological analysis and commentary are widely frowned upon in contemporary economics, especially by those working in the mainstream." (Lawson, 1994, p.106).

This tendency was explicitly expressed and was given theoretical backing by Frank Hahn in his famous -among economic methodologists- article published in the *Royal Economic Society Newsletter* in 1992. Hahn's position concerning methodology was not new, given that in a 1965 article he had stated that "methodological arguments have nothing to teach us" (Hahn, 1965, p.xi; see also Boland, 1989). In the same spirit, Hahn's advice to young economists in his 1992 paper, was to urge them to 'avoid discussion of "mathematics in economics" like the plague', and to 'give no thought at all to methodology'. This position was reinforced when in the July 1992 issue of the same publication, Roger Backhouse put the question: 'Should we ignore methodology?', the heading of a response by Hahn is 'Answer to Backhouse: Yes'. (see Hahn, 1992a, 1992b; Backhouse, 1992). The basic components of Hahn's argument were the following: 1. Economists are not philosophers of science and therefore these issues are best left to specialists. 2. Methodological discussions do not have considerable impact on how economics is practised. Even when they make much difference, the results are by no means unambiguously good (e.g. positivist proselytizing). 3. Economics foundations look after themselves as there is a process of selection whereby economics with good foundations prospers while

economics with bad foundations withers (Hahn, 1992a; see also Hargreaves Heap, 2000, p.96).

A number of papers sprang out of this exchange attempting to justify the usefulness of economic methodology with main examples being Backhouse, 1992; 2010; Lawson, 1992, 1994; Hoover, 1995; Hargreaves Heap, 2000. Most of these papers provided arguments and specific examples in order to counter Hahn's anti-methodology stance. However, the attitude of mainstream economics towards economic methodology does not appear to have changed significantly (see also Davis, 2003).

Although Hahn's piece expressed and justified the implicit contemporary mainstream view on this issue, there have also been anti-methodology approaches originating from non-mainstream authors such as D. McCloskey (1986) and R. Weintraub (1989). However, the foundations of these objections are totally different, since they are mainly derived from a post-modernistic philosophy of science. The core of these approaches is that the possibility of scientific objectivity is extremely problematic and that scientific theories are merely narratives, thus clearly leading to relativism. According to Lawson, the implication of this stance is: "because science seemingly loses possibility of critical engagement with any extra-linguistic reality there can be no scope in science for methodology broadly conceived." (Lawson, 1994, p.126). Therefore, the endeavor of economic methodology falls into this category and it is more or less meaningless. It has to be noted though, that this strand of methodological aversion has not had significant impact on the orthodox attitude towards economic methodology.

III. Main Explanations

The underlying reasons for the observed methodological aversion of mainstream economics have not received adequate attention, although there are a few papers which attempt to provide some possible explanations. One may distinguish two broad approaches towards this important issue. The first category of explanation has to do with the internal and institutional structure of the field. In this sense, it draws from the sociological aspects of economics viewpoint (see for example, Coats, 1993; Hands, 1994). The second category refers to the methodological framework of mainstream economics and therefore, to the philosophy of science. Similarly, one can employ the tools of the Internal and External History of Science approach in order to distinguish the two general lines of explanation relating to the above discussion. Internal history of science focuses on the ways in which evidence and argument lead to scientific change. External history of science concerns how social, technological, psychological, and even natural causal factors have influenced the course of science (Hausman, 2001, p.66).

Even before the Hahn debate, Bruce Caldwell supplied an early explanation by identifying five possible reasons for methodological aversion (Caldwell, 1990). In sum, these reasons were: 1. A knowledge of methodology is neither a necessary nor a sufficient condition for becoming a good economist. This is linked to the time constraint for mastering the standard tools of economic theory rather than to engage in philosophical discussions. 2. Most philosophical discussions about the way to do science are irrelevant for economics. As Caldwell points out, this argument reduces to the simple question of the relevance of studying philosophy (Caldwell, 1990, p.65). 3.

Methodological debates are often sterile never reaching any conclusions. This argument is connected to the previous one. 4. Economic Methodology only interests a small fringe of the profession, often heterodox schools of economics. The standard perception is that "real" economists do not do methodology. 5. Methodology is superfluous for economics. ("we know what economics is"). In view of the above categorization, reasons 1, 4 and 5 obviously relate to the sociology of economics. Reasons 2 and 3 concern the nature of economics as a science.

After attempting to counter these objections, Caldwell argues that the more important reason has to do with the influence of positivism on mainstream economics. Thus, he is implicitly placing more weight to the second category of explanation. In particular, he maintains that positivism has been rejected by philosophers, and the new philosophies of science make economic methodology much more appealing. His earlier work which concentrates on the redundancy of positivism in economics, should be viewed in tandem with the above argumentation (Caldwell, 1982).

A couple of years after the publication of Hahn's essay, Tony Lawson provided an explanation based on the existing philosophical foundations of economic orthodoxy, thus attributing methodological aversion to the philosophy of economics. In particular, his central thesis is that the prevailing influence of positivism is the main factor for this hostility towards methodological discussion. In Lawson's words:

"It has been argued that the belief, seemingly widely held within economics, that philosophical/methodological reasoning has been and must be unhelpful to the discipline arises from a misplaced trust in results rooted in (or derived via a sideways displacement of) the philosophical perspective of (a version of)

positivism… Positivism in all its forms, however, is untenable and the resulting dismissal of methodology is unsustainable". (Lawson, 1994, p.128).

Lawson focuses his criticism on the version of positivism popular in mainstream economics and proceeds to argue that the abandonment of positivism will make methodological reasoning in economics highly desirable (Lawson, 1994). It is clear that Lawson's argumentation concerning the role of positivism has a lot in common with the views expressed by Caldwell.

Another more recent attempt to provide an explanation for the methodological aversion was proposed by Bruno Frey in 2001. Frey's line of thinking is exclusively focused on the sociology of economics. He maintains that the publication process of economics journals is the main cause and more specifically, the formalism bias of top mainstream journals. As he points out, "There is considerable bias in the direction of formalistic papers making minor addition to accepted knowledge." (Frey, 2001, p.43). This is reinforced by the intense competition for publication linked to successful academic careers. In Frey's view, there is a large gap between economic methodology and economic practice, and this will remain as long as external incentives remain the same (Frey, 2001).

In his response to Hahn, Backhouse maintains that because methodology is unavoidable in economics, the study of economic methodology should be taken more seriously (Backhouse, 1992). His call for a more professional attitude to methodology implies that amateurism to methodological matters might be an explanation for the mainstream methodological aversion. In this sense, it can be seen as belonging to the line of thinking emphasizing the sociological aspects of economics. In the same

framework, Hoover in his review of four books on economic methodology, seems to adopt the similar position that economic methodologists are viewed as amateurs. As he writes:

"The argument about the irrelevance of methodology has shifted and become socialised in that it no longer claims that the issues raised by methodologists are irrelevant, but rather that some people do not have the social standing to raise them." (Hoover, 1995, p.718).

In sum, the above two broadly defined approaches have offered important insights into the persisting tendency of mainstream economics to ignore economic methodology.

IV. Physics and Methodological Aversion

Apart from the above explanations for the methodological aversion, the influence of the physics scientific ideal on mainstream economics is the one which has received little attention. The natural science ideal was present even in the writings of many classical economists. Examples of the analogy between economics and physical sciences can be found in Smith (astronomy), Cairnes (chemistry), Say (chemistry and physics) and Mill (geometry) (Smith, 1980ed, Cairnes, 1875; Say, 1803; Mill, 1874). However, the tendency to imitate the methods of physics became much more apparent with the emergence of the marginalist school. Jevons' assertion that the theory of economy presents a close analogy to the science of statical mechanics (Jevons, 1871, p.viii), and Walras' prediction that mathematical economics will rank with the mathematical sciences of astronomy and mechanics (Walras, 1965, p.47, 48), are indicative examples in this respect

(see also Mirowski, 1984, 1989; 1991; Turk, 2012). The work of second generation marginalist F. Y. Edgeworth, represents the highest point of physics and in particular, of classical physics methodological influence. In his main work *Mathematical Psychics* (1881), Edgeworth not only carried the analogy to its extreme, but also provided a thorough methodological justification. Similarly, the work of I. Fisher, the popularizer of marginalism and neoclassical economics in the US, also exhibits the same tendency. Fisher took terms and concepts from classical physics (especially hydraulics) and transferred them directly to economics, also providing the appropriate methodological basis for their use. Thus, the writings of those two influential economists were paramount for the general acceptance of "economics being parallel to physics" methodological paradigm (see also Drakopoulos and Katselidis, forthcoming).

The physics scientific ideal is also relevant in explaining the general hostility towards the study of economic methodology. It can be maintained that the gradual establishment of the physics methodological ideal justified to a large extent, the increased formalization of mainstream economics (Mirowski, 1991; Morgan, 2012). In turn, the increased formalization combined with the scientific prestige of physics methods makes methodological discussion and critique obsolete. Moreover, it shields economics from methodological attacks. One can discern the above argument in the following quotation which provides the core of Fisher's methodological viewpoint.

> "The introduction of mathematical method marks a stage of growth – perhaps it is not too extravagant to say, the entrance of political economy on a scientific era." (Fisher, 1892, p.85).

Fisher was convinced that terms from physics correspond to terms in economics, thus supporting explicitly the close analogy between economics and classical mechanics. He presents a list of terms that economists use and which have been employed from physics. Examples are: equilibrium, stability, elasticity, expansion, inflation , reaction, distribution (price), levels, movement, and friction. In addition, he constructs a table of correspondence of terms and concepts between classical mechanics and economics (Fisher, 1892, p.24, and pp.85-86). Given the establishment of a close analogy between the two disciplines, it follows that methodological questions concerning economics are not necessary since economics has become an advanced science in the manner of physics. The futility of economic methodology is then clearly expressed in the following statement:

> "It has long seemed to me that students of the social sciences, especially sociology and economics, have spent too much time in discussing what they call methodology. I have usually felt that the man who essays to tell the rest of us how to solve knotty problems would be more convincing if first he proved out his alleged method by solving a few himself. Apparently those would-be authorities who are forever telling others how to get results do not get any important results themselves." (Fisher, 1932, p. 1).

Fisher's viewpoint concerning the nature of economics and its relationship with physics is also present in a discussion among prominent American economic theorists of the period. Fisher presented his views regarding the nature of the discipline with such figures as H. J. Davenport, W. H. Hamilton, Richard T. Ely, and B. M. Anderson, Jr. This discussion which

was published in the *American Economic Review*, the physics ideal is present and clear. As he writes:

> "One of the speakers has said that economics is not physics. No, but its method is the method of physics, and I believe a study of physics to be one of the best preparations for a young man intending to enter economic theory. The trouble with economic theory is that economists have entered the field, either from the a priori side of philosophy and metaphysics where the proper importance of cold facts has not been recognized, or on the other hand, from the side of history where only facts and not principles have been studied." (Davenport et al, 1916, p.167).

Fisher's work established a close connection between physics and economic concepts but more importantly, it provided an extensive methodological justification for the physics analogy in economics (see also Drakopoulos, 1994). Given that the high scientific status of economics had been achieved, methodological discourse was deemed to be irrelevant and unnecessary. In this respect, Fisher's approach has had a major influence to the establishment of current orthodox methodological aversion.

The increased formalization of economics continued with the seminal works of John Hicks, Paul Samuelson and John von Neumann. The aim was to construct a mathematical economic theory so as to make it as 'scientific' as the hard sciences. Hicks' adherence to methodological monism exemplified by his statement that "the method of modern economic investigation is the same method of all science" is a clear indication of the target of scientific economics (Hicks, 1939, p.3). The publication of Samuelson's *Foundations* was also full of mathematical methods used in physics as Samuelson himself

admits in an essay dealing with the intellectual development of his seminal work:

> "I was vaccinated early on to understand that economics and physics could share the same formal theorems (Euler's theorem on homogeneous functions, Weierstrass's theorems on constrained maxima, Jacobi determinant identities underlying Le Chatelier reactions, etc.), while still not resting on the same empirical foundations and certainties." (Samuelson 1998, p. 1376).

It is also noteworthy that Samuelson's early views regarding economic method which were outlined in his *Foundations,* were explicitly drawing from the scientific philosophy of operationalism as expressed by physicist Percy Bridgman (Samuelson, 1948; see also, Blaug, 1980, p.99). Finally, his subsequent aphorism to methodological discourse is ultimately based on the hard science argument. As he writes: "Those who can, do science; those who can't prattle about its methodology." (Samuelson, 1992, p.240).

In the same conceptual framework, John von Neumann, who was very influential for the further development of formalism in economics, also advocated and strongly promoted the use of the methods of physics to economic problems (von Neumann and Morgenstern 1944, pp.3-7; see also Rashid, 1994). It is indicative that von Neumann held that even the most advanced theoretical works in economic theory at the time, were seriously lacking in mathematical rigor in comparison to physics.[1] As he writes in a letter to O. Morgenstern: "Economics is simply still a million miles away from the state in which an advanced science is, such as physics" (Morgenstern

[1] For a detailed discussion of the views of von Neumann and Morgenstern concerning the epistemological model of physics, see Mirowski, 1992.

1976: p. 810). However, von Neumann clearly believes that the achievement of the scientific status of physics is attainable and only a matter of time. The following passage is the epitome of the physics ideal in economics:

"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 would have been absurd in physics to expect Kepler and Newton without Tycho - and there is no reason to hope for an easier development in economics." (von Neumann and Morgenstern 1944: 4)

Thus, by the middle of the previous century, mainstream economics had reached a high degree of formalism by mainly employing mathematical tools from physics (see also Debreu, 1991)[2]. During the same period, the publication of the well-know essay by Milton Friedman (1953) was the next major factor after Fisher that influenced the observed mainstream methodological aversion. Obviously, Friedman's work was not anti-methodology per se, but its arguments essentially reinforced the negative mainstream attitude towards economic methodology. Friedman's work was extremely influential among mainstream economics. As Hausman states 'It is the only essay on methodology that a large number, perhaps a majority, of economists have ever read' (Hausman 1992: 162). In particular, most mainstream economists are content with the methodological outline provided by Friedman's (1953) essay which effectively dismisses any methodological discourse concerning the role of assumptions in economics. As Düppe points out:

"On the contrary, his [Friedman] slogan of Who-Cares-About-Assumptions expressed nothing but the futility of philosophical arguments about economic

[2] The high degree of mathematization of contemporary mainstream economics has been the subject of much debate which focuses on the nature and method of the discipline (see for instance, Beed and Kane, 1991; Lawson, 2003; Dow, 2012)

knowledge. And only in this respect could the article be successful. It excused the economists' ignorance about methodology and provoked the philosopher of science." (Düppe, 2011, p.169).

It is suggestive that in this essay, Friedman also uses the analogy of physical sciences in his effort to construct the methodological basis of positive economics:

> "In short, positive economics is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences." (Friedman, 1953, p.2)

Friedman uses examples from physics in order to provide justification for his approach. The case of the simplifying assumptions of a falling body is mentioned as an example where a theory cannot be tested by its assumptions. Furthermore, the case of the billiard player who makes his shots *as if* he knew the complicated mathematical formulas, is used in order to provide support for the *as if theorizing* in economics (Friedman, 1953, pp.11-13). Although, Friedman's essay has been the subject of extensive criticism (see for instance, Mäki 2003; 2009), it still shapes current mainstream perception linked to the high scientific status of economics (deriving from its close analogies to physics) and thus, to the futility of any methodological discussion.

At this point. the views about the decline of popularity of the history of economic thought as a sub-discipline (which is a close neighboring field to economic methodology), are relevant. Although the reasons offered for its decline are more oriented towards the internal dynamics and the institutional structure of economics, there is also the idea of hard science status (see also

Caldwell, 2013). In particular, Mark Blaug has identified this factor in his discussion of the falling appeal and status of the history of economic thought as a subject. As the following quotation indicates: “The hard sciences do not much bother with their own histories—a statement less true than it used to be—and if economics is a real science, neither should economists”.(Blaug, 2001, p.146). Unfortunately, Blaug did not elaborate further this line of explanation.

Finally, a large part of the literature that aimed to respond to Hahn’s anti-methodology views, have also attempted to suggest possible ways of making economic methodology more attractive and more ‘relevant’ to general economics practice. For instance, Hands calls for a redefinition of economic methodology to encompass broader and more progressive areas of inquiry such as science theory (Hands, 2001b, pp.57-58). Mäki argues that methodology “is to be improved by making it less autonomous, by welcoming influences from similar substantive research fields so as to enrich our image of real scientific agents in action”’ (Mäki, 2008, p. 421). Backhouse believes that that it needs to be done better in the future, something which is consistent with his amateurism-based explanation for methodological aversion (Backhouse, 2010). Düppe emphasizes the key role of history: “The programmatic implication of this insight is that no economic methodologist will ever communicate effectively as long as the need for methodological reflection is not historically established – historically because motivations lie in the past! Only then can economic methodology avoid feeding the ghosts that it hopes to cast out.” (Düppe, 2011, p.174).

Undoubtedly, the above prescriptions have their own merits. However, the continuing influence of the physics ideal needs also to be integrated in this debate. It follows that a more systematic discussion concerning the nature of the relationship of economics to natural sciences in general, might be a positive contribution of economic methodology to economics and to the subfield itself.

V. Concluding Comments

It is clear that there is an observed aversion or even hostility of mainstream economics towards economic methodology which was reinforced by Hahn's piece in 1992. Economic methodologists have attempted to provide possible reasons for this phenomenon. We argued that these explanations can be categorized into two broad lines of thinking. The first had to do with the sociological aspects of economics or similarly, with the external histories of science. The second approach focuses on the way that a discipline incorporates evidence and argument or similarly, on its internal history. This paper maintained that the scientific ideal of physics has also played a crucial role to the observed methodological aversion. In particular, the strive to achieve the high scientific status of physics was a significant influence on the formation of mainstream economic thinking about the nature of economics. This was seen by studying the works of extremely influential mainstream economists such as Fisher and Friedman. Fisher, an enthusiastic supporter of the close parallel between physics and economics, was one of the first major figures to dismiss methodological speculation as a research activity. Friedman, with his irrelevance of the assumptions thesis, provided the rational

for the futility of economic methodology. These developments facilitated the dominance of the now established view that the hard science status of economics renders methodological discussions and especially methodological criticism, pointless. The existing prescriptions for the making economic methodology more attractive, do not give much thought to this important aspect of mainstream economics.

The physics imitation explanation for the mainstream hostility to methodological discussion can be seen as belonging to the internal histories of science, because it refers to the method of economics and therefore to its scientific philosophy. The previous discussion indicated that the physics scientific ideal has contributed to methodological aversion since it ascribes the hard science status to mainstream economics. Thus, the mainstream attitude towards methodology will continue as long as economics is perceived as a hard science like physics. There has been some work on the physics influence on economics mainly in the domain of the history of economic thought (the main example here is Mirowski's work). However, if economic methodology is to play a more central role, the topic of the scientific ideal of mainstream economics and its repercussions for the nature of the discipline, needs to receive much more attention by methodologists.

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