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*Economic crisis, economic methodology  
and the scientific ideal of physics*

**Stavros A. Drakopoulos**



# Economic crisis, economic methodology and the scientific ideal of physics

*Stavros A. Drakopoulos*

**Abstract.** The methodological foundations of mainstream economics have been cited as one of the main reasons for its failure to account for the economic crisis of 2008. In spite of this, the status of economic methodology has not been elevated. This is due to the persistent aversion towards methodological discourse by most mainstream economists. The anti-methodology stance has a long presence as exemplified in Frank Hahn's (1992) work. After focusing on the debate originating after the publication of Hahn's arguments, the paper offers a categorization of the main explanations for mainstream methodological aversion. Subsequently, it suggests an explanation based on the role of the physics scientific ideal, arguing that the endeavor to achieve the high scientific status of physics by following the methods of physics, contributed to the negative mainstream attitude towards economic methodology. The relevant writings of the extremely influential mainstream economists Irving Fisher and Milton Friedman, reinforce the assertion that the alleged hard science status of economics renders methodological discussions and especially methodological criticism, rather pointless. The paper also calls for a more systematic discussion of this issue, especially in the wake of the line of argument that links the recent failings of mainstream economics to its methodological basis

**Keywords:** economic methodology, history of economic thought, economic crisis.

## Introduction

Many leading economists have argued that mainstream economics failed to predict and offer useful insights to the recent economic crisis of 2008 (for a review, see Beker, 2016). [1] There were several explanations for this failure, but a substantial number of works focused on the methodological foundations of mainstream economic theory (e.g. Elster, 2009; Krugman, 2009; Lawson, 2012; Boyer, 2013; Bigo and Negru, 2014). Normally, this development should have generated an increased interest to the role and status of economic methodology as a sub-field of economics. However, this was not the case. In fact, methodological discussions concerning the discipline of economics were never very popular among the vast majority of mainstream theorists. The negative attitude towards economic methodology is still quite strong, given that papers on economic method are rarely published in established high ranking mainstream journals. This state of affairs is also acknowledged by leading economic methodologists (e.g. Hoover, 2010; Hands, 2015). Thus, a discussion of the development and causes of the mainstream stance towards economic methodology seems necessary, especially in the wake of the line of argument that links the recent failings of mainstream to its methodological basis.

The persistent lack of interest or even aversion to the field of economic methodology was exemplified by the well-known Frank Hahn's (1992a,b) arguments against the pursuit of methodological discourse. Although in the last decade there is some interest to methodological questions (Hands, 2015), Hahn's line of thinking is still influential among mainstream economists. 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 of the main reasons

for the observed methodological aversion of mainstream economics. Although the literature on this issue remains rather undeveloped, some 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; Lawson, 1994; Frey, 2001).

However, another possible reason which has not received enough attention 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 stance 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 Irving Fisher and Milton Friedman furnish 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 rejects 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. Although Fisher, Friedman and Hahn did not have a common methodological approach, they shared a negative attitude towards economic methodology as a field of study.

The paper will start with a brief discussion of views attributing the recent failure of mainstream economics to its methodological foundations. It will proceed to the mainstream methodological aversion focusing mainly on the debate originating after the publication of Hahn's arguments. In the same sub-section signs of the revival of interest to methodological issues in some influential non-orthodox recent work, will also be discussed. It will advance to a presentation of the main explanations regarding the mainstream methodological aversion 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. The pertinent contributions of prominent figures such as John Von Neumann and Paul Samuelson, will also be studied. With the above in mind, it will

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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.

## **Economic crisis and attitudes to economic methodology**

A considerable part of the discussion concerning the failure of mainstream economics to account for the financial crisis exhibits a noticeable methodological dimension. [2] For instance, some of the arguments suggested are focused on: the need for economic models to evolve with changing circumstances and the need to change the structure of economics education (Shiller, 2010); the misuse of impressive looking mathematics (Krugman, 2009); the need for institutional changes to the economics profession in order to improve the modeling process (Colander, 2010; Solow, 2010); and the need for different behavioral assumptions (Akerlof and Shiller, 2009). However, in spite of the plethora of critical papers and points of view, there was no systematic attempt to seriously examine and challenge the dominant methodological framework (see also Bigo and Negru, 2014, p.10). Against this main tendency, there were a few papers which focused more on fundamental methodological issues such as the excessive use of and reliance on mathematical rigor and mathematical modeling as well as the tendency to imitate physics (e.g. Hodgson, 2008; Elster, 2009; Lawson, 2012; Beker, 2016). In general, the financial crisis 'forced' an interest in methodological issues in spite of the widespread anti-methodology attitudes.[3] As Kevin Hoover (2010, p. 397) has appropriately observed:

Economists who had previously thought that methodology should be avoided as a diversion from practical knowledge found themselves more or less openly examining their own methodology.

### *Persistent methodological aversion*

Although the whole debate emerging from the aftermath of the crisis brought an interest to methodological questions, it did not alter the prevailing attitude towards the discipline of economic methodology. A number of authors have identified the persistent widespread methodological aversion among mainstream

economists. As was mentioned before, this aversion is not a recent phenomenon. Even since the 1980s the period where economic methodology as a separate discipline emerged, most mainstream economists still paid no heed to this rising trend (Boland 1982, pp. 1-2; see also Boland, 2003).[4] 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 points out 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 embedded tendency was explicitly expressed and was given further backing by Frank Hahn in his famous – at least among economic methodologists – article published in the *Royal Economic Society Newsletter* in 1992. Hahn's position concerning the study of methodology was not entirely novel, 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 attitude 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 main components of Hahn's argument were the following: Given that economists are not philosophers of science, methodological issues are best left to specialists. Moreover, 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). [5]

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 delivered arguments and specific examples in order to counter Hahn's anti-methodology stance. The effect of these efforts was not very

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significant given that the attitude of mainstream economics towards economic methodology did not appear to have changed significantly (see Davis, 2003).

*Some recent signs of interest to methodological questions*

There are signs that since the beginning of this century and before the financial crisis, interest towards methodological issues has increased. One source of this change is the rise of criticism of mainstream assumptions by non-orthodox research fields like experimental economics, behavioral economics and evolutionary economics. For instance, the core mainstream assumption of independent consumer preferences has been challenged by a number of behavioral and experimental economics papers. In particular, several experiments have showed that social preferences or other— regarding preferences seem to play a significant role in economic decision-making (see for instance, Camerer, Loewenstein, and Rabin, 2004; Fehr and Schmidt, 2006; Heffetz and Frank, 2011). As many authors have realized, the clear implication of these results is the serious undermining of mainstream economic rationality with the ensuing methodological consequences (for discussions, see Rabin, 2002; Claveau, 2009; Drakopoulos, 2016).

Another source of renewed interest to methodological issues relates to the rise of research on subjective well-being (or happiness and economics) which primarily relies on stated preferences and survey evidence (e.g. Clark, Frijters and Shields, 2008). There is a marked reluctance by mainstream theorists to accept such evidence, mainly because of mistrust regarding empirical findings based on questions related to subjective well-being (see the discussion in Easterlin, 2004). The extensive use of survey analysis and reliance on stated preferences in happiness research has provoked the reaction of many orthodox economists. The predisposition by mainstream economists to believe only what people do and not what they say relates to the methodological foundations of the discipline (for a discussion, see Manski, 2004). Although the influence of behavioral economics and of research on subjective well-being is increasing, they are not considered part of the mainstream theory yet (see for instance, Frey, 2008).

The issue of the methodology of econometrics is related to the above. There is a vast and growing literature on the methodology of econometrics (see for

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instance, Hendry, 2000; Hoover, 2013), but it seems that this line of research has followed its own course with little interaction with the field of economic methodology. This can be viewed as an additional indication of the limited influence of economic methodology on the practice of mainstream economics (see also Frey, 2001). Thus, as in many cases in the past, the renewed interest to methodological questions originates from non-mainstream schools. Furthermore, the recent developments in experimental and behavioral economics and also the recent debates concerning the financial crisis and the crisis of mainstream economics, did not result in a marked increase of the status of economic methodology as a research field (see also Backhouse, 2010; Düppe, 2011). Although economic methodology has the characteristics of a distinguishable subfield with its own dedicated specialist journals, conferences and professional societies (see also Hands, 2001b, 2015; Davis, 2007; Düppe, 2011), economic methodology is still viewed as an 'inferior' research subject. As Hands (2015, p. 62) aptly remarks:

Particularly in the United States, the economics profession still seems to have little or no interest in elevating economic methodology to the status of a legitimate field of inquiry within the discipline of economics.

Therefore, and in spite of on-going criticism of the mainstream methodological foundations induced by the economic crisis, the mainstream attitude towards economic methodology as a field of study has not improved significantly.

## **Methodological aversion: main categories of 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 a viewpoint on the sociological aspects of economics (see for example, Coats, 1993; Hands, 1994). The second category refers to the methodological framework of mainstream economics and therefore, to the



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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 mainstream 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 notes, '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 refer to 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 on 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).

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A couple of years after the publication of Hahn's essay, Lawson provided an explanation based on the existing philosophical foundations of economic orthodoxy, thus also attributing methodological aversion to the second category of explanation. In particular, his central thesis is that the prevailing influence of positivism is the main factor for this hostility towards methodological discussion. Lawson argues that positivism in all its forms, is untenable and this implies that the resulting dismissal of methodology is unsustainable (Lawson, 1994, p.128). Furthermore, 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 attempts to tackle the issue by focusing exclusively on the sociology of economics. He maintains that the publication process of economics journals is the main cause, and more specifically, the formalistic bias of top mainstream journals. As he points out, 'There is considerable bias in the direction of formalistic papers making minor additions to accepted knowledge.' (Frey, 2001, p.43). This is reinforced by the intense competition for publication linked to successful academic careers. In Frey's opinion, 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 asserts 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 clearly implies that amateurism in methodological matters might be an explanation for the mainstream methodological aversion. In this respect, it can be seen as belonging to the line of thinking emphasizing the sociological aspects of economics. In the same framework, Kevin Hoover in his review of four books on economic methodology, seems to adopt the similar position that

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economic methodologists lack social standing in the profession and are viewed as amateurs. As he writes (1995, p. 718):

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.

In sum, Caldwell and Lawson seem to follow an 'internal' explanation while Frey, Backhouse and Hoover lean towards an 'external' approach to the status of economic methodology. The two broadly defined approaches have offered important insights into the persisting tendency of mainstream economics to ignore economic methodology.

## Physics and methodological aversion

Apart from the above explanations for the methodological aversion, the influence of the scientific ideal of physics on mainstream economics is one that has received little attention. The scientific ideal of physics is also relevant in explaining the general hostility towards the study of economic methodology. The argument goes as follows: the gradual establishment of the methodological ideal of physics justified to a large extent the increased formalization of mainstream economics (Mirowski, 1991; Heinonen, 1993; Morgan, 2012). In turn, the increased formalization combined with the scientific prestige of the methods of physics gave the impression that methodological discussion and critique are rather unnecessary, also providing mainstream economics with a shield against methodological attacks.

### *Historical roots*

The physics ideal has had a long presence in the history of economic thought. Its lasting influence can be seen in the following observation by Robert Solow: [6] My impression is that the best and brightest of the profession proceed as if economics is the physics of society. There is a single universally valid model of the world. (Solow, 1986, p. 25)

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, 1874, pp.47, 48), are indicative examples in this respect (see also Mirowski, 1984, 1989, 1991; Turk, 2012). The views of second generation marginalist F. Y. Edgeworth represent the highest point of physics and, in particular of the methodological influence of classical physics. In his main work entitled *Mathematical Psychics* (1881), Edgeworth not only carried the analogy to its extreme, but also provided a thorough methodological justification.

#### *The role of Irving Fisher*

The work of I. Fisher, the popularizer of marginalism and neoclassical economics in the US, was paramount for the general acceptance of the methodological paradigm of 'economics being parallel to physics' (see also Mirowski, 1991; Drakopoulos and Katselidis, 2015). In Fisher's view, the increased formalization combined with the scientific prestige of the methods of physics, would transform economics into a hard science (Fisher, 1892, p.85). Convinced of the close analogy between economics and classical mechanics, 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. The origin of this stance can be found in the influence of theoretical physicist Willard Gibbs, who was one of Fisher's doctoral supervisors. Fisher was much affected and probably impressed by Gibbs' methods (see also Tobin, 1987; Breslau, 2003). Thus, in order to complement the arguments in his doctoral thesis, he built an elaborate hydraulic machine with pumps and levers, allowing him to demonstrate visually how equilibrium prices in the market adjusted in response to changes in supply or demand. Subsequently, he presents a list of terms that economists use, which have been

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directly taken 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 (Fisher, 1932, p. 1):

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's perspective concerning the nature of economics and its relationship with physics is also present in a discussion among prominent American economic theorists of the period (figures included H. J. Davenport, W. H. Hamilton, Richard T. Ely, and B. M. Anderson, Jr.). In this discussion, which was published in the *American Economic Review*, the physics ideal is present and clear. As Fisher writes (Davenport et al, 1916, p.167):

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.

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Apart from establishing a close connection between physics and economic concepts, Fisher's work provided an extensive methodological justification for the physics analogy in economics (see also Drakopoulos, 1994; 2015). Given that the high scientific status of economics had been achieved, methodological discourse was deemed to be irrelevant and obsolete. In this respect, Fisher's approach has had a major influence on the establishment of current orthodox methodological aversion.

### *Samuelson and von Neumann*

The increased formalization of economics continued with the seminal works of 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.<sup>[7]</sup> The publication of Samuelson's *Foundations* (1947) was also full of mathematical methods and tools 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).

Samuelson has also pointed out that the Harvard mathematician and physicist Edwin Bidwell Wilson was one of the main influences of his *Foundations*.<sup>[8]</sup> According to Samuelson, Wilson had encouraged him to believe that economics could use the same mathematics as physics without resting on the same empirical foundations and certainties (Samuelson, 1998, p. 1376). As Roger Backhouse claims, it was Wilson who stimulated Samuelson's lifelong interest in thermodynamics and the realization that economists could learn from physics (Backhouse, 2015, p. 332). Finally, Samuelson's subsequent aphorism concerning 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, whose work was very influential for the further development of formalism in economics, also

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advocated and strongly promoted the use of the methods of physics for economic problems (von Neumann and Morgenstern 1944, pp. 3-7; see also Rashid, 1994). It is indicative that, for von Neumann, even the most advanced theoretical works in economic theory at the time were seriously lacking in mathematical rigor in comparison to physics.[9] 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, 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 (von Neumann and Morgenstern, 1944, p. 4):

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.

Thus, by the middle of the previous century, mainstream economics had reached a high degree of formalism by mainly employing mathematical methods and tools from physics (see also Ingrao and Israel, 1990; Debreu, 1991; Weintraub, 2002). [10]

#### *Friedman's Essay*

During the same period, the publication of the well-known 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, p. 162). Furthermore, most mainstream economists seem to feel 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 (2011, p. 169) remarks:

On the contrary, his [Friedman] slogan of Who-Cares-About-Assumptions expressed nothing but the futility of philosophical arguments about economic knowledge. And only in this respect could the article be successful. It excused the economists' ignorance about methodology and provoked the philosopher of science.

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 his view, 'positive economics is, or can be, an 'objective' science, in precisely the same sense as any of the physical sciences.' (Friedman, 1953, p.4).

Friedman uses examples from physics in order to provide justification for his approach. The case of the simplifying assumptions (e.g., zero air pressure) of a falling body is mentioned as an example where a theory cannot be tested by its assumptions (Friedman, 1953, p.36). The essay is full of further analogies between economics and physics (Friedman, 1953, pp. 4, 5, 10, 32). 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.

## **Physics envy and the status of economic methodology**

The physics ideal which has been thought to shield mainstream economics from methodology is in itself highly questionable. First of all, there are many well-known examples of major physicists engaging in methodological discussions concerning the nature of the field (see for instance, Kragh, 2002). The long history of methodological debates in physics and their continuation in modern physics undermines the mainstream stance. It can be argued that the appeal to physics serves as a window dressing for the application of formalism and the avoidance of methodological issues. Thus, it seems that the reference to physics has a symbolic character, implying that the 'hard science status' makes



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methodological questions rather unnecessary. [11] This consequence of 'physics envy' has also been emphasized by Philip Mirowski (1992b, p. 61) who states:[12]

Problems of 'physics envy' include a certain contempt for the history of economics, a tendency towards the uncritical appropriation of a limited range of mathematical formalisms, and constant intrusions by physical scientists seeking to upgrade the scientific status of the discipline.

Secondly, the recent emergence of econophysics is a further indication of the problematic character of physics-envy. Most econophysicists maintain a highly critical attitude towards the mathematical approach followed by mainstream economics. They also doubt central assumptions of mainstream economic theory (e.g. McCauley, 2004; Keen, 2011). Orthodox economists are rather perplexed and undecided as to how to respond to these challenges that originate mainly from their physics-expert colleagues (for an extended discussion, see Drakopoulos and Katselidis, 2015).

It is also worth noting that the issue of physics-envy has been identified in the recent discussions emerging in the aftermath of the financial crisis. For instance, Jon Elster argued that a flaw in economics is the belief that social science only can become a science on the model of the natural sciences (Elster, 2009). Tony Lawson maintained that the physics-based mathematical method has become the dominant ideology in the economics academy. This ideology consists of '...the extraordinarily widespread and long-lasting belief that mathematical modelling is somehow neutral at the level of content or form, but an essential method for science, underpinning any proper or serious economics.' (Lawson, 2012, p. 17). In the same vein, other economists have claimed that the excessive mathematical modeling which comes from physics imitation was also a crucial factor for the failure of economists to offer insights into the crisis (e.g., Krugman, 2009; Bigo and Negru, 2014; Beker, 2016).

Furthermore, as an indication of the strong hold of the physics-inspired mathematical methodology, most critical accounts of the failure of economics advocate the need for different and improved models, not for a reduced emphasis on modeling. As Bigo and Negru observe: '...many of those reflecting on the

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discipline and its methodology, including those who call for greater realism, argue for the development of newer, improved mathematical models.' (Bigo and Negru, 2014, p.11).

Given the still low status of economic methodology as a field, a few specialists on economic methodology have attempted to suggest possible ways of making it 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 given 'that no economic methodologist will ever communicate effectively as long as the need for methodological reflection is not historically established ...' (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. As a first indication, the prevalent notion that the hard science status somehow makes a discipline free from methodological considerations needs to be investigated. It follows that a more systematic discussion concerning the nature of the relationship of economics to physical sciences in general, might be a positive contribution of economic methodology to economics and to the subfield itself.

## **Concluding comments**

The failure of mainstream economics to account for the financial crisis of 2008 brought to the surface methodological questions concerning the nature of the discipline. Paradoxically, the rise of interest in methodological themes did not bring an elevation of the status of economic methodology. The principal reason

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for this was the continuous aversion or even hostility of mainstream economics towards the field of economic methodology. 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 has 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 in the observed methodological aversion. In particular, the endeavor to achieve the high scientific status of physics was a significant influence on the formation of mainstream economic thinking concerning the nature of economics. This was seen by studying the works of extremely influential mainstream economists such as Fisher and Friedman. The influential justifications for physics-based formalism supplied by Samuelson and von Neumann were also important. These developments facilitated the dominance of the now established view that the hard science status of economics renders systematic methodological discussions, and especially methodological criticism, pointless. Fisher, Friedman and Hahn had different methodological viewpoints, but the notion of 'hard science status' is central in their stance towards the field. The existing prescriptions for making economic methodology more attractive do not give much thought to this important aspect of mainstream economics.

The 'physics envy' 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. In this respect, it is closer to the explanations offered by Caldwell and Lawson and it can also be seen as vitally connected to the role of positivism. The previous discussion indicated that the physics scientific ideal has contributed to methodological aversion since it ascribes a hard science status to mainstream economics. Thus the mainstream attitude towards methodology will probably continue as long as economics is perceived as a hard science like physics. The call (even by many critics of mainstream economics) for improved mathematical models in the aftermath of the economic crisis reinforces this view. There has been some work on the influence of physics on economics mainly in the domain

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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.

## Endnotes

[1] The 'sense of failure' of the discipline can also be discerned in the Dahlem Report (see Colander et al, 2009).

[2] For a definition and a discussion of the basic characteristics of mainstream economics, see Colander et al, 2004.

[3] For an interesting discussion of the relationship between the state of economics and economic downturns, see Dzionek-Kozłowska, 2015.

[4] Methodological discussion concerning the nature of economics as a field of scientific study has much older roots. 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).

[5] It is worth mentioning that Hahn's anti-methodology stance does not prevent him in making a methodological criticism of the theoretical, empirical and predictive success of mainstream economics (Hahn, 1992c).

[6] The physics ideal is still sometimes explicitly mentioned by highly influential mainstream figures as in the case of Edward Lazear (2000).

[7] John Hicks' methodological ideal was also in the same framework given his adherence to methodological monism (Hicks, 1939, p. 3).

[8] It is interesting that Wilson was a protégé of Willard Gibbs who was one Fisher's mentors as was mentioned before (see also Backhouse, 2015, p. 331).

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

[10] 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).

[11] The 'hard science status' has also been offered as a reason for the decline of popularity of the field of the history of economic thought -a close neighboring field to economic methodology-. See the discussions in Blaug, 2001; Caldwell, 2013.

[12] For an opposite viewpoint, especially against Mirowski's arguments, see Hands, 1993.

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