Mathematical formalism and significance in economics: a review of criticism

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The mainstream economics in both its micro and macro search for relevance in respect to people, organizations', and countries' behavior lays its analytical foundations on a rigorous set of methodological requirements.
It is the purpose of this contribution to enlist the main lines of attack against that widespread belief, as well as to provide a substantial discussion as to the extent of the use of mathematical formalism distorts the significance of economic facts. To that end, this material has made recourse to the existing literature and has organized it around two dominant points of argumentation. A first cluster of arguments has been gathered along the history of economic thought with regards to the quantitative adoption of formalism from the point the view of its economic relevance. A second view tackles the issue of alternative perspectives in order to find a way out toward economic relevance.
The reader is advised not to take this material as an exercise in refuting mathematical formalism in economics. Instead, its added value should reside in identifying the limits of formalism under those circumstances when the significance of economic analysis either becomes of no practical importance or assumes doubtful theoretical constructions.

Key words: *Formalism; Economic significance; Economic method; Modeling*

Introduction

The methodological debate in economics has never been deprived of radical attitudes from that of Samuel Bailey who argued that no economic phenomenon is conserved through time, and therefore scientific analysis is impossible (Mirowski, 1989, p. 399) at one extreme, to that of Paul Samuelson (1994) who said that "nothing would impede great leaps forward but some economists' poor knowledge in mathematics" at the other. A more moderate stance has been however embraced by those who see in the particularities of the every-day life the ferment or substance of the economic phenomena. By the 1880s, for instance, the German historicism held all economic generalizations to be relative judgments and, correspondingly, argued that each economic problem must be approached *de novo*. This recourse to realism finds its support in a long tradition of understanding economic life through its diversity rather than unifying correlations. This paper seeks to appropriately nestle the debate on formalism among those different views and find methodological arguments for the proper use of mathematics in revealing the relevant part of economic phenomena.

The current orthodox program in economics has been raised in piecemeal fashion on the building blocks of the marginalist revolution. Listening to its fathers would suffice to leave aside any scepticism. Take for instance the credo of W.S. Jevons (1871, *The Theory of Political Economy*): "Economics, if it is to be a science at all, must be a mathematical science…Our science must be mathematical, simply because it deals with quantities."; similarly, Walras (1900, *Elements of Pure Economics*) exposed its contempt for non-mathematical economics in clear-cut fashion: "As for those economists who do not know any mathematics, who do not even know what is meant by mathematics…let them go their way…They will always have to face the alternative of either of steering clear of this discipline…or of tackling the problems of pure economics without the necessary equipment, thus producing not only very bad pure economics but also very bad mathematics." (Both quotations come from Fullbrook, 2004: pp. 188, 209)

This self assurance has been preserved remarkably accurate in time (see Samuleson's position). Its assertiveness notwithstanding, a widespread criticism gets ever more consistent and visible. A failure to separate statistical significance from plausible explanation has been increasingly shown to produce from hilarious to pernicious effects. The Economist (2004) reports examples of both: "In medieval Holland, it was noted that there was a correlation between the number of storks living on the roof of a house and the number of children born within it. The relationship was so striking that, according to the rules of maths that govern such things, you could say with great confidence that the results were very unlikely to be merely random. Such a relationship is said to be 'statistically significant'. But the Dutch folklore of the time that storks somehow increased human fertility was clearly wrong. More recently and tragically, British mothers have felt the harsh effects of statistical abuse. An expert witness frequently called to give evidence in the trials of mothers accused of murdering their children argued that the odds of more than one cot death in a family were statistically so slim that three such deaths amounted to murder. On this erroneous evidence, hundreds of parents have been separated from their children and many others have been sent to prison."

The following arguments attempt to deconstruct this "cult of statistical significance" and distinguish between exaggeration and necessity in economic formalism. The mainstream economics in both its micro and macro search for relevance in respect to people, organizations', and countries' behaviour lays its analytical foundations on a rigorous set of methodological requirements. It has thus evolved not only as a quantitative science, but as an "exemplar of rationality" next only to physics (Mirowski, 2004: p. 170). It is the purpose of this contribution to enlist the main lines of attack against that widespread belief, as well as to provide a substantial discussion as to the extent of the use of mathematical formalism distorts the significance of economic facts. To that end, this material has made recourse to the existing literature and has organized it around two dominant points of argumentation. A first cluster of arguments has been gathered along the history of economic thought with regards to the quantitative adoption of formalism from the point the view of its economic relevance. A second view tackles the issue of alternative perspectives in order to find a way out toward economic relevance.

Another theme of criticism stems from outright flawed mathematics that is used to quantitatively underpin the economic modelling. From the monumental elaboration of Mirowski (1989) who shows the fatal mistakes in generating the equilibrium conditions to recent observations of outright mathematical errors which persist in economic analysis (Keen 2004) the economist is warned that the economics he is being taught may be not the one which he ought to become familiar with by means of a scientific method proper. However, this author is a non-mathematical economist who does not venture in discussing incorrect mathematics. There is still a wealth of evidence left to prove the point.

The problem of significance

A first hint comes from the scepticism which the very outstanding exponents of quantitative economics expressed at some point along their career, usually after being recognized for their prominent stature within the economics realm. The most notorious examples are John Hicks, John Maynard Keynes, Georgescu-Roegen or Joseph Stiglitz. A different blend of scientists is represented by those econometricians (Neyman, Pearson, Leamer) who always were aware of the limits to which formalism can be of any help to social sciences in general. All these intellectuals' recurrent theme sheds light on the meaninglessness which the modelling of economic phenomena bears for understanding of economic evolutions.

The discussion is timely as flawed causation among elementary economic variables virtually sets the scene for the basis of standard micro and macroeconomics. There are disparate observations which speak for serious, fundamental shortcomings of the conventional method of economics. Noteworthy remarks were uttered from the beginning of economic theory by outspoken economists like William Thomas Thornton who insisted that there were no such things as "laws of supply and demand" (Mirowski, 2004: p. 278) or Oskar Morgenstern who argued that "current theory possesses no methods that allow the construction of aggregate demand curves when the various constituent individual demand curves are not independent of each other" (Fullbrook, 2004: p. 75). In the same vein, modern economic literature witnesses incongruent discussions on fundamental themes of economic interest (Cojanu, 2003).

Several pioneering statisticians like Venn, Neyman, or Pearson long ago warned about the improper use of *statistical* significance of economic correlations for addressing issues of *economic* (substantive) significance. In statistical terms, significance arises when a characteristic of the population under observation may be taken for real (permanent) or is just attributable to chance variation. To put it differently, the significance level the economist chooses for his analysis is a matter of technical nature, of mathematics or statistics alone and may or may not have any policy relevance at all. It is so to be expected that statistical significance still be treated to be the same as economic significance. According to McCloskey and Ziliak (1996), the topic is still discarded by most leading readings in econometrics and worse still the distinction is not yet considered a topic of interest by several authoritative texts in statistics.

In spite of self-confident remarks as the one of Gerard Debreu in 1986 ("the development of mathematical economics was an irresistible current of thought"; quoted by Mirowski, 1991), it is interesting to note that juxtaposing *relevance* and *formalism* has not always seemed so "irresistible". There may indeed be observed a continuous line of mathematical expression in economics beginning with the works of Daniel Bernoulli (1738), but al least until the Second World War it seemed more controversial than evident that formalism dominance is the right methodological approach. As Mirowski (1991) remarked, the mathematical discourse from 1887 to 1924 as reflected in prestigious journals (*Revue d'Economie Politique*, *Economic Journal*, *Quarterly Journal of Economics*, and *Journal of Political Economy*) rarely devoted more than 5% of their pages to mathematical discourse. This may be due to a series of unrelated events which marked the emergence of economics as science[1].

First, doubts about the methodological validity persisted from the very beginning. Precursors in mathematical economics looked to the physics of motion, referred to as "rational mechanics" in the 18th century, to provide them with the analogies needed to guide them in their conceptualization of value. This brutal transfer of physics concepts resulted in the admission that the analogy between rational mechanics and the price system was flawed. That was the case, for instance, with Canard's (1801) attempt to assimilate market exchange to D'Alembert's Principle and the equilibrium of the lever. Similarly, William Whewell in 1831 admitted that his formalization of supply and demand bore a distant resemblance of the state of things produced by the perpetual conflict of such principles with variable circumstances, a theme which resonates from Cournot to contemporary writers with the same ardour.

Second, by the 1920s, few economists placed much credence in the concept of utility, "many mocked it openly" as the American Institutionalist School for example. Historians like Witold Kula (1986) were prompt to infer from observational data that standardized commodity measurements are a relatively recent phenomenon, becoming instituted long after market relations were prevalent. Measures did not exhibit the invariance required to constitute algebra as we know it; in many situations the money price was held invariant in order to carry out calculations, while the various physical measures of the commodities were altered in reaction to alterations in supply.

Third, in contrast to their precursors (e.g. Cournot), most of the second generation of neoclassical theorists were not as well-versed in mathematics or physics. "Bad

[1] The following historical accounts heavily draw on Mirowski (1991).

mathematics", it seems, is not such a rare occurrence in the body of the accepted body of economics. Keen (2004), Mirowski (1989) among others dedicate volumes to dismantle large parts of the accepted theory.

The late 19th century however set out "a state of cumulative self-assured internal development" of formalism in economics which never lost its pre-eminence from 1950s onwards anymore. After 1870, a critical mass of theorists trained specifically in physics, among them William Stanley Jevons, Léon Walras, Francis Ysidro Edgeworth, Irving Fisher, Vilfredo Pareto, each independently or not adopted the same mathematical metaphor – the equilibrium in the field of force – which equated potential energy with "utility" or "rareté" or "ophelimity". As documented by Mirowski (1989), the miraculous emergence of the neo-classical theory from so many independent contemporary economic writers cannot but be explained by the fact that they copied the physical mathematics literally term for term and dubbed the result *mathematical economics*. An even more substantial wave of trained scientists and engineers into economics – Ragnar Frisch, Tjalling Koopmans, Jan Tinbergen, Maurice Allais, Kenneth Arrow – fundamentally paved the way for what has come to be known as *standard economics*. Moreover, noted mathematicians such as John von Neumann, Griffith Evans, Harold Thayer Davis, Edwin Bidwell Wilson "were induced" to turn their attention to economics. They discovered that the corpus of neoclassical theory consisted largely of the formal models which they has already mastered in their earlier training in physics, with "the only difference being that the vintage of the model was clearly that of the later 19th century". Koopmans (1979), for example, explains: "Why did I leave physics at the end of 1933? Because of my reading block, I chose problems that, by their nature, or because of the mathematical tools required, have similarity with physics…What held me back was the completely different, mostly verbal, and to me almost indigestible style of writing in the social sciences." (quoted in Mirowski, 1991)

The new economists however distinguished themselves first and foremost in their familiarity with a new blend of formalism; one based on more up-to-date mathematical techniques such as stochastic mathematics, mathematics of vector fields and phase spaces, linear algebra and constrained optimization. Gerard Debreu in 1984 made the suggestive remark that "the fact that commodity space has the structure of a real vector space is a basic reason for the success of the mathematicization of economic theory" (quoted by Mirowski, 1991). Undoubtedly, as some authors (e.g. Piermartini and Teh) remark, an additional factor which underpins the present dominance of formalism consist of the increased computational and data processing power of computers that have only eased the way the exercises in quantification may be further pursued, but only sticking to the same methodological vulnerabilities. Unless one unnecessarily blurs the distinction between formalism and relevance, it is convenient to pick up two broad directions of mathematical formalism – statistics and modelling – that correspond to the logical foundations of the conventional way of thinking, inductive and deductive reasoning, respectively. The remaining of this section discusses them in turn.

F.Y. Edgeworth in 1885 and John Venn in 1888 are mentioned (McCloskey and Ziliak, 1996; Ziliak, 2004) to have coined first the term "statistical significance" by referring to the division between errors in sampling and what matters form the point of view of economic facts. "Significance" in a statistical context was thus understood in the sense "that we may be tolerably confident that if we took another similar batch we should find a similar difference." In 1933, Neyman and Pearson wrote about the consequence of error in terms of type I and II errors. In their view, mathematical theory show how the risk of errors may be controlled and minimized in a purely technical investigation. This latter qualification was especially introduced for a discussion on the use of these statistical tools in social context. "To convict an innocent man or to acquit a guilty" just hangs on the investigator's perception.

Karl Popper in his *Logic of Scientific Research* added a new element embedded in the principle of *verifiability* to this methodological discourse. The researcher should be prepared to see proofs that dismiss her beliefs (King, 2005: p. 258), to get through the opportunity to deny a systematic relation given that there will always be practically impossible to test all cases. "Is there a pattern or is it just random?" are the interrogative questions of what come to be known as inferential statistics. The scientific character of economics depends on this *inferential* argument (Harberger, 1993) whose absence deprives disciplines such as accounting or lay of any scientific component. The inferential logic allows us to ascertain that the result is significant only if we show that rejecting it is statistically highly improbable (King, 2005: p. 271). The formalization of error so began in the early 20th century by being absorbed in the theory of probability (Mirowski, 2004: 170) and further conceptualized within the discipline of statistics.

Though this distinction leaves things unchanged from a practical point of view (it does not require any recourse to or supplementary treatment of observational data), it imposes a definitive reference to think of differences between sample and population as statistically significant or insignificant. The literature (e.g. Cooper and Schindler, 1998: p. 467; Mirowski, 2004: p. 331) emphasizes two dominant accounts of the treatment of error: the theory of Neyman-Pearson hypothesis testing and the Bayesian theory of subjectivist probability assignment. The first in known as classical statistics or sampling-theory approach: decision making rests totally on an analysis of available sampling data; a hypothesis is established (the null hypothesis), it is rejected and its alternative is accepted or fails to be rejected, based on the sample data collected. Bayesian statistics go beyond sampling data and consider all other available information (subjective probability estimates stated in terms of degrees of belief, based on general experience). The latter usually receives less consideration in economic research because of both its more intricate mathematics and stronger assumptions (in the Bayesian tradition everyone must form identical probability assignments to be 'rational').

A difference has statistical significance if there is good reason to believe the difference does not represent random sampling fluctuations only. Has it any practical significance? The statisticians answer in two steps. First, one has to choose a certain "level of confidence" to distinguish randomness from significant. Ronald Aylmer Fisher (1890-

1962) wrote in 1925 about the first test of significance in his *Statistical Methods for Research Workers* which is now known as "the rule of 2". He explained: "the value for which P=.05, or 1 in 20, is 1.96 or nearly 2; it is convenient to take this point as a limit in judging whether a deviation is to be considered significant or not. Deviations exceeding twice the standard deviation are thus formally regarded as significant." (quoted in Ziliak, 2004) So, if a false indication appears only once in 22 trials one has a "standard of convenience" to verify the hypothesis. The most common level of significance is 5% (95% certainty), although 1% is also widely used, and is largely determined by how much risk one is willing to accept. For instance, the US State Supreme Courts have a standard measure of confidence of a 5% level [of Type I error] for justice cases (Ziliak, 2004).

A second step is to associate a level of risk with errors. There are two potential sources of mistakes (Yeomans, 1968: pp. 69-70), which may be based on relative monetary costs: Type I error is an error of commission; it occurs when action is taken incorrectly or unnecessarily. The null hypothesis actually holds true, but the study says otherwise. Type II error is an error of omission; it occurs when one fails to take some necessary course of action. The null hypothesis does not hold true, but the study rejects the alternative hypothesis. The researcher's task should be "to balance [the risk level associated with a type error] so that they reflect the consequences of the two wrong decisions". Ideally, the best situation is where $\alpha = \beta = 0$.

Accepting the null hypothesis may be reasonable or unreasonable, but it depends on economic context as the two anecdotes extremely show. King (2005: pp. 279-80) warns against taking "statistically significant" for true but if the result seems unreasonable advises to specify a better model. Few econometrics textbooks in fact distinguish economic significance from statistical significance. The only kind of uncertainty the test of significance deals with is the sampling error. "Low uncertainty" stands for "significant" but it does not answer the question how important the variable is. As McCloskey and Ziliak (1996) note, when the *t*-tests first reached the masses between 1975 and 1979 separating statistical significance from other kinds of significance was almost inexistent in the papers authored by Ph.D.'s conferred by that time and few changed in the meantime. According to the evidence they gather, in the 1980s and the 1990s between 70% and 80% of the empirical papers in the *American Economic Review* did not distinguish statistical significance from economic, policy, or scientific significance. They relied too much on numbers, and too little on economic reasoning. 72% of them did not ask "How large is large?" that is, after setting on an estimate of a coefficient, 72% did not consider what other authors had found; they did not ask what standards other authors have used to determine "importance".

A different mix of mathematical formalism emerges from efforts to apply general laws of behaviour like market models to economic phenomena. Here the discourse gets more sophisticated for a non-mathematical economist because the frequency of *incorrect* modeling seems more plausible. However, one cannot help observing the obvious. Keen (2004), for instance, following Anwar Shaikh, discusses the Cobb-Douglas production, function a tenet of neoclassical economists and remarks that the function is simply a transformation of the national income identity according to the following comparison, under conditions of relatively constant income shares and where Q, Production, L, Labour, K, Capital, A, α, β positive parameters.

The national income identity	The Cobb-Douglas production function
$Income = Wages + Profits$	$Q = A\, K^{\alpha} L^{\beta}$

The deceptively obvious consists in showing a powerful model of economic growth only because it is a correlation of X with approximately X.

However, the most frequent problem of modelling remains its strong underlying assumptions which deprives the analysis of any significance. Rational behaviour seems a particularly strong assumption. Derivation of demand curve, for example, implies similar preferences irrespective of income category. Under these circumstances, there is an inherent difficulty in expressing in mathematical terms the context of choice. In a discussion of what are known as the 'Sonnenshein-Mantel-Debreu' conditions, Keen (2001) notes that a number of 'conditions' which economists impose upon their models are actually 'proofs by contradiction' that their theories contain errors. Microeconomics foundation says that, in order to be able to aggregate the individual utility which different consumers derive from consuming different commodities: (a) all consumers' indifference curves must have the same slope; and (b) the slope must be such that spending doesn't change with income. But condition (a) means that all consumers must be identical—so that there is really only one consumer. Condition (b) means that all commodities must be identical—so that there is really only one commodity. This means that utility *can't* be aggregated across different consumers and different commodities—it is a 'proof by contradiction' that what economists are trying to do is impossible. As Mirowski (2004: p. 330) comments, "if Walrasian general equilibrium cannot guarantee uniqueness and stability of equilibrium except under unrealistic circumstances, then these economic laws don't operate in general the way their neoclassical advocates suggest."

In this case, significance or lack of is visible in the series of numerical computation for policy practitioners. The computation appears in the form of a general or partial equilibrium analysis and accounts for all or part of the links between sectors of an economy - households, firms, governments and countries. It imposes a set of constraints on these sectors so that expenditures do not exceed income and income, in turn, is determined by what factors of production earn. These constraints establish a direct link between what factors of production earn and what households can spend. For partial models, it is further assumed that the impact of that sector on the rest of the economy and vice versa is either non-existent or small.

Some unrealistic representations of economic life are encapsulated by the very construction of the model: the puzzling absence of "finance" in an increasingly global world; the high level of aggregation required to be able to use comparable and consistent data; or the difficulties in the specification of parameters and functional forms in the model.

It should be so no surprise that the results significantly diverge from representing a true understanding of economic phenomena. Piermartini and The (2005) admit that "the numbers that come out of the simulations should only be used to give a sense of the order of magnitude that a change in policy can mean for economic welfare of trade." Even by this modest yardstick, the results are hardly relevant. They survey various applications of analytical techniques of Computable General Equilibrium (CGE) and gravity models and find controversial results. Some papers generate simulations which show welfare losses from agriculture liberalization; one other paper shows that trade reform can have quite opposite welfare effects on developed and developing countries; in three other simulations, agriculture is the sector where the greatest welfare gains are derived. These results suggest again that differences in assumptions, in this case about market structure and the presence of scale economies, reflect a huge discrepancy between the impersonal world of models and conditions of actual human and social behaviour (see also Rodrik, 2005).

Alternative views to formalism

In light of these vulnerabilities, there have lately been many attempts to rescue formalism from its demise. A clear awareness of its irrelevance has only worsened its status. Harberger (1993) favours an approach between science and prescription, "one foot planted in science, the other in the practice of that science" in contrast to the methodological discipline of pure sciences like chemistry or physics. His perception is based on a large educational survey applied to American population that signals worries only muted so far in the orthodox economics:

- Both faculty and recent Ph.D. respondents believed mathematical and statistical tools were overemphasized;

- Only 14% of faculty members could say that by the time students completed their comprehensive examinations, most of them or all of them were good at applying theory to the real world;

- About 80% of faculty respondents call for less theory and technique, the most frequently suggested changes include more emphasis on the links between theory and real-world connections and applications.

In his view, the implication is that probably 90% of policy practitioners have to be able "to think on their feet, to know simple tools and how to use them well." The reality a policy practitioner should perceive appears like "more or less OK", "caution" or "danger". The method of economic analysis should give a sense of "the nature and source of the trouble", of "what is normal and what is not". A suggestion consists in collecting data for a large number of countries, going back where feasible for about 30 years on a vast array of subjects such as monetary and banking institutions and magnitudes, national income and product accounts, trade and the balance of payments, prices, wages, interest rates, and exchange rates. Though a definitive split with the orthodox methodology is hardly recognizable, Haberger insists that "formalism should be taught as a 'door-opener', to be followed by a demonstration of its massive failure to explain the facts of the last century or so, to be followed in turn by an explanation of some of the more plausible reasons for this massive failure."

This sort of methodological intervention is better known as the principle of minimalism: methodological simplicity, not methodological overstatement. It is in fact an interchange between two types of researchers, A and B: A introducing a more complicated methodology which appears to be needed in order to accomplish the objective at hand, and B responding that A missed a particular trick and showing that A's task could still be performed in a very simple framework.

This approach is not singular. In a different form but similar mindset, McCloskey and Ziliak (1996) try to understand the link between economic relevance and scientific success, or in their words, between statistically significant coefficients and clever common sense, elegant theorems, new policies, sagacious economic reasoning, historical perspective, relevant accounting.

Some other authors explore a modern symbiosis between mathematical formalism and some sort of instrumental institutionalism. For instance, Mirowski (1991, 2002) begins from the understanding that algebra is necessary to describe modern market activity because the market structures have historically evolved to the point where its formalism patterns reify the impersonal character of appropriation. He explains: "this has absolutely nothing to do with 'equilibrium' or any other metaphor borrowed from physics…Prices conform to specific algebraic structures, but they are not the *a priori* products of nature or of the individual mind, rather, they are provisional invariances imposed upon the motley variety of human perception by various conventions and social structures."

Concluding comments

The formalism of orthodox economics seeks event-regularities *per se* in an effort to resemble (pure) science. It proposes a unifying framework of economic endowments and constraints that is amenable by its very quantitative nature to mathematical treatment. This material argues that in doing so the current orthodoxy escapes for its large part the economic significance of the phenomena under observation. The identification of the underlying mechanisms especially where correlations are not to be found remains the unresolved methodological problem in economic analysis.

The normal line of criticism follows the problem of juxtaposing the issue of *practical* significance with the one of *theoretical* validity. It is recognized here that this treatment is much less elaborated and laborious work is still needed in order to validate the analytical treatment of causation in economics.

The reader is advised not to take this material as an exercise in refuting mathematical formalism in economics. Instead, its added value should reside in identifying the limits of formalism under those circumstances when the significance of economic analysis either becomes of no practical importance or assumes doubtful theoretical constructions.

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