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Artículo

On the Complexity of Economic Reality

and the History of the use of Mathematics in Economics

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Abstract

The role of mathematics in modern economics has been a topic of periodic dispute, which took on a new life with accusations concerning the limitations of mathematical models after the global crash of 2008. This article adds a historical dimension by considering some key past debates over the use of mathematics, including important statements by Alfred Marshall and John Maynard Keynes. It is proposed that the complexities of economic systems and of human motivation do not themselves constitute arguments against the use of mathematics, but they should affect the kinds of mathematical approaches employed and the purposes to which it is oriented. Given this complexity, mathematics is less useful as a predictive tool and more useful for heuristic purposes. Economists should also pay attention to guiding metaphors and analogies that guide the uses of particular kinds of mathematics.

Key-words: complexity, mathematics, economics

Resumen

El rol de la matemática en la economía moderna ha sido un tema de disputa periódica, la cual ha resurgido tras las acusaciones concernientes a las limitaciones de los modelos matemáticos luego de la crisis mundial del 2008. Este artículo aporta una dimensión histórica a dicho debate considerando algunas claves de los debates anteriores sobre el uso de las matemáticas, incluyendo las importantes posiciones de Alfred Marshall y John Maynard Keynes. El artículo propone que las complejidades del sistema económico y de las motivaciones humanas no constituyen en sí mismas argumentos contra el uso de las matemáticas, pero que, sin embargo, deben afectar el tipo de enfoque matemático que se utiliza y el propósito hacia el cuál están orientadas. Dada la complejidad, la matemática tiene menos utilidad como herramienta predictiva y es más útil para fines heurísticos. Los economistas deberían también prestar atención a las las metáforas y analogías que guían el uso de tipos particulares de matemáticas.

Palabras claves: complejidad, matemáticas, economía.

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Introduction.

Economic systems and the motivations of agents within them are extremely complex. This has been widely acknowledged, at least since Adam Smith.[1] At the same time it is widely believed that economics is a mathematical science and the extensive use of mathematics is vital to make economics 'scientific'. Anyone trying to grapple with economic concepts, their development and their applications must have at least some rudimentary knowledge of mathematics and statistics. It is also undeniable that some mathematical formalisations have played a key role in the development of economic ideas, from the link between marginal utility and calculus to the analysis of strategic interaction in game theory. Few would deny that such formalizations have enhanced our understanding. Economics stands way above the other social sciences in its degree of utilisation of mathematics, and consequently in terms of claims of its purported rigour.

Historical debates about the role of mathematics in economics are a useful background to contemporary debates about mathematics in economics. Historically this debate has touched on many issues and the diversity of concerns remains today. For instance there have been calls for a different kind of mathematics (e.g. McCloskey 1994, Potts 2000, Velupillai 2000, Colander et al. 2008), especially to deal with the complexity of economic reality. We find claims that the assumptions used in mathematics should be more empirically grounded (e.g. Leontief 1982). And several scholars call for the role and purpose of mathematics to be reconfigured– addressing the limits to accurate prediction in complex systems for example – rather than proposing that mathematics should be abandoned.

The debate about the nature and role of mathematics in economics was heightened after the Great Crash of 2008. Recognizing that mathematical models have played an enormous role in the generation of advice for financial institutions on how to spread their risks, critics observed that these models had helped to generate dangerous over-optimism and institutional complacency concerning high levels of lending. Many models, calibrated with data stretching back for decades before 2008, broke down during the financial crash of that year. A closely-related concern was that economists had been beguiled by free and deregulated markets largely because their models suggested that deregulated markets were self-adjusting and more efficient. But having a self-righting model of a market does not mean that markets are self-righting in the real world. Standard models as-

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[1] This essay makes extensive use of material from the Introduction to Hodgson (2012).

sume away some of the sources of market destabilization, particularly by making highly optimistic assumptions concerning the availability and use of relevant information.

One incident prompted considerable attention and debate. In November 2008 Queen Elizabeth of the United Kingdom visited the London School of Economics and she was heard asking why so few economists had seen the Crash coming. In response two leading British economists sent a letter to Her Majesty, largely in defence of the economics profession (Besley and Hennessy 2009). This prompted another, more critical, letter to the Queen (Dow et al. 2009). Both interventions received widespread publicity in the press. In the same year Nobel Laureate Paul Krugman (2009) wrote in the *New York Times*:

> Few economists saw our current crisis coming, but this predictive failure was the least of the field's problems. More important was the profession's blindness to the very possibility of catastrophic failures in a market economy … the economics profession went astray because economists, as a group, mistook beauty, clad in impressive-looking mathematics, for truth …

If these claims are true, then the role, scope and nature of the mathematics used in economics must be re-evaluated. An insistence on the complexity of economic phenomena, the prevalence of uncertainty and the heterogeneity of agents is a vital first step. The standard post-Robbins view of economics, as being about optimal decisions facing well-defined choices with ample information, has to go. The real world, with all its complexity, uncertainty and information scarcity has to be brought to bear on economic reasoning. Standard models that omit these elements have also failed to explain adequately the recent crisis. And as David Colander et.al. put it 'the current academic agenda has largely crowded out research on the inherent causes of financial crises.' (David Colander et.al. 2008, p. 2). That agenda now requires radical revision, using models that are more data-driven alongside discursive and historical analyses.

Whatever our view on the nature and role of mathematics in economics, there is no doubt that the twentieth century turned economics into a mathematical science. George Stigler et.al. (1995) examined the use of mathematical techniques in four leading journals of economics. They found a dramatic decrease in articles that use neither geometric representations nor mathematical notation from 95% in 1892 to 5.3% in 1990. After similar studies of the literature, Roger Backhouse (1998), Mark Blaug (1999; 2003) and E. Roy Weintraub (2002) argue that the 'formalist revolution' in economics was consolidated in the late 1950s. This transformation was given some impetus by the long report to the American Economic Association by Howard R. Bowen (1953) that called for more extensive training in mathematics at graduate level.

The historical discussion in this essay shows different ways in which economists have reacted to the challenge of modelling a complex economic reality. Tony Lawson (1997) and others have argued that this real-word complexity and openness at best severely limits the role of mathematics, which essentially involves closed logical systems. While this critique has to be taken seriously, it overlooks the fact that mathematics is not necessarily deployed either as a map of economic reality or a means of predicting its future. An important additional role of mathematics is to aid the development of explanatory heuristics (Sugden 2001; Hodgson 2006, ch. 7). Heuristics focus on specific causal mechanisms and not necessarily the system as a whole. Nevertheless, real-world complexity remains a major challenge.

The aim of this article is to place the concerns raised above in their historical context, confining ourselves primarily to the period before 1945 but highlighting some post-1945 developments. The remainder of this article is organized in three sections. Section 1 addresses the issues surrounding the (limited) employment of mathematics in economics before Alfred Marshall. Section 2 considers the arguments made by Marshall and John Maynard Keynes for limiting the role of mathematics. Section 3 concludes the essay.

Mathematics in economics before Marshall.

The writings of the classical economists are almost entirely discursive, save for a few arithmetical illustrations. Despite this, an early controversy exposed some key issues that were to underlie later debates concerning the role of mathematics. The modes of argument of Adam Smith, David Ricardo, Nassau Senior and Thomas Robert Malthus are manifestly different, with Ricardo and Senior being more concerned with the logical deduction of conclusions from *a priori* assumptions. While Smith attempted judiciously to blend induction with deduction, he also sought general principles and laws. But his classical successors such as Ricardo and especially Senior went much further. They pursued more and more an axiomatic and deductivist method, attempting to derive universal conclusions from a few professedly general and fundamental propositions.[2] Smith and

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[2] I define deduction and deductivism in accord with their classic use in logic. *Deduction* 'is used

Malthus were relatively more inclined to base their logic on empirical facts. Does the age-old philosophical controversy between induction (or empiricism) and deduction mirror later debates about the role of mathematics in economics?[3]

Because mathematical modelling was underdeveloped in economics in the nineteenth century, methodological controversies were typically framed in terms of deduction versus induction, or other versions of that dilemma. But the deductivist versus inductivist debate does not translate completely and directly into one concerning the promoters of mathematics and their critics. Deductivism does not necessarily imply the promotion of mathematical techniques, and empiricism does not necessarily imply their abandonment, as we shall show later with examples below.

Hence one should not be misled into believing that there is a close correspondence between, on the one hand, advocacy of deduction and the promotion of mathematics, and on the other hand, advocacy of induction or empiricism and a more cautious attitude to mathematical methods. This misconception has enjoyed a recent revival.[4]

In part what was at stake in these early controversies were claims concerning the degree to which general principles of economics could be established, the legitimacy of specific simplifications and abstractions (especially in regard to the complexities of economic systems and of individual motivation), and the extent to which any basic assumptions must rest on empirical evidence. These dilemmas relate to the question of mathematics, but in a more indirect and complicated way.

A principal difference between Malthus and Ricardo was the extent to which they believed that simple theoretical models could illuminate economic reality or provide a basis for economic policy. Criticising the

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to denote arguments which are such that if their premises are true their conclusions must also, as a matter of logic, be true' (Urmston 1989, p. 79). Note that some writers (see footnote 4) depart from this classic formulation and confusingly define deductivism in a very different way.

[3] *Induction* is an empirically-driven process involving the attempt to establish generalisations from a set of observations. *Empiricism* is the broad view that knowledge is based primarily on experience rather than on any body of theory.

[4] Note that Lawson's (1997) close association of the use of mathematical modelling with deductivism is based on an odd definition of the latter term. Others follow Lawson in making the association without making it clear that deductivism is being used in an atypical sense. Lawson (1997, pp. 16–17) regards 'deductivism' as presuming 'event regularities' or 'constant conjunctions of events or states of affairs' with regularities of the form 'whenever event x then event y'. This description of deductivism refers to empirical regularities concerning events rather than logical deductions concerning propositions. Hands (2001, p. 323 ff.) also points out that Lawson's use of the term deductivism 'is different from the way in which the term is generally used within the philosophical literature'. Hands (2001, p. 327) suggests and Wilson (2005) argues that in any case neoclassical economics does not fit Lawson's own characterization of 'deductivism'.

emphasis on over-simplification and generalisation, Malthus (1836, p. 4) wrote in 1819: ‘The principal cause of error, and of the differences which prevail at present among the scientific writers on political economy, appears to me to be a precipitate attempt to simplify and generalize.’ (Malthus 1836, p. 4).

Malthus stressed the complex and varied nature of economic reality, and concluded that simple or general conceptual frameworks or formal models could at best be of highly limited use. In contrast, Ricardo upheld that simple models could somehow be representative of a wide set of varied and complex phenomena. This key methodological difference divided two classical economists and reverberated through the history of economics for the next two hundred years. Significantly, John Maynard Keynes opined in 1933: ‘If only Malthus, instead of Ricardo, had been the parent stem from which nineteenth-century economics proceeded, what a much wiser and richer place the world would be today!’ (Keynes 1972, pp. 100-1).

One of the earliest and most important attempts to use mathematics systematically in economics was by AugustinCournot (1838) in his *Researches on the Mathematical Principles of the Theory of Wealth*. Cournot saw his approach as deductive as well as mathematical. A pioneer of mathematical economics, he also laid down some of the foundations of the marginalist or neoclassical approach in economic theory. He introduced the notions of function and probability into economic analysis and he was the first to express and illustrate supply and demand curves as functions of price. Despite the prescience and brillianceof his work, Irving Fisher (1898, p. 119) noted with regret that it was ‘passed over in silence, if not contempt’.

The rise of the concept of marginal utility signalled a further opportunity for much greater use of mathematics in general and calculus in particular. Although it was not directly measurable, utility seemed an ideal candidate for uni-dimensional quantification.[5] Albeit with contrasting modes of application, this idea is manifest in the seminal marginalist treatises of William Stanley Jevons (1871), Carl Menger (1871) and Léon Walras (1874).

But while Jevons, Menger and Walras are often lumped together, their approaches are quite different (Jaffé1976). Menger was the founder of the Austrian school and they regarded mathematical models as of limited use, despite the deductivist stance adopted by several members of that school.

In contrast, for Jevons (1871, pp. 50, 52, 70), economics ‘must be per-

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[5] See Fisher (1892). Problems within this vision came later, with concerns about interpersonal comparisons of utility and the emergence of formulations in terms of ordinal rather than cardinal utility.

vaded by … the tracing out of the mechanics of self-interest and utility.' He believed that 'all economic writers must be mathematical so far as they are scientific at all.' In part he justified this stance on the grounds that economics 'deals in quantities', particularly in the form of prices.[6] Jevons seemed to overlook that much of mathematics is not about quantities. His complementary presumption of utility as the goal of human behaviour also prompted a utilitarian analysis that subsumed other psychological notions and any lingering Smithian 'moral sentiments'.

Francis Edgeworth pioneered a similar line, embracing both Benthamism and mathematics. Defending the idea that individuals maximize their own utility or pleasure, in his *Mathematical Psychics* Edgeworth argued that '*the conception of Man as a pleasure machine* may justify and facilitate the employment of mechanical terms and Mathematical reasoning in social science.' (Edgeworth 1881, p. 15).

Further evidence for this connection comes from the work of the American neoclassical economist Irving Fisher. Originally trained as a mathematical physicist, Fisher was one of the earliest and most forceful evangelists for mathematics in economics. Fisher (1892, p. 85) drew up a table of 'mechanical analogies' where a 'particle' in mechanics 'corresponds to' an 'individual' in economics, 'Space' 'corresponds to' 'Commodity', 'Force' to 'Marginal utility or disutility', 'Work' to 'Disutility' and 'Energy' to 'Utility'. Vilfredo Pareto justified the appeal to mechanics in similar terms:

> Rational mechanics gives us a first approximation to the theory of the equilibrium and of the movements of bodies. … Pure economics has no better way of expressing the concrete economic phenomenon than rational mechanics has for representing the concrete mechanical one. It is at this point that there is a place for mathematics. … It therefore appears quite legitimate to appeal also to mathematics for assistance in the solution of the economic problem. (Pareto 1897, p. 490)

The mention by Jevons, Edgeworth, Fisher and Pareto of 'mechanics' was no accidental turn of phrase. As with Walras (1874), the search for usable mathematical techniques had quickly gravitated towards physics (Mirowski 1989). The particular kind of late nineteenth-century physics provided specific formalisms (particularly those involving calculus),

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[6] Mirowski (2012) points to the severe difficulties in this and other presumptions of an underlying quantitative ontology. Prices depend on suppositions of invariance that are contingent rather than historically universal. The idea of using the presumed quantitative nature of the object of investigation as justification for the prevalent use of mathematics is thus undermined.

guided the general approach to modelling, enshrined the preoccupation with equilibria and elevated the supreme goal of prediction. Equipped with these metaphors, admiration for the genuine powers of mathematical technique, and expectations of greater forecasting ability, a number of leading neoclassical economists launched their campaign for formalism and precision into the twentieth century.

The persistence of what has been described as 'physics envy' among economists has remained a major impulse to extend the scope of mathematics in their subject. But much of theory in the natural sciences is not articulated mathematically. Furthermore, when some physicists have cast their eye on economics they have complained about the unfalsifiability or lack of empirical grounding for its assumptions (e.g. Chatterjee and Chakrabati 2007, pp. 250 ff.).

Cambridge qualms: Marshall and Keynes.

But while the marginalist or neoclassical approach invited a great deal of mathematical enterprise, the role of mathematics in economics then remained a matter of debate, even within the neoclassical camp. The key witness here is Marshall, the great synthesizer of neoclassical economics (Ekelund and Hébert 2002). His global status as the most important microeconomic theorist endured at least until the 1940s (and the delayed development and adoption of Walrasian general equilibrium theory).

Making relatively less reference to mathematical physics, Marshall drew from the then-influential discourse of Spencerian evolution (Moss 1990;Thomas 1991; Hodgson 1993a; 1993b). His primary invocation of Spencerianbiology rather than physics signalled a recognition of the complex and evolutionary character of economic systems (Raffaelli 2003; Cook 2009). The fact that he adopted a quite different underlying metaphor – taken from complexity-oriented Spencerian evolution rather than mechanics or physics – partially accounts for his much more cautious attitude to the use of mathematics in the development of economic theory.[7]

In methodological terms, Marshall tried to steer a middle course between deductivism and empiricism. In multiple editions of his *Principles,* Marshall (1920, p. 29) quoted and endorsed Gustav Schmoller's statement that: 'Induction and deduction are both needed for scientific thought as the left foot and the right foot are both needed for walking.'[8]

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[7] It may also help to explain why Marshall was unable to develop his theoretical system further. When Marshall published his *Principles* in 1890 Spencer's work on evolution was in its heyday. Fifteen years later it was widely abandoned (Hodgson 1993a; 1993b).

[8] Schmoller was a leading member of the German historical school. Marshall's strong and re-

Marshall was highly sceptical of naïve empiricism, on the one hand, and of excessive deductivism and formalism, on the other. He saw economic theory as an essential precondition of empirical enquiry, rather than something that emerged automatically from the gathering of facts. But he also saw limits to highly general 'pure theory' of the type found in the works of Ricardo and Jevons. Theory had to be immersed and qualified by empirical material. Hence on 12 October 1899 Marshall wrote to William A. S. Hewins, the first Director of the London School of Economics, concerning the economics curriculum at the School:

> The fact is I am the dull mean man, who holds Economics to be an organic whole, and has little respect for pure theory (otherwise than as a branch of mathematics or the science of numbers), as for that crude collection and interpretation of facts without the aid of high analysis which sometimes claims to be part of economic history. (Whitaker 1996, vol. 2, p. 256)

Marshall again wrote to Hewins on 29 May 1900:

> Much of 'pure theory' seems to me to be elegant toying: I habitually describe my own pure theory of international trade as a 'toy'. I understand economic science to be the application of powerful analytical methods to unravelling the actions of economic and social causes, to assigning each its part, to tracing mutual interactions and modifications; and above all to laying bare the hidden *causas-causantes*. (Whitaker 1996, vol. 2, p. 280)

Marshall thus emphasised the goal for the economist of understanding underlying causes. Marshall wrote to Francis Edgeworth on 28 August 1902. Again he stressed that the role of theory was both essential and limited:

> In my view 'Theory' is essential. … But I conceive no more calamitous notion than that abstract, or general, or 'theoretical' economics was economics 'proper.' It seems to me an essential but a very small part of economics proper: and by itself sometimes even – well, not a very good occupation of time. (Whitaker 1996, vol. 2, p. 393)

Marshall not only charted a middle course between pure induction and deduction, he also was wary of excessive generalisation. For him, mathematical tools were of great use in the task of constructing and developing

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peatedly expressed sympathies for that school have been grossly underestimated by both his devotees and his critics (Hodgson 2001).

a theory, but they were useful primarily as means to clarify and render consistent the argument. Mathematics was not theory as such. Hence on 27 February 1906 Marshall gave the following advice concerning the use of mathematics a letter to Arthur Bowley:

> (1) Use mathematics as shorthand language, rather than as an engine of inquiry. (2) Keep to them till you have done. (3) Translate into English. (4) Then illustrate by examples that are important in real life (5) Burn the mathematics. (6) If you can't succeed in 4, burn 3. This I do often. (Whitaker 1996, vol. 3, p. 130).

Cambridge colleague and former student Arthur Pigou reminisced about Marshall:

> Though a skilled mathematician, he used mathematics sparingly. He saw that excessive reliance on this instrument might lead us astray in pursuit of intellectual toys, imaginary problems not conforming to the conditions of real life: and further, might distort our sense of proportion by causing us to neglect factors that could not easily be worked up in the mathematical machine. (Pigou 1925, p. 84)

These sentiments are radically different to thosethat prevail in economics today.[9] Marshall's attitude to mathematics in the early years of the twentieth century serves as a benchmark for much of the discipline at that time, alongside the strong and growing mathematical enthusiasm of a minority of academic economists. But by the end of the twentieth century, mathematics had indeed become the principal 'engine of enquiry' within the discipline, rather than a subservient tool of discursive theoretical development. Abundant published models may be accused of neglecting'factors that could not easily be worked up in the mathematical machine'.

Marshall's cautious attitude to the role of mathematics found later expression in the work of his Cambridge student John Maynard Keynes. Like Marshall, Keynes was no enemy of mathematical analysis, but he raised similar concerns regarding its possible misuse:

> Too large a proportion of recent 'mathematical' economics are mere concoctions, as imprecise as the initial assumptions they rest on, which allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols. (Keynes 1936, p. 298)

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[9] Weintraub (2002) argues that Marshall's and Keynes's views became rapidly unrepresentative even within Cambridge.

Keynes wrote to Roy Harrod on 16 July 1938: ‘In economics … to convert a model into a quantitative formula is to destroy its usefulness as an instrument of thought’ (Keynes 1973, p. 299).

Keynes also addressed the rise of econometrics as a sub-discipline. ‘The Econometric Society, an International Society for the Advancement of Economic Theory in its Relation with Statistics and Mathematics’ (to give its full title) was formed in the USA in 1930. The birth of this society was an expression of the growing belief, particularly by the younger generation of economists, that mathematical and statistical techniques were essential to provide rigour and predictive potential for economists. Among this younger generation was the Dutch economist Jan Tinbergen, who published a major treatise on the statistical testing of business-cycle theories, which was later to help him win the Nobel Prize. Tinbergen’s (1939; 1940a) appeal for statistical testing was directed among others at the institutional economist Wesley C. Mitchell (1927) and his team in the National Bureau of Economic Research. They had developed methods of national income accounting and assembled extensive data on business cycles. Tinbergen wanted to use econometric methods to help generate and refine models of the business cycle.

Keynes (1939) responded with a detailed critique of the econometric approach outlined by Tinbergen. Mary Morgan pointed out that Keynes ‘had clearly not read the volume [by Tinbergen] with great care’ (Morgan 1990, p. 121) and Tinbergen (1940b) was to draw attention to this deficit. But Keynes had some forceful arguments. First, because a complete list of variables could not in principle be provided(for reasons of ignorance, availability, difficulty of measurement or whatever),the selection of variables for regression analysis was inevitably partial and biased.

Second, judgemental bias also entered the analysis with unavoidable but empirically ungrounded assumptions (typically concerning linearities and normal distributions) and in the choice of analytical techniques (including intrinsic estimates of time lags). Even if the full set of variables were specified (which is impossible in practice) different econometricians would be likely to reach different conclusions because of their reliance upon judgement and their differences in prior assumptions. Keynes suggested that no combination of data and technique could resolve this issue: some judgemental biases would always be involved. Even with the best techniques available, the data do not guide us unambiguously or objectively to the truth. Unempirical, *a priori* assumptions are always and unavoidably involved. Comparing econometrics to ‘alchemy’, Keynes (1940, p. 156) did not mince his words.

Keynes's second argument has been tested by experiment. Jan Magnus and Mary Morgan (1999) specified a single data set and asked eight teams of researchers to carry out the same set of applied econometric tasks, with whatever methods they chose. The eight participating teams produced different versions of the variables, constructed different models and used different measurement procedures.

In his critique of Tinbergen, Keynes voiced misgivings about the use of mathematical and statistical techniques and upheld that empirical evidence could not guide enquiry unaided by prior assumptions. Tinbergen advocated these techniques and adopted a version of empiricism, believing that evidence could guide enquiry to the truth. With the development of econometrics, the empiricist wing of the age-old methodological debate could also find solace in mathematical technique. Advocates of mathematics no longer had to be champions of deductivism over empiricism. Keynes offered a powerful philosophical critique of this tendency.

Significantly, Keynes's position echoed that of Marshall (1885, p. 166) who argued that 'facts by themselves are silent' and insisted that some prior assumptions were always involved.[10] Clearly, with both Keynes and Marshall, their qualms concerning the use of mathematical and statistical techniques did not emanate from naïve empiricism. Quite the opposite – their insistence was that prior assumptions were always involved. Consequently economists should be educated in matters of judgment as well as technique.

Both Marshall and Keynes showed that much powerful economic theory can be articulated verbally, and they are far from alone in this achievement. Other greats, including Smith, Ricardo, Mill, Marx, Veblen, Schumpeter and Hayek, come to mind. Mathematics plays a useful and sometimes vital complementary role, aiding conceptual clarification and providing thought-stretching heuristics. But historically much important theory in economics is verbal.

But despite the influence of Keynesian macroeconomics from the 1940s to the 1970s, Keynes's own ideas concerning the limitations of econometrics did not prevail. Keynesian models were somewhat remote from Keynes's own theory (Leijonhufvud 1968; Robinson 1975).

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[10] In this regard both Marshall and Keynes anticipated the decisive philosophical argument against empiricism by Willard van Orman Quine (1951). Quine showed that all empirical enquiry rests on prior ontological assumptions that cannot in principle be tested empirically. Thorstein Veblen (e.g. 1900, pp. 241, 253; 1908, p. 397 ff.) had previously made the same point a number of times, but always in his typical, elliptic style.

Econometrics became established after 1945 as a major and prestigious branch of economics. The development of econometrics was both useful and important for economics and it gave the discipline a new character. Those that believe that the primary route to understanding is through empirical enquiry can be attracted to econometrics as a sophisticated and systematic set of techniques, to make sense of the interrelations and correlations within a complex and extensive set of data. The more naïve their empiricism the less they would be concerned by the problem of unverifiable *a priori* judgements and assumptions that had been raised by Marshall and Keynes. After Tinbergen and others, mathematical economics could attract empiricists as well as deductivists. In the absence of more sophisticated methodological and philosophical discussion, its victory would be assured.

Alongside econometrics, Paul Samuelson (1947) established the foundations for post-war mathematical economic theory. His work combined Walrasian general equilibrium theory with the 'Keynesian' macroeconomic modelling that was ironically more the achievement of Keynes's followers than Keynes himself. Samuelson's formalised Walrasian-Keynesian synthesis set the style for much of mathematical economics for the next 30 years.

Concluding remarks.

Modern economics is highly diverse and includes a variety of approaches. Since 1980 game theory has become a major engine of theoretical enquiry in microeconomics. Yet dynamic stochastic general equilibrium models are still highly influential in mainstream economics. Behavioural and experimental economics have produced major challenges to the assumption of rational, maximizing behaviour, yet the rational expectations hypothesis remains the convention in many macroeconomic models. Rational expectations modelling is generally based on the assumptions of well-ordered aggregate demand and a unique equilibrium, yet insurmountable problems with aggregation from individual to macroeconomic demand functions led precisely to the rise of game theory and the discovery in many games of multiple equilibria.[11]

The post-1980 promotion of game theory within mainstream economics meant a major change of mathematical repertoire. The turn means that much of mainstream economics is no longer linked so closely in its meta-

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[11] See Kirman (1989) and Rizvi (1994) for incisive accounts of the theoretical results that undermined the general equilibrium approach and opened the door for game theory.

phors to mechanics or physics.[12] But modern mainstream economics often claims to build models that generate predictions. Despite the changes in the mathematics, the influence of Milton Friedman's (1953) famous argument – that the test of a theory is its capacity for prediction – remains strong. 'Physics envy' endures, but principally in terms of a predilection for prediction. More systematic research would be useful to determine the extent to which these impressions are valid. But there is already strong evidence of a remaining predilection for prediction in terms of the kinds of relevant mathematics that have been relatively neglected within the mainstream.

This may help to explain why some alternative approaches, includingSraffian economics and Post Keynesian modelling, have been neglected by the mainstream. Sraffian analysis seems more concerned with theoretical explanation than prediction (Sraffa 1960). Post Keynesian models vary in their predictive claims (e.g. King 1996; Cornwall and Cornwall 2001), but any inclusion of Keynes's (1936) concept of uncertainty (which by definition excludes any calculable probability) is bound to limit the predictive capacity of the model (Hodgson 2011).[13]

But the relative neglect in terms of potential mathematical exploitation is most obvious in regard to chaos theory and complexity theory. When chaos theory emerged in the 1980s and demonstrated that outcomes in complex, non-linear systems are often unpredictable, some mainstream economists met the new mathematics with a significant flurry of interest.[14]

But mainstream discussion of chaos theory has since waned, and it has never been given the attention warranted by the ubiquity of non-linearities in the real world. A bibliometricanalysis shows that the frequency of the term 'chaos theory' in all JSTOR[15] journals in economics reached a low level by 1995. Mainstream economics is not simply focused on mathematical models; it elevates those models that purport to yield predictions

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[12] Mirowski (2002) sees modern mainstream economics as a 'cyborg science', using the metaphor of an information processor or automaton.

[13] Elsewhere I suggest additional reasons why works in Sraffian and Post Keynesian economics receive low citation levels (Hodgson 1997; 1999). Neither tradition has paid much attention the analysis of human agent or individual decision-making, by using insights from psychology and elsewhere. Consequently it they have done little to displace the mainstream notion of the rational utility-maximiser.

[14] See, for example, Baumol and Quandt (1985), Grandmont (1987), Barnett et al. (1989), Baumol and Benhabib (1989), Brock et al. (1991), Benhabib (1992), and Bullard and Butler (1993). Furthermore some Keynesians saw this as an opportunity to embrace a form of modelling that was consistent with their ideas (Day 1983; Day and Shafer 1985; Rosser 1991).

[15] JSTOR is an online journal database that includes all the prestigious journals and many of more heterodox inclination.

and downplays those that show that our powers of prediction are limited.

For example, a model of the business cycle developed by Richard Goodwin (1972; 1990) involves non-linear dynamics where (with some parameter values) outcomes are highly sensitive to initial conditions, thus eroding its predictive capacity. Despite its reputable mathematics and relatively appealing formal characteristics, the Goodwin model has remained rarely cited in leading journals since its inception. Hodgson (2011) shows that to date since its initial publication it has received only eleven citations in articles in the ten most prominent journals of economics. It seems that ostensive relevance and a strong mathematical component are insufficient to gain mainstream attention.[16]

Neither has the so-called 'complexity revolution' yet shifted mainstream economics from its predictive goals. Although there has been significant discussion of complexity in mainstream journals, it has not yet shaken the belief that the main aim of economics is to build models that yield useful predictions. Rather than look real-world complexity in the face, economists have remained in an artificial world of much simpler models, partly to maintain the rhetoric of prediction. As yet, established techniques for dealing with complexity, such as agent-based modelling and Kauffman-type *NK* models (Kauffman 1995), are generally rare in leading mainstream journals of economics (although they are much more common in organisation science). A possible reason for the neglect is that these kinds of model are intended more as explanatory heuristics and as devices to explore emergent properties of complex systems. Their capacities for prediction are limited at best.

Hence perhaps the focus on prediction has been a major factor in determining the kind of mathematics that mainstream economists have adopted. The hypothesis is confirmed by the evidence above, but would merit further investigation and detailed research.

If the hypothesis were correct then it would reinforce another supposition about modern economics. This is the relative lack of reflection on insights from the modern philosophy of science on how sciences operate. Friedman's (1953) paper is regarded by philosophers as a somewhat crude and ambiguous statement, where one is 'free to choose' one's favoured interpretation (Mäki 2003). One wonders what scientific methodological criteria leading mainstream economists would claim to follow. Popperian falsification would probably top the list, alongside Friedman's test by

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[16] Another relevant example is Velupillai's (1996; 2000; 2005) argument, based in part on the consideration of computability, for an alternative form of mathematics for economics.

predictions. In practice, however, attempts at falsification are rare. More common are attempts to subject empirical results to ever-more stringent robustness checks. Knockout empirical tests are elusive, and in principle extremely difficult to assemble because of the number of variables involved (Boland 1989).

Another remarkable fact is that leading advocates and critics of the mathematics in modern economics play little attention to the philosophical status of mathematical discourse (Mirowski 2012). Rival schools in the philosophy of mathematics discuss whether mathematics is itself real, or representative of reality, or a logical language, or a mental construction, or whatever (Benacerraf and Putnam 1984). For anyonewho is doing (or criticizing) mathematics, this matters.

Consider also the difference between believing that reality is essentially mathematical and believing that it can be helpfully represented in mathematical terms.Most applied economists would probably fall into the latter camp. But some (and perhaps a growing number of) post-war economic theorists seem to belong to the former.Yet there are philosophical gulfs between these views. A problem is that economists are discouraged from submitting their own philosophical reflections on the ontological and epistemic status of mathematics to leading journals of economics. It would be a worthwhile project to investigate the philosophical preconceptions, partly by polling leading economists for their views.

It would be wrong to claim that contemporary mainstream economics has detached itself from empirical evidence. David Colander (2005) has surveyed young economists in leading universities and detected a strong empirical turn in the discipline. It is also clear from inspection of leading journals in economics that articles involving the analysis of empirical data are commonplace. Much of this work is undeniably worthwhile. But a typical feature of this prestigious output is that more attention is paid to robustness tests of the econometric analysis than historical or other grounding of the basic questions or assumptions. Consequently, an arms race of econometric technique has escalated in the leading journals, in excess of other theoretical and judgemental considerations.The empirical turn in economics is welcome, but it should not be diverted into matters of competitive technical display, with technical steps of often-diminishing marginal benefit.

A related problem is that while great stress is place on the benefits of precision yielded from the deployment of mathematical techniques, much less emphasis is placed on precision concerning the concepts involved.

For example, there are a number of articles that usefully test propositions concerning the role of institutions in economic development (e.g. Acemoglu et.al. 2002; Rodrik and Trebbi 2004). Typically, much more attention is played to the econometrics than to understanding the nature and functioning of institutions. Although institutionally-orientated analysis has become fashionable, there is a striking lack of attention to the meaning and definition of an institution. Despite the 'institutional turn' in economics and other social sciences, one waits in hope for an essay devoted to a forensic discussion of the concept of an institution to be published in a prestigious mainstream journal.

Another case in point is the theory of the firm. Models in the area abound (e.g. Aoki 1990; Hart and Moore 1990), but by contrast little attention is given to determining what a firm is. There is simply no consensus on the definition of the firm. But conceptual precision is just as important as mathematical precision.

Overall, the foremost matter of concern is that the predominant emphasis on technique diverts attention away from key matters of assumption and judgment that always precede the application of any technique and the building of any model. Students of economics are given insufficient encouragement to question basic assumptions. Economists need to be trained to make critical judgements as well as producing models. As with the expert chess player, trained judgement involves both analysis and intuition (Simon 1987; Frantz 2005).

Models have to be put in their place alongside conceptual, philosophical, historical and other considerations. We need to be able to criticise assumptions and discriminate between models. Given that decisive empirical tests are rarely possible, other factors have to be taken into account when evaluating different models. Broadly-trained judgement is vital. Its role is enhanced precisely because of the complexity of the phenomena at hand.

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