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What Is the Critique of the Mathematization of Economics?

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I. INTRODUCTION

This paper classifies into an overview the ad hoc and disparate criticisms that have been made of the mathematization of economics over the past seventy years. Some strands of this critique have been developed extensively. For example, MIROWSKI since 1984 has pursued the case that the mathematics of nineteenth century physics provides an unsound base for mathematical economics. But there have been few comprehensive reviews of the arguments against the stress on mathematics in contemporary economics (one example is WOO [1985]).

In this paper, the term 'mathematization of economics' is used to mean the increasing emphasis given to mathematical economics. As defined in the literature, 'conventionally ... mathematical economics is reserved to describe cases employing mathematical techniques beyond simple geometry, such as matrix algebra, differential and integral calculus, differential equations, difference equations, etc' [CHIANG, 1984, p. 3]. This is not greatly different from KOOPMANS' [1954] definition. However, the term is often used more loosely. For instance, ARROW and INTRILIGATOR [1989, Preface] define mathematical economics to include 'various applications of mathematical concepts and techniques to economics, particularly economic theory'. But, whether it is defined precisely or loosely, mathematical economics is nowadays usually differentiated from quantification, econometrics, and the measurement of economic data, which were included in KOOPMANS' definition.

Objections to the mathematization of economics arose well before the 1920s [MIROWSKI, 1989]. But, to retain manageability, this paper starts chronologically with complaints by the likes of MARSHALL and KEYNES in the 1920s (as outlined, for example, in O'DONNELL [1990]). However, the paper is more

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concerned with the content of the critique than its historical development. Accordingly, it situates the historically formed critique in the context of orthodox economics as it progressed after the Second World War and, as far as possible, as it is practised today. Also, the overview is constructed in the light of the continuing debate about the meaning of science as it emerged in the philosophy and sociology of science from the 1950s and 60s (e.g., CHALMERS [1982]).

No great emphasis is put on evaluating the case against mathematical economics. It encompasses a diverse range of claims, many revolving around unresolved and controversial epistemological issues, such as what is the 'correct' way to evaluate scientific theories. Given that such matters face continuing disputation, a researcher's pre-held values may well determine his/her attitude to a particular issue. Value judgements are needed to come to a conclusion about a number of the criticisms raised for they are not amenable to determinate solution.

A further reason for de-emphasizing evaluation reflects the sparse tradition of debate between the two sides. Some of the issues have not been considered in common. For instance, the advantages of mathematical economics are now taken for granted by mathematical economists, for debate on that matter occurred in the late 1940s and early 1950s and is now regarded as 'old hat'. Occasionally, an exponent of the pro-case will present it again, without acknowledging the range of criticisms that have been made against it (e.g., SCHWARTZ [1978], DEBREU [1986, 1991]). Textbooks of mathematical economics adopt this approach, too. Typically, a few pages at the beginning justify the importance of mathematics and dismiss criticism as misplaced (e.g., CHIANG [1984]). Far less frequently does a symposium appear discussing the pros and cons of the matter (e.g., CHARLESWORTH [1963]). To all intents and purposes, the case has been sown up. Criticisms voiced in the 1940s and 50s debate are either thought of as having been answered or are ignored. Certainly, they have been drowned in the flood of application of mathematical methods to economics. Development of technique has been the overwhelming characteristic of mathematical economics since the 1950s, while reflection on its meaning, limitations and practical relevance is less common.

One interesting feature of the anti-mathematical disfavour is that it shows few signs of disappearing. Given the above context, it might be thought that mathematical dominance would have silenced the non- and anti-mathematicians long ago. Yet, there is almost a paradox — the stronger has become mathematics in economics, the more it has produced a reaction. Signs of this are in the academic economics journals that sprang up from the 1960s eschewing a dominant role for mathematics. Of course, many new and existing

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axiomas ou
1ª Crítica: Dado que os/pressupostos usados possuem duvidosa validade empírica ou dedutiva, a dedução a partir deles não têm validade empírica.

journals became *more* mathematical after the 1960s, but the number of new 'non-mathematical' economics journals is surprising, as suggested by the impressionistic list in Appendix 1.

If pressed, the authors would pay tribute to the gains that have been made in the development and enuciation of economic principles by the use of mathematical methods. Our concern, therefore, is not with the use of mathematics as such but with the extent to which these gains might have been made at the expense of other equally valid or even more appropriate methodologies.

During the last seventy years, at least seven criticisms have been directed at the mathematization of economics. These are:

1. The axioms of mathematical economics do not correspond with real world behaviour.
2. The number of empirically testable hypotheses generated by mathematical economics is small compared with the volume of mathematical economic analysis.
3. Some/much of economics is not naturally quantitative and therefore does not lend itself to mathematical exposition.
4. The translation of the description of economic processes from a natural language (such as English) to mathematics can be naive and illegitimate.
5. There is no objective way to gauge whether mathematical economics is more precise than less mathematical economics.
6. There is no *one* 'best' system of mathematical logic.
7. Because of all the above problems, mathematics is often an unnecessary adornment to economic discovery about the real world, but serves other purposes.

Each of these claims is examined below.

II.

1. The axioms of mathematical economics do not correspond with real world behaviour

One criticism of mathematical economics is that the axioms on which much of it rests have dubious descriptive or empirical validity (e.g., LEONTIEF [1971, 1982]). The stricture then proceeds to allege that since the descriptive validity of the assumptions may be questionable, deduction from the axioms cannot have empirical validity either. Strictly, this criticism is equally applicable to non-mathematical reasonings (verbal and geometric) that follow a similar procedure rather than to mathematical deduction per se. However, since the

1ª crítica, porem, é aplicável também ao raciocínio não matemático (desde que usados os mesmos mathematics may heighten focus on the axioms and the chains of reasoning, the issue is considered here.

The point is contentious on two main grounds. First, to what extent are the
1 axioms of mathematical economics unreal? Second, even if they are, does this
2 matter? In FRIEDMAN's [1953] view, for example, the descriptive unreality of assumptions of a theory is immaterial to the theory's value.

1. The first aspect of the criticism holds that only particular restricted sets of assumptions about human behaviour can be used in mathematical economics. These are those from which mathematically tractable deductions can be made, but their empirical validity is rarely considered. A common example of this is in textbook mathematical expositions of microeconomic theory. For example, consider BINGER and HOFFMAN's [1988] Chapter Five on Consumer Preference Theory. They develop the calculus of constrained extrema/maximization/minimization from the six conventional axioms of rational preference (transitivity, completeness, reflexivity, continuousness, nonsatiability, and that indifference curves show diminishing marginal rates of substitution). Neat, mathematically tractable deductions and results are achieved through twenty-six and two-thirds pages developing the mathematical structure of the theory [pp. 103-127]. Only in two and a third pages at the end of the chapter [pp. 127-129] do they raise the question of the empirical validity of the underlying assumptions of rational preference. This is approached via considering the empirical validity of revealed preference theory through experiments with rats and pigeons. Experimental evidence is cited to show that rats and pigeons obey the weak axiom of revealed preference, that 'choices are never directly contradictory' [BINGER and HOFFMAN, 1988, p. 120]. That some animals in some experiments obey the weak axiom of revealed preference for food and water casts little light on whether humans obey the six axioms of rational preference above. But at least BINGER and HOFFMAN raise the question of the empirical validity of these axioms. Many other microeconomic texts do not pursue the issue as far as this (e.g. VARIAN [1984], NICHOLSON [1985], MANSFIELD [1985]).

Debate about the descriptive reality of the rationality (and maximization) assumptions is as old as neo-classical theory itself [BLAUG, 1985] and has never reached universally accepted conclusions. Periodically, the debate gathers steam, as it did most recently from the 1970s and continues today. In the late 1980s, a veritable plethora of studies questioning the empirical and non-empirical nature of the rationality and maximization assumptions emerged in economics, psychology and behavioural science[1] (e.g., GILAD and KAISH [1986], FURNHAM and LEWIS [1986], ELSTER [1986, 1990], SCHWARTZ [1986], TVERSKY and KAHNEMAN [1986], EARL [1986, 1988], HOGARTH and REDER [1987], MACHINA [1987], LEA, TARPY and WEBLEY [1987], ALBANESE [1988], BAXTER [1988], HODGSON [1988], HARGREAVES-HEAP [1989]). But, just as in earlier disputations, the current one is no nearer to consensus. The most agreement that exists among the protagonists is that in some situations the conventional assumptions may apply in the real world, whereas in others they may not.

2. To economists, a familiar defence to the criticism above is FRIEDMAN's [1953] dictum that the reality of assumptions is irrelevant, that 'to be important ... a hypothesis must be descriptively false in its assumptions' (p. 14). This controversial claim spawned an enormous response in economics that continued into the 1980s. FRIEDMAN's position is probably best described (by WONG [1973], BOLAND [1979], CALDWELL [1980] and MUSGRAVE [1981]) as instrumentalism, that theories are 'neither true nor false but merely as instruments' for prediction' [LAKATOS, 1970, p. 95]. But instrumentalism is professed by few scientists today. It started to fall into disfavour from the 1940s as broader and more encompassing conceptions of scientific validation were developed [CALDWELL, 1980]. Even the seminal covering law model of scientific explanation, articulated by HEMPEL and OPPENHEIM in 1948, declared that 'the sentences constituting the explanans must be true'. The explanans, or statements of antecedent conditions and general laws, including assumptions, 'have to satisfy some condition of factual correctness' (p. 137). Possibly, the most common philosophy of science held today is realism, 'the theory that the ultimate objects of scientific inquiry exist and act (for the most part) quite independently of scientists and their activity' [BHASKAR, 1989, p. 12]. In this view, 'the aim of science is to construct the theories of ever increasing truth-likeness about what the physical world ... is like' [CURRIE, 1988, p. 205], 'to have true theories about the world, where "true" is understood in the classical correspondence sense' [MUSGRAVE, 1988, p. 229]. Insofar as scientific realism places great store on the correspondence of theoretical construction with the real world, it is not sympathetic to the idea of assumptions of theories that do not show the same truth connection with reality. Thus, a realist philosopher who has written extensively about economics, argues that 'without assessments of

1. Debate about the empirical validity of the assumptions is conceptually distinct from the equally vibrant contemporary debate about the internal consistency of the assumptions, of which ANAND [1987] is an example.

realism (approximate truth) of assumptions, the process of theory modification would be hopelessly inefficient' [HAUSMAN, 1989, p. 21]. This is in spite of the fact that some realists hold that metaphysical statements play a role in theory construction referring to entities which are impossible to observe (e.g., ARONSON [1984]). The great contemporary challenge to scientific realism is the relativism of the sociology and anthropology of science (e.g., KNORR-CETINA and MULKAY [1983]). But insofar as scientific theories rest on 'unreal' assumptions, this would be regarded as fuel for the charge by the sociology of science that scientific discovery is relativistic; for example, that the use of particular assumptions in neo-classical mathematical microeconomics reflects mainly the mind-sets of users of the theory, and not the real world.

The argument about the continued use of particular (unreal?) assumptions in mathematical economics — ones that lend themselves to mathematical manipulation — is an illustration of a broader problem. This is the extent to which formulations of economic processes that do not lead to 'neat' mathematical results (e.g., provable theorems), or are mathematically intractable, are relatively downgraded. For instance, a range of economic behaviours is given little attention, relative to their probable real world importance, in recent textbooks used at undergraduate and graduate levels. Thus, texts such as VARIAN [1984], NICHOLSON [1985], MANSFIELD [1985], LIPSEY et.al. [1985], SAMUELSON and NORDHAUS [1985] and BINGER and HOFFMAN [1988], compared with the space they devote to conventional mathematical (and geometric) topics, give little space to the following:

- LEIBENSTEIN's work on X-efficiency and the importance of 'information constraints' and 'bounded rationality' more generally
- utility based on relative not absolute levels of consumption
- equity as a social objective on a par with allocative efficiency
- multiple criteria objective functions where indifference curve analysis may be of little practical value
- functions which are not 'continuous and twice differentiable', whose extrema are determined, for example, by dynamic programming
- non-maximizing theories of the firm (e.g., evolutionary theory, satisficising, organisation theory)
- theories of consumer behaviour embodying non-rational elements

Much of the above does not lend itself to determinate mathematical analysis, and most do not lead to unique solutions. Its relative downgrading in the teaching of contemporary economics suggests that economic analysis is being moulded by the bounded rationality of mathematics. The mathematics may be driving the theory.

2. The number of empirically testable theories generated by mathematical economics is small compared to the volume of mathematical economics.

This criticism stems from the first above. If the mathematical expression of economic phenomena *does* rest on unreal assumptions, it may not be able to produce empirically relevant predictions; that is, 'unrealistic assumptions (in the sense of false assumption) will always result in false predictions' [HAUSMAN, 1989, pp. 120-121]. Therefore, while the enormous volume of mathematical economics explores the formal properties of models, it is little interested in generating predictions capable of empirical test.

How might the empirical validity of this criticism be assessed? One morsel of evidence is LEONTIEF's [1982] classification of the articles published in the *American Economic Review* between 1972 and 1981. Over fifty percent were 'mathematical models without any data', and less than thirty percent were empirical analyses of various kinds, with the first type growing proportionately over time. LEONTIEF's analysis has been updated by MORGAN [1988]. He analyzed articles in both the *American Economic Review* and the *Economic Journal* from 1982 to 1986. 'Mathematical models without any data' had declined to 42 percent in the *AER* but had risen to 52 percent in the *EJ*. MORGAN compared this with a prominent journal each in political science, sociology, chemistry and physics. 'Mathematical models without any data' showed up at 18, 1, 0 and 12 percent respectively [MORGAN, 1988, p. 163].

Another line to help assess the empirical validity of this criticism might be to look at textbooks of mathematical economics. Invariably, these concentrate on the construction of theory rather than on its empirical relevance. ARROW and HAHN's *General Competitive Analysis* [1971] is an example. It is difficult to disagree with MORISHIMA who claimed that it was 'poor in terms of empirical content' despite it being a volume in a series whose stated aim was to 'relate the theory to relevant empirical work' [1984, pp. 51-2]. The situation may not have altered much since ARROW and HAHN's classic. For instance, in VARIAN [1984], there is no demonstration of how or whether any of the mathematical theorems produce empirically testable predictions. NICHOLSON's less mathematically advanced treatment [1985] has, at the end of each chapter, a few pages devoted to one or more applied examples of the theoretical microeconomic exposition. But in no example is the correspondence between the mathematical terminology and the empirical situation shown. Rather, each applied example is told in the form of a verbal historical story in which no mathematics occurs. Since each case study can apparently be analysed non-mathematically, the question arises as to the purpose of the mathematical symbols in the theoretical exposition.

Or, consider the seminal *Handbooks in Economics*, especially the three volumes of the *Handbook of Mathematical Economics* [1989], edited by ARROW and INTRILIGATOR. The twenty nine chapters in the three volumes cover the entire gamut of the application of mathematics to economics. But there is minimal reference to empirical tests of prediction from the theories presented. Instead, emphasis is on the abstract and formal properties of the systems of equations constructed, and proofs are offered for various theorems. Indeed, there is scant relation to anything empirical.

It could be argued that recent work in industrial organisation theory has departed radically from the neo-classical view and is attempting to develop mathematical models that do correspond with real world behaviour, e.g., TIROLE [1988], SCHMALENSEE and WILLIG [1989]. However, in his review of the *Handbook of Industrial Organisation* [1989], PELTZMAN [1991] observes '.. a retreat from policy analysis, a deemphasis of empirical work, and a focus on formal theory' [p. 204]. He notes a striking departure from the normative theory of a generation ago, which relied heavily on verbal discourse, as the discipline has '... drifted toward an ingrown fascination with formalism'. We would agree with him that this has introduced a rigour which was often lacking in earlier work, but also agree that the end result has not been a set of testable models designed to help explore observable facts or empirical regularities. The objective seems to have been based more on a desire for logical completeness than a desire for new understandings.

If the books cited above are typical of the thrust of mathematical economics, the conclusion seems inescapable that the theory dwarfs the empirical application (e.g., LEONTIEF [1971], DENNIS [1982]). The essence of this criticism is that too much of theory and model construction in economics is not being tested against real world data. The complaint is not so much about modelling per se. Clearly, abstraction has to be made from the endless detail and variety of behaviours found in the real world. Otherwise, it would not be possible to aggregate or summarize across observed behaviours without a model to make predictions that can be tested against empirical data. Abstraction is made from the real world and the resultant model is manipulated. But for the process to be relevant, reference must be made back to the real world. The complaint about mathematical modelling is that too frequently this last vital step is ignored.

Partly, this may be because adequate econometric techniques do not exist to judge between the claims of competing theories. The availability of a quantitative empirical testing procedure is the ultimate sine qua non of mathematical modelling. But if econometrics is not up to the task of discriminating between competing theories, the question of the purpose of mathematical modelling persists. Complaints about this problem surface repeatedly, in studies pointing to the inability of econometric testing procedures to discriminate between the predictions of competing theories (e.g., SARGENT [1976], MCCALLUM [1979], PESARAN [1982]). This criticism would seem to reduce the role of econometrics to one of estimation only. One might hope that in the longer term, the availability of better datasets and better theories (particularly at the micro level) would improve the position. One must be concerned, however, that as the level of disaggregation approaches that of the individual firm or household, we do not face a 'Heisenberg's Uncertainty Principle' where the behaviour of the individual unit is simply not predictable.

2 Another reason why the development of mathematical models has outstripped their empirical testing may concern their debatable behavioural content, raised in the previous section. If the theory of economics cannot provide an adequate behavioural base, the mathematics is not going to enhance confidence in using the model for predictive or policy purposes. Indeed, the mathematics may even limit the behavioural content of a model, as noted in the previous section. Few mathematical models can claim to have won decision makers' confidence and trust through repeated and successful application in practice. In the absence of such proving, it will be the behavioural content not the mathematical structure that gives a decision maker confidence in a model. Models need not be mathematical nor their predictions quantitative to be of practical policy use.

3. Some/much of economics is not naturally quantitative

A third criticism of mathematical economics contests SAMUELSON's claim that economics is 'naturally' quantitative and thereby lends itself to mathematical analysis [SAMUELSON, 1952, p.63]. The alternative view to SAMUELSON's is that mathematics draws attention away from qualitative problems in economics. This view emphasizes the tendency to bias the information set by reducing the importance of those aspects of a problem that are not conveniently quantifiable or included in the narrow framework of a mathematical model. For instance, if policy models are constructed on the basis of neo-classical theory, how long might it be before considerations of equity and the hardships of the transitional period are ignored completely? If government policy advice is proferred on the basis of such models, a disservice is being done to the community. Further, what is the value of an economic adviser who defines 'economics' and her model to exclude the realities of the political economy. For economics to remain a live discipline, its practitioners must be able to include *all* key aspects of the problem in their presentation to the decision maker. //

Economists who assess qualitative issues as of major importance in economics have seldom been sympathetic to the trend to quantification and/or mathematization as charting the way forward for economics (e.g., KEYNES [1924, 1939], VON MISES [1949], SHACKLE [1972]. A softer version of this critique might note that mathematics can handle many qualitative factors including uncertainty and risk, that 'qualitative differences are equally subject to mathematical analysis' as are quantitative relationships [BOULDING, 1948, p. 187]. It might agree with SAMUELSON's observation of 'the convenience of mathematical symbolism for handling certain deductive inferences' [1952, p. 64]. But this begs the question of whether those 'certain deductive inferences' are incisive in explicating real-life economic activity. The risk is that SAMUELSON's 'problems of economic theory' become limited to those for which the mathematics of 'certain deductive inferences' are appropriate; that is, the mathematical theory drives and shapes the problems that are analysed.

Both the hard and soft versions of this criticism dispute the image that mathematical expression can give to the representation of economic processes. This image can be that processes are closed and determinate rather than open-ended and indeterminate, that economic theory is precise rather than loose, that economic 'laws' exist rather than tendencies. The maths can convey a tidiness and order about economic processes that do not exist in reality.

How might the empirical validity of this type of criticism be assessed? If some/much of mathematical economics rests on empirically invalid assumptions (Point One) and if some/much does not yield empirically testable predictions (Point Two), it may be a reasonable presumption that theories of mathematical economics have not captured the complexities of the real world. Perhaps the complexity derives from factors impinging on economic processes that cannot be expressed in mathematical symbolism. Or, perhaps the factors will be expressable mathematically only when they are discovered. Perhaps, also, there is an additional rationalization. As JOSKOW [1975, p. 273] put it, 'somehow one gets the distinct feeling that the important messages are being carried by the informal theories, stories, and behavioural observations, and that the formal models are trotted out ex-post to demonstrate that some kind of formal apparatus can explain or incorporate some of what is actually being observed'.

The alleged disconformity between some qualitative aspects of economic processes and mathematical expression can be thought of as part of a more encompassing criticism. This is the extent to which it is linguistically and logically possible to represent human action mathematically.

4. The translation of economic processes from a natural language to mathematics can be naive and illegitimate

Some early proponents of the application of maths to economics were adamant that there was and should be a 'strict equivalence of mathematical symbols and literary words', epitomized in SAMUELSON's contention that 'mathematics is language'. On this assertion, 'the two media (mathematics and words) are strictly identical' [1952, p. 56] and it should always be possible to translate one directly to the other; in STIGLER's view, 'translation is absolutely necessary, not merely desirable' [SAMUELSON, 1952, p. 56; STIGLER, 1949, p. 45][2].

Subsequent critics have contested this view in two ways. First, the claim has been made that there is not a direct equivalence between expression in the vernacular English language and mathematics, that is, mathematics is not a natural language. Second, it has been asserted that the expression of economic processes in mathematical terms may read into the maths a behavioural content it does not possess.

An example of these criticisms is DENNIS [1982] who examined four published papers using mathematical economics terminology. He claimed that the *mathematical* meanings of the equations employed were 'propositions concerning numerical equalities and inequalities (and their empirical counterparts), and the conditional relationships between those relations'. However, the *verbal* translations of those formulae, as expressed in the relevant discussion in the four papers, said something quite different. They asserted that the equalities and inequalities described human motivation — 'forcing', 'wishing', 'must' — that is, human 'belief, capability, allegation, motive or intention'. The relatively simple mathematical formulae employed in the four papers were given meaning they did not possess; they were asserting *causal* connections when really they were expressing only *conceptual* connections between numerical phenomena. As another example, DENNIS [1981] argued re ARROW and HAHN's *General Competitive Analysis* that all they had 'proved formally is a series of abstract mathematical propositions mainly about numerical sequences approaching finite limits, mathematical propositions possessing in and of themselves no economic (or extramathematical) significance even though some economic meaning has been attributed to, or 'read into' those mathematical theorems through the habitual practice of naive translation' [p. 104]. Again, this criticism has older roots; for example, as put by MACHLUP [1952, pp. 69-70], 'the basic

2. To what extent this advice has been followed in mathematical economics is not investigated here.

human attitudes that underlie economic conduct . . . cannot be described and analyzed exclusively in mathematical language'.

If this argument is valid, the claimed rigour of mathematical economics is dependent on *two* attributes: one, the veracity of the translation from the literary vernacular to the mathematical symbolic, and, two, the self-contained accuracy of the mathematical manipulation. As MIROWSKI [1986] put it, 'rigour and conciseness derive as much from the precision with which the analyst is capable of performing the translation between the subsystem of the mathematical model and the English commentary, as it does from the appropriate handling of the rules of mathematical manipulation' [p. 199]. DENNIS, for one, obviously thinks that some mathematical economics falls down on the first attribute, that it presents a 'double standard of high mathematical rigor and low semantic comedy' [1982, p. 1060].

In this view, 'mathematics is not a language, but a field (or several fields) of logic'. A language is 'a social skill consisting of the use of signs for the purpose of expressing and communicating feelings and ideas' whereas logic 'involves a more limited range of activities: mainly, the making and appraising of inferences' [DENNIS, 1982, p.697]. Since mathematics is not a natural language, it is not suited to expressing the complete range of human actions and relationships. As noted in Point Three, some qualitative concepts may be unsuited to mathematical expression, especially those not separated from each other by clear-cut boundaries. Such concepts possess multi-dimensional attributes, what GEORGESCU-ROEGEN [1971] called 'dialectical terms', such as equity, efficiency, workable competition, utility, wants, abstraction, belief, judgement, good, evil, democracy, power, and so on [GEORGESCU-ROEGEN, 1979], ideas that emphasize forms and qualities.

Theories employing dialectical or heterogenous concepts, like evolutionary theories of economic change (e.g., NELSON and WINTER [1982]) and behavioural theories of consumer behaviour (e.g., EARL [1986]) will be inexpressable in mathematical symbolism. Explanations of economic processes that seek to include cultural, sociological, historical, psychological influences and so on, are not easily reducible to mathematical expression. Dialectical problems are not confined to variables in social science theories, however. They occur also in the physical sciences. Thus, for thirty four years, it was held that VON NEUMANN had provided the definitive proof in 1932 that 'hidden variable' theories in quantum physics were impossible. But, gradually, VON NEUMANN's 'proof' was exposed as invalid. Whereas 'hidden variable' theories, such as BOHM's [1952], involved qualitative and dialectical concepts, the mathematical and logical form of VON NEUMANN's proof was such that it could not handle dialectical terms within an axiomatic framework [PINCH, 1977]. One of VON

NEUMANN's assumptions was eventually exposed [1966] as inconsistent with any 'hidden variable' theory. The selection of assumptions had pre-determined VON NEUMANN's conclusion.

5. *There is no objective way to gauge whether mathematical economics is more precise than less-mathematical economics*

One of the advantages often claimed for the injection of mathematics into economics is that it produces greater clarity, precision and conciseness in economic expression. For instance, in the analysis of utility, SAMUELSON contended that 'it is demonstrable from the literature that symbolic methods have been an aid to clear thinking and the advancement of analysis' [1947, pp. 92]. STIGLER, who was sympathetic to many aspects of the mathematization of economics, felt that this Samuelson-type view was 'almost the opposite of the truth' because mathematical symbols could, and had been used to refer to 'confused ideas', to 'ambiguous' ideas, and to unclear concepts [1949, pp. 39-40] — the mere expression of ideas mathematically was no guarantee of their precision, a view similar to the translation problem above.

STIGLER was unconvinced that it was possible to define terms such as 'clarity' with any precision. Even if the concept could be clarified, 'it is difficult to conceive of a method of testing the claim; it seems necessary for each person to accept or reject it on faith' [1949, p. 40]. STIGLER was ahead of his time in anticipating a theme that was to carry weight in the re-assessment of the meaning of science from the 1960s. This was the extent to which objective or neutral procedures exist for assessing the conduct of scientific activity (e.g., CHALMERS [1982], OLDROYD [1986], RICHARDS [1987]). A dominant view to emerge from that re-assessment in the philosophy and sociology of science is that neutral, objective procedures do not exist, that each procedure is contaminated by human judgement which differs inevitably between people even though 'best' efforts are made to seek a truth that exists outside of human consciousness (e.g., LAWSON [1987]).

The absence of a neutral, objective definition for 'clarity' conforms with these views. Scientists (and everybody else) may differ about the meaning and applicability of terms such as 'precision', 'conciseness', 'simplicity', 'elegance', 'scientific', 'rigorous', 'validity', 'truthful', and so on in any one particular concrete situation. How might 'simplicity' be defined, for example? Even HEMPEL, a neo-positivist philosopher of science, conceded that 'no satisfactory general characterization of simplicity is available'. He used a mathematical example to show that simplicity can only be defined in relation to a prior mathematical frame of reference [HEMPEL, 1966, pp. 41-42]. This is akin to the realist philosopher of science, NEWTON-SMITH's idea, that 'relative simplicity to a large extent lies in the eyes of the theoretician and not in the theory, while 'there is no reason to see greater relative simplicity of this sort as an indicator of greater verisimilitude' [1981, pp. 230-231].

Where economists have grappled with defining 'simplicity', they have been forced to recognize the above views. For instance, FRIEDMAN [1953] noted the problem in discussing ways in which the same empirical evidence can be consistent with alternative hypotheses. He suggested that the choice between hypotheses could be assisted by applying criteria such as 'simplicity'. FRIEDMAN defined 'simpler' as 'the less the initial knowledge needed to make a prediction within a given field of phenomena' [1953, p. 10]. This is similar to HEMPEL's observation that 'the number of independent *basic assumptions* is sometimes suggested as an indicator of complexity', but HEMPEL rejected this as a satisfactory criterion; 'assumptions can be combined and split up in many ways; there is no unambiguous way of counting them'. Even if there were, 'different basic assumptions might in turn differ in complexity and would then have to be weighted rather than counted' [HEMPEL, 1966, p. 42]. Although FRIEDMAN did not discuss these problems, he did concede that notions such as simplicity 'defy completely objective specification' [1953, p. 10].

Even if a consensual definition could be reached that mathematical analysis was 'simpler' or more 'precise' than less-mathematical analysis in particular instances in economics, it would not necessarily point to its superiority. Perhaps the less-mathematical analysis was sufficiently precise for the task at hand, perhaps the analysis did not lose anything by being expressed less-mathematically, perhaps the less-maths was more comprehensive and more true. Subjective trade-offs would be necessary to decide on what basis the maths or less-maths was 'better'.

Specifying the relative qualities of mathematical and less-mathematical expression in economics has usually revealed only the value judgements of the commentator. A nice example is STIGLER who 'did not need to argue the proposition that verbal reasoning can also be extremely subtle and complicated and rigorous — and even beautiful' [1949, p. 42]. An opposite viewpoint was KLEIN's [1954, p. 360] for whom verbal reasoning tended to be 'fat, sloppy and vague ' while mathematical expression could be 'compact', and possess 'beauty' and 'elegance'. There is no objective definition of any of these terms, they are in the eye of the beholder. Certainly, an arbitrary definition could be constructed (say, 'simpler' is that which uses fewer literary words, more symbols etc.), but any judgement would only be as valid as the arbitrary definition on which it rested. There may well be economic processes analysed more adeptly (another value judgement) by maths than less-maths. But, like vernacular thinking, mathematical thinking can also be subject to disputation within its framework, to contested logic and to subjective definition. It is not a complete, closed, unambiguous, determinate system of symbolic logic. This contention is considered below.

6. *There is no one 'best' system of mathematical logic*

Since the 1930s, the view has become increasingly accepted that mathematics does not stand as one complete and closed system of logic. Instead, different schools of mathematics emphasize different conceptualizations of logic, and no one school can, via empirical test or otherwise, be shown as intellectually superior. This recognition weakens claims that mathematics is necessarily a more consistent system of logic than vernacular reasoning, but, equally, does not 'prove' that mathematical logic is inferior to verbal reasoning.

The historical development of the process whereby the idea of mathematics as *the* epitome of logical reasoning gradually eroded during the early twentieth century is traced by KLINE [1980]. He showed that by 1930, 'four separate, distinct, and more or less conflicting approaches to mathematics had been expounded (logicism, intuitionism, formalism and set theory), and the proponents of the several views were, it is no exaggeration to say, at war with each other. No longer could one say that a theorem of mathematics was correctly proven. By 1930 one had to add by whose standards it was deemed correct' [KLINE, 1980, p. 257]. According to KLINE, this loss of certainty has continued. GÖDEL's incompleteness theorem [1931], 'produced a debacle' and it was followed by a succession of theorems undermining the certainty of mathematics such as the LOWENHEIM-SKOLEM theorems [1920-33] and CHURCH's theorem [1936]. With GÖDEL's later work and COHEN's independence proofs [1963], 'mathematics reached a plight as disturbing as a the creation of non-Euclidean geometry' [KLINE, 1980, pp. 6,271-2,267, 269].

The net result of these developments, according to KLINE, is that 'the present state of mathematics is anomalous and deplorable. The light of truth no longer illuminates the road to follow.' 'The loss of truth is ... a tragedy of the first magnitude', in which 'the concept of a universally accepted, infallible body of reasoning ... is a grand illusion ... The Age of Reason is gone'. Any thought that mathematics has 'absolute certainty or validity of its results, could no longer be claimed' [KLINE, 1980, pp. 275, 278, 6, 7, 263, 325]. Other philosophers of mathematics have drawn similar conclusions (e.g., BARKER [1964], DAVIS and HERSH [1981, 1986]). Thus, in the absence of the attainment of 'absolute truths', the choice between different methods of analysis or discovery can only be made on pragmatic grounds. Subjective trade-offs become necessary to choose between degrees of 'rigour', 'comprehensiveness', 'approachability', etc.

What might the relevance of all this be to mathematical economics? It seems that mathematical economics may be unfamiliar with the above. Often, one of the main schools of mathematics is followed in economics as though it had none of the blemishes to which KLINE points. For instance, formalism is common in economics but there is little discussion of the criticisms that have been levelled at it.

Formalism in mathematics has long emphasized the consistency of deduction from the given axioms. As HILBERT, the founder of formalism, put it, 'if the arbitrarily posited axioms together with all their consequences do not contradict one another, then they are truth and the things defined by these axioms exist. For me, this is the criterion of truth and existence' [KLUGE, 1971, p. 12]. Even at the time, however, the logistic mathematician, FREGE, declared that he 'cannot admit such an inference from consistency to truth' [KLUGE, 1971, p. 21]. To FREGE, existence and truth had a priority over consistency. This disagreement is pertinent to the use of formalistic mathematics in economics, for formalism 'shifted the emphasis in mathematics from questions of truth to questions of deductive relationships' [RESNIK, 1980, p. 108]. In this emphasis, mathematics became 'abstract, symbolic, and without reference to meaning ... the formalists sought to buy certainty at a price, the price of dealing with meaningless symbols' [KLINE, 1980, pp. 247, 248]. Formalism came to have little relevance to cognition, material meaning or real-world data.

A related aspect of the critique of formalist mathematics concerns its game playing nature — it may be a great game to play, but it does not necessarily reveal truth. For instance, VON NEUMANN, who was to influence the direction of mathematics in economics, was drawn to formalism. To him, 'we must regard classical mathematics as a combinatorial game played with the primitive symbols' [VON NEUMANN, 1931]. In formalism, 'mathematics simply is a game of symbol manipulation' [RESNIK, 1980, p. 17]. But as one philosopher of mathematics asked, 'if we regard mathematics as a game played with meaningless marks, what then is the point of the game?' [BARKER, 1964, p. 98]. An alternative view from formalism might be that a mathematical system is useful only if it enables relationships between empirical entities to be described. As we have seen, a persistent criticism of mathematical economics is that it does not do this. But, even so, a powerful critique has all but destroyed the empiricist view in mathematics, which seems no more reliable than formalism.

A third criticism of formalism relevant to its use in economics stems from GÖDEL's incompleteness theorems. No set of axioms can be specified that will define fully the reality of any phenomenon. In the last two hundred years, approaches to mathematics such as formalism and logicism have 'tacitly assumed that each sector of mathematical thought can be supplied with a set of axioms sufficient for developing systematically the endless totality of true propositions about the given area of inquiry. GÖDEL's paper [1931] showed that this assumption is untenable'. This was so because 'given *any* consistent set of arithmetical axioms, there are true arithmetical statements that cannot be derived from the set'. Since this is the case, 'no absolutely impeccable guarantee can be given that many significant branches of mathematical thought are entirely free from internal contradiction' [NAGEL and NEWMAN, 1958, pp. 6, 58, 6].

The above three criticisms of formalism in mathematics can be related to its application in economics. One example of the alleged incompleteness of axiomatics in economics occurs in general equilibrium analysis. INGRAO and ISRAEL [1985] contended that the aims of the Walrasian research programme — in their interpretation, 'to demonstrate the *existence*, the *uniqueness* of the economic equilibrium and the *global stability* of the system' — have not been realized. In their view, DEBREU's axiomatic system did not encompass global stability so that he 'never touched on the problem . . . he identified the sole central issue to be the existence theorem' [INGRAO and ISRAEL, 1985, pp. 109, 114]. Because of these types of problems affecting efforts to specify axiomatic structures for economic systems, SOLOW speculated that 'the attempt to construct economics as an axiomatically based hard science is doomed to fail' [1985, p. 328]. GÖDEL's theorems suggest the likelihood of this possibility. This whole problem is similar to the alleged 'unreality' of the neo-classical assumptions specifying the behaviour of economic agents. If the set of axioms does not depict the system to be analysed, due to 'unreality and because of 'incompleteness', mathematical deduction may be unable to define theorems that represent the actual system.

DEBREU's work exemplifies a second of the criticisms above that has been directed at mathematical formalism, namely, that it is unrelated to empirical content. Of course, DEBREU has long conceded this point, that the 'acid test' of the mathematization of economic theory is 'removing all their economic interpretations and letting their mathematical infrastructure stand on its own' [DEBREU, 1991, p. 3]. It is not saying anything new to note that DEBREU has never sought to consider how real-world data might relate to his theorems. In the twenty paper reprint of his work [1983], none touches on how the theorems might apply in reality. All this underlines the third criticism of mathematical formalism above, that it is no more than an esoteric game played by applied mathematicians. It seems to be a peculiarity of economics that a segment of the profession does mainly formalist mathematical 'theory' unrelated to the empirical world and enjoys the highest status in the profession. Perhaps no other physical or social science is in this position.

A more charitable view might be to claim that 'pure', formal, theoretical research may eventually lead to empirically testable hypotheses. It might have done so in fields other than economics; for example, EINSTEIN's theory of gravitation [1911] was not immediately testable empirically, but within twenty years, various of its predictions did become so [POWERS, 1982, pp. 114-118]. This line of argument raises at least two potentially unanswerable questions for economics, one of which was discussed earlier. This was whether the body of mathematical economics to date *has* led to many empirically testable hypotheses. A second question is the continuing and unresolved one of exactly *how* theories can be tested empirically. Suffice to say that this question is far from settled (e.g., in science generally, HARDING [1976], in economics, GILBERT [1989], SAWYER, BEED and SANKEY [1991].

The more charitable view could be extended to argue that mathematical modelling performs the useful function of showing how the elements of economic systems fit together in a clearer way than verbal modelling. This is a view that DEBREU holds, for instance. But it leads back into the points discussed earlier, namely, whether economic phenomena can always be translated from vernacular English to mathematics, and whether clear-cut definitions of terms such as 'clearer' exist. Some older practitioners of mathematical economics were not too impressed by the 'fit together' claim. For example, SAMUELSON [1952, p. 63] played down PARETO's belief 'that the chief virtue of mathematics is in its ability to represent complexly interacting interdependent phenomena. I think we must accept this with a grain of salt . . . ' in the sense that the maths 'may not add very much more', a view that echoes BOULDING's [1948, p. 192].

In any case, the 'fit together' idea does not *have* to lead to mathematical 1 modelling. In the study of any system, the first step must be to construct a 'verbal model' of the system; that is, to identify and isolate the key component parts of the system and the causal relationships that exist between them. The 2 mathematical economist might then translate such a statement into a mathematical model which could be manipulated to derive relationships that might not have been apparent in the initial specification, and to derive predictions about real world behaviour that can be tested empirically. For this second stage, there is, however, an alternative to the traditionally employed mathematics of 'continuous and twice differentiable functions'. Computer simulation modelling (either deterministic or stochastic) can readily be used to translate a verbal model into a quantitative and testable form without the need for extensive mathematical skills and without the need to over-simplify the model to such an extent that it becomes mathematically tractable but potentially unrealistic. These models have the rigour and clarity of traditional analytical models while retaining the flexibility and approachability of the verbal model. In the area of general equilibrium theory (whether or not one accepts their neo-classical basis), such 'Computable General Equilibrium' simulation models have already demonstrated that systems of a complexity quite intractable to analytical methods can be used as a basis for a rigorous scientific enquiry. In macroeconomic modelling, only the simpler and unrealistic linear models have proved amenable to mathematical solution, with the 'interesting' non-linear specifications already having to concede to computer simulation methods.

But, there are further indeterminancies with any sort of modelling. Just as there is more than one way of analyzing the same economic processes verbally, so there is more than one way mathematically. Although the above discussion concentrated on the application of formalist mathematics to economics, other mathematical approaches can be employed to analyse the same phenomena. For instance, ROEMER [1981] used formalist axiomatic maths to interpret the foundations of Marxian theory while MIROWSKI [1986] used group theory maths. They each ended up with different interpretations of components of the theory. To ROEMER, one aspect of MARX's theory, that 'prices of commodities are such that equivalent exchanges for equivalent' was 'incidental' to MARX's analysis and must be discarded [1981, p. 150]. MIROWSKI, however, showed that for MARX, the trade of equivalents 'was a prior condition for the quantitative comprehension of a capitalist economy', and that MARX had a consistent analysis of the problem in the first six chapters of Volume 1 of *Capital* that can be interpreted in group theory mathematical terms [1986, pp. 221-232]. There is no value-judgement-free answer to the question which is the 'right' or 'best' approach. The particular methodology chosen (axiomatics versus group theory) to some extent pre-determined the outcome. Of course, the conclusion is more apparent in verbal models of Marxist theory of which numerous competing varieties exist. But to suppose that mathematics gets rid of priors is dubious.

The list of ways in which mathematical reasoning has been alleged historically to possess indeterminate qualities, like verbal reasoning, could go on. For example, the conception of what is regarded as a mathematical proof changes over time (e.g., WILDER [1981]), no one unassailable system of mathematical logic exists nor do mathematicians try and practise one (e.g., LEHMAN [1979]), and, therefore mathematical proof is not inviolate (e.g., LAKATOS [1976], PINCH [1977], BLOOR [1976], KLINE [1980]). A nice overview of these issues is CROWE [1988] who illustrated ten misconceptions about mathematics and its history, including the erroneous claims that:

- mathematics provides certain knowledge
- mathematics is cumulative knowledge
- mathematical statements are invariably correct
- mathematical proof is unproblematic
- standards of rigour are unchanging
- mathematical claims admit of decisive falsification

None of these disputable claims means that mathematics in economics should be abandoned. Nor do they show that the disputed philosophical bases of mathematics put mathematical expression on a par with literary expression. They do imply that the expression of ideas in mathematical form carries with it a higher degree of uncertainty than was recognised before the 1930s. Unfortunately, mathematical economics has given little weight to the discussion of these philosophical issues.

7.

7. *Mathematics adds little to the understanding of real world processes but serves other purposes*

The seventh criticism of mathematical economics stems from the previous six. This is the claim that because of the six earlier problems, mathematical economics has discovered little about the real world, and certainly little that less-mathematical approaches have not achieved or could not have achieved. An aspect of this criticism is the contention that where mathematics is used, it adds little to the process of discovery. This view would contradict, say, SAMUELSON's [1952, p. 61] that 'a careful review of all the literature since the 1870's would show that a significant part of all truths since arrived at have in fact been the product of theorists who used symbolic techniques'. The claim was noted in the previous section that symbolic manipulation is not necessarily the path to discovery about the real world.

An example of this seventh criticism is MCCLOSKEY [1986, pp. 87-112] who dissected the structure and content of MUTH's seminal [1961] article. MCCLOSKEY showed that MUTH's case could equally well have been presented in the vernacular without suffering loss of meaning. Linguistic analysis is growing in economics (e.g., SAMUELS, ed., [1990]) where attempts are made to isolate the modes of persuasion and rhetorical devices used by economists in their writing. Among these modes, mathematics enjoys high prestige currently. Like so many of the other points of criticism, this seventh may also have long-standing roots.

Dados os seis problemas anteriores, a matemática econômica descobriu pouco sobre o mundo real, e certa-

A possibilidade (que a mat. oferece) de aumentar dimensões à análise se incorre rapidamente na lei de retornos decrescentes.

Germs of it are found in BOULDING [1948] who raised the question, 'how important, for economics, is the gain in generality which comes from extension of the analysis to an indefinite number of dimensions' beyond the two of geometric explication? BOULDING's opinion was that 'the adding of dimensions to our analysis runs into a law of diminishing returns ... n-dimensional analyses ... add much to the aesthetics of economics but surprisingly little to its substance' [p. 192]. We may well accept the gains that mathematization has brought but still wonder if the prominence increasingly given to the search for a state of the art mathematical solution has not begun to displace the need for understanding of the economic process itself.

This seventh criticism can be extended. If 'mathematical economics, by and large, does live and operate in a world of its own, with little or no contact with the real world of economic events' [BLATT, 1983, p. 184], the claim can be made that it has little practical application. So, if an instrumental view is taken of economics, that it should constitute 'useful' knowledge which, say, the business world or government can actually apply, mathematical economics falls down on this score. Or, looking at it from the point of view of how business uses economic knowledge, no great emphasis is put there on mathematical modelling as against verbal modelling. Certainly, there will be elements of business and government that do engage in mathematical modelling — and some applications of maths to economics may have practical value like input-output analysis and linear programming — but this is only the tip of the iceberg of a far greater amount of less-mathematical economic modelling. *If* this is so, it raises the further question of how the emphasis that is put on mathematics in economics courses can be justified in terms of relevance to the community, or to sections of it, such as the business world. The counter to this argument is that mathematical modelling can reveal relationships between economic agents that verbal modelling cannot. However, this takes us back into all the earlier discussion of this paper about whether this is so.

This general criticism is often carried further to speculations about *why* mathematics enjoys such high prestige in economics today, unlike in any other social science. The pro-view might be that it shows the scientific standing of economics, that it underlies the practice of economics as science, deserving all of the legitimacy that is accorded to the physical sciences which rely heavily on mathematics; that the extensive use of mathematics is an indication of the rigour of economics which has developed many empirically testable theories that can be used for predictive purposes. Critics, of course, contest this picture of orthodox economics (e.g., KATOUZIAN [1980], perhaps even BLAUG [1980], ROSENBERG [1983], EICHNER, ed. [1983], WILES and ROUTH, ed. [1984], McCLOSKEY [1986], VAN MEERHAEGHE [1986], HODGSON [1988], MIROWSKI

A matemática serve para dar estatus científico à teoria econom...

Crítica: a matemática é usada para esconder o grão pouco produzir de predições reprodutíveis e empiricamente confirmadas.

[1989]), to which could be added many papers by Post-Keynesians, Behaviouralists, Institutionalists, Austrians and Marxists. In their view, little of the adulation for orthodox economics as science, with its reliance on mathematics, is deserved. This paper is not an appropriate vehicle for evaluating this continuing and unresolved debate.

Comments from such critics about the supposed intentional and unintentional effects of mathematics in economics, the alleged 'other purposes' it serves, are legion. There is the allegation that mathematization has prospered to hide the fact that economics has produced few replicable and empirically confirmed predictions, that mathematics gives the impression of science but it is only form without empirical substance (e.g., EICHNER [1983]). So, mathematics in economics has grown because 'the more abstract and arithmomorphic the object the more authority it commands' [PINCH, 1977, p. 207]. This initially mesmerised the uninitiated, such as industry, into believing that economics was a science that could provide answers to policy questions. But the irony is that as mathematization within economics accelerates, economics increasingly finds itself in 'splendid isolation' [LEONTIEF, 1982, p. 107], not only from the rest of the social sciences (e.g., HIRSCH, MICHAELS and FRIEDMAN [1987]), but from policy relevance and therefore the community which, allegedly, is demanding its services decreasingly. For instance, TOWSE and BLAUG's 1987-1990 survey of the British economics profession claimed that 'the complaint that economics degrees are too theoretical, too impractical and too unrelated to the possible uses of economics in business and government was a constant note in just about all the interviews' they conducted with employers of professional economists; 'industry no longer wants economists' [1990, pp. 230, 231]. Such complaints lead to periodic pleas by academic economists in the popular media to drastically redesign economics degree courses (e.g., SIMPSON [1989]).

retórica

A plausible story can be told about why there might be a decline in demand for economists by industry. Decision makers in industry face a problem space as if it were an 'organic' entity consisting of a myriad of complete interactions and patterns. For each individual decision maker, this will have been built up from practical experience, and will consist of a range of procedures and heuristics, some ad hoc, some implicit, some explicit, some mathematical, some not. The problem space resembles a living organism into which economists attempt to graft the grossly simplified but hopefully helpful 'metallic' structure of a mathematical model. But for the graft to take, each component of the organic entity has to be related carefully to the intruding model. Without this careful surgery, without a detailed explanation of its connections to the world of the politician, the sociologist, etc., the model will simply be rejected by decision makers. The almost 'take-it-or-leave-it' attitude of mathematical

 A teoria econômica torna-se cada vez mais

em "esplêndida isolação". Irrelevante ...

A teoria económica pode estar seguindo o caminho da pesquisa operacional.

economic modelling may well explain the decreasing demand for economists' services.

It is possible that economics today is following the same path as Operations Research (OR) did in the 1960s and 70s, leading to its heavily reduced status in industry. OR started life as a discipline for creative minds seeking original solutions to real world problems. But within twenty to thirty years, the search for mathematical rigour and the solution to mathematical optimisation problems was allowed to become an end in itself. Today, there is little demand for its services to help solve the big problems of government and industry. It persists there among isolated specialists not involved with the strategic decisions of the firm but, more typically, reporting to a 'production manager' whose problems are conceptually simple and for which a mathematically obtained optimum can reduce costs.

Through mathematization, continues the main criticism of this section, economics has become insulated from criticism by the non-mathematical — a maths degree now seems to be a pre-requisite to participate in its evaluation. Even though it was noted early that higher mathematics in economics can act as 'a serious bar to understanding' [BOULDING, 1948, p. 199], the critics go further and maintain it has 'a certain ritual efficacy over and above its content', like saying Mass in Latin rather than English [MIROWSKI, 1986]. Reification and mystification of the subject are consolidated. This serves also to increase the complexity of socialisation into the economics profession, for 'the more arithmetised and restricted a science, the more extended is its initiation programme and the more it seeks to distance itself from everyday concerns and beliefs by emphasising professional training and arithmetic manipulations'. The process works in the opposite direction simultaneously for 'the more a science ... is concerned about professionalisation the more [it] will emphasise arithmetic reasoning and extended periods of training in mathematical techniques' [WHITLEY, 1977, p. 164]. Thus, the drive to mathematization is another example of 'rent seeking' by a profession, only in the case of economics, what might impress other economists means little to those who pay the rent.

Within the economics profession so created, a hierarchy of prestige is built up, with status depending on facility in formalist abstraction [LEONTIEF, 1971, p. 3]. More publications per unit time can be constructed via this approach than for time-consuming empirical or applied economics [EARL, 1983]. Given a weak basis for comparing publications between economists, facility in mathematics becomes an easy rule-of-thumb screening device for entry into and promotion within the profession. Effective social control is thereby maintained within it. These 'other purposes' that mathematical economics is supposed to

A matematização é uma forma de rent-seeking

1. subtítulo: pesquisa empírica. Ao contrário do cientista das ciências físicas, o economista levanta poucos dados

serve help explain why mathematics has become dominant in economics. Even if mathematical economics is to be questioned as the most appropriate path to truth, there may be further reasons for the trend to mathematics.

1. First, the attitude to empirical data collection in economics is almost the opposite from that in the physical sciences. The bulk of the work of a physical scientist is spent in finding data or creating it by building an experimental apparatus. The value of many published papers in the physical sciences is not in the theoretical proposition but in the data itself which may be used by other researchers. In economics, the gathering of data on the real world is an arduous task which, as things stand, would force an economist to spend a large part of his/her working life in a firm or government department. WILLIAMSON [1989] is encouraged by the way in which researchers in 'transaction cost economics' have started to collect their own data and comments on '.. the degree to which a subject becomes a science when it begins to develop its own data ...' [p. 174]. But given the way the academic economics industry has long been organized, few economists have the desire, experience or time for this practical activity — there are no 'brownie' points for it in academia. Therefore, research carried out in academia restricts itself to a type for which no data is needed (applied mathematics) or uses data from the government's statistical service (econometrics). When data *is* gathered, it is 'of course' not made freely available, since the collector's career advancement depends on her maximizing publications from the data.

2. Second, another barrier forestalls real world involvement. The first step in model building should be to build a verbal model that is comprehensive but nevertheless an abstraction from the real world. This would again involve the economist seeking first-hand experience of the world; for example, how do business people make decisions, how do they relate the short and long term, how do they allow for risk, how did they react when the exchange rate fell etc., etc. Again, this behavioural approach is foreign to the desires and experience of the academic economist. Instead, stylized or arbitrary assumptions are made about behaviour which serve as the basis for modelling.

Finally, mathematical rigour is probably easier to teach and examine than verbal rigour. Far too often, verbal reasoning by students can indeed become either opinionated or 'fat, sloppy and vague'. For the academic, the mathematization of economics is no small gift and, struggle though the economist may, the siren song may prove too strong.

2. Subtilizar a compreensão verbal do problema, com a construção, primeiro, de um modelo verbal, pelo uso de pressupostos estilizados arbitrários

3. Mais fácil de ensinar e fazer exames.

... críticas ao uso abusivo da matemática a esse respeito: ou (1) a possibilidade da teoria econômica ser uma ciência precisa; e/ou (2) contra a sua formulação matemática.

Na Economia, ao contrário da Física, o modelo verbal está longe de estar completo.

III. CONCLUSION

The anti-mathematicians have a lengthy, unfinished and continuing case against mathematical economics. Some of their criticisms could be directed as appropriately against economics *per se* as against its mathematical formulation. For example, if reliable statements cannot be made about economic relations, neither mathematical nor any other form of economic analysis will produce empirically reliable conclusions [DUESENBERRY, 1954, p. 362]. A sceptical view is that 'by the standards of accuracy applied to predictions in the natural sciences, economics makes a poor showing' [BLAUG, 1985, p. 703]. Perhaps contemporary theoretical economics is no more than sixteenth century medicine wrapped up in twentieth century mathematical clothes. One might argue, perhaps in contemporary physics (?), that the understanding of the real world is such that the 'verbal model' is now complete and that the only knowledge left to be gleaned is that which can be derived from a painstaking mathematical analysis. In economics, the verbal model is far from complete, and our concern is that by concentrating on the mathematization of what we know, we may fail to give enough attention to discovering what we do not know.

Unfortunately, many of the planks of the anti-mathematical case have not been debated by the mathematicians. Perhaps the anti case is an eccentric backwater perpetuated by economists unwilling and unable to come to grips with higher mathematics. On the face of it, this explanation might be plausible because the anti case is typically put by those who are not expert in mathematics. Yet, at least four mathematical competents (GEORGESCU-ROEGEN, LEONTIEF, MORISHIMA and MIROWSKI) have been in the forefront of the anti case. Nor is it reasonable to suppose that all the 'antis' are 'second-rate' economists (however that might be measured). At least six Nobel Prize economists have questioned aspects of mathematical economics (MYRDAL, HAYEK, STIGLER, LEONTIEF, SIMON and SOLOW). Perhaps there is more to the anti case than a disinclination to learn new techniques.

Even if we cannot arrive at an 'objective' answer to the question, 'is the present domination of economics by mathematics justified?', there seem enough grounds in the anti case to preserve *both* the literary and the mathematical approaches to economic analysis. The conscious maintenance of a policy of methodological pluralism in all its aspects — not just mathematics and non-mathematics — would appear to be desirable (e.g., CALDWELL [1989]), what MACHLUP [1952, p. 70] called 'polylinguistic scholarship'. Methodological diversity is necessary in all fields of knowledge to preserve the richness from which enduring discovery may emerge.

It seems impossible to give a determinate answer to any of the seven issues considered in this paper. If this is so, ambiguity must remain about any mode of expression of ideas, mathematics included. Certainly, different modes may have different strengths and weaknesses, but this stops a long way short of claiming that one mode is universally superior to others and should dominate a field of knowledge.

Perhaps we can proceed no further than suggesting that an economist's personal preferences are likely to mould his/her choice of technique. In the case of mathematics versus less-mathematics as technique, one's preference may depend on how one sees economics and the world. One extreme could be, rely on maths if one believes that social processes can be represented precisely and rigorously, leading to the formation 'of principles which will be permanently valid: an economic science'. If one does not believe that social processes possess this form, perhaps greater reliance will be placed on literary devices as technique, employing 'reason and appeals to logic' but as 'a user of language at its full compass, where words are fingers touching the keyboard of a hearer's mind' [SHACKLE, 1983, p. 116].

APPENDIX I

Academic economics journals (and starting dates since 1960) in which mathematical economics is not dominant

Journal of Economic Literature (formerly Journal of Economic Abstracts)	1963
Journal of Economic Issues	1967
History of Political Economy	1968
Review of Radical Political Economics	1969
Journal of Behavioral Economics	1972
International Journal of Social Economics	1974
Cambridge Journal of Economics	1977
Journal of Post Keynesian Economics	1978
Journal of Economic Behaviour and Organization	1980
Journal of Economic Psychology	1981
Contributions to Political Economy (annual)	1982
Research in the History of Economic Thought and Methodology (annual)	1983
Economics and Philosophy	1985
Journal of Economic Surveys	1987
Journal of Economic Perspectives	1987
Journal of Interdisciplinary Economics	1988
Review of Political Economy	1989
Journal of Evolutionary Economics	1990

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CLIVE BEED AND OWEN KANE

SUMMARY

Over the last seventy years, a range of criticisms has been levelled at the mathematization of economics. While *particular* criticisms have been pursued extensively, for example, that the mathematics of classical physics provides an insecure base for economics, few general overviews of the critique of mathematical economics have been made. This is the focus here where seven objections to the mathematization of economics are identified. They range from the claim that the axioms of mathematical economics do not correspond to real world behaviour to the assertion that mathematics has often been an unnecessary adornment to economic discovery about the real world but is promoted because it serves unstated social and political purposes. While no determinate answers can be provided to each of the seven criticisms raised, it does appear that the case *for* the mathematization of economics is not without its limitations.

ZUSAMMENFASSUNG

In den letzten siebzig Jahren sind gegen die Mathematisierung der Ökonomie eine Vielzahl von Kritikpunkten angeführt worden. Während einzelnen dieser Punkten umfassend nachgegangen wurde — beispielsweise der Aussage, dass die Mathematik der klassischen Physik eine unsichere Basis für die Ökonomie bilde — sind wenige generelle Übersichtsartikel über die Kritik an der mathematischen Ökonomie geschrieben worden. Letzteres ist der Gegenstand des vorliegenden Artikels, in welchem sieben Einwände gegen die Mathematisierung der Ökonomie untersucht werden. Diese reichen von der Behauptung, dass die Axiome der mathematischen Ökonomie nicht mit der realen Welt übereinstimmten, bis hin zu der Behauptung, dass die Mathematik häufig eine unnötige Verzierung zur ökonomischen Erklärung der realen Welt sei, jedoch gefördert werde, da sie sozialen und politischen Zwecken diene. Obwohl die sieben Kritikpunkte nicht eindeutig bestätigt werden können, so scheint es doch, dass die Argumente für eine Mathematisierung der Ökonomie ihre Grenzen haben.

RÉSUMÉ

Depuis plus de 70 ans de nombreuses critiques se sont élevées contre la mathématisation croissante des sciences économiques. Des critiques particulières — comme le fait que les mathématiques de la physique classique ne forment pas une base solide à la science économique — ont été bien souvent exprimées.

Mais jusqu'à présent on n'avait vu que très peu de critique globale à la mathématisation de la science économique. Cet article se concentre donc sur l'identification des sept principales objections à cette mathématisation. Parmi ces objections, on trouve l'affirmation selon laquelle les axiomes des sciences économiques mathématisées ne correspondent pas au comportement du monde réel. Mais on trouve aussi l'affirmation selon laquelle les mathématiques ont souvent constitué un ornement inutile de l'explication économique du monde réel, leur utilisation ayant servie des objectifs politiques et sociaux occultes. Alors qu'on ne peut apporter de réponse spécifique à chacune de ces sept critiques, il apparaît bien que la cause de la mathématisation de la science économique n'est pas toujours évidente à plaider.