THE OVERUSE OF MATHEMATICS IN ECONOMICS: NOBEL RESISTANCE

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INTRODUCTION

In 1963 Leonid Hurwicz could confidently claim that "Mathematical economics has 'arrived' " and had become a "growth industry." Not only did Hurwicz use the usual sociological criteria — periodicals, courses, enrollments and so on — to make his case, but he also went on to provide some examples which met the challenge J. E. Cairnes had handed down in 1875 — the production of new economic truth by mathematical economics. [Hurwicz, 1963, 1] In the last 30 years the advance of this growth industry has been so phenomenal that it sometimes seems to have taken over the profession. From a position in which mathematics added insight, we have moved to one in which it is now the primary vehicle for conveying knowledge; from being one of the several pillars of modern economics, some now regard it as *the* foundation of economics.

Success has bred its own problems. The dean of modern general equilibrium theory and a Nobel laureate, Gerard Debreu, wondered in an address to the American Economic Association whether the profession had not become polarized.

> Our profession may take pride in its exceptional intellectual diversity.... Yet that diversity is strained by the increasing impenetrability to the overwhelming majority of our Association of the work done by its most mathematical members. [1991, 6]

Yet another Nobel laureate, Maurice Allais, had much sharper words on the current state of the profession.

> For almost fifty years contemporary economic literature had developed too often in a totally erroneous direction with the construction of completely artificial mathematical models detached from reality; and too often it is dominated more and more by a mathematical formalism which fundamentally represents an immense regression. [1989, 13]

Is it not odd that so caustic an assessment comes from the highest rank of mathematical economics?

Mathematical economics has not lacked for critics. From the early eighteenth century onwards, scholars have cautioned against the use of mathematics and warned about the dangers of its misuse [Quddus and Rashid, 1990]. Leon Walras dismissed the criticism of those who did not understand mathematics and Paul Samuelson warned young economists that they had best be prepared for psychological problems if they ignored mathematics. Increasingly isolated and frustrated by the new mathematics, they would either stay away from economic theory or strike back by criticizing the new methodology.

> ... without mathematics you [younger economists] run grave psychological risks. As you grow older, you are sure to resent the method increasingly. Either you will get an inferiority complex and retire from the field of theory or you will get an inferiority complex and become aggressive about your dislike of it. [Samuelson, 1952, 65]

One implication is that much of the criticism of mathematical applications — past and present — can be explained as an irrational reaction, because the critics did not understand the mathematics and felt personally threatened by its application to economics.

How *is* this new situation, in which mathematics is omnipresent, to be evaluated? One fruitful source of opinion consists of the views of those whose mathematical abilities are unquestioned. Authority alone can never settle a question, but it can provide us with food for thought. Furthermore, by focusing on their views we avoid Samuelson's irrelevant but painful barbs.

It is our claim that the worries expressed by Debreu and the fury of Allais are not isolated, idiosyncratic examples. Nor are they peculiarly French. Here are two examples from Nobel laureate British economists; first, Sir John Hicks, in an interview with Arjo Klamer,

> I do feel that most of this stuff that I pick up and see in the journals seems to have very little relevance to the sort of practical problems that really bother people.... I mean what have these mathematical theories got to say about whether Britain should go into the EMS? Nothing! That is the sort of question about which economists should have something to say.... A lot of these mathematical models, including some of my own, are really terribly much in the air. They lost their feet off the ground. [Hicks, in Klamer, 1991, 180]

and secondly, Sir Richard Stone, in an interview with Hashem Peasaran,

> Spinning theories is good fun, especially when they are expressed mathematically; testing them quantitatively is a swot but is the only way of finding out whether they have any validity. I know that theorizing is considered a nobler pursuit than number crunching and is therefore held up as the highest achievement to all who aspire to fame. As a consequence, thousands of theoretical papers are published every year. I doubt whether thousands of worthwhile theories are produced every year. [Stone, in Peasaran, 1991, 112]

We hope to convince the reader that there is widespread concern in some of the highest echelons of mathematically competent practitioners of economics regarding the current dominance of mathematics. The perception of the profession is distorted by several facts. First, several early criticisms — such as the inability to quantify economic variables or the inappropriateness of marginal methods — have in fact been successfully answered by mathematical economists. However, the question whether *some* mathematics is useful has now turned around to whether *only* that which is mathematical is useful. The very sociology of the profession has changed. This is made clear by the second fact important to this paper. Outright criticisms are both rare and published in relatively inaccessible outlets. (Note that both the Hicks and Stone quotes derive from interviews.) As a result, a critical mass of opposition never develops or succeeds in gaining publicity. In order to demonstrate our thesis, it is essential for us to quote directly from the economists in question. To us quotes are what data is to a statistical paper. The reader's indulgence is requested because we cannot make our case without frequent quotes. The points themselves have been raised on many occasions. The surprise is that they are still being voiced by people who should know, and the puzzle is why they have had so minimal an impact on the profession.[1]

CRITICISMS OF MATHEMATICAL METHOD IN ECONOMICS

The introduction of mathematical techniques in economics received a great boost in the waning decades of the nineteenth century at the hands of those economists engaged in the "Marginal Revolution." This group of economists — Walras, Jevons, Edgeworth, Pigou, Fisher — deliberately introduced mathematics into economic analysis. According to Philip Mirowski [1989], they did so to gain respectability by attempting to elevate the stature of economics to that of physics. By borrowing the metaphors and the methods of physics, they hoped to create a "science."[2]

Whatever the truth of this charge of borrowing from physics may be on historical grounds, it does not diminish the value of mathematics to economics; one cannot use its origins to condemn an idea. When told that many economic quantities could not be measured mathematicians could cheerfully retort that the same had been said for some physical entities, such as heat [von Neumann and Morgenstern, 1944, 3]; when told that mathematics required numbers, mathematicians, quite rightfully, provided a wide array of theorems that needed no numbers [Kemeny, 1961]; when told that

their methods assumed far too much continuity, mathematicians took this as a challenge to develop newer (and often harder!) mathematics [Aumann, 1964]. Indeed, much of the historical criticism of mathematics in economics is vitiated by the fact that the critics aimed to determine the issue *a priori*. This left them open to the sensible retort that whether or not mathematics was useful was best decided by trying the issue.

Misleading Quantification

Some of the earliest critics of mathematical methods opposed these methods on grounds that economic variables are inherently qualitative and, therefore, not measurable. Political economy deals in concepts that often cannot be quantified: utility, desire, want, welfare, preference. Attempts to quantify these essentially subjective and qualitative variables are suspect. J. B. Say, an influential nineteenth century French economist, opposed the use of mathematical and statistical tools in economics on such grounds [Menard, 1980].

Modern critics take a somewhat more sophisticated view. While they do not deny that quantification has merit, they point to its real or perceived *costs*. Kenneth Boulding, for example, argued that quantification leads to something that might be described as "reducingism," that is reducing complex structures and sets to single numbers, a procedure that can seriously mislead us when dealing with human welfare [1985, 14]. Thus to quantify in economics is necessarily to oversimplify. This theme reverberates in the writings of Nicholas Georgescu-Roegen. He writes about having *proved* some negative results, a very different thing from simple criticism:

> All these painstaking endeavors [to measure utility] should be viewed with pride because science should leave no stone unturned. However, through these endeavors we gradually came to realize that measurability, whether cardinal or ordinal, requires very stringent conditions... *I was able to show* ... that neither wants nor expectations fulfill the conditions of measurability. [Georgescu-Roegen, 1966, 119-120, emphasis added]

In his review of Samuelson's *Foundations* for the *Journal of Political Economy*, Boulding posed the question: "What makes a subject "mathematical?" His answer was that economic variables tend to be *internally heterogeneous*, thereby making it difficult to capture them with numerical averages, since the significance of any part is determined only by its relation to the whole [1948, 188]. Some tend to dismiss Boulding as mathematically naive but he is only elaborating upon the arguments of Henri Poincare, one of the greatest of modern mathematicians. [Poincare, 1905] In the same vein, Georgescu-Roegen emphasizes the role of cultural and institutional elements in economics, which cannot be captured by mathematical models. He states,

> ... the difficulty of the subject of economics does not lie in the mathematics it needs, but in the fact that the subject itself is "much too involved to be fully accessible to mathematics." And what makes this subject (economics) not fully amenable to mathematics is the role that cultural propensities play in the economic process. [1966, 124]

Thus the difficulty lies in the very nature of economics — multidimensional, moral, philosophical, metaphysical, rich in institutional and cultural details — that prevents widespread quantification. Mathematical modeling, no matter how sophisticated, is thus incapable of capturing the complexity and richness of the real-world economy. As a result, quantification is often a false prophet, promising much more than it can ever deliver. This criticism is especially applicable to the modern econometric model-building and forecasting industry, whose record in forecasting is widely perceived to be woefully inadequate.

The first step in useful econometric analysis is the acquisition of data. Arnold Zellner notes that much economic data is not only of poor quality, but that economists show little interest in improving this situation. With such poor raw material it is no surprise that large-scale econometric models lacked predictive power. The surprise is that one reaction, in the form of vector autoregression, has been to add a lot more mathematics and a lot less theory! Zellner writes of vector autoregressive methods:

> To throw out all economic knowledge and rely on complicated multivariate statistical models that even time series statisticians believe are inadequate is an extreme proposal. And indeed this approach has yielded little in the way of dependable economic understanding of the behavior of national economies. [1987, 6]

We end this section with some harsh words from Allais. Concerning the abuse of statistical tools in modern economic literature, he writes,

> But the abusive use of mathematics is unfortunately not the only failing in contemporary literature, which too often has generated a crop of pseudo-theories based on the mechanical application, devoid of any real intelligence, of econometrics and statistical techniques. All these theories have the same characteristics — the elaboration of models of linear correlation which are in reality only *pseudo models*, accompanied by a mathematical-statistical panoply of *untamed*, totally unjustifiable econometrics which seem to the naive to be scientific theories whereas they are generally just empty shells; — a *blind and brutal* application of linear correlation programs and tests associated with them, although these *tests generally are not applicable to the cases studied*; — and the use of models, too often applied to a single country for a short period, where the number of explanatory

> variables and the number of arbitrary parameters *are such that the fittings can have no real meaning*. [1989, 13-14; emphasis in original]

Seductive Precision

It is the privilege of genius to argue on both sides of an issue. Shortly after telling the younger economists that they risk an inferiority complex by ignoring mathematics, Samuelson goes on to tell them that "The danger is almost greater that you will overrate the power of the method for good or evil" [Samuelson, 1952, 65]. George Stigler alluded to just this danger in the course of a lecture given at the London School of Economics in 1949.

> Because the mathematical method is so powerful and beautiful and its possession still sufficiently rare to command distinction, the mathematical economist is under constant temptation to use it just for the sake of using it. [1949, 43]

Many critics, including Georgescu-Roegen and Stigler, have expressed concern that mathematics will come to dominate our subject. Many of these critics accept that mathematics has a useful role, but maintain that it must be to serve economic analysis, not rule over it. This concern is credible because of the nature of mathematics. For ages, mathematicians have been criticized for pursuing abstractions for their own sake. This propensity creates the danger of delinking the researcher from the real world and can result in empty research paradigms and propositions. According to Stigler, even great economists who knew mathematics have committed errors because they became preoccupied with the form and neglected the substance. Stigler considers Marshall's utility theory and Keynes's multiplier as two examples of logical errors that resulted precisely because the mathematics overwhelmed the economic content. This criticism is particularly potent because Keynes himself was one of the earliest critics of the abuse of mathematics in economics. He wrote in the *General Theory,*

> ... symbolic pseudo-mathematical methods of formalizing a system of economic analysis... allow the author to lose sight of the complexities and interdependencies of the real world in a maze of pretentious and unhelpful symbols. [1936, 297-98]

Stigler noted a deficiency in the mathematical approach to lie in its exclusion from view of those features of a problem that cannot be made mathematical. While Stigler was a noted theorist, he was hardly a mathematical economist. Yet exactly the second half of Stigler's claim is made by Maurice Allais, who sums up the present state of affairs as follows:

> Indeed a large part of contemporary theoretical literature has progressively come under the control of pure mathematicians who are

> more concerned with mathematical theorems than with analysis of the real world. A new scholastic totalitarianism has arisen based on abstract and apriorist conceptions, detached from reality. [1989, 13]

Grubel and Boland [1986] in their recent survey of the profession found that the majority of respondents felt that mathematics has a *stranglehold* over academic economics — something they consider less than desirable.

Those who support the application of mathematical techniques in economics have often justified it on grounds that mathematics is but a language. Furthermore, it has additional merits as the language of logic and of all sciences. To translate sentences into equations clarifies the logic of the argument, removes ambiguities, and achieves conciseness.

The modern variant of this claim is that mathematical analysis is superior to literary analysis because it forces errors to the surface. Poisson is credited with the quote: "Mathematics has no symbols for confused ideas." The practice of the profession appears to have accepted the substance of Poisson's claim. From its earlier status as one of the conveyors of economic insight, mathematics is now seen as a *sine qua non* for expressing economic truth [Grubel and Boland, 1986]. But if so, then we must be aware of the fact that mathematics has its own biases.

While mathematics may be considered a language with some strengths, it also suffers from several deficiencies which may make it unsuitable as a medium for economic analysis. As far back as 1948, Boulding wrote: "I know of no mathematical expression for the literary expression 'I love you' " [1948, 188-9]. Boulding returns to this theme in a more recent paper:

> This [mathematics] is a language — or perhaps we should say a jargon — with an extraordinary paucity of verbs — it is hard to think of more than four: equals, is greater than, is less than, and is a fraction of [1989, 14].

Mathematics can be purely theoretical or applied. The history of its development is rich in the story of this tension between the development of theory and emphasis on application. Economics, on the other hand, is largely an applied, policy-oriented discipline. Some writers believe that mathematical analysis has a strong pro-theory bias.[3] The use of mathematics in economics imports this into economic research.

The pro-theory bias manifests itself in the choice of what is analyzed. Cournot's statement that he only dealt with problems which could be dealt with mathematically was at least an explicit acknowledgement of the limitations of his chosen approach. With the adoption of mathematics as the most respectable language of economic science, we have silently adopted new biases. Gerard Debreu expresses this concern very clearly:

> Essential to an attempt at a fuller explanation are the values imprinted on an economist by his study of mathematics. When a theorist who has been so typed judges his scholarly work, those

> values do not play a silent role; they may play a decisive role. The very choice of questions to which he tries to find answers is influenced by his mathematical background. Thus, the danger is ever present that part of economics will become secondary, if not marginal, in that judgment. [1991, 5]

It is curious that this transformation has come about despite perceptive and furious criticism from several mathematically knowledgeable and eminent economists, of whom Wassily Leontief, an early Nobel laureate, the founder of modern input-output analysis, is perhaps the most prominent.

> Not having been subjected from the outset to the harsh discipline of systematic fact-finding, traditionally imposed on and accepted by their colleagues in the natural and historical sciences, economists developed a nearly irresistible predilection for deductive reasoning. As a matter of fact, many entered the field after specializing in pure or applied mathematics. Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions. [1982, 104]

> Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same set of data without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system. [ibid., 107]

Impossible Demands

This helplessness of economists to achieve systematic advance in understanding becomes evident when we see how another Nobel laureate, Robert Solow, treats the confusion visible in macroeconomics, the one subject where the public seeks our guidance most.

> Why, then, is macroeconomics in disarray? "Disarray" is an understatement. Thoughtful people in other university departments look on with wonder. Professional disagreements exist in their fields too — at the frontier there is always disagreement — but as outsiders they are shocked at the way alternative schools of thought in macroeconomics describe each other as wrong from the ground up. They wonder what kind of subject economics is. [1983, 279-80]

Are we not clever enough or are we demanding too much from the subject?

Sometimes there is an undercurrent of doubt which suggests that progress may not be possible. Franklin Fisher is one of the leading economic theorists in industrial organization. In reviewing the current state of the field he noted how enthusiastically game theorists had adopted industrial organization and that oligopoly theory had become "totally dominated" by the game-theoretic approach. Nonetheless, after a compact survey of the history and current status of game-theoretic applications, Fisher claims that no substantive progress has been made. The difficulty lies in the fact that the theoretically challenging and tractable problems are not those which real corporations worry about.

> There is a strong tendency for even the best practitioners to concentrate on the analytically interesting questions rather than on the ones that really matter to real-life industries. The result is often a perfectly fascinating piece of analysis. But so long as that tendency continues, these analyses will remain merely games economists play. [Fisher, 1989, 123]

If we turn from game theory to econometrics the situation is not much improved. Trygve Haavelmo (echoing very early doubts by Ragnar Frisch) stated in his presidential address to the Econometric Society that he was disturbed and discouraged by the seeming regression in our comprehension.

> The concrete results of our efforts at quantitative measurements often seem to get worse the more refinement of tools and logical stringency we call into play. [Haavelmo, 1958, 354]

Haavelmo feels that this nightmare for quantitative economists can be partially removed by more imaginative model building and econometrics, but he does not avoid the disconcerting conclusion that these perverse facts may have forced us to recognize a plain fact, "that the laws of economics are not very accurate in the sense of a close fit, and that *we have been living in a dream-world* of large but somewhat superficial or spurious correlations" [1958, 354-5].

(The knowledgeable reader will of course know that both Haavelmo and Frisch are Nobel laureates.) While the dominant figures applying mathematics appear to be keenly conscious of the limitations of our claims, the majority of the economics profession appear far too trusting of the general capabilities of mathematics [Crowe, 1988].

Deadening Intuition

An oft-repeated theme in the literature critical of mathematical economics is that while mathematical methods have benefits for economics, with these benefits come certain sacrifices. We will be less than fair to ourselves and to society at large if we fail to practice what we often preach — that is to look at both sides of the equation in deciding what is the optimum level of mathematics economics can usefully absorb.

James Buchanan, who won the Nobel prize for his work on public choice theory, has argued that the use of higher mathematics leads to rapidly diminishing benefits. Buchanan does not deny that mathematics has a useful role to play in economic science; after all, geometry — the graphical approach — is a branch of mathematics, and it is an approach he supports. What he is concerned about is the seeming "infatuation" with ever-more advanced mathematics, not realizing that the addition to our understanding may be marginal and there is definitely a cost that the profession must pay. On the challenges faced by the discipline in the 1980's Buchanan wrote,

> I do deplore the waste that such (learning mathematics) investment of human capital reflects. The intellectual achievement comes at a major resource cost, and, as with any such commitment, the opportunity cost is measured in benefits that might be expected from the alternative that is sacrificed. In modern economics, that which is sacrificed is an understanding of the principles of market process and of the relationship of this process to the institutional setting within which persons choose. [1985, 15]

Thus the costs, which must be balanced against the benefits, include the understanding of *economics* that could have developed during the time spent in mastering mathematics, the possibility of policy mistakes from unjustified faith in complicated mathematical models, and the lost influence on the society because unnecessarily technical discourse shuts out the general public.

Some authors have distinguished between the use of mathematics in economic research and in the teaching of economics. In a famous passage, Alfred Marshall, the dominant figure of late nineteenth century Anglo-Saxon economics, firmly opposed using mathematics in the exposition of economic ideas. Marshall wrote to A. L. Bowley in 1906:

> I had a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics: and I went more and more on the rules — (1) Use mathematics as a shorthand language, rather than as an engine of inquiry. (2) Keep to them until you are done. (3) Translate into English. (4) Then illustrate by examples what are

> important in real life. (5) Burn the mathematics. (6) If you can't succeed in (4), burn (3). This last I did often. [Pigou, 1966, 427]

Other authors have also commented that the use of mathematics tends to be "elitist," as it often has the unhealthy effect of distancing the researcher from the society being studied. Carried to extreme, this undermines the ability of a discipline to influence social opinions and policies.

There has also been a growing literature on whether mathematics is at all necessary to teach basic economic theory and applications. It has been argued that the criterion of how much mathematics is appropriate for teaching undergraduate economics should be the level of mathematics necessary to understand popular readings in economics and business. Based on this criterion, very little mathematics is necessary for teaching economics at the introductory college level. A recently published report on the economics major by Siegfried, McCloskey and several other educators [1991], commissioned by the American Economic Association, concluded that the teaching of calculus is neither necessary nor sufficient to an understanding of economic theory at the introductory or intermediate level. The gasp of budding economists when faced by mathematics is well captured by the feelings of practical biologists when faced by mathematical biologists. "Like most mathematicians, he takes the hopeful biologist to the edge of a pond, points out that a good swim will help his work, and then pushes him in and leaves him to drown" [Kingsland, 1985, 129]. So the attempted mastery of a language has not only failed to advance our knowledge as expected, it has led us into questionable pedagogy and to avoiding those questions that cannot be asked or answered by this new language.

CONCLUDING REMARKS

This paper has documented the concerns of a significant number of mathematical economists and econometricians of the highest rank. Having fought for mathematical technique, they now feel overwhelmed by the absence of nonmathematical insights. On occasions a pregnant hint is dropped. The Russian Nobel laureate, Leonid Kantorovich, whose work lay in mathematical programming, notes the wide dissemination of mathematics and says "One cannot embrace the unembraceable" [Kantorovich, 1989, 18]. Surely a significant doubt for a Nobel memorial lecture! William Baumol, on the other hand, expresses the dilemma very clearly:[4]

> As one of those who worked with some determination to change this state of affairs and to introduce some grounding in mathematics as a standard part of postgraduate curriculum in several universities, it may be pardonable for me to suggest that things may have gone a bit far in the opposite direction.
>
> ...there are at least two grounds on which such a state of affairs is to be deplored — its preclusion of other promising lines of attack, and its

> consequences for those students whose talents are for approaches other than the mathematical. [1991, 2]

That mathematics is a wonderful language for some, even many, economic problems is undoubted. But it is significantly incomplete in framing economic questions and the actual record exposes the inadequacy of complete reliance on mathematical method. In the words of Sir Richard Stone, "Speaking as an economist, I suspect that in quite a few cases the failure of our models may be due to our disregard of noneconomic factors" [Stone, in Peasaran, 1991, 112]. Mathematical and econometric models have been unable to do justice to the complexity of the real world and the use of such models has raised false hopes. Within the profession, the bias of mathematics towards pure theory has influenced the way we ask and answer questions. If economics is to be applied and policy oriented, this bias does not serve us well. Furthermore, the approach filters down into the textbooks, encourages poor pedagogy and discourages the talent of those with direct intuition and insight into the real world. The most recent issue of the *American Economic Review* has a "Petition to Reform Graduate Education," signed by faculty from over 400 institutions that embodies such concerns [*American Economic Review,* 1993, ii-iii].

It is our opinion that there are nuggets of wisdom in the literature just examined. Perhaps it is true that left to itself the market for mathematical economics will tend to overproduce. Perhaps the market for mathematical economics suffers from an absence of competition. Criticism that economists have been able to overindulge themselves in academic research and writings which only they care about, because of an absence of market accountability, is pertinent here. These issues deserve the attention of the profession, since the cycle whereby initially inflated claims are moderated by practice is well-known to historians of science [Grabiner, 1988].

What is the proper role of mathematics in economics? There is no doubt that mathematics is useful in many ways. One must agree that mathematics can and does provide a pillar on which the structure of modern academic economics is built. However, in our race for technical virtuosity we often forget that this foundation can only be one of several pillars ("a" foundation, not "the" foundation) on which the structure of economics should be based.[5] Other equally important pillars include institutional knowledge of the economy, history, sociology, politics, and even art, literature and philosophy of the society. Today in many branches of economics the sensitivity and balance that is so important when dealing with a tool such as mathematics is lost.

What should be done to correct the situation? John von Neumann was not only one of the greatest mathematicians of the twentieth century but also one of the founders of modern mathematical economics. His comments on the proper inspiration for mathematics are worth repeating.

> As a mathematical discipline travels far from its empirical source, or still more, if it is a second or third generation only indirectly inspired by ideas coming from "reality," it is beset with very grave dangers. It

> becomes more and more purely aestheticizing, more and more purely *l'art pour l'art*. [Dore, *et al.*, 1989, i]

The following quote from a highly respected mathematical economist is pertinent, as it continues von Neumann's thoughts and suggests steps which would be part of any solution to this problem:

> ...the reason for present-day economics having lapsed into the wretched state of affairs we have noted above is the fact that so deep and extensive has been the mathematization of economics since 1940 that it has lost all sense of balance, becoming divorced from knowledge of economic systems and economic history. There is only one medicine which will cure this malaise, and that is for the theorists to make a serious effort in the direction of the institutionalization of economics, in the sense of slowing the speed of all development towards mathematization and developing economic theory in accordance with knowledge of economic organizations, industrial structure and economic history. [Morishima, 1984, 70]

John Maynard Keynes was not only a great economist but also a vigorous and perceptive participant in earlier debate on this issue. He accurately identified the problem of the balance necessary to produce worthy economists and fruitful economic analysis when he wrote that "[t]he master economist must possess a rare combination of gifts. He must reach a high standard in several different directions and must combine talent not often found together. He must be mathematician, historian, statesman, philosopher to some degree." [Keynes, 1936, 173-4] Unless the profession wakes up to the many pertinent concerns discussed in this paper, and actively seeks to reverse these trends, there is a real danger that economics will decline in respect and influence with the larger society.

NOTES

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1. Since we were originally moved to write this paper by the words of several Nobel laureates, we have used their presence in the title. However, the reader will see that we do use many others, some of whom may yet win the Nobel Prize!
2. An early and forceful critic of this historical development was Nicholas Georgescu-Roegen, an important contributor to modern mathematical economics and an early and persistent critic of how mathematical economics has evolved in the 20th century. In *Analytical Economics* [1966] he protested against "imposing a gospel from physics" because economists could have direct contact with the primary units of analysis such as wants, attitudes and expectations while physicists have no options but to treat only that which was externally observable.

3. Philip Morse was the most active promoter of Operations Research after World War II. Yet even he found his patience tried in bringing together mathematicians and their real-world counterparts. "Those mathematicians are much more worried about having the correct words for everything than they are in getting an actual solution to an actual problem." [Rider, 1992].
4. It is notable that another author with a similar background in quantitative economics, Thomas Mayer, has written a book on this issue [Mayer, 1993].
5. This section owes much to the general comments of Boulding [1985].

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