"Has Formalization in Economics Gone too Far?"

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The following five papers by Professors McCloskey, Katzer, Leamer, Caldwell, and Solow respectively were presented at the joint session of the International Network for Economic Method and the American Economic Association at the Annual Meeting of the latter in Washington, D.C., 28–30 December, 1990. The session was chaired by Professor Daniel R. Fusfeld.

– Editor

Economics Science: A Search Through the Hyperspace of Assumptions?

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It would of course be silly to object to the mere existence of mathematics in economics. No one wants to return to the time, not so distant, in which economists could not keep straight the difference between the movement of a curve and a movement along it.[1] Economics made progress without mathematics, but has made faster progress with it. Mathematics has brought transparency to many hundreds of economic arguments. The metaphor of the production function, the story of economic growth, the logic of competition, the facts of labor force participation would rapidly become muddled without mathematical expression. In fact muddled they once were. Most economists and I agree with Léon Walras, who wrote in 1900, "As for those economists who do not know any mathematics, who do not even know what is meant by mathematics and yet have taken the stand that mathematics cannot possibly serve to elucidate economic principles, let them go their way repeating that 'human liberty will *never* allow itself to be cast into equations' or that 'mathematics ignores frictions which are *everything* in social science" [p. 47].

But economists know that a qualitative argument for something does not automatically fix its optimal quantity. When America has market power in some exportable, and takes a selfish view, the economist can assert qualitatively that some tariff would improve on free trade. But an argument for the existence of an optimal tariff does not automatically tell how large the tariff should be, quantitatively speaking. Likewise, if some industries are monopolized, then forcing other industries to price exactly at marginal cost may be a bad idea, as a matter of qualitative, logical, on-off, what-might-possibly-happen truth. But the scientific question is quantitative. How far from competitive is the economy? What closeness to marginal cost would trigger the second best? How much marginal cost pricing can the economy stand?

In other words, economists do not need more existence theorems about the role of mathematics in economics – "there does not exist a mathematical economics that can take account of human liberty" or "there does not exist a rigorous economic argument unless in Bourbaki-style mathematics." To answer the quantitative question about the role of mathematical formalism in economics we need quantitative standards.

Comparison provides a quantitative standard. On several grounds, physics is a good standard for comparison. For one thing, economists share some human qualities with physicists. Economists like to think of themselves as the physicists of the social sciences, and they are. Like physicists they are political animals, in love with conferences and competition. They are hedgehogs, not foxes; they know one big thing ($F = ma$; $e = mc^2$; $P = MC$; $MV = PT$) not a large number of little things. They like to colonize other fields, the way biology was colonized after the War by physicists ashamed of making bombs. And economists are approximately as arrogant about the neighboring fields as physicists are.[2] The jokes that economists tell about sociologists and political scientists, without knowing anything about sociology or political science, are matched in physics by jokes about chemists and engineers. The chemist in the Manhattan Project who made the trigger for the bomb (a brilliant trick, without which no bomb) was praised by one of the physicists: "George, you're an absolutely first-rate chemist – which is to say, a good third-rate physicist!" Very funny.

For another, economists admire physicists and judge themselves, as do most people in our culture, to be intellectually inferior to the physicists. Like philology in the centuries of Scaliger, Erasmus, and Bentley, physics nowadays is at the top. Physicists have the most prestige among intellectual workers. The peculiarly English word "Science" (all other languages use the word to mean "systematic inquiry") has come to mean "a field of study close to what non-physicists imagine physics is like." The first-rate economists imagine themselves to be good third-rate physicists. Comparisons with sociology, then, would not be to the point, since economists, without knowing any sociologists, imagine sociologists to be inferior to economists. The standard of comparison should be the field we look up to rather than the one we look down upon.

Robert Solow has a good story on the matter. He was complaining at lunch one day to the Nobel laureate Victor Weisskopf about the dearth of really bright people in economics, "unlike physics," said Solow, in a bit of ill-advised humility. Weisskopf replied that such a situation did not sound like an equilibrium: if there were too few bright people in economics, then some of the marginally bright in physics could move over into economics and make their intellectual fortunes. So in equilibrium the marginal person would be equally bright. Solow was stunned and embarrassed. Here was a physicist inventing on the spot the economist's favorite argument and using it better than Solow himself to show that self-deprecation was not in order. The story says either that physicists *are* brighter than economists or, at another level, that they are not, and likewise for sociology. It's that way in the human sciences.

Most economists would accept physics as a standard for the use of mathematics. The empirical result of applying it is this: physics is less mathematical than modern economics.

The proposition sounds crazy. The average economist knows a lot less mathematics than the average physicist, as is apparent from the courses both take in college. Walk the aisles of the college bookstore and open some of the upper-division undergraduate books in physics (or in the much-despised civil engineering, for that matter). It makes the hair stand on end. Even the mathematically more sophisticated economists know less math than comparable physicists, if by "knowing math" one means "knowing about Bessel functions" or "knowing six ways to solve an ordinary differential equation" or even "knowing a lot about the theory of groups."

The proposition, however, does not say that economics *uses* more math; it says that economics is "more mathemaitcal." In the Economics Department the Spirit of the Math Department reigns. The spirit is different over in the Physics Department. The great theoretical physicist Richard Feynman, for example, introduced a few simple theorems in matrix algebra into his first-year class at Cal Tech with considerable embarrassment [1963, Vol. I, 22-1]: "What is mathematics doing in a physics lecture? ..... Mathematicians are mainly interested in how various mathematical facts are demonstrated ..... They are not so interested in the result of what they prove." Feynman's rhetorical question startles an economist. In most first-year graduate programs in economics it would be rather "What else but mathematics should be in an economics lecture?" In physics the familiar spirit is Archimedes the experimenter. But in economics, as in mathematics, it is theorem-proving Euclid who paces the halls.

Economists know little about how physics operates as a field, and the physicists are amazed at the math-department character of economics. The new Santa Fe Institute, which brings the two groups together for the betterment of economics, has made the cultural differences plain. In 1989 *Science* described the physical scientists there as "flabbergasted to discover how mathematically rigorous theoretical economists are. Physics is generally considered to be the most mathematical of all the sciences, but modern economics has it beat" [Pool, p. 701]. The physicists do not regard the mathematical rigor as something to be admired. To the seminar question asked by an economist, "where are your proofs?", the physicist replies, "You can whip up theorems, but I leave that to the mathematicians" [701]. A physicist at the Institute solved a problem with a computer simulation, approximately, while the economist found an analytic solution. Who is the more mathematical?

Economists think that science involves axiomatic proofs of theorems and then econometric tests of the QED, which therefore will test the axioms. In truth the physicists could care less about mathematical proofs. Even the theoreticians in physics spend much of their time reading the physical equivalent of agricultural economists or economic historians. Pencil-and-paper guys are uncommon in physics departments. Physics is finding driven. Economics is theorem driven. Ask your local physicist what he thinks about proofs. He'll say, "Well, I prefer to depend on an existence theorem about existence theorems: if the mathematicians tell me they exist, fine; I reckon they know. But it ain't physics."

The economists, to put it another way, have adopted the intellectual values of the Math Department – not the values of the Departments of Physics or Electrical Engineering or Biochemistry they admire from afar. The situation is odd on its face. Philip Anderson, the distinguished physicist who brought the Sante Fe Institute together, explained the differences with "the differences in the amount of data available to the two fields" [701]. But economists are drenched in data, as hard as they want them to be. Odd.

No one would make the absurd claim, of course, that axiom and proof have no place of honor in economic reasoning. They do, and should, though economists might be more sensitive to Alfred Marshall's remark long ago that "the function then of analysis and deduction in economics is not to forge a few long chains of reasoning, but to forge rightly many short chains and single connecting links" [1920, p. 773]. We had better know that assumption A leads to conclusion C, although it would be a poor economics that only knew this.

But at the heart of axiom and proof as practiced in economics is a rhetorical problem, a failure to ask how large is large. As our own William Brock put it in 1988:

> We remark, parenthetically, that when studying the natural science literature in this area it is important for the economics reader, especially the economic theorist brought up on the tradition of abstract general equilibrium theory, to realize that many natural scientists are not impressed by mathematical arguments showing the "anything can happen" in a system loosely disciplined by general axioms. Just showing existence of logical possibilities is not enough for such skeptics. The parameters of the system needed to get the erratic behavior must conform to parameter values established by empirical studies or the behavior must actually be documented in nature. (p. 2 of typescript).

The problem, to put it formally, is that economists have fallen in love with existence theorems, the beloved also of the Math Department. There *exists* a canopener, somewhere. The most famous of these theorems is of course Arrow-Debreu, though I intend the word "existence theorem" to apply to all the qualitative theorems with which economists wile away their hours between 8:00 and quitting time. Significantly, what are commonly regarded as the first formal proofs of the existence of a competitive equilibrium, advanced during the 1920s and 1930s, were devised by professional mathematicians, John von Neumann and Abraham Wald. (Equally significantly, one could plausibly claim that Edgeworth had already proven the theorem; for merely N = 2, of course, but so what?)

From everywhere outside of economics except the Department of Mathematics the proofs of existence of competitive equilibrium, just to take them as concrete examples, will seem strange. They do not claim to show that an actual existing economy is in equilibrium, or that the equilibrium of an existing economy is desirable. The blackboard problem thus solved derives more or less vaguely from Adam Smith's assertion that capitalism is self-regulating and good. But the proofs of existence do not prove or disprove Smith's assertion. They show that certain equations describing a certain blackboard economy have a solution, but they do not give the solution to the blackboard problem, much less to an extant economy. Indeed, the problem is framed in such general terms that no specific solution even to the toy economy on the blackboard could reasonably be expected. The general statement that people buy less of something when its price goes up cannot yield specific answers, such as $4598 billion.[3] The proofs state that somewhere in the mathematical universe there exists a solution. Lord knows what it is; we humans only know that it exists.

The usual way the quest for existence is justified is to say that, after all, we had better know that solutions exist before we go looking for them. Ask an economist why she's so interested in existence theorems and this is the answer you will get. Of course, the economist giving it does not then go out and look for parameterized and empirical solutions. But nobody's perfect. The answer anyway sounds reasonable: if you can't actually find it, you nonetheless know that what you're looking for exists. Judging again from physics, however, it is not reasonable. Physicists have happily used the Schrödinger equation since 1926 without knowing whether it has solutions in general. The three-body problem in Newtonian physics does not possess known solutions in general. Yet astronomers can tell you with sufficient accuracy for most of the questions they ask where the moon will be next year. For that matter, poets can write particular *terza rima* poems without knowing whether the form has in general a solution possessing optimal properties. Whether a solution under assumption A exists in general is irrelevant if the physical or economic or poetic question has to do with a particular finite case under assumption A' merely close to A.

The way the mathematical rhetoric has been transformed into economic rhetoric has been to *define* the economic problem as dealing with a certain kind of (easily manipulable) mathematics. One searches under the lamppost because the light there is good. The notions of "equilibrium" and "maximization" in economics have been treated in such a way [Weintraub 1991; Mirowski 1990]. Many economists have claimed that Adam Smith's question *is* the mathematical one of existence. The move is doubtful as intellectual history. Smith used the phrase "the invisible hand" only once (albeit in each of his books) and it is not until the coming of mathematical values in economics that the matter of existence was considered to be important.

But what is more unhappy is that a proof of existence leaves every concrete question unresolved, while enticing some of the best minds in the business into perfecting the proofs. With certain assumptions about preferences and technology one can write down equations which can be shown to have somewhere out there a solution (and sometimes, more to the point, even a stable solution, insensitive to trembling hands). Naturally the result, which is about the equations, not about the economy, depends on the assumptions. The task has been to vary the assumptions and see what happens. Unsurprisingly, under some assumptions the equilibrium does exist and under others it does not; under some assumptions the equilibrium is efficient and under others it is not.

Well, so what? Sometimes it rains and sometimes it does not. In some universes the moon is made of green cheese and in others it is not. None of the theorems and countertheorems of general equilibrium theory has been surprising in a qualitative sense. *But this is the only sense they have.* They are not quantitative theorems. They are mathematics without numbers, of great and proper interest inside the Department of Mathematics, but of little interest to quantitative intellectuals.

The problem is that the general theorem does not relate to anything an economist would actually want to know. We already know for example that if the world is not perfect the outcomes of the world cannot be expected to be perfect. This much we know by being adults. But economists arguing over the federal budget next year or the stability of capitalism forever want to know *how big* a particular badness or offsetting goodness will be. Will the distribution of income be radically changed by the abandonment of interest? Will free trade raise American national income? It is useless to be told that if there is not a complete market in every commodity down to and including chewing gum then there is no presumption that capitalism will work efficiently. Yet that is a typical piece of information from the mathematical front lines. It does not provide the economic scientist with a scale against which to judge the significance of the necessary deviations from completeness. Chewing gum or all investment goods: it does not matter for the proof.

Practical people, including most economists, understand Adam Smith's optimism about the economy as asserting something like this: economies that are approximately competitive are approximately efficient, if approximate externalities and approximate monopolies and approximate ignorance do not significantly intervene; and anyway they are aprpoximately progressive in a way that the static assertion does not pretend to deal with, even approximately. The claim has analogies to the theorems of general equilibrium theory (say: similar fuzzy but highly relevant claims are made in other parts of economics). But except on the knife edge of exact results, where a set of measure zero lives, the theorems are not rigorously relevant. If we are going to be rigorous we should be rigorous, not rigorous about the proof and extremely sloppy about its range of application.[4] The theorems are exact results, containing no definition of the neighborhood in which they are approximately correct.

The exact existence theorems may be worth having, though why exactly [redacted] needs to be argued more rigorously than it has been so far – a matter of rhetorical, not mathematical, rigor, but rigor all the same. Mathematical economics has not been sufficiently rigorous about its arguments.

To put it rigorously, the procedure of modern economics is too much a search through the hyperspace of conceivable assumptions. In the second of his *Three Essays on the State of Economic Science* (1957) Tjalling Koopmans argued for precisely such a program of research, referring to a "card file" of logical results connecting a sequence of assumptions A, A', A", A"', ..., A^N to the corresponding conclusions C, C', C", and so forth. He specifically wished to separate blackboard economics from empirical economics, "for the health of both." Economists should have a theoretical branch and an empirical branch (which he thought was going to result in an imitation of physics). The theoretical branch should devote itself to "a sequence of models".

Koopmans' program has been widely accepted. In 1984, for example, Frank Hahn thought he was answering the objection that anything can happen in general theorising by saying: "It is true that often many things can be the case in a general theory but not that anything can be. Everyone who knows the textbooks can confirm that" (p. 6). What he means is that the textbooks line up the sequence of assumptions A, A', A", ... with the conclusions C, C', C", ... True enough. But of course it is not an answer to the objection that in economic theorizing, contrary to its declared love of rigor, in fact anything goes. I conjecture the following important

Metatheorem on Hyperspaces of Assumptions

For each and every assumption A implying a conclusion C and for each alternative conclusion C' arbitrarily far from C (for example, disjoint with C), there exists an alternative assumption A' arbitrarily close to the original assumption A, such that A' implies C'.

I have not been able to devise a proof, but you can whip one up; anyway, as an empirical scientist, I leave that to the mathematicians. The empirical evidence is overwhelming. Name a conclusion, C, in recent but not last year's formal economics – say, that rational expectations obviates government policy or that interaction in many different markets makes for closer collusion of oligopolists. Observe that by now there have appeared numerous proofs that alternative assumptions A' or A", which for most purposes look awfully close to the original A, result in C' or C" – that government policy outwits rational expectations or that the oligopolists are nonetheless unable to achieve collusion.[5]

The problem, to repeat, is a rhetorical one. The prestige of mathematical argument led economists to believe, contrary to their discipline, that the economist could get something intellectually for nothing, proving or disproving great social truths by writing on a blackboard. Programs of research since the 1940s that focused on existence theorems have for a time been rhetorically successful, until the economist have realized once again that after all nothing has been concluded. Besides the general equilibrium program itself, one can mention the $2 \times 2 \times 2$ program of international trade, the theory of international finance, and the rational expectations revolution in macroeconomics. The economists responsible for these excellent ideas have wandered off into a discussion of whether or not an equilibrium exists for this or that "setting" and what its character might be, qualitatively speaking.

They have seldom asked in ways that would persuade other economists how large the effects were. They have not asked how large is large. Eventually they have gotten bored with the formal tool of the day and have walked off to develop a new one. For example, game theory is beginning (for the third time in its brief history) to bore economists; evolutionary theory stands enticingly ready to fuel careers and then to be abandoned in its turn. The economists, though they talk about it quite a lot, and sneer at lesser breeds without the law such as lawyers and sociologists, have not taken the rhetoric of science seriously, and have retreated from the library and laboratory to the blackboard. The research in many fields of economics does not cumulate. It circles.

The problem was brought into focus by the philosopher Allan Gibbard and the mathematical economist Hal Varian some time ago. "Much of economic theorizing," they noted (without intent to damn it), "consists not ..... of forming explicit hypotheses about situations and testing them, but of investigating economic models" [1978, p. 676]. That's right. Economic literature is largely speculative, an apparently inconclusive exploration of possible worlds. In defending the excess of speculation over testing in economics journals Gibbard and Varian use a phrase heard a lot in the hallways: "When we vary the assumptions of a model in this way to see how the conclusions change, we might say we are *examining the robustness of the model*" [same page]. Economists commonly defend their chief activity by saying that running through every conceivable model will show them the crucial assumptions. They have embarked so to speak on a fishing expedition in the hyperspace of possible worlds.

The trouble is that they have not caught any fish with the theoretical line. The activity works as science only when it gets actual numbers to fish in. But economic speculation does not use actual numbers. It makes qualitative arguments, such as existence theorems. (Paul Samuelson, who founded the present paradigm in economics, spent a good deal of time in his book of marvels published in 1947 trying to derive *qualitative* theorems; he did not show the way to empirical work. Maybe for all his astounding excellences Samuelson in this respect set economics off in the wrong direction.)

What economics needs, say Gibbard and Varian, is a quantitative rhetoric, telling how large is large:

> When a model is applied to a situation as an approximation, an aspiration level epsilon is set for the degree of approximation of the conclusions. What is hypothesized is this: there is a delta such that (i) the assumptions of the applied model are true to a degree of approximation delta, and (ii) in any possible situation to which the model could be applied, if the assumption of that applied model were true to degree of approximation delta, its conclusions would be true to degree epsilon. [pp. 671-72]

That sounds good. Yet they realize that the degree of approximation of this desirable, physical, engineering rhetoric to economics is poor. In the next sentence they concede that "Of course ..... few if any of the degrees of approximation involved are characterized numerically" [p. 672]. Oh, oh. Wasn't that the point? If the literature of economics consists largely of *qualitative* explorations of possible models, what indeed *is* its point? Don't we already know that there exist an unbounded number of solutions to an unbounded number of equations? Where, one might ask, will it end?

Gibbard and Varian are uneasily aware of how crushing their remark is. They conclude lamely "but the pattern of explanation is, we think, the one we have given" [same page]. Well, be quantitative. Within what neighborhood of radius epsilon does economic theory, high-brow or low, approximate the quantitative procedures that are routine in physics, applied math, labor economics, or quantitative economic history?

Varying the assumptions of economic models with no rhetorical plan in mind – because "it's interesting to see what happens" when assumption A is replaced by assumption A' – is not science but mathematics. It is the search through the hyperspace of assumptions. A long time ago I helped interview a young man who had written a thesis weakening one of the assumptions in Arrow's Impossibility Theorem. We asked him mildly what the scientific uses of such a result might be. The youth waxed wroth: "What! Don't you understand? I have *weakened* an assumption in *Arrow's Impossibility Theorem*!" Here was someone from the Math Department, at least in spirit.

Scientists think differently. When the economic historian Robert Fogel varies an assumption he plans to strengthen his economic case by biasing the findings against himself. When Richard Feynman cut the safety seals of the space shuttle engine with a kitchen knife he also had an *a fortiori* plan in mind. The most prestigious research method in modern economics, imitated at all levels of mathematical competence in the field, has no such rhetorical plan.

The rhetorical problem, to repeat, is that economists have taken over the intellectual values of the wrong subjects. It is not that the values or the subjects are intrinsically bad. No reasonable person could object to such values flourishing within the Department of Mathematics. Splendid. Some of all our best friends are mathematicians. Capital. The problem comes when the economists abandon an economic question in favor of a mathematical one, and then forget to come back to the Department of Economics. Questions of existence or questions that ring the changes on the mathematical object itself might be of interest to mathematics, regardless of how remote. Unless they can be shown to bear directly on a dispute in economic science, however, they are not of interest to economics.

The problem lies in the sort of mathematics used, which is to say the details of the formal methods. Physicists and engineers routinely state the bounds within which their assertions hold approximately true and then they tell how true. Listen to page 3 of one of the leading textbooks in engineering mechanics:

> In mechanics models or idealizations are used in order to simplify application of the theory ..... A *particle* has a mass but a size that can be neglected. For example, the size of the earth is insignificant compared to the size of its orbit ..... *Rigid Body*. ..... In most cases, the actual deformations occurring in structures ..... are relatively small ..... *Concentrated Force* ..... We can represent the effect of the loading by a concentrated force, providing the area ..... is *small* compared to the overall size of the body. (Hibbeler, 1989, p. 3)

Such talk about magnitudes is foreign in economics. It is surprising to both their students and to their colleagues in physics and engineering that in what economists regard as their chief scientific work they do not talk about magnitudes at all. Of course, when they come to advise on policy or reconstruct past economies the bounds of error must be stated, and often are, with wonderful skill. On the blackboard, where they spend most of their time, however, economists routinely forget to say how large is large. They have taken over unawares the intellectual ideals of that admirable, excellent department where existence is all important and magnitude is irrelevant. The economists are in love with the wrong mathematics, the pure rather than the applied.

It is not fair, in other words, to blame the Department of Mathematics for the economist's love of existence theorems. In fact, it is not fair to blame the mathematical economists themselves. Even non-mathematical economists have always loved existence theorems. It is said that economists would have had to reinvent the calculus for their own lovely marginal analysis if it had not already been invented; likewise they would have had to reinvent fixed point lemmas.

It's not a matter of the use of mathematical notation. A mathematical spirit pervades the works of David Ricardo (Schumpeter called the spirit The Ricardian Vice), who used no mathematics. The physiocrats, too, were attempting to solve great social questions by manipulating definitions. The Ricadian Vice has little or nothing to do with the use of *mathematical* formalism. It is formalism, whether of words or statistics or mathematics, that creates the false hope that the blackboard is all we need. The wholly verbal Austrian economists are as much in love with their own sort of formalism, and hostile to the notion that science might have to come off the blackboard, as is the most math-besotted graduate of Berkeley or Minnesota. The older sort of Marxian economists are, too. System is what people want, as Francis Bacon promised in sounding the bell that gathered the wits, "the business done as if by machine."

Among the oldest questions in economics, after all, is a theorem about whether, as Bernard Mandeville put it in the early 18th century, private vice can be a public benefit: "Thus every Part was full of Vice, Yet the whole Mass a Paradise." Are social systems automatically virtuous as well as automatically stable? No numbers are expected in the answer, which is a tip-off that social philosophy, not social physics, is in question. It is to be done at the blackboard or the lecture podium, not in the world of measurement. A modern student of the matter, known as the Hobbes Problem, is the non-mathematical but Nobel economist James Buchanan; and another the philosopher Robert Nozick; and another the lawyer and judge Richard Posner; and scores of lesser lights, none of whom can be accused of making a fetish out of mathematics. The non-mathematical existence theorems are as peculiar as the mathematical ones. Why would it matter for a worldly philosophy whether or not a knife-edge existence theorem could be proven? Unless it concerns the relevant quantitative questions – *how* full of vice, *how* paradisical – the theorem will not enlighten economics. The problem, again, is not the presence of logic or mathematics – plainly, systematic imagination will often need them. The problem, as one can see clearly in these non-mathematical cases, is the strange rhetoric of existence theorems.

The classic [redacted] definition of economics is Marshall's: "a study of mankind in the ordinary business of life" (p. 1). The literary critic Northrup Frye would extend the definition: "The fundamental job of the imagination in ordinary life ..... is to produce, out the society we have to live in, a vision of the society we want to live in" (1964, p. 140). Mathematical economics, and indeed theory generally, should be viewed as poetry in this act of imagination. Poets are not mere luxuries. We need their constructs – although it should be noted that we do not need large numbers of third-rate constructs any more than we need lots of third-rate poetry. The third rate in empirical work is still useful, something on which one can build. The third rate in theoretical work is perfectly useless, even bad for one's soul, the way that Edgar Guest or even Robert Bridges is.

The advantage of looking at theorists this way is that they are cut off from their false claim of physics-mimicking scientificity. They are bards, imaginaries, mathematicians. One of them, Brock again, speaks of his work explicity in such terms, as for example (1989, p. 443), "chaos theory unfetters our imagination ..... [L]ike much of abstract economic theory, it may give us a hint of how to formulate better empirical models even though the guidance is still rather limited".

The intellectual values of poets are not to be taken as a guide to science. The rhetoric of existence theorems elevates consistency to the only intellectual virtue – not merely the most important or the one necessary, but the only one. "A foolish consistency," the American philosopher said, "is the hobgoblin of little minds, adored by little statesmen and philosophers and divines." The singleminded pursuit of consistency is the Math Department's value. In economics is it too often a foolish consistency.

Alan Turing, the great mathematician, had a good-natured debate in 1939 with Ludwig Wittgenstein, the still greater philosopher (who was trained, not incidentally, as an aeronautical engineer):

> *Wittgenstein:* the question is: Why are people afraid of contradictions? It is easy to understand why they should be afraid of contradictions in orders, descriptions, etc., *outside* mathematics. The question is: Why should they be afraid of contradictions inside mathematics? Turing says, "Because something may go wrong with the application." But nothing need go wrong. [redacted] And if something does go wrong – if the bridge breaks down – then your mistake was of the kind of using a wrong natural law .....
>
> *Turing:* You cannot be confident about applying your calculus until you know that there is no hidden contradiction in it.
>
> *Wittgenstein:* There seems to be an enormous mistake there ..... Suppose I convince Rhees of the paradox of Liar, and he says, "I lie, therefore I do not lie, therefore I lie and I do not lie, therefore we have a contradiction, therefore $2 \times 2 = 369$." [D.N.Mc.: Wittgenstein here refers to the logical proposition that an accepted contradiction allows one formally to prove any false proposition whatever.] Well, we should not call this "multiplication," that is all .....
>
> *Turing:* Although you do not know that the bridge will fall if there are no contradictions, yet it is almost certain that if there are contradictions it will go wrong somewhere.
>
> *Wittgenstein:* But nothing has ever gone wrong that way yet .....

Andrew Hodges, a mathematical physicist and the biographer of Turing, writes of this exchange:

> But Alan would not be convinced. For any pure mathematician, it would remain the beauty of the subject, that argue as one might about its meaning, the system stood serene, self-consistent, self-contained. Dear love of mathematics! Safe, secure world in which nothing could go wrong, no trouble arise, no bridges collapse! So different from the world of 1939. *[p. 154]*

The mathematician's mad pursuit of consistency (for which in the 1920s and 1930s the gods rewarded him, in part through this very Alan Turing, with rigorous proofs of its ultimate impossibility) is aesthetic, not practical. The poet's values surface again. Whatever may be its merits in mathematics (and there are doubters even there; Kline 1980, p. 352), the aestheticization of science is bad. The main argument that economists appear to have in favor of an argument is that it is "deep" or "elegant," as against "ad hoc." I would suggest that the reason they have such a non-rigorous vocabulary of persuasion is that they are not aware they are persuading. In any event, taking over the persuasive rhetoric of the Math Department is not a good idea. Economics is a science, not a branch of mathematics.

In the end the engineer's criterion is what matters: does it work? Quantitatively speaking, has the formalism of economics resulted in good science? The question is complicated, but by now, after forty years of rigorous trial, it is fair to ask what has been learned. It is not fair to claim in answer simply the number of theorems or papers. The mathematician Stanislaw Ulam calculates that some 200,000 theorems are proven annually in mathematics (1976, p. 288). The NSF reckons that some 2,000,000 articles are published each year in science, from 20,000 journals. Such figures suggest the question whether much of it matters. What ideas that matter have come out of the formalization of economics?

In 1965 one could stand back from the program of formalization in economics and remark wisely that economics needed to invest a little in searching the hyperspace of assumptions, because perhaps in 50 years one of the theorems will become empirically relevant. We should tolerate the mathematical economists for a while. After all, said the tolerant sages, non-Euclidean geometry was useless at its birth but proved to be just what Einstein needed.

There are three practical problems with such tolerance. First, it is no longer 1965. It is 25 years on, and we have yet to see the payoff. In the meantime the empirical parts of economics have taught us all manner of things, that we now will always know, about how economies actually work. The second problem is that economists think that being a social physicist means not having to read anything that is older than the last round of xerox preprints. So economists reinvent the wheel, and the claim that old theorems will come back into use is undermined. Monopolistic competition, for example, keeps getting reinvented. Likewise, economists reinvent every few years the point that pure bargaining, being language (which always can be trumped by itself), has no solution. There is no point piling up theorems whose half-life is six month. Or more exactly, to recur to the quantitative theme, the number of them we presently produce seems grossly non-optimal. Wassily Leontief recently categorized the articles in the leading journals in physics, chemistry, economics, and sociology. In physics and chemistry the theoretical paper were about 10 or 15 percent of the total. In economics (and sociology, perish the thought) the figure was about 50 percent.

I hesitate to articulate the third practical problem with continuing to tolerate the large scale of formalization in economics, because it will seem mean spirited. It probably is. I feel mean about it. But someone has to say it, because everyone knows it is true: A dominant coalition of the formalizers are not themselves tolerant of science. It is an open secret that they *want* economics to become a branch of the Math Department. What is most objectionable about their want is that they are willing to act as *homines economici* in a rigorous sense to achieve it. One economics department after another has been seized by the formalists and marched off to a Gulag of hyperspace searching. Few graduate programs in economics teach economics, especially to first-year students. They teach "tools," tools which become obsolete every five years or so.

Partly this is because of the vocabulary we use. The leading middle-aged economists laugh when Gary Becker is described as a "theorist" and the leading young economists do not even think it is funny. The way first-year graduate programs are structured is a direct result of such terminological confusion. If you do not know anything about any actual economy, the argument goes, perhaps you had better be assigned to the "theory" sequence. "Theory" will at least be your comparative advantage.

Whatever the merits of the argument for static allocation, it has had dismal effects dynamically speaking. It has resulted in graduate students who believe (until experience drives the madness out) that economics is about certain mathematical objects called "economies". The students have no incentive to learn about the economy. When Arjo Klamer and David Colander asked graduate students whether having a thorough knowledge of the economy was a very important thing to have for academic success in economics only 3.4 percent said it was [1990, p. 18]. Nor do the students have an incentive to learn about economic theory beyond that embodied in certain mathematical books (only 10 percent said it was very important to have a knowledge of the literature of economics). Such students become teachers and practitioners who do not understand economics: micro teachers, say, who do not grasp opportunity cost and cannot think about entry; macroeconomists who have not read Keynes; policymakers who do not know the history of their section of the economy.

Their ignorance is commonly defended by saying that, well, they certainly did acquire in their education a lot of "tools". But the tool kit turns out to be filled mainly with bits of mathematics that in five years will become unfashionable again (in favor of other "tools": witness the history of linear programming in economics). Broken power routers and defective power jigsaws have crowded out the hammers and nails.

The problem of a training in technique that does not deal with life appears to be a widespread modern problem. Look at modern art, School of Manhattan, if you can, or modern architecture, from Bauhaus to our house. In a recent essay the critic John Aldridge attacks what is known in English departments as "the workshop writer," that is, the product of one of the numerous programs that teach writing in imitation of the University of Iowa's original Workshop. His description of "that odd species of bloodless fiction so cherished by the editors of *The New Yorker*" would fit most graduate programs in economics. Try substituting "academic economists" for "writers," "economic research" for "writing," and "economics" for "literature":

> [W]hat finally counts ..... is not the quality of the work produced but the continued existence and promotion of writers. Any question raised about quality would surely be considered a form of treason or self-sabotage ..... [I]t is entirely possible for a young writer to be graduated from one of these programs in almost total ignorance of the traditions of his craft and, for that matter, with only superficial knowledge of literature ..... [O]ften the promise they show is the variety most young people show up to the age of about twenty-five, while other qualities more essential to the continued productivity of writers are not so immediately detectable ..... [T]hese writers are not only estranged from their culture but seem to have no impressions of, or relations to it at all. In fact, they show no symptoms of having vital social and intellectual interests of any kind or any sense of belonging to a literary tradition ..... [A]ny of their novels and stories might conceivably have been written by almost any one of them. (pp. 31, 32, 33, 37)

The benefits, I claim, have been meagre. The physics standard shows that something is wrong. Another standard is the scale of promised and then boldly claimed accomplishments. To hear mathematical economists say it, you would think that mathematical economics would bring us to a Newtonian stage in the science. In a review of Kenneth Arrow's collected works Frank Hahn made the following assertion: "The theory which Arrow and his coevals and successors have built is all we have of honest and powerful thinking on the subject." "The subject" appears to be economics. Suppose, to be generous, that Hahn means rather "the subject" to be the narrower matter of thinking about the desirability of capitalism since Adam Smith. Even so, his claim that general equilibrium has afforded "that precise formulation which would allow [Adam Smith's arguments] to be evaluated and their range of applicability discussed" will seem unreasonable to many economists. It is similarly unreasonable to say that "the case for modern economics" rests on the achievements of one who "has only concerned himself with establishing what it is that can be claimed as true if certain assumptions are made," when the "assumptions" are formal only, the product of the blackboard rather than of the library or of the world.

To ask the question ofwhat we have learned from formalization since the War is to suggest that the yield has been rather modest.[6] We have learned more in economics from our continuing traditions of political arithmetic and economic philosophy. Human capital, the economics of law and society, historical economics, and the statistics of economic growth have come from economists who trade with someone besides the Math Department.

This is not, I repeat, to set *The Journal of*

Economic Theory below its proper value. Surely we should have people doing some sort of philosophical job, finding out how much can be wrung from this or that convenient assumption, though we should not assign quite so many people to the job as at present. We are all very thankful to Smith and Marx and Keynes for having inspired those fine theorems by Hahn and Arrow and Samuelson. But we should not be thankful for the reduction of "theory" to a certain brand of mathematics.

In other ways, however, I stand four square with Frank Hahn: "[A]ll these 'certainties' and all the 'schools' which they spawn are a sure sign of our ignorance ..... [I]t is obvious to me that we do not possess much certain knowledge about the economic world and that our best chance of gaining more is to try in all sorts of directions and by all sorts of means. This will not be furthered by strident commitments of faith" [pp. 7-8].

And I stand, too, with our sainted Léon, he of general economic equilibrium a century ago. He attacked then (p. 48) "the idea, so bourgeois in its narrowness, of dividing education into two separate compartments: one turning out calculators with no knowledge whatsoever of sociology, philosophy, history, or [even, McC.] economics; and the other cultivating men of letters devoid of any notion of mathematics. The twentieth century, which is not far off, will feel the need, even in France, of entrusting the social sciences to men of general culture who are accustomed to thinking both inductively and deductively and who are familiar with reason as well as experience." The 21st century hurries near. We may hope by then, after a century of experiments in educational compartments, that Walras' vision of an undivided economics may be fulfilled.

Notes

1. Look for instance at the presidential address of Harry A. Millis to the American Economic Association (delivered December, 1934), especially pp. 4-5 on marginal productivity and the labor problem. [redacted] He did not understand the notion of a function [redacted] in [redacted] Hicks' *Theory of Wages*.
2. Richard Palmer, a physicist from Duke University recalling a conference of physicists and economists, told Robert Pool, "I used to think physicists were the most arrogant people in the world. The economists were, if anything, more arrogant" [1989, 700].
3. I am speaking of neoclassical economics; but anti-neoclassicals should not therefore rejoice. They do the same thing. In Marxian economics, for example, the general statement that commodities are made with commodities cannot be expected to yield specific answers to any question worth asking, either. The various impossibility theorems that make institutional economists happy ("But after all the economy is obviously not competitive and so all that neoclassical talk is rubbish") are equally vacuous.
4. The recent works of William Milberg at the University of Michigan-Dearborn and of Hans Lind at the University of Stockholm have noted the lack of rigor in the opening and closing paragraphs of theoretical papers. Great rigor in the middle; touchie-feelie on the ends.
5. For that last see Fisher 1989, p. 122.
6. A full catalogue is examined in Henry Woo's book, *What's Wrong with Formalizaiton in Economics.*

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