Knowledge and persuasion in economics

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CAMBRIDGE
UNIVERSITY PRESS

1994

chapter 10

The rhetoric of mathematical formalism: existence theorems

The main figure of economic rhetoric has become the conspicuous use of mathematics. The rise of a scientistic style in economics has been accompanied by the rise of mathematical formalism. In 1972 Benjamin Ward identified a "formalist revolution" beginning in the 1940s (Ward 1972, p. 40). Philip Mirowski has recently dated what he calls "the second rupture" in mathematical rhetoric to the 1930s, attributable he argues to a belief by young physicists and other scientists that economic problems urgently needed their skills (1991b, p. 151; compare the similar entry of physicists into biology after the Second World War). In 1986 Herbert Grubel and Lawrence Boland surveyed the results. From 1951 to 1978 the number of pages containing a mathematical expression without empirical use rose in the *American Economic Review* from 2.2 percent to 44 percent (Grubel and Boland, 1986, p. 42; cf. Debreu 1991, p. 1). The rise was matched in other high-prestige journals, accelerated by the birth of journals devoted entirely to such products. (The products have not pleased the customers. Grubel and Boland sampled several hundred economists of various types and found that over two-thirds believed that excessive space in the journals is devoted to purely theoretical articles [1986, p. 433]).

It would course be idiotic to object to the mere existence of mathematics in economics. No one wants to return to the time, not distant, in which economists mixed up the movement of an entire curve and a movement along it. Look for example at the presidential address of Harry A. Millis to the American Economic Association (delivered in December, 1934), especially pages 4–5 on marginal productivity and the labor problem. Because he did not understand the notion of a mathematical function Millis misunderstood Hicks' *Theory of Wages*.

Economics made progress without mathematics, but has made faster progress with it. Mathematics has brought transparency to many hundreds of economic arguments. The ideas of economics – 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. 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'" (Walras 1874/1900 [1954], 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? (See the classic article by Paul David and Albert Fishlow making this point in 1961, simulation before the computer made it fashionable.) 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 it is expressed in Bourbaki-style mathematics." Grubel and Boland put it economically: "Is the quantity of mathematics applied to the production of [economic] knowledge and human capital [of students] efficient?" (1986, p. 421). To answer the quantitative question about the role of mathematical formalism in economics we need a quantitative standard.

Comparison provides the 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 in a few ways they are. Like physicists, and unlike, say, historians, economists are frankly competitive, in love with conferences and conflict. 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 (cf. Khalil 1992, who wisely adds a third category, "owl"). They like to colonize other fields, in the way biology was colonized after the war by physicists ashamed of making bombs. And economists, as we have seen, are approximately as arrogant as physicists are.

For another, economists admire physicists and judge themselves, as do most people in our culture, to be intellectually inferior to them. Like the philologist in the centuries of Scaliger, Erasmus, Bentley, and Housman the physicist today is at the top. Physicists have the most prestige among intellectual workers, and are able therefore to persuade government to give them expensive toys. The first-rate economists imagine themselves to be good third-rate physicists. Comparisons with sociology, say, 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 a field economists look ignorantly up to rather than one they look ignorantly down upon.

Most economists, then, 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 for that matter in the much-despised civil engineering). 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 mathematical." In the economics department the spirit of the math department reigns. The spirit is different over in the physics department. The physicist Richard Feynman introduced a few simple theorems in matrix algebra into his notoriously difficult freshman class at the California Institute of Technology with evident embarrassment (1963, vol. I, 22-1): "What is mathematics doing in a physics lecture?" He answered by distinguishing the physicist's results from the mathematician's proofs: "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 – why math? – 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?" The anthropologist Sharon Traweek reports that "theoretical physicists may be chastised by their peers for being 'too mathematical'" (1988, p. 79), a charge that would not be rhetorically effective in economics. 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 in turn 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 1989, p. 701). The physicists do not, actually, feel "beaten," since unlike economists they do not regard 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" (Pool 1989, p. 701). A physicist at the Santa Fe Institute solved a problem overnight with a computer simulation, approximately, while the economist found an exact analytic solution. Who is the more mathematical?

Economists think that science involves axiomatic proofs of theorems and then econometric tests of the QED (*quod erat demonstrandum,* not quantum electrodynamics), which therefore will test the axioms. As Paul Feyerabend remarks, "It is to be admitted that some sciences going through a period of stagnation now present their results in axiomatic form, or try to reduce them to correlation hypotheses. This does not remove the stagnation, but makes the sciences more similar to what philosophers of science think science is" (Feyerabend 1978, p. 205). In truth the physicists could care less about mathematical proofs and very little about correlation hypotheses. Even the theoreticians in physics spend much of their time reading the physical equivalent of agricultural economics or economic history. Pure pencil-and-paper guys are common enough in physics departments, but they do not set its intellectual agenda. Physics is finding driven. Economics, like mathematics in the heyday of Nicholas Bourbaki, is proof driven. Ask your local physicist what he thinks about proofs. He'll say, "Well, I prefer to depend on an existence proof about existence proofs: 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 mathematical economist Gerard Debreu, in an address to the American Economic Assocation, notes that the mathematical economist "belongs to the group of applied mathematicians, whose values he espouses" (1991, p. 4); and he speaks of "the values imprinted on an economist by his study of mathematics" (p. 5). Debreu realizes that physicists do not share these values: unlike economics, "physics did not surrender to the embrace of mathematics and to its inherent compulsion toward mathematical rigor," but on the contrary occasionally was led "to violate knowingly the canons of mathematical deduction" (p. 2). But economists, says Debreu, do not have enough experimental data, and therefore must rely on deductive methods. A similar remark was made by Philip Anderson, the distinguished physicist who (with Kenneth Arrow) brought the Santa Fe Institute together, explaining the differences in attitudes towards mathematics on the part of economics and physics by reference to "the differences in the amount of data available to the two fields" (Pool 1989, p. 701).

As an economic historian I can attest that Debreu and Anderson are mistaken, as they would probably agree if they looked into the matter. Economists are drenched in data, as hard as may be, and recently even experimental data. And unless astrophysics and geology are to be accounted non-sciences because they do not experiment much, observational data are data, too, what we mainly can hope to have in paleontology or history or economics. Wassily Leontief attributes the tradition of fact-boredom in economics to the long and lazy era in which economics could advance on the basis of facts available to any alert person and, when these were exhausted, on the basis of easily acquired government statistics (1982, p. 104). His argument seems plausible. In any event, the old era is past. The new economic historians have revolutionized what even a lazy economist can extract by way of data (Sutch 1991; McCloskey 1976).

The manuscripts of the American Census of Production after 1840, for example, have provided stunning quantities of data. The word "data" anyway shows the real problem: it means in Latin "things given." The better, less mathematical, and more scientific word would be *capta*, "things seized" in long, cold nights at the telescope or long, dry days in the archive. The data are not "available" to physics: they are seized, with great difficulty. An astrophysicist studying neutron stars has thin and puzzling data, but she examines them closely, and lusts to have more. A theoretical economist, by contrast, fabricates some "stylized facts" out of his head and then devotes the rest of his career to axiom and proof.

No one would make the absurd claim, of course, that axiom and proof have no place 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" (Marshall 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. True though it is that a science is axiomatized only on its death bed – but it is after all the owl of Minerva, the mathematizers would reply, that takes wing at dusk – no one should spurn knowing $A \rightarrow C$.

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 the mathematical economist 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 that "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.
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> Brock 1988, 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. (They are not the beloved of the physics department.) Faced with a can of beans on a desert island the economist proves that there must exist a can opener, somewhere.

The most famous of these theorems is of course the Arrow-Debreu proof of the existence of competitive equilibrium, though I intend the word "existence theorem" to apply to all the qualitative theorems with which economists wile away the hours between 8.00 a.m. and quitting time. The problem of formalism in economics extends beyond the admitted vacuities of general equilibrium theory. It reaches down to the way the average economist sets up a problem, as a theorem rather than a simulation. When the chemist Linus Pauling, age ninety-one, sets to work in the morning he is reported to carry a calculator; an equivalently eminent economist carries a pencil alone or perhaps a piece of chalk, the better to prove theorems.

But general equilibrium is a leading case, and is often though wrongly taken as the core of economics. 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. From everywhere outside of economics except the department of mathematics the proofs of existence of competitive equilibrium will seem strange. The proofs 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 actual 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 $4,598 billion. 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.

Incidentally, 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. The various impossibility theorems that make institutional economists happy ("But after all the economy is obviously not competitive, and so all that neoclassical rhetoric is rubbish") are equally vacuous, equally in love with Kant's synthetic a priori, equally unlike the procedures of physics or any other science.

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 reply you will get. Of course, the economist giving it does not then go out into the world and look for parameterized and empirical solutions, ever. Nobody's perfect. The reply anyway sounds reasonable to someone who has never studied another science, coming to economics from the department of mathematics: if you can't actually find the solution, nonetheless you can know that what you're endlessly looking for exists.

Judging again from physics, however, the reply is not reasonable, and is not the procedure in science. Mathematicians believe it is, but the physicists do not agree. Physicists have happily used the Schrödinger equation since 1926 without knowing whether it has solutions in general. The *N*-body problem in Newtonian physics, which mathematicians have been working on for three centuries, does not possess extant 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 (though in the long run its orbit is in fact instable). 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 particular finite cases covering assumptions A' or A'' or A''' merely close to A. For that question you need approximations and simulations and empirically relevant parameters, not existence theorems.

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 and then to run the field as though math-department questions were in fact important for the science. It is a search under the lamppost because the light there is so good, as the drunk explained after losing his keys in the dark.

The notions of "equilibrium" and "maximization" in economics have been subject to such a mathematizing treatment, as historians of economic thought have noted with alarm (Weintraub 1991a, 1991b; 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 in each of the two books published in his lifetime (1776 [1981], IV, ii, p. 456 and 1790 [1982], IV, i, 10, p. 184) 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 (I note that the same diversion of talent occurred in the ruminations on ordinal utility from 1910 to 1950 and now is occurring on a bigger scale in game theory). With certain assumptions about preferences and technology one can write down equations that 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 modernist 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, or else they are not believed, and their assumptions are perturbed until the lack of surprise is reinstated. *But the qualitative sense 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. Among mathematicians an error however small is not to be overlooked (an intellectual value that Bishop Berkeley threw in the face of the early users of the calculus (Davis and Hersh 1981, p. 243). In physics and engineering and economics a small error *is* to be overlooked.

The problem is that the general theorem of Arrow and Debreu or any of the other qualitative theorems do not, strictly speaking, relate to anything an economist would actually want to know. We already know for example that if the world is not perfect then the outcomes of the world cannot be expected to be perfect. We know it 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 outlawing of interest? Will free trade with Mexico raise American national income much? 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 perfectly efficiently. Yet that is a typical piece of information from the mathematical front lines. It does not provide the economic scientist with a quantitative scale against which to judge the significance of the necessary deviations from completeness. It is social mathematics, not social physics. Chewing gum or all investment goods: no 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 approximately 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 (similarly fuzzy but 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 (cf. Cowen 1990).

If we are going to be rigorous we should be rigorous, not rigorous about the proof and extremely sloppy about its range of application. William Milberg of the New School for Social Research (1988, 1991) and Hans Lind of the University of Stockholm (1992) have in a series of papers documented the lack of rigor in the opening and closing paragraphs of theoretical papers. Lind has exhibited the rhetoric of evasion in the work of one Swedish economist, a specialist in international trade theory. The Swedish economist does not fall below the usual standard, explored by Milberg in other fields of economics: great rigor in the middle; utter laxity on the ends. The argument put forward, in its middle parts so very precise, is usually vague to the point of scandal in its beginning parts – about why the assumptions A' or A'' are to be preferred to the old A. The new assumptions are said to be "more realistic" or "less restrictive" or something else obscure and untried. After a middle passage through pointless precision the argument again becomes fuzzy, at the end, where the "policy implications" are brought forward. More intellectual scandal. Because of the tangency of curves on a blackboard we are to adopt a policy of free trade. Because of a lack of tangency we are to overthrow capitalism.

The theorems are exact results, containing no definition of the neighborhood in which they are approximately correct. They choose the wrong rhetoric. They are fine for math but useless for science. For science we need quantitative simulation, not qualitative theorems.

Richard Lanham has pointed out that the ancient practice of *declamatio*, that is, the rehearsal of rhetorical techniques, the mock court of law schools, is precisely simulation. The rhetoric of science and life speaks of "trying out" this or that notion. It is useless, on the contrary, to prove by analysis that there exists an ideal speech at law or an ideal design of a bridge. Simulation tells us what we want to know: what works, how well. And simulation by computer becomes cheaper every few years by another order of magnitude. "Just as the rhetorical practice of declamation put dramatic rehearsal at the center of classical thought," Lanham notes, "the computer has put modeling at the center of ours" (Lanham, forthcoming, ch. 4, p. 14). The technique of simulation suits the postmodern economist, or the pre-modern, Gothic economist – anything but the modernist in love with existence theorems. The ancient prestige of analytic solutions and existence theorems, one might predict, will fade as the cost of computation falls. It is already happening in some branches of pure mathematics. The Greek idea of mathematics may be under a sentence of death by electronics.

The exact existence theorems are perhaps worth having, though why exactly they are worth having needs to be argued more rigorously than it has been so far – a matter of rhetorical or philosophical, not mathematical, rigor, but rigor all the same. The philosopher Gary Madison said, "the trouble with this kind of [philosophically unsophisticated] call for intellectual rigour is that it is not rigorous enough. On the one hand it naïvely accepts positivistic myths as to what natural science is . . . On the other hand, it does not raise any critical questions as to what the object of economics is or ought to be" (Madison 1990, pp. 35–36). Mathematical economics has not been sufficiently rigorous about its arguments – the way mathematical physics has been forced to be. As it was put by Wittgenstein, who was rigorous about the place of mathematics in our intellectual culture, "Confusions in these matters are entirely the result of treating mathematics as a kind of natural science. And this is connected with the fact that mathematics has detached itself from natural science; for, as long as it is done in immediate connection with physics, it is clear that *it* isn't a natural science" (quoted in Monk 1990 [1991], p. 326).

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 (trained as a physicist in the 1930s) 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 protection of both. It recommends the postulational method as the principal instrument by which this separation is secured" (Koopmans 1957, p. viii). 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" (p. 147 and throughout).

Koopmans' program has been widely accepted. In 1984, for example, Frank Hahn thought he was answering the objection that anything can happen in general theorizing 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" (Hahn 1984, 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. That's nice. 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: choose A', A'', A''', as you will, like a gentleman preferring blondes to brunettes. I conjecture the following important

Metatheorem on Hyperspaces of Assumptions

For each and every set of assumptions A implying a conclusion C and for each alternative conclusion C' arbitrarily far from C (for example, disjoint with C), there exists an alternative set of assumptions A' arbitrarily close to the original assumption A, such that A' implies C' (see figure 1).

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. Any experienced economist knows of examples. Name a conclusion, C, in recent (but not last year's) formal economics – say, that rational expectation 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'', disjoint with C – conclusions such as that government policy outwits rational expectations or that the oligopolists are nonetheless unable to achieve collusion (on the latter see F. Fisher 1989, p. 122). We have discovered empirically in economics over the past forty years that blackboard proofs that $A \rightarrow C$ are *not*

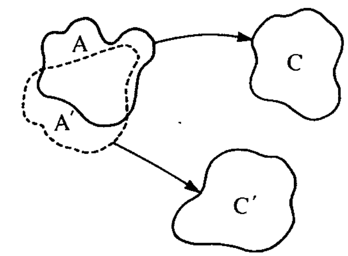

fig. 1 The A-prime, C-prime theorem

robust, cannot in principle be robust, because there always exists $A' \rightarrow C'$, A close to A', where C' is the negation of C.

Richard Feynman told of a game he used to play in graduate school at Princeton with the mathematicians. He offered to tell them at once, on the spot, whether any theorem they could explain to him was true or false. He listened closely to the set of assumptions A that the math students reported to him, and to the conclusion, C, making them express their topological theorem in terms he could understand, such as the cutting up of an imaginary orange. But then he merely guessed randomly at its truth or falsehood, without any attempt to think up a proof validly connecting the assumptions, A, to the conclusion, C. In the instances in which he guessed right, the game ended and the mathematicians went away impressed by Feynman's mathematical insight. If he guessed wrong, he ran the *A*-prime-*C*-prime exercise, and being a very bright boy he could always do so. If he called a true theorem false, for example, the mathematicians would exclaim "Ha! We got him . . . It's So-and-so's [true] theorem of immeasurable measure!" (Feynman 1985, p. 85). But then Feynman would exploit the fact that in order to explain the theorem the mathematicians had had to make particular assumptions, A, about, say, the imaginary orange. Under alternative assumptions *A*-prime, which Feynman was sure to find with a few moments of reflection, the theorem was false, as Feynman could assert without fear of contradiction. The mathematicians had assumed perhaps an infinitely divisible orange, whereas Feynman (if he wanted to reverse the result) claimed after the fact that *he* had been assuming one made of actual atoms. "So I always won." If you are as smart as Richard Feynman you can always win such a game. But it isn't physics, or even interesting mathematics. It's the game economists have been playing with growing unease for the past fifty years.

So have other departments, though not physics and engineering. A computer scientist has complained about the way the subject is taught in universities, a triumph of math-department values over those of electrical engineering:

> While programmers would certainly like . . . to be able to prove mathematically that their programs are bug-free, experience has shown this to be impossible in any practical sense. Real-world programs are too large ever to be proved "correct" and too ill-specified, in that the requirements for a large program are rarely completely understood in advance . . . Nearly every useful program has bugs, and nearly every program simple enough to be bug-free is likely to be of very little practical utility. In general, this doesn't matter, because big programs with occasional small bugs have proved to be so useful in the real world . . . One cannot fault a mathematician for seeing a computer program as a mathematical object. After all, mathematicians see everything as a mathematical object, and rightly so . . . However, none of this implies that a mathematical perspective is necessarily the most useful one for the practicing programmer.
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> Borenstein 1992, pp. B3–4

The problem, to repeat, is a rhetorical one. The prestige of mathematical argument led economists to believe, contrary to their discipline, that they 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 economists have realized once again that after all nothing has been concluded, that $A' \rightarrow C'$ and $A'' \rightarrow C''$ and so forth without limit, as everyone who has read the textbooks can confirm.

The pattern has been repeated in most parts of economics. Besides the general equilibrium program itself, one can mention among others 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 discussions of whether or not an equilibrium exists for this or that "setting" and what its character might be, qualitatively speaking. They have rarely 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. Terence Hutchison instances the rises and falls of growth theory and welfare economics: "In both cases, after decades of ingenious manipulation, efforts petered out with the level of abstraction almost as far-fetched as it had been, 'optimistically' [he refers to Joan Robinson's dictum that one must be patient and optimistic with simplifications], from the start, with any significant real-world relevance as far away as ever" (1992, p. 36). The Tin Lizzie of growth theory has been cranked up again, and can be expected to sputter to a halt in the mud of A', A'', A''', . . . once more, around 1997. Hutchison quotes Ward's complaint twenty years before that the formalization tends "to proliferate beyond the ability of anyone to vouch for its connection with the real world" (Ward 1972, p. 39). Nothing about the real world of economics has been concluded except that $A \rightarrow C$ and $A' \rightarrow C'$ and $A'' \rightarrow C''$ . . . . The orange is assumed to be discrete or infinitely divisible or something else. So?

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 Science quite a lot, and sneer at lawyers and sociologists, have not taken the rhetoric of science seriously, and have retreated from the library and laboratory to the blackboard. Research in many fields of economics (though not all) 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" (Gibbard and Varian 1979, p. 676). That's right. Economic literature is largely speculative, an apparently inconclusive exploration, as I say, of the hyperspace of assumptions A, A', A'', . . . In defending the excess of speculation over testing in economics 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, my italics). But it doesn't work out that way and their rhetoric shows they realize it does not: notice the diffident phrasing, "we might say." Economists commonly defend their chief activity by saying that running through every conceivable model will show the crucial assumptions. Ha.

The economists have embarked 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 much of his book of marvels in 1947 trying to derive *qualitative* theorems; his rhetoric of positivism not withstanding, 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 rhetorical direction.

What economics needs, say Gibbard and Varian with much justice, 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.
>
> Gibbard and Varian 1979, pp. 671–672

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). But 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 exists an unbounded number of solutions to an unbounded number of equations? That *A*-prime implies *C*-prime? 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, engineering, 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-prime – is not science but mathematics. It is the search through the hyperspace, A-prime-C-prime economics. As Benjamin Ward observed sourly, "The lesson of economics is that it is not always enough that . . . practitioners are in substantial agreement as to the properties of acceptable puzzles and their solution to insure that a science is seriously engaged in the attempt to understand the relevant natural phenomena" (1972, p. 255).

Around 1980 a young man getting his Ph.D. at an important department of economics was interviewed for a job at another important department. He had written a thesis weakening one of the assumptions in Arrow's Impossibility Theorem. The economists interviewing him in a hotel room at O'Hare Airport listened to his spiel and then asked him encouragingly what the scientific uses of such a result might be. Why, they inquired politely, should we care if you have found an *A*-prime to substitute for the *A*? It was late in a tiring day and the youth waxed wroth: "What! Don't you understand? I have *weakened* an assumption in *Arrow's Impossibility Theorem*!" Oh. Yes. I see. Here was someone from the math department in spirit. (The department in the story did not hire him, but in most departments of economics he would have been a leading candidate.)

Scientists think differently. When the economic historian Robert Fogel varies an assumption he thereby plans to strengthen his economic case by biasing the empirical findings against himself. When Richard Feynman cut the safety seals of the Challenger space shuttle with a kitchen knife he also had an *a fortiori* plan in mind. But the most prestigious research method in modern economics, imitated at all levels of mathematical competence in the field from Debreu to the local undergraduate, has no such rhetorical plan. Economics does *A*-prime-*C*-prime scholarship, in the spirit of the math department.

The rhetorical problem is that economists have taken over the intellectual values of the wrong subject. It is not that the values or the subject are intrinsically bad. No reasonable person would object to such values flourishing within the department of mathematics. Splendid. Some of 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. As A. J. Oswald put it with an empiricist's frustration, "Economics is in an equilibrium in which large numbers of researchers treat the subject as if it were a kind of mathematical philosophy" (Oswald 1991, p. 78). Questions of existence or questions that ring the changes on the mathematical object itself might be of interest to mathematics, regardless of how remote from an economy. Unless they can be shown to settle 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 extent of the formality, not its existence. Physicists and engineers routinely state the bounds within which their assertions hold approximately true and then they tell how true. As Gibbard and Varian put it, the applied mathematicians seek accuracy of "aspiration level epsilon." When Richard Courant, a mathematician of some repute, brought a bit of applied mathematics into his elementary but difficult calculus book, he emphasized the principle involved, so foreign to the analysis he had been expositing for 300 pages: "We wish to direct special attention to the fundamental fact that the meaning of an approximate calculation is not precise unless it is supplemented by an estimate of the errors occurring, i.e. unless it is accompanied by definite knowledge of the degree of accuracy attained" (Courant 1937, p. 342). The rhetoric is scientific, not mathematical.

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; my italics, except, characteristically, on the "small" in the last line, which is Hibbeler's.

Such rhetoric of magnitudes is foreign in economics and would be revolutionary on page 3 of a leading text in microeconomics. Their colleagues in physics, chemistry, and engineering and their elementary students even in economics are surprised that in what economists regard as their chief scientific work they do not talk about magnitudes at all. They talk about the existence of unique solutions, not the magnitude of the approximations. As Richard Palmer, a physicist involved in the Santa Fe Institute's attempt to instruct economists in science, put it: "while these ingredients might reasonably be expected to lead to many possible economic states or equilibria, it is not clear whether or not they do so in practice. The situation is somewhat obscured by the tendency in economics to look only for unique solutions, and to reject or modify models that do not provide them" (Palmer 1988, p. 179).

Of course, when economists come to advise on policy or to reconstruct past economies the bounds of error must be stated, and often are, with wonderful skill. Empirical simulation runs policy and

history, and should. Economists practice *declamatio*, trying out in the computer or the board room this or that way of saying it.

At the blackboard, where they spend most of their time, however, academic 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.