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FORMALISM IN ECONOMICS, RHETORICALLY SPEAKING

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Even the Nobel science of the economy cannot by-pass rhetoric. This is no bad thing. By the ancient definition, "rhetoric" is the whole art of argument – not ornament and bombast alone. And the non-English definition of "science" is merely, as de Felice and Duro say in their *Dizionario della lingua e della civiltà contemporanea*, for instance, "the speculative, agreed upon inquiry which recognizes and distinguishes, defines and interprets reality and its various aspects and parts, on the basis of theoretical principles, models, and methods rigorously cohering". If science is to cohere it must use the art of argument; and if it is to be agreed upon by free people it must be argued persuasively.

The academic culture of the West, even the English-speaking culture, is turning in such a direction. We children of Plato and Descartes are growing up, surrendering the notion that science or mathematics or some other device yields certitude happily ever after. Pragmatic persuasion, the certitude that an adult can use, is replacing the metaphysics of the perfect, the ultimate, the forever. The humanities and the sciences are growing together.

Rhetorical adulthood is not confined to literary subjects. Mathematics, law, ethnography, political science, history, economics, computer science, nursing, statistics, biology, paleoanthropology are discovering their rhetorical character (see Nelson et al. 1987; McCloskey 1986).

The people who resist such adulthood are disproportionately speakers of English. In English the words "science" and "rhetoric" quarrel childishly. Since the 1840s the English word "science" has come to mean what the lay

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person means by physics. "Rhetoric" has gone the other way. Since 1600, and especially since the early 19th century, and most especially since the days of Mussolini and Hitler, English speakers have thought of rhetoric as the opposite of science. Yet in Italian and in other languages the opposition is less sharp. "*Retorica*" is not forgotten as an element of Italian education, by way of Vico and Croce. And "*scienza*" means merely systematic inquiry, lacking the haughty certitude of the English word. The Italian half of the big *Cambridge Italian Dictionary* warns of English "scientific" that "*nell'uso comune non si riferisce ai principi filosofici classici*": that is, in the common English use, by contrast with Italian, the "science" word excludes knowledge earned beyond the laboratory.

The assertion that economics is rhetorical but still a science, therefore, sounds more believable in Italian and in other languages than in English. The English language has absorbed the message of British empiricism; or perhaps British empiricism sounds especially persuasive in English. The master equivocations of British philosophy trip easily off English tongues: "empiric*ism* = the discipline of fact" and "scient*ism* = the pursuit of knowledge" and "rational*ism* = the exercise of reason". The linguistic fact makes anyway the main point here: that a language and the culture it bears will affect a science.

Most economists and others in love with number would not at first agree. Science, they would say, avoids being Italian or English by being formal. Like the international style in architecture or the modernist style in literature, formalism is a scientific Esperanto, a machine producing the same output regardless of the minder. The wordless imagery of machinery has dominated modern science since its beginnings. Francis Bacon recommended that "the mind itself be from the very outset not left to take its own course, but guided at every step, and the business done as if by machinery" (1620, Preface). "It is by instruments and helps that the work is done..." (aphorism ii). "The understanding must not therefore be supplied with wings, but rather hung with weights to keep it from leaping and flying" (aphorism civ).

Yet the history and sociology of science has found recently that even experiments, the best of machines, are run by humans. The British sociologist of science Harry Collins, for instance, doubts the "algorithmic model", which "encourages the view that formal communication can carry a complete recipe for experiment" (1985, p. 159). As a participant-observer in laser research he found that "no scientist succeeded in building a laser by using only information found in published or other written sources" (p. 55). He and most other recent students of science favor an "enculturational model", which emphasizes "the acquisition of skill as opposed to formal instruction. The locus of knowledge is not the written word but the community of expert practioners" (p. 159). As the crystallographer and philosopher Michael Polanyi put it some time ago (1966), much scientific knowledge is "tacit".

At any rate, most economists would attest after reflection that the formal, written word is not enough for the science of economics. The formalities are easily reduced to mathematics, pleasing to Platonists, but the easy formalities make bad economics. The official philosophy in the field says that if you, oh student, will learn enough engineering mathematics you will become an economist. Unhappily, the official philosophy dominates graduate programs, and the students know where their interests lie. In a survey of student opinion Arjo Klamer and David Colander found that mathematical technique and problem solving were considered very important by 65 and 57 percent; having a thorough knowledge of the economy was considered very important by 3 percent (1987, p. 100).

But formalization does not save the student from having to learn to argue like an economist, somehow. He argues unaware even in his most technical courses. The formalization is itself rhetorical.

The two main formal methods in economics are (1.) specification and regression; and (2.) axiom and proof. Obviously, both are appeals to the *ethos*, the character, of the Scientist (whereas *scienziato* means merely "learned one"). It is this point that people are making when they laugh at the formalistic pretensions of social science; and the joke is a good one. The American *Journal of Irreproducible Results* has long prospered on such nonsense as "A Stress Analysis of a Strapless Evening Gown". We laugh at an incongruous appeal to scientific status (scientology, Christian Science, the more risible jargon of sociology and the more ludicrous models of economics) as we laugh at the bourgeois gentilhomme. "Desire for status", wrote the American sociologist C. Wright Mills, "is one reason why academic men slip so readily into unintelligibility.... To overcome the academic *prose* you have first to overcome the academic *pose*" (1959, pp. 218-219). For all its sober achievements, modern economics and its imitators in other social sciences exhibits a good deal of foolishness, strutting about claiming dignity in the manner of John Cleese.

But that is not the point I want to make. The point I want to make is that economic science is rhetoric all the way down. The rhetorical element in economics is apparent not merely in its sometimes comical claim to scientific status or its sometimes tragic advocacy of social engineering. The formal methods themselves, I should like to argue, stand on hidden arguments, unspoken metaphors, unargued appeals to authority.

Rhetorical methods expose the arguments. Some of the arguments are very good indeed, and I shall love them evermore. But some of them are startlingly bad, because unexamined. Economists do not cease to be rhetoricians when they hang weights of axiom and specification on their understanding, letting their scholarly business be done as if by machine. They are anyway human arguers, and do better to recognize the fact. The search for machinery of intellect that eludes human rhetoric is naïve.

1. *Statistical Significance is Useless*

The first of two examples comes from econometrics – the machinery of specification and regression. The economist thinks of herself as miles ahead of sociologists and psychologists in the use of statistics, and is prone to sneer at the lesser breeds without the law, who stumble about on paths and factors. But in one important matter the economist is the one stumbling along behind (cf. Morison and Henkel 1977). The matter is statistical significance.

If you ask economists who use the notion of statistical significance whether they know what they are doing you will find that quite a few do not. The ignorance is not confined to the inexpert. A sample of statistical works in the *American Economic Review* (McCloskey 1985) found that a significant portion of the papers – three quarters, in fact – significantly misunderstood the use of statistical significance.

A "significant" regression coefficient, of course, means that a *sampling* problem has been solved, or at any rate solved well enough to satisfy conventional standards. (The statistician John W. Tukey has given recently some reasons for doubting the conventional standards (1986)). In other words, *the sample is large enough to assure that if you took another sample it would give roughly the same result.* The sampling variance, which is the population's variance divided by the square root of the sample size, has been driven down to some nice, low figure. As John Venn put it a century ago, when the procedure was a mere twinkle in the statistician's eye, the coefficient (or the mean or the difference between two means or the estimated variance or the R-squared or whatever other statistic you are examining) would probably be "permanent". You would probably come up with roughly the same estimate again.

But a coefficient that is permanent with respect to sampling is not necessarily an important coefficient. High corn yields in little Iowa would raise the income of the United States a little, of course, and with a large enough sample a proper regression analysis of income on the Iowa corn yields would show it. A large enough sample of years would make the relationship register, and would make it keep on registering in successive samples. (Never mind what "successive samples" of *years* could mean: that's a philosophical problem with statistical significance; the subject here is the rhetorical problem). The coefficient would be statistically significant. The Iowa effect would show. Yet for most human purposes it would not matter. "Statistically significant" does not mean "substantively significant".

What matters is oomph. Oomph is what we seek. A variable has oomph when its coefficient is large, its variance high, and its character exogenous. A small coefficient on an endogenous variable that does not move around can be statistically significant, but it is not worth remembering. Oomph is what we mean when we talk about money being "important" for explaining the price level or about capital being "important" for explaining income per person. The Iowa corn yield certainly does affect average national income, but has little oomph because the coefficient is low. Likewise, the existence of oxygen in the atmosphere certainly does affect combustion, but it does not vary enough to give it oomph in an explanation of why a house burns down. The stock of money in the hands of Iowa Citians certainly does determine their expenditures, but because it is entirely endogenous it has no oomph.

Statistical significance, which now guides a large part of the intellectual life of economists, has nothing to do with oomph. It implies, to repeat, that you have acquired some control over sampling error as a source of doubt. But sampling error is seldom the main source of doubt. The main source of doubt is whether a variable matters, or whether it matters to such-and-such a degree. What matters is whether foreign prices affected American prices under the gold standard significantly (that is, with oomph), or whether American wages affected migration from Europe significantly, or whether social security wealth affects capital accumulation significantly. *Statistical* significance will not reveal the substantive significance, the blessed oomph.

The best way to see the point is to suppose that you really do know the coefficient. For sure. God has told you, with no nonsense about confidence intervals. Sampling error is zero. The t-statistic is infinite.

Well, then: Has the variable got oomph? *You don't yet know*. To find out you have to ask and answer questions having nothing to do with statistical significance, and especially the question whether the coefficient is large. How large? That's the crucial question. The answer has to be something like: Large enough to matter in some conversation of scholars or policymakers or other human beings. And it matters for oomph whether the variable could vary enough to produce effects you consider important. How important? Important enough to matter to human beings, in some dispute they are having.

Note the character of the ancillary questions. They concern the state of a human conversation. The questions are rhetorical, concerning how humans come to be persuaded. They do not concern what exists in the mind of God. For most scientific questions the answer that across successive samples having a nice, random character the coefficient would be permanent is only mildly interesting. And even the standard of permanence (the disastrously named "significance level") is the result of a collective, human decision about what is persuasive. Science is human persuasion all the way down.

That statistical significance is only "mildly interesting" is not to say that it is not interesting at all. Occasionally an economist will have a genuine sample and because of its small size will have a genuine worry about the sampling problem. But mainly the economist's problems have nothing to do with sampling error. They have to do with other statistical problems (bias, for example: Leamer 1985) or, most commonly, with oomphelimity. And all these have to do with honestly persuading other economic scientists.

At this point I need to treat some objections:

[At first confident, the Significance Tester smiles indulgently]. "But statistical significance is an approximate test of what you call 'oomph'".

Educate me. Tell me how the permanence of an estimate over successive samples tells how important the variable is. To be sure, large coefficients will ceteris paribus have larger significance. But why not look directly at the largeness, and ask directly whether it is large enough to matter? Why be approximate and irrelevant when you can be precise and relevant? Why put the coefficient through an irrelevant transformation? Calculating a significance level will fool people into thinking they've solved the central intellectual problem, namely, how important a variable is to a human audience of economic scientists. But you know in you heart that the calculation can't do it. It must be done by us, the community of economists persuading ourselves: we must decide how large is large. God can't help, unhappily. We do not know what His standard of bigness or smallness is. Tables of *t* tell us how large is large *with respect to the permanence in sampling*. (Though they do not tell us what to take as the null hypothesis for the test; this, I repeat, is a question of human substance, not of statistics). The test does not tell us how large is large with respect to the economic argument in question.

[Worry clouds his face]. "Uh, sure. But statistical significance provides a good initial hurdle for the variables. They should at least be statistically significant. Those that survive can be tested later for oomph".

No. There is no reason to make a necessary hurdle out of a merely desirable quality – the quality, remember, of appearing to be permanent within such-and-such bounds, at least so far as sampling error is the problem, as it usually is not. Doing so would be like choosing academic colleagues "first" on the basis of their geniality. Geniality is a desirable quality, Lord knows, but not so desirable that it should head a list of lexicographically ordered "priorities". The proceduce would make it impossible to hire a brilliant person with a slightly sub-par amount of geniality. Anyway, for all the talk of "priorities" in public discourse, lexicographical orderings are irrational. The irrationality is worse, of course, because the "later testing" for other qualities is not in fact carried out. In actual, middle-brow econometric practice it seldom is. (See the papers cited earlier for some examples). Most economists pack up their statistical package and go home as soon as they find "significant" results "consistent with the hypothesis".

[Sweat oozes from his brow]. "Hmm. Gosh. But, come on, everyone does it. It has survival value in producing good economics. And someone who knows more about statistics than I do must have decided that it is a good practice. After all, the eccanned programs and all the papers in the journals are filled with it".

The argument here is from authority. Arguments from authority are not always wrong, though this one seems to be. I do not know why economists and other quantitative folk have misread their statistics books. It would make a good paper on the rhetoric of econometrics to trace the literature back to the authoritative turnings. (I record an impression that the reliance on significance tests for dropping and adding variables is not recommended in so many words by the textbooks, but in practice has figured more and more heavily as computers have become cheaper). One can merely quote authorities in reply, and note that the authorities who sharply distinguish statistical from substantive significance are of the best sort. Again I refer to the articles mentioned above and the works cited there: for instance, the article by William H. Kruskal (University of Chicago, past president of the ASA, etc., etc.) "Statistical Significance" in the *International Encyclopedia of Statistics* (1978); or the elementary book, *Statistics* (1978) by David Freedman, Robert Pisani, and Roger Purves (Berkeley, well-known statisticians, youngish turks, etc., etc.). The point has been recognized almost as long as statistical significance has been used. But only 3.14159% of economists seem to be aware of it (a short list would include some specialist econometricians such as Griliches and Leamer and a few amateurs such as Arrow and Mayer; I learned it from Eric Gustafson).

[He loosens his tie, sweat dripping from his nose]. "But there's nothing else to do. I want to use statistical procedures. What do you propose to substitute? How will I fill up my days?".

Fill them up with statistical calculations that are to the point. Find out what economic scientists consider to be a large coefficient and then see if your data show it. Do sensitivity analysis. Bend over backwards to see how robust your argument is. Encompass your opponent's model with your own, showing how his results follow as special cases of yours. Take collecting "data" seriously (the word means "givens"; we should prefer "capta", things *taken*). There's plenty of useful econometric work to be done (see Sims, Leamer, Hendry among others in econometrics and Mosteller, Tukey, Hogg among others in statistics) that does not rely on the misuse of statistical significance.

[He is shaking uncontrollably and his palms are wet. This is an unhappy would-be scientist. But after a time he calms, and a smirk spreads over his face. He has found a way out]. "Well, to hell with you, then. The misuse of statistical significance is *profitable*. As long as editors publish articles that misuse statistical significance I'm going to keep on submitting them. I've got a career to run".

Shame on you. The reply is immoral. However much some economists take their model of selfish behavior as a guide to action, it is immoral to lie, and a lying scholar is a contradiction in terms. That 96.8584% of editors fall into the group of economists who do not know the difference between statistical and substantive significance does not justify someone who does know the difference going on pretending she does not. Scholarship that depends on convenient lies will not last. To put it sharply, it is gradually becoming plain that the econometric work of the past quarter century that relied inappropriately on statistical significance (which is most of it, sadly) has got to be done over again. [The Significance Tester strides off, muttering to himself].

Note the rhetorical methods and result. One asks, "What is the argument here?". Unlike a philosophical analysis, the rhetorical analysis does not take the official rhetoric of the paper as candid. It finds unspoken argument in the turns of phrase and turns of logic, in the assumptions about which arguments matter to the scientific community and which do not, in the pragmatic context of the scientific text. The analysis embarrasses the claim that econometrics is a machine, free of human argument. A routine procedure of econometrics turns out to be mistaken on rhetorical grounds. It does not answer the question raised by the arguments of scientists: What's the oomph?

A rhetorical analysis does not attack econometrics, a point on which I especially do not want to be misunderstood. I am by profession a quantitative economic historian, using mathematics and statistics with pleasure (McCloskey 1987; and in practice McCloskey 1989). No reasonable person could object to making formal what is formalizable. Perhaps formality is not the only good, but it is at least one good. Contrary to humanistic opinion untrained in the mysteries, the use of mathematics and statistics is good. What is bad is an econometrics unaware of its rhetoric, falling therefore into arguments that are beside the scientific point.

How did it happen? It happened by adopting the values of the statistics department, in a way that the statisticians themselves would find distasteful. The men who put statistical testing on a formal basis – Jerzy Neyman, E.S. Pearson, and Abraham Wald – were emphatic that the persuasive way to run a statistical test is with an explicit loss function in mind. Wald pointed out that the loss function had to arise from the substantive argument at hand: "The question of how the form of the weight (i.e. loss) function... should be determined, is not a mathematical or statistical one. The statistician who wants to test certain hypotheses must first determine the relative importance of all possible errors, which will entirely depend on the special purposes of his investigation" (1939, p. 302). But the statisticians know that they cannot decide "the special purposes" of an investigation in economics or biology. Only the economists and the biologists can do that. The users of statistics cannot merely join the statistics department at the crucial step, and wash their hands of their own rhetorics. They cannot hand over to statisticians the most important of their rhetorical jobs, deciding how large is large.

2. *Existence Theorems are Useless, Too*

The other main formalism in economics is axiom and proof. It would be absurd, of course, to argue 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... 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). But I reaffirm my enthusiasm for mathematics in economics. No one wants to go back to a time when economists could not keep straight the difference between the shift of a curve and a movement along it.

But at the heart of axiom and proof as practiced in economics is a rhetorical problem similar to the econometric one – another adoption of foreing intellectual values and another failure to ask how large is large.

The problem is that economists have fallen in love with existence theorems. There *exists* a canopener, somewhere. Since the late 19th century and Léon Walras the chief such theorem has had to do with competitive markets. Walras saw that supply and demand curves could be generalized to sets of supply and demand curves covering all commodities; and he saw that the set of curves captured the economic idea that things depend on each other, directly and indirectly.

The mathematical question came to be posed, Can one presume that a particular set of curves has a solution? The question is one that would occur to a mathematician, not to an economist. Since real economies quite plainly do have solutions of some sort the question has been from the beginning mathematical in spirit. It is significant that 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 (that same) Abraham Wald. ("Commonly regarded" because, after all, Edgeworth did prove it for $N = 2$). The ultimate in existence theorems of course is the Arrow-Debreu theorem, in the 1950s proven by mathematically inclined economists.

From everywhere outside of economics except the Department of Mathematics the proofs of existence 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 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. (I am speaking of neoclassical economics; but in marxian economics a similar remark could be made: in a world lacking intellectual free lunches the general statement that commodities are made with commodities cannot be expected to yield specific answers to any question worth asking). 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 justification of the quest for existence is that, after all, we had better know that solutions exist before we go looking for them. One does not hunt a snark without proofs of its existence. The remark sounds reasonable at first, but not at second. Physicists have happily used the Schrödinger equation since 1926 without knowing whether it has solutions in general. The three-body problem in Newtonian physics is not known to possess 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 exists in general is irrelevant if the physical or economic or poetic question has to do with a particular finite case.

The way the mathematical rhetoric has been transformed into economic rhetoric has been to *define* the economic problem as mathematical. The notion of "equilibrium" in economics has been treated this way (Mirowski 1989). 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" exactly once, and it is not until the coming of mathematical values into 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. 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 even a stable solution). Naturally the result (which is about the equations, not about the economy) depends on the assumptions. The task in general equilibrium theory since the 1950s 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. As the mathematician Paul Halmos put it,

> If you think that your paper is vacuous,
> Use the first-order functional calculus.
> In then becomes logic,
> And, as if by magic,
> The obvious is hailed as miraculous (p. 216).

None of the theorems 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 no interest to quantitative intellectuals.

For instance, if markets do not exist in which people can buy or sell insurance or investment goods or some other important good, then evidently a competitive equilibrium may not be efficient. J.M. Keynes taught economists to imagine the disastrous equilibria that might result. Fine. Adam Smith himself would have understood the assertion. Interference in the market for some commodity – sugar, say, from Britain's 18th-century colonies – is bad. If nature or society interferes with trade in some commodity (insurance being a commodity) then badness will result. (Or, strictly speaking, might result, since anything is possible).

The problem is that such a general theorem does not relate to anything an economist would actually want to know. We already know that if the world is not perfect the outcomes of the world cannot be expected to be perfect. This much we know from 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. How large is large? 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 the information content of the existence theorems. The theorems say, to pick out one of their assumptions, that if "complete" markets exist then efficiency will be achieved. They do not provide the economic scientist with a scale against which to judge the significance of the deviations from completeness. Chewing gum or all investment goods: it does not matter for the proof.

The point can be illustrated by the most eloquent defender of existence theorems in economics, Frank Hahn of Cambridge. He asserts that the very scrupulosity of the Arrow-Debreu result has political consequences: "This negative role of Arrow-Debreu equilibrium I consider almost to be sufficient justification for it, since practical men and ill-trained theorists everywhere in the world do not understand what they are claiming to be the case when they claim a beneficent and coherent role for the invisible hand" (1973, pp. 14-15).

I would suggest on the contrary that the practical people, and even the ill-trained theorists, understand pretty well what they are claiming. Here is the Practical Person's Claim: that 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 no connection with Arrow-Debreu, except on the knife edge of exact results, where nobody lives. The Arrow-Debreu result is an exact result, containing no definition of the neighbourhood in which it is approximately correct. No wonder: the neighbourhood is a rhetorical issue, unexamined in mathematical economics.

Hahn grossly overstates its worth by identifying it with the Practical Person's Claim. The existence theorem is worth having, I suppose, though why exactly it is worth having needs to be argued more rigorously than it has been so far – a matter of rhetorical, not mathematical, rigor, but rigor all the same. The existence theorists have not been sufficiently rigorous.

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 been rhetorically successful, at least until the economists have realized 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 and the rational expectations revolution in macroeconomics. The economists have wandered off into a discussion of whether or not an equilibrium exists and what its character might be, qualitatively speaking. They have commonly ignored the only question worth asking: how big are the effects? They have not asked how big is big, at any rate in ways that would persuade other economic scientists. They have not taken the rhetoric of science seriously, and have retreated from the library and laboratory to the blackboard.

Another aspect of the problem was highlighted by Allan Gibbard and 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 philosophical and speculative, an apparently inconclusive exploration of possible worlds. To defend the excess of speculation over testing in economics journals Gibbard and Varian make an argument much heard 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). This sounds like what engineers do. 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 space 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. That is where the economist's fishing diverges from the engineer's. 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 trying to derive *qualitative* theorems; he did not spend any time with the facts. Maybe for all his many excellences Samuelson in this respect sent 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. 671-672).

Hear, hear. Yet in the next sentence they acknowledge that the degree of approximation of this rhetoric to economics is poor: "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, uneasily aware of how crushing their remark is, conclude lamely "but the pattern of explanation is, we think, the one we have given" (same page). Well, be quantitative. Within what neighbourhood of radius epsilon does economic theory, high-brow or low, approximate the quantitative procedures that are routine in physics, applied math, 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. When the economic historian Robert Fogel varies an assumption he plans to strengthen his economic case by biasing the findings against himself. When a rocket engineer cuts the safety seals of the space shuttle engine with a kitchen knife he also has an a fortiori plan in mind. What rhetorical description could one give of the dominant research program in modern economics? Overlapping generations of tenure seekers with asymmetric information and no long-term memory?

The rhetorical problem 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 departments of mathematics or statistics. Splendid. And some of all our best friends are mathematicians and statisticians. Capital. The problem comes when the economists abandon an economic question in favor of a mathematical one (or, as in the case of significance tests, a statistical one), and forget to come back to the Department of Economics. Questions of existence or questions that ring the changes on the mathematical object itself are of proper interest to mathematics, regardless of how remote from the world. But they are not of interest to economics unless they can be shown to bear directly on a dispute in economic science.

A much heard argument in favor of such mathematizing in economics is that the physical scientists act this way. We economist, as the physicists of the social sciences, are just following the practice of the high energy boys. The argument misunderstands physics. Physicists know more mathematics than economists do, but do not therefore adopt the intellectual values of the mathematicians. As Eric Livingston argues in *The Ethnomethodological Foundations of Mathematics* (1986, pp. 185-189):

> [I]n the reflective discussion... physicists' mathematical practices are predominantly spoken of as... those of professional mathematicians.... In practice, when they are actually engaged in their work, physicists have a much more circumspect regard for their derivations and mathematical descriptions.... The physicists' proof of the divergence theorem [for example]... has the interesting feature that it preserves the physical interpretability of the equations throughout the derivation.... [T]he relationship between mathematics and physics... is disengaged [by outsiders] from consideration of physicists' actual work practices.

The theoretical physicist Richard Feynman introduced some theorems in matrix algebra into his first-year class at Cal Tech with considerable embarrassment (1963, Vol. I, pp. 21-22): "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 that they prove". In physics the familiar spirit is Archimedes the experimenter. But in mathematics, and in economics, it is theorem-proving Euclid who paces the halls.

The problem, to repeat, is not the use of mathematics. I am not arguing against formal methods in economics. That would be silly. 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 tell how true. Surprisingly, in what they regard as their chief scientific work the economists do not, although when they come to advise on policy or reconstruct past economies they state bounds of error with wonderful skill. On the blackboard, where they spend most of their time, they forget to say how large is large, because they have taken over unawares the intellectual ideals of that admirable, excellent department in which existence is all important and magnitude is inconsequential. The economists are in love with the wrong mathematics, the pure rather than the applied.

It will be said that we economists are theorizing in order to fit the models econometrically. Later. *Pasado mañana*. After all, the parameters are a mere empirical matter. In what is believed to be an imitation of physics, the mathematical economists have made a pact with the econometricians to specialize: we will imagine unfittable models if you will imagine unuseable methods for fitting them. But the pact is a sham, and by now we all know it is. Econometric fitting of the models has been disappointing at best. Much ink has been spilt describing how economists might go about deciding whether rational expectations, say, fitted the American economy, if the data were good enough. But the actual fitting, in a manner than would persuade sceptics, has not been accomplished. The mathematics of existence theorems has no results; and neither does the econometrics that is supposed to make it into science.

Put it this way. What are the scientific results of general equilibrium? Unhappily, there are none.

It is not fair to fault the Department of Mathematics for the economist's love existence theorems. Even non-mathematical economists have always loved them. 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. The oldest question in economics, after all, is whether, as 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". Does vice map into itself, yielding paradise?

A modern student of the question, for example, 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 question Buchanan asks is, Can one state conditions under which a mass of selfish individuals would form into a paradise of cooperation, without compulsion? The question dates to Hobbes, who began his project of social analysis, it will be recalled, after reading a startling theorem of Euclid's.

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 exsistence theorem could be proven? Unless it concerns the relevant quantitative questions – *how* full of vice, *how* paradisical – the theorem will not enlighten economics.

Actually, the way the Hobbesian theorems are set up guarantee their irrelevancy. Hobbes and his successors begin with humans outside of a civil society, and ask whether the imagined humans will create one. But why would an economist care about such an exercise? Why would a hypothetical world deprived of fellow feeling be any more interesting than one deprived of speech or backbones or oxygen? Intellectuals of the 17th and 18th century delighted in hypothetical history, for which an imagined state of nature provided a backdrop. But we have by now real history and real anthropology. Humans are always already in civil society. That in some more or less arbitrary logic a state of selfish anarchy leads or fails to lead to civil society is irrelevant to the study of people who were raised as citizens. What a social scientist needs to know is how selfishness might affect an actual existing society, not whether it would yield a civil society out of an imagined counterfactual. The problem, again, is not the presence of logic or mathematics – plainly, systematic imagination will need them. The problem, as one can see clearly in these non-mathematical cases, is the strange rhetoric of existence theorems.

3. *The False Dawn of Formalization*

Formalization was supposed to end controversies in economics. It has not, which is a decisive test of the hypothesis that economic science depends on more than the official formalities. Robert Merton drew attention to the suggestiveness of agreement and simultaneous discovery in science. But disagreement and neglected discovery have lessons to teach as well.

The formalization of economics came out of a philosophical not a scientific program. The claims that significance tests and existence theorems can run our intellectual lives came not from economics as an empirical and speculative science but, as Keynes would have said, from madmen in academic authority distilling their frenzy from some mathematical scribbler of a few years back. Not surprisingly: a chain of philosophers from Plato through Hobbes to the younger Wittgenstein has believed that philosophy could be reconstructed on mathematics. That the trick has not been turned after 2500 years of trying does not discourage the philosophers one bit; but philosophers, bless their hearts, are more patient than empirical scientists.

As H. K. H. Woo has noted of economics, "It is a big mistake... to presume that the formal approach, which has proven... to be of... great service to man in making up for part of his cognitive weaknesses, is capable of ultimately supplanting man's other cognitive potentials and strengths" (p. 109). On the next page he quotes Whitehead, who was in a position to know: "Logic, conceived as an adequate instrument of the advance of thought, is a fake".

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No serious scholar wants to break the machines of science. When the argument in economics arrives at a matter of sampling or a matter of existence, then the economist had better know how to work them. Humanism provides a method of reading the manuals for the machines – call it "rhetoric" defined broadly. The method discovers that the official machines produce nothing like the whole argument. The whole argument, revealed by rhetorical analysis, depends on every way of speaking. It is merely childish to believe that working one lever of speech can say everything an adult in science needs to say.

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Summary

JEL 030

FORMALISM IN ECONOMICS, RHETORICALLY SPEAKING

Even formal methods in economics are "rhetorical", in the sense of "argued to other scholars, not proven forever and ever". The rhetoric of inquiry, in other words, is not confined to flowery language. Two examples of formal methods that have defective rhetorics in economics are significance tests and existence theorems. The defect does not arise from statistical or mathematical techniques as such. It arises from adopting the intellectual values of the wrong department: specifically, not the department of economics. A humanistic economics would not abandon mathematics, but would examine all the arguments, mathematical or not.

Riassunto

JEL 030

IL FORMALISMO IN ECONOMIA, DAL PUNTO DI VISTA RETORICO

Anche i metodi formali in economia sono "retorici", nel senso di "esposti ad altri studiosi con l'intento di persuaderli mediante il ragionamento, e non dimostrati una volta per tutte". In altri termini, la retorica dell'indagine non riguarda soltanto il linguaggio fiorito. I test di significatività e i teoremi di esistenza sono due esempi di

metodi formali in economia che presentano difettose retoriche. I loro difetti non discendono dalle tecniche statistiche o matematiche in quanto tali; dipendono piuttosto dal fatto che chi le impiega fa propri i valori intellettuali di un dipartimento sbagliato, e cioè di un dipartimento che non è quello di economia. Un'economia umanistica non dovrebbe certamente abbandonare la matematica, ma dovrebbe prendere in considerazione tutti i tipi di argomentazioni, matematiche o di altra natura.