Mathematical Formalism and Political-Economic Content

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March 23, 2010

Abstract

Human economic interactions spontaneously express themselves in the quantitative form of prices and transactions quantities. This makes it difficult to avoid quantitative reasoning in political-economic research altogether. Mathematical methods, however, are only one moment in a layered process of theory generation in political economy, which starts from Schumpeterian vision, progresses to the identification of relevant abstractions, the development of mathematical and quantitative models, and the confrontation of theories with empirical data through statistical methods. Mathematical formalism is subject to the "garbage in, garbage out" principle; its conclusions can have no more validity than the assumptions supplied to the formalism. Samuelsonian mathematical economics and its general equilibrium variants imported optimization techniques from statistical physics to the problem of studying of full-information allocation, but neglected to include theories of statistical fluctuations in the resulting models. This encouraged the "Samuelsonian vice" of modifying the relevant abstract problems of political economy to fit available mathematical tools. The role of empirical research in disciplining theoretical speculation, on which the scientific tradition's integrity rests, was undermined

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by specific limitations of nascent econometric methods, and usurped by ex cathedra methodological fiats of theorists. These developments systematically favored certain ideological predispositions of economics as a discipline. There is abundant room for New Thinking in political economy starting from the vision of the capitalist economy as a complex, adaptive system far from equilibrium, including the development of the theory of statistical fluctuations for economic interactions, redirection of macroeconomic and financial economics from path prediction toward an understanding of the qualitative properties of the system, introduction of constructive and computable methods into economic modeling, reconceptualization of the macroeconomy as a social coordination problem, and the critical reconstruction of econometric statistical methods from a Laplacian perspective. Interdisciplinary dialogue between political economists and researchers in substantively and methodologically related fields is essential to end the recent intellectual isolation of economics.

1 Sad truths

From time to time I encounter students with a keen critical interest in economics and a strong aversion to mathematics. These students tend to be interested primarily in the philosophical and historical aspects of economics and political economy, and are diverse in their ideological backgrounds: they range from radical Marxist critics of the capitalist economy to Austrian true believers in private property and markets. Most of the time, however, even when instructors and mentors encourage their intellectual aspirations and protect them from the extreme consequences of degree requirements, they discover through pursuing their own work that some element of quantitative or mathematical method is necessary for them to make progress. This is one of those unpleasant aspects of human life, such as the often-observed fact that practicing a musical instrument generally makes players sound better (or the observation that riskier portfolios on average make higher returns).

There are, of course, great models of political economic work that avoids explicit mathematics, including Adam Smith's *Wealth of Nations* (Smith, 1937), and David Ricardo's *Principles of Political Economy and Taxation* (Ricardo, 1951), but on closer examination these examples tend to confirm my basic observation. A close reader soon perceives Smith's avid interest in the sparse economic data available to him, and the almost geometrical structure of his underlying reasoning. Few readers of Ricardo survive the experience without wishing that he had formulated the core of his ideas formally, given how rigorous and axiomatic is the substance of his system. Ricardo's correspondent Thomas Malthus explicitly bases his theory of population (Malthus, 1985) on a "mathematical" principle. In the twentieth century Piero Sraffa (Sraffa, 1960) wrote his classic *Production of Commodities by Means of Commodities* almost as a Borgesian parody of an essentially mathematical argument shoehorned for stylistic reasons into a purportedly "literary" form.

It is not surprising that economics and political economy are so closely intertwined with quantitative and mathematical methods. The social interactions human beings enter into in the course of production, distribution, and exchange take a peculiarly direct quantitative form through prices as ratios in exchange and productive transformation. We can certainly count and analyze other aspects of human social life quantitatively, as we do when we collect statistics on suicides or fertility rates, but in these cases it is the social scientist who generates the numbers, while in economic transactions the numbers are an irreducible aspect of the social interaction itself. A good case can be made, in fact, that economic interactions have as often been the source of mathematical invention and development as astronomical speculation or "pure" thought. Geometry originated in problems of land surveying for taxation in ancient hydraulic societies. Algebra and arithmetic are frequenters of the bazaar. Double-entry bookkeeping contributed important features to physical conservation laws and thermodynamics. Social statistics were one inspiration for statistical mechanics. In my lifetime game theory has developed from a somewhat dubious and moribund branch of industrial organization to a vigorous dynamic mathematical theory of evolution in biology and related fields.

It does not make sense, however, to think of economics and political economy as essentially mathematical sciences, or as branches of applied mathematics. Economic interactions, quantitative as they are, arise from the same deep cognitive and emotional springs as the rest of human life. As "animals of the community" in Aristotle's terms, humans also create a complex web of relationships as they go about their economic business. The economy as a whole is what we have come in the last fifty years to think of as a "complex, adaptive system, far from equilibrium", a system characterized by an astronomical number of combinatorial degrees of freedom. As Karl Marx (another "literary" figure whose notebooks reveal a vigorous interest in quantitative models and results) explains, our chief tool for dealing with such systems is *abstraction* (particularly when the scope of experimental methods is limited due to the scale or historically unrepeatable nature of the phenomena under study). Abstraction starts with what Josef Schumpeter called a "vision" of the economy, which is expressed as a simplified account of a basic feature of economic processes. For example, the classical political economists, observing the turbulent character of market prices in particular times and places, devised the abstract notion of a "natural price" around which market prices fluctuate or "gravitate". Their abstract vision posed the question of how decentralized movement of capital and labor under conditions of production would regulate natural prices, and what patterns of accumulation would result. Neoclassical economics posed a similar abstract question from a somewhat different point of view, the question of what equilibrium allocation of privately owned scarce resources would result from competitive market interactions, and what would be the welfare implications of those equilibria. These abstractions are far from realistic, but when they are appropriate approximations to complex reality they can offer surprisingly deep and illuminating insights. In some cases abstract representations of economic interactions raise interesting mathematical questions at their own level, such as the stability of the dynamics of the processes they suggest. Unfortunately the human mind is all too prone to reify abstractions, and substitute the abstract world for the concrete phenomena the abstraction is intended to illuminate. The great political economists had a reliable ability to see the abstract in the concrete and the concrete in the abstract without losing track of the difference in levels of thought involved.

If mathematical and quantitative considerations are an unavoidable aspect of economic reasoning, they are only one moment of a much more layered and sophisticated process of theoretical development. In a very stylized sketch, we could think of this process as involving Schumpeter's vision, the clear statement of corresponding abstract systems, the logical and mathematical investigation of the properties of these abstract systems, the identification of abstract concepts with operational observables, and the confrontation of the resulting elaborate conceptual system with empirical data. Some moments of this process are well-adapted to mathematical or statistical reasoning; others are inherently conceptual, social, historical, or philosophical, and require sophisticated critical examination using non-mathematical tools. The neglect of the conceptual and critical side of political economy leads to just as much error and confusion as the refusal to formulate problems in mathematical or statistical form when they present themselves in that guise.

The often-quoted exchange between Bertrand Russell, who said he gave up economics because it was too easy, and Max Planck who found it too hard illustrates the subtlety of the situation. Russell was thinking of the abstract marginalist model of economics as the solution to a constrained maximization problem, which he found mathematically trivial; Planck was considering economics as a serious attempt to understand a complex dynamic system, which he found formidably challenging.

2 Garbage in, garbage out

Current generations' experience with information technology systems has made us acutely aware of the problem systems programmers refer to as "garbage in, garbage out". Mathematical and statistical methods, no matter how carefully they are crafted and how deep the insights on which they are based, cannot yield any better results than the problems and data that are input to them. If, to take a relevant example, one uses sophisticated mathematical methods to analyze a complex adaptive system far from equilibrium under the prior assumption that it is an equilibrium system, the sophistication of the mathematics is not going to correct the fundamental conceptual error. Inherent in the application of mathematical methods to economics is the risk of what I will venture to call the "Samuelsonian vice", the temptation to change the formulation of the abstract problem to fit the mathematical tools available rather than to seek mathematical tools that are appropriate to the actual problem at hand.

Samuelson's initiative to re-found economics as a mathematical science was an astounding success in terms of academic politics, but in retrospect looks increasingly flawed as a research program. Samuelson's vision saw economics as a constrained optimization problem, and his genius was to adapt a subset of the powerful arsenal of tools developed by mathematical physicists to attack constrained optimization to an economic context. He did this in a much more subtle and critical manner than many other would-be translators of physical methods into economics, who too often wind up merely re-labeling physical models with economic categories through defective analogies. Nonetheless, there are puzzling lacunae in Samuelson's cross-disciplinary lexicon. For example, the optimizing methods he imported into economics were mostly developed as a branch of physical thermodynamics, which neoclassical economics has a family connection to (Smith and Foley, 2008). But Samuelson's translation omits the specifically statistical character of physical thermodynamic reasoning, an omission which is all the more surprising because statistical fluctuations and regularities appear in very important ways in economic data, including wealth and income distributions, firm and city size distributions, and asset price movements.

Samuelson's intervention also benefitted from the fallout of the "socialist calculation" debate of the nineteen-thirties. From our current point of view, what is striking about this debate was that the participants (with the exception of Hayek) shared the view that capitalist and socialist economies were addressing the same economic problem, namely the full-information allocation of scarce resources. Thus the market system was idealized by both the socialist and anti-socialist sides of this debate as a zero entropy, achieved equilibrium system in which information was free. In this setting the differences between socialism and capitalism are reduced to details of decentralization and computation. The unintended consequence of the debate was to make mathematical economics ever more confident of the relevance of optimization methods as a general approach to economic theory.

The consequences of Samuelson's approach have been far-reaching in economics and economic education. For one thing, economics graduate students, though they spend an enormous part of their curricular time on mathematical techniques, often learn a rather idiosyncratically selected subset of mathematical topics. Economics graduate students, for example, are likely to know more about topological theorems which have little direct bearing on real economic problems outside collective choice than about thermodynamic theories of approximation and fluctuations, which do have a direct bearing on economic phenomena. Where entropy maximization plays a central role in physicists' understanding of dynamic complex phenomena, economists are pointed toward stochastic optimization theories. Economics comes to a vision of the economy as an exact optimizing process rather than as a chaotic, self-organizing process that approximates orderliness.

Samuelson's way of introducing mathematics into economic thinking and education also left out what seems to me to be a critically important aspect of statistical physics, which is its ability to establish direct quantitative links between simple models and observed data. Empirical observation and theory are unified methodologically in statistical physics to a far greater degree than in contemporary mathematicized economics, where models tend to be seen as indirect inspirations for econometric specifications, rather than as sharp constraints on the interpretation of data.

Thus economics has been shaped in ways that make it vulnerable to misleading illusions and interpretive errors. The abstract problem of general equilibrium (which arises plausibly from considering some aspects of interactive economic phenomena) is prone to become reified as an accurate picture of a much more complex economic reality. Theories based on mathematical optimization that address issues raised by the general equilibrium vision flourish and are elaborated in ever-more sophisticated mathematical contexts. Econometric tests of these theories are based on highly indirect specifications which depend to an unacceptable degree on maintained statistical assumptions and the adoption of particular statistical methods. The whole enterprise is carried on in an echo-chamber in which economists talk to each other with very limited critical input from other disciplines. The discourse is finely calibrated to pull the wool over the eyes of academic administrators and interdisciplinary review committees.

Let me hasten to qualify this characterization, which is more of a parody than a nuanced portrait. For one thing, these ills, which are quite important in some subfields of economics, particularly macroeconomic modeling and finance, have moderating counter-tendencies in other subfields. For example, there has been a vigorous input of psychology into the shaping of modern theories of economic behavior despite stubborn resistance of traditionally-minded economists to overwhelming evidence contradictory to the homo economicus model of rationality. Much applied microeconomics is indistinguishable from applied sociology in using more or less reliable statistical techniques to draw what appear to be supportable inferences from cross-section data. The impact of evolutionary thinking on economics (and of economics on biological evolution) has created important sub-discourses in economics that avoid the dogmas of optimization and reified rationality. Economic history and the history of economic thought have fought an honorable rearguard action despite the threat of their practical elimination from the graduate curriculum.

3 Quis custodiet custodes ipsos?

The Western scientific tradition as it has developed since the Renaissance depends heavily on the principle of empirical verification and falsification to discipline the speculative excesses of theory. Economics has a rather ambiguous connection to this tradition. On the one hand, political economy is a policy science, and has been from its origins. Thus it is supposed to pay its way in social terms by having something practical to say about public finance, trade policy, control of externalities and the like, which it cannot very well do without studying the specifics of these problems and the institutions that are involved with them. On the other hand, economic theory has not had very good luck with direct empirical verification of its abstract theories. As long as data is absent economists are happy to fill in the gaps from prior reasoning, but the appearance of economic data generally presents a picture of forbidding complexity, in which theoretical regularities are buried in exceptions, qualifications, and measurement noise.

The role of empirical verification in the mathematized economics of the Samuelson era was supposed to be played by econometrics, a field, curiously enough, in which Samuelson himself rarely, if ever, worked. With some economic data traditional statistical methods work very well. I remember Richard Ruggles showing a slide of what appeared to be a perfect bell curve in a talk, and remarking that it was in fact a visualization of real census data points. With macroeconomic time series, however, several problems gang up on econometrics to make life very difficult.

First, although it may appear that there are a lot data points in macroeconomic time series, a little acquaintance with the data shows that it is highly "autocorrelated", which means in practical terms that the amount of independent information is much smaller than the number of measured data points. For example there may have been twelve or thirteen separate business cycles since 1929, which suggests that at business cycle frequency the effective statistical sample size is only on that order, which greatly limits the power of any statistical methods to find reliable regularities.

Second, econometrics was born under some unlucky scientific stars. Though economic theory (of all schools) bristles with inherently nonlinear relationships, the dominant and best-developed statistical methods available in the first decades of serious econometric investigation firmly rested on linear specifications. Linear regression is best adapted to understand equilibrium systems undergoing small perturbations from stable equilibrium configurations, a situation where the small size of the variations makes the assumption of linearity plausible. This is emphatically *not* a good description of macroeconomic fluctuations in industrial capitalist economies. The particular "frequentist" philosophy that dominated econometric thinking and teaching frames statistical inference as a problem of "estimating" models, and generally evaluates procedures by their "asymptotic consistency", that is, their theoretical performance with unboundedly large samples from "stationary", that is, essentially, unchanging repetitive experiments. This is cold comfort for a science where data points are scarce, and a recipe for disaster in macroeconomic research where history is in principle unrepeatable. Early econometric theory remained blissfully innocent of the problem of over-fitting limited data with excessively parameterized specifications, so that its methods tended to confirm pretty much any theory whatsoever. The absence of any theory of fluctuations in the optimizing mathematics that underlies the various flavors of general equilibrium theory means that theory itself offers no guide to the statistical specification of econometric models, further opening the floodgates to whatever method supports the point of view of the investigator.

The wild-west character of econometric investigations into macroeconomic problems from the 1940s to the 1970s led to a swing of the pendulum, exploited by the advocates of "rational expectations" macroeconomic modeling, in which the traditional scientific role of empirical confirmation in policing theoretical speculation was usurped by philosophical/theoretical general principles. The filter for publication of macroeconomic research became, not the ability of the theory to explain real features of the data (of which there wasn't that much to begin with), but the fidelity of the theory to strictures of modeling purity announced ex cathedra by leading senior authorities. The test of the ability of theories to explain data, which was never very strong in macroeconomics, was further watered-down to ad hoc procedures such as "calibration", which have essentially no protections against over-fitting built into them.

The problems of financial economics are somewhat different. For one thing, there is a lot of data available on financial transactions. Financial economic research is dominated more by "financial engineering" than by the demands of economic policy makers which preoccupy macroeconomic discussion. Financial engineering faces the test of direct market success in generating profits for its constituency, rather than the indirect tests of scientific explanation of observed data. The direct quantitative form of financial data and its relatively high temporal frequency are a great temptation to apply statistical and mathematical modeling methods from other fields. There are, however, two big, related, problems in analyzing financial phenomena with conventional mathematical and statistical tools. From a technical point of view financial data is not stationary; the dynamic patterns we observe undergo sharp changes in different historical periods, so that the inductive generality of statistical measures is doubtful. This reflects, in my opinion, the reality that financial markets are a part of a complex, adaptive system far from equilibrium. Our limited understanding of such systems suggests that they frequently manifest *long-range correlations* in temporal and other dimensions. These correlations arise because the surface phenomena we are measuring is the result of complex institutional and historical patterns beneath the surface that are difficult to observe directly, but have fundamental influences on the dynamics of the system. For example, the statistical distribution describing financial institutions balance sheets is not easy to recover from publicly available data, but can have, as we saw in the 2007-9 financial crisis, enormous impacts on the behavior of market prices. It is because financial markets are a part of a system far from equilibrium that their statistics fail to be stationary. Ultimately non-stationarity is a signal that we are observing the outcomes of self-referential human actions, with all the paradoxes of self-fulfilling expectations and strategic manipulation built into them. The garbage in problem for finance is the assumption that markets are universally liquid and competitive, which at one stroke supposedly renders the data amenable to a wide range of mathematical and statistical modeling methods and systematically blinds the analyst to the very real phenomena of large, unanticipated fluctuations. Thus the problems of financial analysis are perceived as “fat tails” rather than the macroeconomic malady of poor “out-of-sample fit”. The MBA executives with their limited mathematical and statistical expertise had no better luck in penetrating the mystification of quantitative finance, however, than academic administrators and tenure review committees have had in leavening the monolithic dominance of macroeconomic orthodoxy.

Let me hasten to acknowledge that these problems have not gone unnoticed nor unaddressed by econometricians. Frequentist time-series techniques were souped up with rule-of-thumb information criteria intended to protect against the grosser forms of over-fitting. Tests for nonlinearity, and procedures for estimating nonlinear perturbations of linear models have been developed. In the hands of talented practitioners gifted with common sense, even the rather ramshackle procedures of frequentist statistics will yield plausible analyses of macroeconomic time series data. Mostly what these types of studies show is that there isn’t enough information in macroeconomic time series data to draw more than a few solid conclusions, most of which were known even before the advent of formal econometrics to informed observers such as Keynes: macroeconomic quantity data is dominated by trends; the main deviations from trend in all series reflect a common business-cycle component; over the business cycle the labor market does not work according to simple supply-and-demand principles. In many ways, it seems to me that macroeconomics as an academic subfield knows *less* about the real dynamics of industrial capitalist economies today than it did in the early nineteen-sixties when I began my studies of economics.

4 Ideology

One could view this story as an account of an aberrant episode in the history of science, and argue, with considerable plausibility, that over a long enough historical perspective corrective mechanisms built into science will come into play in economics. The convergence of limited mathematical perspective, narrow range of modeling strategies and weak disciplining of theoretical speculation by empirical data are a kind of perfect methodological storm, which will blow over sooner or later, given the inexorable pressures of scientific process. Other sciences, even physics, run into similar heavy weather, particularly in addressing problems where data is thin and hard to come by, such as cosmology before the discovery of the microwave echo of the Big Bang, or the particle physics of energies beyond the reach of human experimental manipulation. After all, the problem of macroeconomic fluctuations in industrial capitalist societies is inherently difficult, and we should not be surprised that scientific efforts in this field remain flawed, and have reached a limited level of understanding. I think, however, that this understanding of the state of economics leaves out a crucial aspect, without which our view of the situation will remain distorted and limited, namely ideology.

Let me hasten, even a third time, to clarify this remark. The word "ideology" has a dubious status in scientific discussions because it is associated with a kind of ad hominem name-calling. When one side of a debate accuses the other of being "ideological" in thinking or motivation, the implied claim is that the accuser's position is "scientific" or "value-free". Stalinist Marxists and neoclassical economists in my experience are both rather attached to this tactic, just to name two examples. This is not where I am going. I have argued in my book *Adam's Fallacy* (Foley, 2006) that values or ideology, or whatever term you prefer to invoke attempts to persuade and motivate people to action, are built-in to the economic project as part of its DNA, so to speak. Schumpeter's category of vision is incomplete without this dimension. Whatever knowledge we have of economic phenomena and the political economic side of human social life comes intertwined with ideological roots, which influence the way problems are framed, the problems addressed, the type of solution that is proposed as acceptable, and the data and conceptual tools brought to bear. Economists are certainly aware of the omnipresence of ideology in their discourse, though in my opinion they would do better to acknowledge it openly rather than retreat to strategies of denial or prevarication such as the distinction between "positive and "normative" economics. It is striking that this flimsy language, which seems to satisfy so many otherwise critical and powerful intelligences engaged in economic discourse, comes from Milton Friedman, whose work expresses a formidably consistent ideological drive.

If we look at the methodological failures of economics addressing the problem of macroeconomics through the spectacles of ideology, several otherwise inexplicable features of the story form a much more coherent pattern. Samuelson's import of the optimizing mathematics of thermodynamics as the foundations of a mathematical economics without including statistical theories of fluctuations contributed to an economic vision of market interactions smoothly implementing social goals. As I have argued in Foley (2010) the suppression of the irreversible aspects of market interactions in finding Pareto allocations also systematically suppresses the study of redistributions of economic surpluses inherent in decentralized market interactions. Looked at in this way, the market appears to be a "neutral" mechanism for achieving economic efficiency through the universal discipline of the price system. The econometric catastrophe of "Keynesian" macro-models of the 1970s was intimately connected to the pressures on economists to find a "neutral" presentation of the conflict over distribution that underlay the inflationary stagnation of the period. The sudden triumph of rational expectations theories rested in an important way on their presentation of macroeconomic policy, particularly monetary policy as "neutral", that is, by implication, without impact on distribution. From this point of view it is easier to see why the balance of power in disposing of theoretical speculation shifted from econometricians to the theorists themselves. Even relatively crude examination of macroeconomic data reveals the central importance of distribution (between wage and non-wage incomes, for example) in the dynamics of business cycle fluctuations. To those who share the tendency of neo-liberal economic policies to accept and encourage increasing inequality in the distributions of wealth and income, research on the actual dynamics of wage setting and its relation to unemployment is as irrelevant as it is unwelcome.

This is emphatically not to argue that other points of view in macroeconomic analysis are free of ideological motivation. Those economists who see business fluctuations as an expression of inherent instabilities in market organization, and who focus their attention primarily on the distributional dynamics of the business cycle clearly start from their own values and vision. As you can imagine, no matter how much they share an interest in the details of financial institutions or macroeconomic fluctuations it is not easy for people who instinctively see the capitalist market economy as a miracle of efficiency, freedom, and coordination of information to interact constructively with others who regard it as an exploitative, unstable, irrational, historically limited system. I hope the story of economics, however, alerts us all to the pervasive influence of ideology in the choice and framing of scientific problems, as well as in the systematic strengthening and weakening of various elements of scientific method and procedure.

5 New economic thinking

The reason to rehearse all this controversial and contested history is to clarify the nature of the situation an Institute for New Economic Thinking confronts.

On the positive side, the story I have sketched here confirms that there are enormous opportunities to introduce constructive new ideas into economics, perhaps particularly, but not exclusively, in the fields of economic theory, macroeconomics, and finance. In my personal opinion, these new opportunities are all connected in one way or another to the vision of the economy as a complex, adaptive system far from equilibrium. As I have argued in my Schumpeter lectures (Foley, 2003), I believe that this change in point of view would return economics to its most fertile and intellectually challenging roots in the work of the classical political economists, and to a philosophically more open and dialectical view of the economy as an aspect of human social life.

At the level of abstract theory this vision implies at least a reformulation of the problem of economic equilibrium and allocation to include a theory of fluctuations, and the adoption of a view of equilibrium consistent with, if not slavishly imitative of, the notions of statistical equilibrium in common use in thermodynamics and other sciences. This line of thinking offers the prospect of unifying theories of allocation and distribution in a way that can incorporate the strong statistical regularities we see in economic distributional data.

We now know quite a lot about the implications of a full-information general equilibrium analysis for macroeconomics through the examination of dynamic, stochastic, general equilibrium models. It seems to me that this effort has led to two very important results. First, this type of modeling by assuming complete, liquid, and competitive markets omits critical aspects of real monetary-financial economies that have important impacts on their overall stability and performance. Second, the DSGE episode, regarded as the last phase of the bad old Keynesian macroeconomic modeling program of the 1960-80 period, should prompt a fundamental re-thinking of the methodological goal of macroeconomic modeling. Is the goal of a macroeconomic model to help us understand the general type of dynamics that a complex, dynamic system can exhibit, or to emulate or simulate the specific dynamics of the particular (largely unobservable) complex dynamics of the current economy? I personally suspect the second goal is unrealistic and self-contradictory, and that our modeling effort will be much more productive of insights if it is redirected along the lines of the first alternative.

Current mathematical economics inherits from its Samuelsonian origins (seasoned with Bourbakist axiomatics) an unquestioned tendency to represent economic magnitudes as real numbers and to accept non-constructive methods of proof for key propositions. These may seem at first to be very esoteric issues, but in an age where we depend increasingly on computational power to manage enormous streams of data the desirability of framing economic theories in computable, constructive and even algorithmic form is increasingly evident (see Velupillai, 2004).

One unfortunate side-effect of the preoccupation of the profession with DSGE research has been the slow development of macroeconomic models based on the idea that social coordination problems are central to macroeconomic dynamics (see Diamond, 1982; Cooper and John, 1988). This theory starts from the assumption that mass market interactions inherently produce important *externalities* that link the behavior of the interacting agents outside their market transactions. Keynes' famous picture of asset markets as a beauty contest in which the goal is not to choose the prettiest face but to predict the choices of the other contestants is an early and canonical example of this approach. We know from work already done that strategic complementarities of this type produce complex dynamics in the form of multiple and unstable equilibria, so that abstract models of this phenomena offer the prospect of understanding the market-level roots of macroeconomic fluctuations. This line of thinking also has evident relevance to understanding the dynamics of financial markets.

I believe there are promising approaches to addressing the problems of econometrics as well. A re-orientation of econometric theory and teaching to the basic logic of inverse probability developed in Laplacian and Bayesian statistical theory would have several salutary effects. We know that methods based on Bayesian priors (however they are constructed) are the only methods of statistical inference guaranteed to be free of potential contradiction (Jaynes and G. Larry Bretthorst (ed.), 2003). Many commonly used frequentist methods in many cases can be shown to correspond to particular priors, but others appear to be ad hoc rules of thumb that are likely to fail unpredictably. The general framework of inverse probabilistic reasoning widens the statistical horizons for researchers in encouraging them to think more broadly about the many ways data can speak to particular hypotheses beyond the procedures enumerated in statistical cookbooks. The inverse probability method also has built into it both mechanisms for detecting over-fitting and signals (in the flatness of posterior distributions) that alert us to the inability of available data to resolve questions due to the lack of relevant information. The increasingly widespread use of nonlinear and semi-parametric approaches in econometric investigations has revealed a wide range of robust relationships in economic data that linear regressions methods have left obscure. As in the case of theory, it seems to me, however, that a radical, critical re-thinking of the goals of statistical work in macroeconomics and finance is necessary. Econometricians cannot solve the unsolvable problem of predicting the behavior of essentially self-referential systems that arise out of complex human interactions. When historians of science look back on the late twentieth century in economics I suspect they will view macroeconomists' preoccupation with the "correct" specification of expectations something like the medieval effort to find philosophers' stones or the Holy Grail. But there are philosophically valid goals of statistical investigation that avoid these pitfalls.

Philosophers, psychologists, sociologists, anthropologists, biologists, physicists, mathematicians and historians have contributed in major ways to our understanding of economic phenomena in the past, both through substantive and methodological interventions. It seems to me that an attempt to foster new thinking in economics must also nourish, strengthen, and re-vitalize these interdisciplinary connections. Continuing dialogue with informed and interested scholars and scientists working on related problems in other disciplines is one the best guarantees of open, critical debate within economics.

It is not an easy project to change the direction of a well-established academic field like economics, or even to make a significant mark on research activity. The sociology of academic scholarship has evolved to protect the autonomy and self-regulation of disciplines to a high (I would say excessive) degree. It would be a mistake to limit the ambitions of the Institute for New Economic Thinking too narrowly, and particularly to focus disproportionately on the specific flaws of recent work in macroeconomics and finance. I have argued here as forcibly as I can that it is a serious error to indict mathematical thinking or the use of mathematics per se as the source of these flaws. What economics needs is not more or less mathematics and statistics, but mathematics and statistics better adapted to its problems and its limitations. In the long run the discipline of economics will be shaped as much by its sociology and the philosophy of science and scientific interchange that commands its consensus as by particular methods or theoretical approaches.

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