

American Academia in Transition: Fifty Years, Four Disciplines

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Economics -- The Current Position

David M. Kreps is Paul Holden Professor of Economics at the Graduate School of Business at Stanford University.

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IN THE FIFTY-ODD YEARS SINCE World War II, economics has undergone a substantial transformation. Before the war the discipline was defined by the subject matter it encompassed, i.e., things connected with prices and markets. But the tools and theories used to study, say, international trade bore only scant resemblance to those used to study labor markets. In the two decades that followed the war, this largely changed. Mathematical modeling rose to preeminence in economics, and a sparse set of canonical hypotheses—Robert Solow has characterized them as greed, rationality, and equilibrium—became the maintained hypotheses in almost all branches of the subject. At first, this process was accompanied by a narrowing of the issues economists were willing to address; the detailed study of economic institutions (beyond prices and anonymous markets) fell out of favor. But beginning in the mid-1960s, technical innovations and increased access to data broadened the scope of greed, rationality, and equilibrium. The issues addressed by mainstream economists broadened, and continues to broaden, in consequence.

Economists do not form a monolithic army. There have been and continue to be those who decry mainstream tendencies. Greed, rationality, and equilibrium have each been the target of well-shot arrows by well-regarded scholars. But economists by and large have steered clear of what Jim March has affectionately termed the "saloons of sociology," both fearing the anarchy that (doubtless) reigns there and cherishing how much has been learned by pushing

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ahead with the canonical principles.¹ Indeed, what trade in ideas there has been between economics and the other social sciences has largely been one way, through missions established to sociology, political science, and the academic discipline of law. (Previously established missions to the fields of academic finance and accounting have led to an almost total conversion of the native populations.) Economists may scoff when unconverted scholars in the other social sciences attribute to us many of the aspects of a religious imperium, but this has been a pretty accurate analogy.

While the missions to new contexts continue, however, strange things are happening at the core. As a result of increasing access to both experimental and field-based data, pressure from important constituencies, and the desire to have something new to work on, the canonical principles are under attack in our nearest approximation of high temples—journals such as *Econometrica*, the *Journal of Political Economy*, and the *American Economic Review*. These attacks have been resisted to some extent by the faithful, but they have increasingly come from previously conformist supporters of the canon. To date, no one has been anathemized. Indeed, some of those who previously were viewed as heretics have been welcomed back into the congregation.

It is not clear where this internal revolution is leading. It could end in schism, with a number of powerful surviving factions and a breakup of the old imperium. The old monolith could arise to expel the apostates. Economics might finish stronger, as overly restrictive canons are replaced by weaker but still workable principles that are fitted empirically to specific contexts and situations. While I do not have the crystal ball needed to predict what will happen, I will close with some hopeful speculations.

QUALIFICATIONS AND METHODOLOGY

Before fleshing out this thesis, let me be clear about its subjective nature. Economics is a broad and diverse discipline, and I occupy the fringe of a wing of the subject: To most people, economics means the (mis)behavior of the macroeconomy, both national and global; I am a microeconomist, whose concerns are much more the (mis)behavior of single consumers, firms, or industries. Economics, at least at its best, is empirical; I am a theorist, who has never

run a regression or systematically collected data. Finally, economics is driven by a core of ideas that are typically taught to first-year graduate students. I missed all that; my undergraduate training was primarily in mathematics, and I studied applied math as a graduate student. And I continue to miss this inculturating experience from the other side; I am employed by a business school and spend my time teaching aspiring managers. Indeed, my place of employment probably warps my view of the discipline considerably. Thus for me to address the current state of the discipline of economics requires me either to report on what (I think) is the mainstream view—and probably get it wrong—or to provide a much more personal view of the matter, and thereby give a misleading picture of what most economists think about these things.

Believing that the latter is better but still unsatisfactory, I have consulted approximately twenty colleagues worldwide. (Acknowledgments come at the end; let me apologize here for my bastardization of their opinions.) Of course, my contacts in the profession tend to be, like me, microeconomists and theorists, although I did include a few macroeconomists and others whose focus is more empirical.

From the perspective of this essay, the results were helpful primarily in a negative fashion. It is an understatement to say that there is no consensus; those queried gave diametrically opposed responses, both in general and to specific questions. The exercise did help me refine my own opinions, which I proceed to give. But the reader should be clear that 1) my report will be skewed towards the experiences and perceptions of a microeconomic theorist who works at a business school; 2) the opinions expressed are my personal opinions; and 3) I am aware of widespread disagreement in the field both as to the substance of the assertions I will make and about the current state of economics.

Indeed, an important weakness of this essay and in its author's qualifications is that I have virtually no training or background in macroeconomics, which most laypersons (and many economists) regard as the main branch of the discipline. I will write primarily about the state of microeconomics today; while I have and will briefly offer some opinions about the recent intellectual history of macroeconomics, they are formed largely out of prejudice and hearsay.

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THE MATHEMATIZATION OF ECONOMICS

There is no question that economics underwent a revolutionary change around a half-century ago; mathematical modeling, a small piece of the subject until the 1940s and 1950s, became the all-encompassing (some would say suffocating) language of the discipline. Mathematical modeling and formal deductive logic have been a part of economic theory for much longer than that, but their embrace by the mainstream of the profession came in this period. For very persuasive evidence on this point, consult Gerard Debreu's 1989 Presidential Address to the American Economic Association . 2

The embrace was so complete that, as a neophyte economist in the mid-1970s, I had little exposure to less mathematical, more verbal treatments of economic questions. "Little" does not mean "none"; in the one macroeconomics course I took, we read both Marx and Keynes's *General Theory*, neither of which is exactly overrun with equations or symbols. But the teacher in this course assumed without the slightest hesitation that to understand these books, it would be extremely helpful to reduce the theories to systems of equations (which he provided, and which did help, at least me).

No single book had canonical authority in 1975, but from my perspective, Debreu's *Theory of Value* came the closest.³ Debreu's classic develops the general theory of (market) value—what determines how much each item costs in the marketplace, including an analysis of why markets are efficient⁴ in the setting of general equilibrium (all markets considered simultaneously)—in 114 pages. To read this, one needs a fairly high level of mathematical sophistication—although Debreu begins by defining the notion of a set, it would be hard to learn the requisite mathematics for the first time from his synopsis—but the economic development is self-contained.

The book was and remains a powerful advertisement for deductive reasoning in economics. Beginning with assumptions about what drives the market behavior of individuals and firms, one proves formally that an overall market equilibrium exists and is "nice" in certain very precisely defined ways. The book contains important extensions of the basic model; while the analysis is

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conducted statically, as if all economic activity took place in a single instant, it points out how this can be extended to economic activity that involves time, uncertainty, or both. Further, because of its resolutely deductive nature, it allows the reader who objects to the conclusions reached to trace her objections back to basic assumptions made. For example,

the efficiency of an economic outcome depends crucially on the assumption that each consumer derives satisfaction entirely from his own private consumption; he does not compare himself to his neighbors, nor does the activity of his neighbor disturb or improve his own well-being.

I should be clear that *Theory of Value* was canonical in terms of basic principles or laws. Its formal setting and its reliance on pure deduction was not embraced as a workaday methodology by the majority of the profession; most economic analysis relied more heavily on special assumptions drawn from something between careful and casual empiricism. (For a description of how most economists use mathematics and models, see Solow's essay in this volume.) Also, because of its very abstract and general level, *Theory of Value* lacks some techniques—e.g., methods of comparative statics—used to study more specific contexts. But the principle of rigorous deduction within the framework of a mathematical model, as well as the basic tenets of *Theory of Value*—purposeful behavior by firms and consumers, and price-mediated market equilibration of those behaviors—were widely embraced as economic canon law.

The allure of mathematical modeling and reasoning in economics comes first and perhaps foremost from the power it gives us to define our terms (say, "efficiency") precisely and unambiguously and to show that certain precise assumptions lead to other precise conclusions. It also allows us to stretch our analyses and to unify them; once we have worked our way through the logic that assumptions A imply conclusion X, we may see how assumptions A' lead to conclusion X' by the "same basic argument." It allows us to appreciate how critical are certain (often implicit) assumptions: If A leads to X, but a slight change in A to A' leads to not X, then we can appreciate that X or not X depends on the seemingly slight differences between A and A'; hence X is not a very robust conclusion. Taking these logical deductions back to the real world, where the satisfaction of assumptions A or A' is a matter of some contro

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versy, our developed intuition concerning what assumptions lead to which conclusions, together with a sense of how closely the real world conforms to A or A', gives us the courage to assert that X will or will not pertain with very high probability.⁵

Debreu points to another explanation for why deductive reasoning has allure to economists. This concerns our (relative) lack of controlled data. In the absence of data with which to

test our theories, if we allow logical inconsistencies or lacunae, we let in too many possible conclusions. The test of logical coherence is a poor substitute for data when one is trying to understand the real world, but it gives some discipline and sharpness to theorizing.

Of course, the rise of mathematics in economics and its powerful hold on economic theory in 1975 (and today) does not trace solely to these advantages. The use of a powerful and somewhat obscure tool confers power on the user. As economists became convinced of the value of mathematical rigor, the reward system (based on peer review) reinforced this tendency.

THE RISE OF STATISTICAL METHODS

Allied with the rise of formal mathematical models was the rise of statistical methodology in economics. Formal statistical techniques— hypothesis testing, inference, regression, and the like—certainly existed prior to this, but their embrace by mainstream economists also came during the postwar period. Of course, these techniques require mathematical specification of a hypothesis or model to be estimated, so the development of mathematical skills among economists was reinforcing. And those relatively more skilled at understanding statistical methods tended to be those relatively more skilled at deduction; as a social phenomenon, the rise of one reinforced the rise of the other. Indeed, one of the most important forces for the development of both in economics was the Cowles Commission, which was formed to advance both branches simultaneously, given how natural was the (seeming) connection between them.

For much of the postwar period, most empirical work was substantially more difficult than theory-building for two reasons: data were hard to gather and relatively expensive to process. In cases in which the government paid for data collection and was

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willing to subsidize its processing—national income accounting and the like -- careful statistical modeling was relatively more prevalent. But the real proliferation of empirical work in economics trailed the proliferation of formal modeling by a bit.

ROMER'S HOURGLASS

The rise of mathematical modeling and statistical methods in economics meant a change in the way economists reasoned and communicated. But it had two further effects that are

crucial to my history. Both are captured broadly by the image of an hourglass, suggested by Paul Romer. Within economics, there are many different topical concerns, such as international trade, development, economic history, labor markets, public finance (and, more broadly, the interplay of economic and political institutions). Before the mathematicization of economics, these fields maintained significant intellectual autonomy. Things a student would learn in, say, development economics would not resemble all that closely what was taught in public finance. Emphasis was placed on typologies and on the institutions that pertained to the subject in question. There were, if you will, a number of regional dialects of "Economese," dialects that were close to being distinct languages. As economics was mathematicized, the power of formal deductive modeling and statistical methods, and the status their use conferred, resulted in their acceptance by the more applied fields. This was not a matter of the applied fields recognizing the power of these new techniques and, thus persuaded, welcoming them with open arms. Acceptance was often grudging. Moreover, the process often involved imperialism, with an economist trained in mathematical technique in one area invading the topical domain of one of the applied fields. But however it happened, in most cases it did happen. The dialect of international trade today is very close to the dialects of industrial organization, development, and labor economics. Nowadays the languages used by different branches of economics sound a lot more like branches of a single methodological (rather than topical) tongue.

The process of unification meant something of a reduction in, or consolidation of, what was studied in economics. The mathematical techniques were developed first and most powerfully in the

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context of so-called competitive market exchange.⁶ The adoption of mathematical techniques, often imported from price theory (the theory of competitive market exchange), meant a concomitant adoption of the modeling choices and concerns of price theory. In particular, the study of economic institutions—a big portion of mainstream economics before World War II—dwindled in importance. The newly dominant dialect of mathematical modeling lacked some topically important vocabulary; rather than speak in an unfashionable dialect, some things were just not discussed.

This contraction of topical concerns had its positive points. The unification of dialects, for example, means that insights obtained in one context can be easily transferred to another. Insofar as a graduate student in economics is meant to master several applied fields, having a unified language promotes the educational process. And, for reasons given above, I assert that the dominant dialect of mathematical deduction possesses some powerful grammatical advantages.

Nonetheless, the loss of certain topical concerns represented a real loss. Price theory for competitive markets is empirically very useful, but there are economically important matters that it simply misses. From 1975 to the present, what we have seen in economics is 1) the development of further techniques that play by the basic rules of established (mathematical) economics but that broaden its scope and 2) the application of those techniques to specific contextual questions, with the result that the field now seems to be returning to something like the breadth of the discipline before World War II.

Hence the image of an hourglass: The vertical axis represents time, and the horizontal axis the scope or topical breadth of economics. As time passes, we see first a narrowing of topical concerns as the language is unified and then a widening of concerns as the language develops. Throughout, it is important to note, the discipline of economics did not entirely abandon subjects such as institutional economics. Pockets of resistance to the revolution persisted; in some overseas locations and in some domestic departments, they dominated. The hourglass describes roughly the development of orthodox or mainstream economics, and (at that) primarily in the United States.

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ENVIRONMENTAL INFLUENCES ON SUBJECT MATTER

As mainstream economics has broadened again, there has been something of a shift in the center of gravity of concerns, and new topics—for example, an increased emphasis on nonmarket exchange—have come to the fore. This shift in topical concerns may be seen as a response to an array of environmental influences.

As economic activity has become more global, demand for and interest in comparative economic systems has increased. There has always been an interest in comparing broadly different systems, such as socialism and capitalism. But recently interest has become more microeconomic. These days we discuss the merits of Japanese production techniques, the

German system of (employee/employer) co-determination, and the impact of venture capital on newly created American firms.

At the same time, economic ideas have permeated a number of applied and professional fields, such as business, law, finance, accounting, and medicine. Practitioners in these fields have increasingly sought the advice of economists; and economists, seeing an increased demand for their services in these domains have responded by shifting toward them.

As economic ideas have permeated such fields, academic economists are increasingly found outside of arts and sciences economics departments—in business, law, engineering, and even medical schools. Both the quantity and quality of professional-school economists has risen, and with them the influence of professional-school subjects within the larger community of academic economists. Within arts and sciences departments, a steep increase in the number of undergraduate majors with professional and professional-school aspirations has led to an increased demand for courses that serve their needs; in consequence, the field has seen an increase in courses that emphasize the normative applications of economic reasoning in the private sector while de-emphasizing abstraction for its own sake and downplaying economics as descriptive social science.

One should not overstate this shift in the center of gravity of the discipline. Economists have not flocked en masse to new subjects, nor have they abandoned the old. But, on the margin, changes in topical emphasis have occurred. And the increased interest in

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nonmarket economic exchange—i.e., exchange within firms and other nonmarket institutions—represents more than a marginal change.

METHODOLOGICAL INNOVATIONS

Together with these environmental influences, the current portfolio of economic concerns and techniques has been influenced by some significant methodological innovations.

Our ability to process data has been increased enormously. This is not to say that empirical testing has only recently arrived, but the quantities of data we can process, and the ease with which the data can be manipulated, have increased enormously. Statistical tests are no longer limited to a small number of theoretically manageable parametric specifications; Monte Carlo methods have dramatically increased what can be tested and how.

The information age has given us a bounty of empirical data to study, including data at a microscopic level of aggregation. Financial market theory and accounting have been virtually revolutionized by the general availability of transaction-by-transaction trading data. Data on purchasing activities of individual households are increasingly available. We are close to the day when economists will easily access the Census Bureau's data base.

Where data do not exist, economists are increasingly willing to create them using controlled experimentation. Experiments in economics include testing choice behavior by individuals, behavior in competitive/cooperative situations, and behavior in marketlike settings. The growth in the computing power at our disposal has also made it possible to solve increasingly large and complex problems and to uncover via simulation the properties of heuristic decision rules. Standard economics places great stress on the idea that economic actors optimize their actions; we are increasingly able to compute what optimization means and to evaluate how short of this goal economic actors fall in complex settings.

SMALL NUMBERS, INFORMATION, AND DYNAMICS

These have been important methodological innovations; but the innovation that has had the greatest impact on economic thought

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has been the enrichment of the language of the field with the vocabulary and grammatical forms of information economics and noncooperative game theory. This enrichment has been crucial to the rewidening of the subject because it has allowed us to tackle with mathematical models three important classes of issues: small numbers interactions, privately held information, and true dynamics. Textbook microeconomics in the 1960s and early 1970s was largely about markets with large numbers of buyers and sellers who interacted anonymously. Markets were entirely driven by the prices of things; the theory of markets was, for all intents and purposes, the theory of prices—hence the name price theory. There was a theory of monopoly (one seller and many buyers), and there were simple theories of oligopoly (a few sellers with many buyers); but the abbreviated models that existed tended to be fairly wooden, with sheeplike buyers and artificially imposed conjectures. Buyers and sellers were assumed to operate on the same informational basis; the idea that, say, the seller of a used car knew more about the car than did the buyer did not enter the story. And it was held to be a great virtue of the theory that any sort of

dynamic interaction could be reformulated as an equivalent static, one-time interaction, without any loss of generality. That is, when planning what to buy at the corner grocery store for the coming week, the consumer in an economic model would solve the problem of what to consume this week, the next, and for the rest of his life—contingent on uncertainties concerning his future income, the dividends his investments might pay, and so on. He would find the optimal lifetime strategy for consumption and then proceed today to implement its first step.

Today, the first theory course for acolytes (first-year Ph.D. students) begins with these basic models and assumptions, but then moves on. We consider with a limited amount of success how bargaining transpires between the single buyer and seller, when each has only limited alternatives in trading partners. We discuss with more success how terms of trade are set in specific institutions for price-setting, such as in auctions and highly organized exchanges. A large part of the second half of the typical first theory course concerns information economics, an innovation dating from 1970 or so. This concerns, for example, how markets in used cars and health insurance operate when sellers know more

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about their cars than do buyers, and insurees know more about their state of health than do insurers, or how the market in managerial talent will be affected when managers pursue agendas that do not automatically accord with those of the boards of directors and shareholders for whom they work. And a good deal more emphasis is put on how the dynamic character of an economic interaction can affect its outcome, e.g., what it takes for large industrial firms to collude, or how a monopoly firm might successfully deter entry into the markets it controls.

These factors become most interesting when they interact. To take a topic near and dear to my own concerns, organization theorists have for a long time remarked that many large firms operate so-called internal labor markets (ILMs): new hires enter the firm in entry-level positions and conduct their entire career within the firm, advancing up the hierarchy. This is a far cry from the perfectly competitive labor market that is implicit in *Theory of Value* (implicit because *Theory of Value* does not distinguish labor from any other economic commodity), where labor is a commodity that sells at a given equilibrium price. But a student leaving the first-year theory course today would be prepared to discuss such matters as how ILMs weaken the bargaining power of workers, because of inferences

alternative employers will draw if a worker leaves an ILM prematurely (small numbers and private information); how a firm's desire to maintain its reputation among its other employees might protect the individual worker from exploitation (small numbers and dynamics); how an ILM might offer the firm a superior monitoring capability (and hence more efficient incentives), because of such factors as a delay in the realization of output measures, group-measurement effects, and/or risk-sharing through time (private information, dynamics, and small numbers); and what all these things mean for the worker's allocation of effort through time (private information and dynamics).

EVOLUTION OR REVOLUTION? THE THREE CANONICAL PRINCIPLES

Moreover, the student will discuss all these issues according to some fundamental principles that are basic in *Theory of Value*. These are:

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1. *Far-sighted rationality*. On deciding to join firm A, worker X anticipates the contingent future of his employment relationship. This is not to say that worker X can foresee the future; but he has a detailed probabilistic picture of the future in mind, as does the firm (or its managers), and he uses this picture in forming his own actions.
2. *Behavior is purposeful*. Employees (and the firm employing them) act purposefully, to achieve a well-defined goal. This goal is put into the model as a numerical index of the individual's well-being, for example, utility for consumers and (usually) profits for firms.⁸ Combining (1) and (2), we have individual firms, employees, and consumers whose behavior is modeled as optimizing some numerical index in (generally) fantastically complex decision problems involving vast stretches of time and substantial uncertainty. Moreover, the solutions they arrive at fit together extremely well in an—
3. *Equilibrium*. Workers correctly anticipate how their colleagues will act and how the firm will act, now and in the future. The firm correctly anticipates the actions of workers. Given these correct anticipations of current and future actions, each party is doing as well for itself as it can, within the constraints of actions available to it.

While the contextual market assumptions of *Theory of Value*—large numbers and anonymity, all information is shared, all analysis is static—have lost canonical status, these three basic principles have not. Of course, the loss of the contextual assumptions makes a difference. Nowadays we speak of promises firm A might make to its workers, yet the only

promises that are made are those that are credible in the sense that it will be in the interest of the firm to keep them when the time comes. We speak of information held by one party and withheld by that party from others, yet if employee X withholds information from her employer, the employer knows that X has information and correctly anticipates what X would do if that information were other than what it is. Considerations like this add enormously to what we can discuss and model. But the

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rules of these models have remained (1), (2), and (3). Note well: These three canonical principles were not born in the *Theory of Value*, nor with the general mathematization of economics. In less formal terms, they go back more than a century. But with the general mathematization of (micro)economics, they became the bedrock of the discipline; *Theory of Value* had something akin to canonical status because it was a particularly powerful formulation of these three principles, and an equally powerful derivation of some of their consequences, for a particular set of contextual assumptions about the economy.

In this sense, while the mathematization of economics was certainly a revolution, the subsequent expansion from the base of 1970 until today has been evolutionary. Most graduate students spend their first few months of microeconomics learning demand theory more or less as it was taught in the 1970s, and at some point in the first year they tackle general equilibrium theory, the basic subject of *Theory of Value*. They do less of this than did their predecessors, to make time for newer contextual assumptions. But they do it, because it is generally believed that such an understanding remains the essential starting point for more modern developments.

CONTINUING EVOLUTION

There continue to be developments of this evolutionary type. Now that economists speak a more or less common dialect, topical fads are common. For a year or so, everyone is doing industrial organization. Then international trade becomes hot. At the moment, microeconomic theorists are by and large chasing organization theory, the closely related subject of corporate finance, and/or growth theory (and, in particular, its connection to the human capital of a region). These fads do not appear entirely unbidden—the recent airwave spectrum auction created a boomlet in auction theory, and events in Eastern Europe and the Far East have prompted more activity in the topic of restructuring economies, with spin-

offs into certain aspects of the theory of contracts, law and economics, and growth theory. But from wherever they come, these forays are part of the ongoing evolutionary development of the subject. Attacks on the context-of-the-year are made according to

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the three principles identified in the previous section, albeit enlivened with some specific contextual novelties, such as increasing returns to industrial production, or complementarities in various activities, or an assumption that no deal will go un-renegotiated (as long as both parties to the deal can profit from a renegotiation). To put it briefly, the theoretical tools we have are pretty good, and many of my colleagues are happily engaged in their employment.⁹ At the same time, the increasing availability of data, the now universal availability of computing power (used to compute solutions to optimization and equilibrium problems, to simulate suboptimal but reasonable behavior, and to process the data), and increased levels of laboratory experimentation have led to substantial increases in the empirical testing of economic theories. I am not enough of a statistician to be able to relate intelligently the latest innovations in these fields. But as an observer, I am impressed with the increasing level, quality, and scope of these endeavors.

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A COMING REVOLUTION

Many of my colleagues will consider this assessment delusionally upbeat. A question we often ask (as do, I suspect, all academics) is: "Read any exciting papers lately?" With some of my theory colleagues, the answer recently has often been "No." Theory development in economics seems fairly moribund, at least when compared with the late 1970s and early 1980s. ¹¹

Put it this way: the Econometric Society—the leading international professional society for economic theorists (and others)—holds a World Congress every five years. The most recent was held in Tokyo, in August of 1995. As part of the program, a number of plenary sessions are held on topics chosen by the program committee to summarize activity over the previous five years and to anticipate (or try to) future developments. These sessions concern relatively more applied topics (industrial organization, finance, and so on), the development of statistical methodology (time series, nonparametric methods, and similar

approaches), and—because the Society has a large constituency of tool creators—sessions on recent developments in the tool-fashioning subindustry of

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theory. For the Tokyo Congress, there was no dearth of interesting sessions in the first two categories. Indeed, there were plenary sessions on topics that twenty years ago one would not have expected at this society's World Congress—economic history, political economy, organization theory, and restructuring socialist economies—reflecting the widening of topics and unification of language described above. But suggestions for session topics in tool-creation were few and somewhat forced; this sort of activity seems to have died back. I say "seems to" because I do not actually believe this is so. Instead, a somewhat revolutionary shift in the economic paradigm has begun—one that mostly missed being on the program of the Econometric Society's 1995 World Congress because its pieces were and are not yet sufficiently advanced for plenary reviews. The shift involves a partial abandonment of the three canonical principles of rationality, purpose (or greed), and equilibrium.

First and foremost, the assumption that economic entities are hyper-rational and hyper-perspicacious is under renewed attack. The assumption of hyper-rationality has been under attack since it began to be taken hyper-seriously; Herbert Simon has been a strong and convincing critic for at least forty years. But until recently mainstream economists mostly shrugged their shoulders at such attacks and claimed that the alternative was theoretical anarchy—ad hockery run amok. Nowadays we see more measured attempts to find alternatives, and while no widely accepted standard alternative has emerged (and may never do so), mainstream economists are increasingly willing to try the alternatives that do arise. In particular, the study of adaptive learning and behavior by the individual has been an area of significant activity recently.

Second, the economic entities of *Theory of Value*—the firm and the consumer—are no longer purposeful monoliths. The attack on the firm has been going on in the guise of organization theory for quite some time. But increasingly one sees papers about consumers whose behavior, tastes, and expectations are socially determined or affected by psychological processes of perception and cognition. We also see papers that pursue a biological metaphor, where behavior is "instinctive," given exogenously, with the population of behavioral types determined through some sort of selection

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dynamics (whether seriously modeled in processes of imitation or just specified exogenously as part of the model).

Third, non-equilibrium states are discussed and sometimes even modeled. Interactive models of adaptive learning, in which individuals struggle to understand an environment that changes with the state of their struggle, are in vogue. Evolutionary models of behavior are, of course, also interactive. Whether one calls them temporary equilibrium or disequilibrium systems, they are certainly a step away from the full-equilibrium (and fully-rational, optimizing) assumptions of the current standard economic theory.¹²

This suite of changes responds to the maturation of the previous wave of developments and is a continued response to the environmental and methodological factors that helped shape the previous wave.

These changes respond in the first place to the limits the standard canon places upon us, especially when it comes to economic exchange involving a small number of parties and long stretches of time. For example, it takes very little empirical power to see that in the real world of exchange between employee and employer or between supplier and purchaser the relationship is ongoing and the "contract" between the two is seriously incomplete, in that what is exchanged, when, how, and at what price are determined as contingencies arise.

Transaction cost economics¹³ is based on this observation and on the notion that in such contracts one must focus on the governance structure implicit or explicit in the contract, i.e., how contractual adaptations will be made. As mainstream economists have become increasingly interested in the dynamics of exchange in general, and in non-market, non-anonymous markets in particular, they have recognized that an understanding of this contractual incompleteness and the relative efficiency of different forms of governance is crucial. Some very insightful models have been built to study contract incompleteness and governance that play within the confines of the three canons, wherein one restricts exogenously the terms upon which a contract can be based.¹⁴ But in this context, the three canons, especially the canons of full (hyper-)rationality and equilibrium, imply that the individuals involved fully and correctly anticipate what contingent adaptations will take place. Hence explanations and investigations based on the three canons are somewhat tortured and miss the

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mark. Interest in this sort of exchange pushes theorists to explore revolutionary alternatives.

These changes respond as well to the increased availability of data, both collected in the field and produced in the laboratory. Debreu's *Theory of Value* correctly identifies the need for formal reasoning (and, I assert, canonical assumptions) as arising in part from the economist's relative lack of data. Compared to physical and biological scientists, economists are still relatively poor in controlled data. But our relative poverty is lessening, and we can (and are increasingly able to) afford a degree of theoretical ad hockery—if we can justify our modeling assumptions with data.

Recent trends in experimental economics are salutary in this regard. Most experiments are tests of the canonical models, either of how individuals choose in relatively controlled settings (usually, not as economic theory would have us believe) or testing equilibrium predictions of the theory. (Market equilibrium theory has tended to fare a bit better.)

Recently, though, there has been an increase in tests outside the canonical models, both trying to refine our understanding of real choice (going from the observation that the frame of a choice matters to an investigation of how systematically the frame matters) and the processes of learning in multiperson settings that might get us to an equilibrium.

This oncoming revolution has also meant increased contacts with other scholarly fields, contacts that will surely increase. To put it on a purely personal level, I am currently engaged in writing a book on employment and human resource management with a card-carrying sociologist. Ten years ago, had I admitted the profession of my coauthor to my colleagues in economics, most would have said that my business school experiences had corrupted me irreparably. Today they may still think this, but they express substantial interest both in the project and what I am learning from it. Specifically, they recognize that standard economic models of long-term interaction suffer enormously from two lacunae: the standard models leave unexplained and unmodeled the idea that the interaction will change what individuals value; and while the standard model makes clear that what individuals expect of their trading partners is crucial, it does not have much to say about where those expectations come from. In the language of economics, sociological considerations help us understand how the social

context of exchange affects both the preferences of the individual and what the individual expects of others. For example, sociological considerations offer explanations for how the symbolic character of employee benefits—presented as a gift, or as just another form of market compensation—might spill over into the day-to-day relationship between employee and employer.

To put it less personally, the interest in evolution means connections with biologists, the interest in the social formation of expectations and preferences means connections with sociologists, and the interest in the processes of perception and cognition means connections with psychologists. This is not missionary work, where economists issue epistles for others to follow, but is more like two-way trade (and, to judge by the testimony of some of my colleagues from these other lands, it may have become missionary work the other way around).

This probably sounds like a substantial and exciting amount of activity. So why, one may ask, does the question "Read any good papers lately?" draw "No" as the answer? The reason, and the best sense in which this is a revolution, is that this activity seems to imply a decrease in the importance of deduction in economics. For people like myself who were raised with *Theory of Value* as canonical authority, a good paper is one with an innovative new model that concludes with an interesting theorem (or, at least, proposition). A paper that reports provocative experimental data or even just a provocative simulation of some dynamic system does not pass the test. If I am right about the coming revolution, though, these standards will change, as will our willingness to report simulations along with the provocative experiments and empirical papers that we increasingly see.

WILL THE REVOLUTION THREATEN THE EVOLUTION?

Most of my colleagues agree that this potentially revolutionary activity is taking place. Some have decried its existence, claiming that the three canons are essential to economics. Perhaps a larger fraction, with empirical instincts, note that the cry "Bounded Rationality!" has been with us nearly as long as the three canons, and predict no lasting impact from the current flurry of activity along these lines.

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But suppose that the revolution does come, and we learn how to reason fruitfully without the three canonical principles. Will this revolution threaten the accomplishments of the past

twenty or thirty or even fifty years? I do not believe so. The advent of private information, small numbers, and nontrivial dynamics has not routed models that live (nearly) fully within the contextual and modeling assumptions of *Theory of Value*. One of the most productive fields of microeconomic study since 1970 has been the study of financial market valuation, which plays largely by the full set of assumptions. This field continues to thrive. These models can and do tell us a lot about what determines the market value of a share of AT&T or an option to buy a share of AT&T. But even so they do not tell us everything; this form of modeling financial market value has been enriched by developments in the study of market microstructure (how stock markets operate transaction by transaction) and of corporate finance (what determines the level of debt assumed by public corporations)—developments that rely on the three canons but depart from the contextual assumptions of large anonymous markets, shared information, and essentially static optimization.

Similarly, the three canonical principles, while limited, have their sphere of relevance. Indeed, some of the models that step outside the three canons do so with the explicit intention of testing them, e.g., when do we derive (even approximately) an economic notion of equilibrium from a biological process of natural selection? Within that sphere, the three canons will continue to guide informative study of specific questions. Perhaps needless to say, some of the sharpest debates to come will be over the boundaries of that sphere of relevance.

So is this a revolution after all? Some readers of early drafts of this essay assert that a revolution, by definition, must overthrow all of the old system. (Webster's supports this assertion.) So perhaps what I call a revolution is more like the earlier introduction of information economics and the language of game theory into economic discussions; that is, a burst of increased diversity that is evolutionary rather than revolutionary in character.

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WILL THE ESTABLISHED PARADIGM IMPEDE THE REVOLUTION?

Conversely, will the established paradigm impede these new developments? There are good reasons to think that it will.

First, economists have an unfortunate tendency to believe that rational behavior by individuals means behavior that accords with the basic axioms of revealed preference

enriched, in the case of choice under uncertainty, by Savage's axioms or, in the case of dynamic choice, by the principle of full dynamic consistency. These economists believe that behavior that does not accord with the standard model is irrational, unpredictable, and even somewhat unsavory—not a fit subject for serious discussion. I recall a conference on organization theory at which a prominent labor economist, within the space of fifteen minutes, decried models in which employees were concerned with their compensation relative to that of fellow workers and then related that his experience as department chair had taught him that his colleagues behaved in just this fashion. The second, though, was dismissed as "just an anecdote"; the first was "something serious."

Second, economics, at least as practiced by mainstream economists, is a field that values tradition. If phenomenon X can be explained within the standard canon, such an explanation is usually preferred to another explanation that ventures outside the canon because, given our relative paucity of data, adherence to canonical assumptions provides us with a measure of discipline. And, turning this seeming virtue into a vice, we have become adept at tweaking our true-to-the-canon models to produce almost anything we desire. We can empirically reject specific models that employ the canon, but our inventiveness with models has gotten us to the point where rejecting the canon is well-nigh impossible. Finally, economists, or at least economic theorists, have a strong and developed taste for answering the question, "How efficient (in the precise economic sense) is this or that outcome?" Efficiency, in the precise economic sense, needs consumers with well-anchored preferences. To relax the first and second assumptions of the canon (far-sighted rationality and purposeful behavior) means at least a reformulation of the notion of efficiency, and perhaps something more dramatic—not a price most theorists will be happy to pay.

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For these reasons and others besides, the very success of the continuing evolution—the playing out of the second wave of developments since World War II—may act to impede the third wave. Why abandon a seemingly winning hand? But, to give a purely personal opinion, I think the third wave will only be a little impeded: the empirical deficiencies of the canon, where it is deficient, are too glaring. The rich array of things to learn from other disciplines is too rich. And not least of all, the need for economic theorists to work on something new and different is too impelling.

MACROECONOMICS (AS VIEWED BY AN OUTSIDER)

I am quite content with the assertions in this essay insofar as microeconomics is concerned. But there remains the subject of macroeconomics. As already noted, I am very poorly qualified to talk about the recent history and current position of macroeconomics. At the same time, no essay on the current state of economics could possibly be complete without addressing those topics. So with the strong *caveat* that my opinions on the matter compound ignorance and prejudice, I proceed to give my impressions.

To begin with the "facts" concerning recent history (the scare quotes are intended to recognize that others will dispute these facts): Macroeconomics over the period from 1970 until 1990 or so was split into two camps, so-called saltwater and freshwater approaches. The names are derived from the geographic locations of the institutions that practiced these contending views. Thus, the temples of saltwater macroeconomics were MIT, Harvard, Pennsylvania, Princeton, Stanford, and Yale, while freshwater macroeconomics was developed at Carnegie, Chicago, Minnesota, and Rochester. 15 Saltwater macroeconomics is (by descent) the macroeconomics of Keynes. In terms of the models constructed, it is more behaviorist; consumers act in mathematically modeled ways but do not have quite the foresighted rationality of consumers who are making microeconomic decisions. Markets can fail. When we see workers on the dole, they are there involuntarily because, say, firms will not lower wages to clear markets. Freshwater macroeconomics, in contrast, begins with microeconomic-style consumers, who are not fooled by government attempts to manipulate them through fiscal or monetary policy (hence

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freshwater's alternative name, rational expectations macroeconomics). And markets in freshwater macroeconomics do work; any unemployment we observe is voluntary in a real sense.

The saltwater brand of macroeconomics was the establishment brand; freshwater was the revolutionary challenge to the status quo. The challenge was based on something akin to canonical purity: Saltwater macroeconomists accept (and accepted) the power of mathematical, formal models—they certainly accepted the relevance of statistical modeling and fitting—but they were skeptical about the canon of farsighted rationality and, as a reduced form, were willing to live with a bit of "disequilibrium" in their models. Greed, rationality, and equilibrium had their place in the saltwater pantheon, but that place was not

above all other considerations. The freshwater challenge, then, was to take as paramount greed (or purpose), rationality, and equilibrium.

Battles between the two camps were common; as an outsider, I was privileged (if that is the right word) to observe these battles on various selection and review panels. Saltwater types would argue that they were being pragmatic, using the canons to guide their models as far as common sense and the data indicated was appropriate. (Macroeconomics is one field in which data have been fairly readily available, supplied by the government whose practical interests in the subject are obvious.) Freshwater types would argue that an edifice could only be as strong as its foundations; if you did not play by the rules of the discipline, you could not be sure what you had. In support of this argument, the freshwater types could cite evidence; during this period one of the fundamental empirical regularities of establishment macroeconomics, the Phillips curve (which said that governments could trade off unemployment for inflation), was badly mauled by stagflation.

The battlegrounds were grants, publications, positions, and the minds and souls of the next generation—otherwise known as graduate students. Both sides had victories and defeats. My sense, as an observer, was that the freshwater types had victories especially in the arena of graduate students; better graduate students can excel more quickly when excellence depends on the mastery of mathematics and logical purity rather than on an experience-based sense of how far to push on canonical principles, hence better graduate students are strongly drawn to topics and approaches

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that stress technical expertise and novelty. I can recall an archdeacon (at least) of the saltwater church sitting on a selection panel for fellowships for younger faculty members and complaining about how all the bright young macroeconomists, including his own students, were reading, believing, and (according to him, worst of all) doing freshwater-style work.

Today the war has largely abated. In part, interest among macroeconomists has moved on to questions about the determinants of long-term growth, which run orthogonal to the saltwater/ freshwater debate. And in part, having built and studied the simple models, freshwater macroeconomics has proceeded to more complex models, with richer information, strategic and dynamic effects (the evolution in microeconomics), and, very recently, models in which the individual consumers are not hyper-rational (the revolution).

The models that emerge may not take us all the way back to saltwater macroeconomics as it was in the 1960s. But saltwater types maintain that once these real-world complications are added to freshwater models, what emerges is just a somewhat more detailed version of what they (the saltwaters) have been chasing all along. Hence both sides claim a brackish victory. The saltwater types insist that the freshwater folks have begun to abandon the ivory tower for more realistic models of individual behavior. And the freshwater types insist that, at last, the saltwater types are taking seriously what impels individual behavior, which is just what the freshwaters wanted.

From the perspective of the basic thesis of this essay, the story of macroeconomics (if I have it right) is interesting on two related grounds. First, it gives an example of the tension in economics between treating the canonical principles as ironclad precepts or as loose guidelines. In his essay in this volume, Solow argues that serious economists (i.e., everyone except perhaps the high theorists) use mathematical models opportunistically, constructed to shed the most light on the phenomenon under consideration, without doing violence to empirical observations. It should not go unnoted that Solow is a high priest of the saltwater school of macroeconomics. This is not to say that Robert Lucas—a high priest of freshwater—would disagree in principle with Solow's description of how economists should use mathematics and ca

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nonical principles. But they would disagree on how ready one should be to bend canons to perceived circumstances.

Second, the story may give some guidance for what will happen as the third wave of bounded rationality and disequilibrium breaks on mainstream, more doctrinally pure, microeconomics. At the conference for which these essays were prepared, it was commonly observed that economics was different in an important way from the other three fields discussed—English, philosophy, and political science. Economists by and large agree on what they are doing and how to do it. The discipline has not been the scene of major methodological conflict, based on different generations or anything else. The freshwater/saltwater wars were as close to this as we have come. And in those wars, the common heritage and shared respect for the canon (even if some economists are less observant than others) led to an armistice with both sides feeling victorious. In the third-wave story there are important differences; most importantly, the revolutionaries are

seeking to weaken the canonical assumptions instead of increasing adherence to them. But notwithstanding the differences, as one of the new revolutionaries, I can hope that if the wars to come will not be friendly, neither will they be internecine.

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ENDNOTES

1 Jim March was contrasting economics and sociology; his counterpose to the "saloons of sociology" was the "well-ordered Methodist Church of economics." As evidence that economists are well-ordered Methodists, unacquainted with saloons, I can report that an economist friend, reading this line in an earlier draft, was concerned that my spell checker had converted "salons" into "saloons."

2 Reprinted as "The Mathematization of Economic Theory," *American Economic Review* 81(1991): 1-7.

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3 Gerard Debreu, *Theory of Value* (New Haven, Conn.: Yale University Press, 1959). While Debreu's work (and that of his colleagues in general equilibrium theory) played a role in the mathematicization of economics, I do not mean to assert that they were solely or even principally responsible for this revolution. To attribute credit (or blame) for this revolution is outside both my charge and my expertise, although I would be remiss not to mention in this regard Paul Anthony Samuelson's vastly influential *Foundations of Economic Analysis* (Cambridge, Mass.: Harvard University Press, 1947). I am only asserting here that in 1975, when the revolution was (largely) over, *Theory of Value* was as close to canonical as anything, at least for microeconomists.

4 The term "efficient" here does not conform to the normal-usage meaning of the word. Roughly, a market outcome is efficient if there is no way to reorder economic activity that

would be unanimously preferred by all the individuals in the economy. Needless to say, this is subject to conditions and qualifications.

5 For further development of these points, together with some examples, see Debreu, *Theory of Value*, or David M. Kreps, *Game Theory and Economic Modeling* (London: Oxford University Press, 1990), chap. 3.

6 "Competitive" here means that no economic entity has so much power as to be able to affect any market price.

7 Because I work at a business school, I may see more of this than there actually is.

8 In Robert Solow's trinity of "greed, rationality, and equilibrium," greed is a code word for purposeful behavior. Most economists populate their models with greedy individuals—those whose purpose is self-interest, very narrowly defined. In this sense, Solow's code word is entirely appropriate. But the formal canon is that individuals act purposefully, and in theory (if not in many models) the goals pursued could be an equitable distribution of resources or a "just" outcome somehow defined.

9 To add a darker note here: Economists are often accused of trying to force the world to fit their modeling forms. Within economics, the context-of-the-year phenomenon gives rise to a related accusation, viz., the theory community will descend upon a particular context and write a bunch of papers that are motivated in large measure by their theoretical appeal and only in small measure by empirical observation. Then the theory steamroller moves on, leaving scholars whose careers are in the context-of-last-year to deal with the litter left behind, viz., papers that may have been fashionable but that too often miss the contextual mark. Context specialists need to have a healthy skepticism concerning what they can learn from general-purpose theorists. Happily, their skepticism is usually in robust good health.

10 And to add a darker note here: It is universally appreciated, I think, that theorists are able to tweak their assumptions in order to reach any conclusion they wish. The believability of the conclusion depends not only on the fact that it was reached but on how hard the theorist had to tweak the model to get there. On the other hand, it is less clear how much we appreciate that with increased availability of data and computational power, and with journals that naturally select papers that are novel, we collectively face a data-mining problem, at least as far as claims of statistical significance are concerned.

11 Another piece of data, perhaps a bit more objective, is the large number of textbooks being produced on the subjects developed over the past two decades.

12 There has been enough work on these topics so that a plenary symposium was held at the Tokyo Congress on adaptive learning and evolutionary models of behavior.

13 See, for example, Oliver E. Williamson, *The Economic Institutions of Capitalism* (New York: Free Press, 1985).

For a recent survey, see Jean Tirole, "The Current State of Incomplete Contract Theory" (University of Toulouse, 1995, mimeographed).

14 The past tense in this sentence is intentional. Most saltwater temples have at least one practicing priest of freshwater in residence nowadays, and at least one of the saltwater temples has been largely taken over by the other side. Moreover, the distinctions are no longer so severe, although that gets a bit ahead of the story.