ECONOMICS AT THE MILLENNIUM

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History teaches that the future will be unkind to those foolhardy enough, or well compensated enough, to attempt prediction. The most eminent American economist of a century ago, Irving Fisher, predicted in October 1929 that "the end of the decline in the stock market will … be a few more days at the most." IBM chairman Thomas Watson estimated in 1943 that "there is a world market for about five computers." Charles Duell, of the U.S. Patent Office, asserted in 1899 that "everything that can be invented has been invented." In 1894, on the cusp of the golden age of physics, physicist A.A. Michelson concluded that "the more important fundamental laws and facts of physical science have all been discovered."

Economics also teaches skepticism about prediction—forecasts of any real value are unlikely to remain valuable for long. Still, the opportunity to speculate on the future of economics is irresistible, not least because any guess at the future must begin with the present, and with the history that brought us here.

Jacob Viner once said, only partly tongue-in-cheek, that "economics is what economists do." Economists today do many and diverse things. Testifying to this diversity, the *New Palgrave Dictionary of Economics* contains nearly two thousand essay-length entries. *How* economists do economics remains more monolithic. In this sense, neither of your correspondents is a representative economist. In a discipline that has adopted the techniques and *ethos* of applied mathematics, we mostly conduct our arguments in prose. Moreover, our research interests include the history and philosophy of economics, fields that today occupy the periphery of the discipline, so our views may not be representative.

To partially compensate for these biases, we present views other than our own. This might also improve the quality of our forecasts, based on an analogy with quantitative forecasting. There, the consensus forecast—which averages individual forecasters' predictions—has been shown to be more accurate than many of its component forecasts. Many distinguished economists have since 1990 risked diagnosing the discipline's situation or future, including the twenty-two papers in the *Economic Journal's* centenary volume (January 1991), Solow and Kreps in *Daedalus* (Winter 1997), Lipsey (*Journal of Economic Methodology* June 2001), Colander (*Journal of the History of Economic Thought* June 2000), and a symposium in the *Journal of Economic Perspectives* (Winter 2000). One remaining bias arises from our professional specialization in microeconomics, the study of individuals, firms and industries. Macroeconomics, the study of economy-wide phenomena like output, inflation, and money, will get short shrift in this review.

Prediction is not used here in the narrow sense of forecasting the magnitude or direction of change of a particular variable, such as a stock price index. Instead, we identify what we—and especially the scholars we cite—see as important tendencies within the profession, and guess which of these tendencies might strengthen. By virtue of their broader and vaguer character, our conjectures on "where economics might be going" obviously risk less than did Irving Fisher and his client, the Yale University endowment.

Species of Economists and Theory vs. Evidence

Perhaps the hoariest methodological debate in economics concerns the weight that economics ought to give to theory versus evidence. Theory by itself is empty, and of limited value for a discipline that aspires at times to be a policy science, but empirical inquiry uninformed by theory is likely to be blind. For more than a century, and in various guises, economics has revisited, sometimes unintentionally, the matter of how to weight theory versus evidence. Today, what different types of (academic) economists do reflects rival views of what the weights ought to be.

We divide the genus economist into five species. While there is too much overlap for taxonomic precision, the different species usefully stand in for different attitudes on theory versus evidence.

"Pure theorists" are in the business of logically deducing the implications of a set of behavioral axioms taken as fundamental (Hahn in *Economic Journal* 1991: p. 47), an enterprise describable as Euclidian in spirit (Solow in *Daedalus* 1997: p. 42). Pure theorists prove theorems and lemmas. Most of game theory is of the pure theory type. The connection between pure theory and the economy runs from tenuous to none. At its most abstract and most archetypal (for example, the Nobel-prize-winning 1959 work of Gerard Debreu), pure economic theory does not even purport to have empirical consequences. As theorist Ariel Rubinstein puts it (in his 1998 volume *Modeling Bounded Rationality)*, pure theory "does not pretend to predict or to advise … the most [it] can do is to clarify the concepts we use" (p. 194).

A second species, "applied theorists," refashion pure theory so it *can* explain or predict real-world phenomena. One successful example, cited by Sutton (in his 2000 book, *Marshall's Tendencies: What Can Economists Know?*), is options pricing. An option is the contractual right to buy or to sell an asset, such as common stock, at a specified price and by a future date. Applied theorists devised a way to predict an option's value, given only the specified price, the current stock price, the time until expiration, a discount rate, and an estimate of the stock-price volatility. Robert Merton and Myron Scholes won the Nobel Prize for their contributions to valuing options. Another successful example, also from Sutton, is auction design, which makes use of auction theory, a branch of non-cooperative game theory, to design rules that result in successful auctions. As the examples suggest, applied theory works best in fairly "controlled" empirical settings, that is, locations where the theoretical assumptions are more likely to obtain. Successful application of theory also often requires a good dollop of elementary economics. Klemperer *(Journal of Economic Perspectives* Winter 2002) has argued that "(w)hat really matters in auction design are the same issues that any industry regulator would recognize as key concerns: discouraging collusion, entry-deterring and predatory behavior. In short, good auction design is mostly good elementary economics" (pp. 169-170).

Our third species works on pure statistical theory. These "theoretical econometricians" develop statistical theory motivated, in part, by the particular theoretical problems that arise in econometrics. A fourth species of economist comprises the empirically oriented. One sub-species, "applied econometricians," uses sophisticated econometric tools—statistical techniques developed for economic applications—to analyze data. Though they work with data, their interests are mostly "tool-driven"; they apply new econometric techniques to established areas of empirical inquiry. A second sub-species of empirical economists is more data-driven. They use established econometric techniques to analyze new (or expanded) empirical data sets. A third sub-species of empirical economists, still something of a novelty in economics, uses experimental methods—especially laboratory games—to test a variety of behavioral propositions.

A fifth category, "applied economics" is not entirely a separate species. These economists are distinguished more by what they study than how they study it. Methodologically eclectic, they use applied theory and empirical methods to analyze issues in their field of interest: for example, labor economics, economic history, or urban economics. The vast majority of economists can be located in the applied economist category. Indeed, applied econometricians or applied theorists will at times, as their research focus shifts, fit the applied economics category.

The different species in our crude taxonomy roughly proxy different attitudes toward the proper mix of theory and evidence in economics. As we will see, this question of how to weigh theory and evidence is a central concern of the distinguished scholars we survey.

Economics Defined by Its Method

When outsiders judge modern economics to be monolithic, especially as compared with its cognate disciplines, they are referring to economic method—how economics is done. What contemporary economists do, says Robert Solow, Nobel Laureate and one of the discipline's ablest ambassadors, is build models: "modern mainstream economics consists of little else but examples of this process" (Solow in *Daedalus* 1997: p. 43). In a typical economics paper, the section immediately following the introduction is invariably entitled "The Model." A model is "a deliberately simplified [mathematical] representation of a much more complicated situation.... The idea is to focus on one or two causal or conditioning variables, excluding everything else, and hope to understand how these aspects of reality work and interact" (ibid).

The determined historian can locate unexpectedly venerable quantitative, mathematical and statistical precursors to modern economics: there is, for example, the "Political Arithmetick" of the 1660s, and Augustin Cournot's (1838) prescient and sophisticated mathematical treatments of monopoly and duopoly. But scholarly economics journals before World War II are virtually free of even rudimentary mathematical notation. In today's journals, every other page is "pockmarked by algebra," to use Baumol's phrase (in the *Economic Journal* 1991, p. 2).

But while mathematics is obviously useful for economic modeling, it is not sufficient. Pure theorists, who are occupied with formal considerations, do not bother with models, because models are meant to isolate and refer to observable empirical phenomena. Models are also routinely designed for special cases, involve fairly basic mathematics, and are therefore neither general nor fancy enough for the pure theorist.

Because economic models are empirically oriented, and because they can be stated (if less compactly) in non-mathematical terms, Solow regards as misplaced the common criticism that economics is excessively formal. Model builders, says Solow, are "obsessed with data" (1997: p. 57). Solow even explains the post-war transition in American economics from prose to algebraic expression as the product of more and better statistical data. "Technique and model-building," says Solow, "came along with the expanding availability of data, and each reinforces the other. Facts ask for explanations, and explanations ask for new facts" (ibid, p. 47). In fact, "the modern approach to economics is mostly about accounting for data" (ibid: p. 53).

We might quarrel with Solow's historical thesis that better data explains the ascendancy of model building technique, but there is no disputing his characterization of the discipline's prevailing current method as "fact-driven model building." Thus does Alan Blinder wryly describe an economist as "someone who sees that something works in practice and wonders if it also works in theory" (in the *American Economic Review*, May 1988, p. 7). What most unifies economics is how economics is done. Colander (in *Journal of the History of Economic Thought* 2000) concurs. "Modern economics" has become "enormously broad in its acceptance of various assumptions and content." But it is "extremely narrow when it comes to method.... *The modeling approach to problems is the central element of modern economics*" (p. 137).

Economics Defined by its Canonical Ideas

What ingredients have been used to construct economic models? Two ideas have typically formed the heart of the structure—*maximization and equilibrium*. Maximization says individual agents make optimal choices consistent with a completely specified objective or "maximand": "utility" for consumers and profit for firms. Equilibrium says that the aggregate consequences of these individual choices are equilibria *stable* and *unique enough* to permit prediction. We can, after Kreps (in *Daedalus* 1997), call maximization and equilibrium the canonical principles.

If agents are to maximize, they must not only be purposeful and forward looking, they must also have a detailed probabilistic picture of the future (Kreps 1997: p. 71). Maximization also requires that agents' preferences over possible states of the future be "well-behaved," that is, amenable to a single, complete, and transitive rank ordering. Rationality consists only in doing what one most prefers.

Preferences are typically treated as primitive concepts—given, and beyond evaluation or analysis. The economist's pared-down treatment of rational choice is sometimes called "thin rationality," because "it leaves unexamined the beliefs and desires that form the reasons for the actions" (John Elster, *Sour Grapes*, 1985). More broadly, economic rationality embeds Lionel Robbins' conception of economics, where the fundamental economic problem is resource allocation under scarcity. Economics is the science, Robbins said famously in 1935, "which studies human behavior as a relationship between ends and scarce means which have alternative uses."

The term equilibrium most commonly refers to an aggregate outcome that is *stable* in the sense that none of the maximizing agents can change it, or, if they can change it, none would want to. In so-called "general equilibrium" accounts, agents are ordinarily powerless to affect outcomes, sometimes by virtue of their large numbers and small size relative to the market. They are called "price takers." In game-theoretic settings, where agents *can* influence outcomes ("price makers"), an equilibrium that no (maximizing) agent wishes to deviate from is, roughly speaking, a Nash equilibrium. Economic method focuses on equilibria more than on the out-of-equilibrium processes by which economic agents arrive at these equilibria. If there are multiple equilibria, as there often are, especially in game-theoretic settings, then prediction requires an additional theoretical explanation of how economic agents come to settle on a *unique* equilibrium outcome.

In sum, economics uses its canonical behavioral principles, maximization and equilibrium, as a source of ideas and in order to organize its characteristic way of doing business, the enterprise of writing down, testing, and refining models.

Scholars on the Current State of the Discipline

How well does the method of fact-driven model building actually work? Later we will take up predictions about the future of the canonical principles, and about the scope of economics. Take first pure theory, which is unconcerned with evidence. Pure theory occupies a curious place in contemporary economics. On the one hand, its practitioners are regarded as among the most able of economists, and mathematical talent is disproportionately admired and rewarded in the profession. On the other hand, said the eminent theorist Frank Hahn in 1984 (in *Equilibrium and Macroeconomics*) there is something unworldly about pure theorizing. "It cannot be denied," Hahn said, "that there is something scandalous in the spectacle of so many people refining the analysis of economic states which they have no reason to suppose will ever, or have ever, come about" (p. 88). This non-empirical orientation may be why, as Solow claims, the vast majority of economists pay almost no attention to pure theory (Solow 1997: p. 43), even as they honor the pure theorists.

Solow wants to distinguish workaday model-building, which employs mathematics, from the formalism of pure theory. His argument is that the discipline's use of mathematics, by itself, is neither formalism nor any other methodological sin. We mostly agree, but two related questions remain: (1) has the *extent* of mathematical expression in economics gone too far—in the sense that too much of a good thing has created real intellectual costs, and (2) is the economic method of fact-driven model building scientifically successful?

Richard Lipsey (in *Journal of Economic Methodology* 2001, p. 84) thinks that the mathematization of economics has had several adverse effects: (1) a tendency for "(g)enerality to be desired for its own sake, even when it obscures the simplicity of solutions to some problems"; (2) obscurantism: using mathematics "even if it adds nothing to your verbal analysis"; (3) intellectual crowding out: "the high cost of learning advanced mathematics [pushes] more descriptive and factual material out of the curriculum"; and (4), perhaps most dangerous of all, the confusion of validity and truth: "the implicit assumption that if some result is derived from a complex model containing all the OK assumptions ... it must be true."

William Baumol (in *Economic Journal* 1991) seconds Lipsey in the concern that excessive focus on mathematical expression works to crowd out "other lines of [scholarly] attack." "(T)hese days few specialized students are allowed to proceed without devoting a very considerable portion of their time to the acquisition of mathematical tools, and they often come away feeling that any ... writing they produce will automatically be rejected ... if it is not liberally sprinkled with ... algebraic symbols." This, Baumol says, has led to a misallocation of intellectual resources within economics. "Mathematics," says general equilibrium theorist Michio Morishima "has gone too far, leading theorists to have an inadequate concern for actuality" (in the *Economic Journal* 1991: pp. 73-74). Nobel Laureate Milton Friedman in the same symposium also worries that economists use mathematics for obscurantist ends. He argues that contemporary economists rely "on mathematics and econometrics beyond the point of vanishing returns." The cost, says Friedman, is under-investment in empirical work. Friedman's explanation is economic. It is cheaper per publication to produce theorems than it is "to gather original data ... to explore their reliability and accuracy ... and derive a full understanding of the historical and institutional circumstances in which they were generated" (*Economic Journal* 1991: 37).

Friedman's last point, which returns to the relationship between theory and evidence, is echoed in one form or another by many scholars in our sample. Many agree that theory, which here means analytical puzzle-solving, is cheap and that data are expensive, so economic and statistical theory tends to outrun the evidence for it (Solow 1997: p. 57). "Without a close relation between evolving theory and empirical observation," says Lipsey, "new theory tends to be developed in unconstrained ways that are empirically relevant only by accident" (2001: p. 174). Pencavel (in the *Economic Journal* 1991) concurs, asserting that economists often avoid meaningfully confronting theoretical hypotheses with empirical evidence, in favor of theorizing alone, which offers quicker or more certain returns.

The harder question, on which scholars disagree, is to what extent theory *should* outrun the evidence for it. The critique of modern economics as insufficiently empirical is a venerable one. In his version, Andrew Oswald (in the *Economic Journal*, 1991) cites the grumbles of two Nobel Laureates, and a former editor of *The Journal of Economic Literature*: "Wassily Leontief (1982) has argued that our discipline has deteriorated into a second-rate branch of applied mathematics in which, unscientifically, researchers eschew empirical investigation. James Heckman (1986, p. 384) says that the subject is 'widely perceived to be discredited because it has so little empirical content and cares so little about developing it.' John Pencavel (1989, p. 1) concludes that economists do not want applied work to be done, because it is likely to reveal the irrelevance of their hypotheses and undermine their ability to derive sweeping implications from theoretical models" (Oswald, p. 75).

Leontief's critique (in *Science*, 1982) derived from an analysis of *The American Economic Review* (*AER*) from March 1972 to December 1981, which found that more than 50 percent of the papers contained mathematical argument with no empirical data; in total, about two-thirds contained no empirical work. Theodore Morgan (*Journal of Economic Perspectives* 1988) updates Leontief by examining the *AER* from March 1982 to December 1986. He finds an increase in empirical analyses, rising from roughly one third in the Leontief period up to one half of all papers in the later period. Morgan also surveyed Britain's leading journal, the *Economic Journal*, over the entire 1972-1986 period, finding a roughly constant 58 percent of papers with empirical work. Morgan also compared economics (as represented by the *AER* and the *EJ*) to a sample from four other social and physical sciences—politics, sociology, chemistry and physics—for the 1982-86 period. Economics is more mathematical than political science and sociology, which produce strictly mathematical papers only in 18 and one percent of all cases, respectively. Economics is also less empirical than its sister fields—non-empirical work in political science and sociology is 42 and 22 percent, respectively, well under the 50-60 percent in Leontief's and Morgan's surveys. Papers without empirical work are unheard of in chemistry (zero percent) and rare in physics (12 percent).

One standard defense, that economics lacks recourse to the controlled experimental techniques characteristic of the natural sciences, is as old as the critique. The Nobel laureate and pure theorist Gerard Debreu argues, for example, that economics' inherent empirical disadvantage requires relatively greater investment in pure theory. "Being denied a sufficiently secure experimental base, economic theory has had to adhere to the rules of logical discourse and must renounce the facility of internal inconsistency ... [on pain of] being useless..." (*AER,* 1991, p. 3). While changes are afoot with respect to experimental economics, most empirical work in economics still consists of statistical inference—econometric attempts to find law-like regularities in historical observations.

Inference from historical observations presents hazards. Unlike physical phenomena, which are ontologically well behaved—that is, relatively invariant over (human-scale) time and place—economic phenomena are not. Economics, says Lipsey, "has nothing like the theories of physics that predict specific quantitative outcomes."

The first hazard of this kind of historical inference, argues Solow, is assuming too much: economists' reliance on physics-like modeling techniques can lead them to downplay the ontological difference between physical and social phenomena. There is, says Solow, "the temptation to believe that laws of economics are like laws of physics: exactly the same everywhere on earth and forever. But the part of economics that is independent of history and social context is not only small but dull" (1997: p. 36).

The second hazard is assuming too little: economists can invoke the relative inferiority of social data as a crutch for ignoring empirical anomalies. In natural science, Lipsey says, because there *are* "tight theories" based on stable quantitative relations, "conflicts between theory and new evidence ... anomalies ... are immediately obvious. They typically encourage research until either the new evidence is proven to be erroneous or the theory is amended to accommodate it" (2001: pp. 172-173). In economics, in contrast, "anomalies, particularly those that cut across the sub-disciplines and that can be studied with various levels of technical sophistication, are tolerated on a scale that would be impossible in most natural sciences—and would be regarded as a scandal if they were." And, since economics lacks truly stable quantitative relations—widely accepted, precise empirical regularities—the discipline instead unifies around theoretical commonalities. This, in turn, says Lipsey, fosters a "lack of communication among economists operating at various levels of theoretical abstraction and empirical sophistication," which "causes them to be unaware of many of these anomalies, so their existence often does not induce research to resolve them" (2001: p. 173).

The disadvantages of empirical work in social science compared to the natural sciences are real enough. But even if we do not require of economics the kind of scientific success demanded in the natural sciences, when do we judge an economic model successful? Solow proposes that success consists in explaining what the data show. He means "explain" not fundamentally but pragmatically, in the sense of capturing a relationship between economic variables to "a fair degree of approximation" (1997: p. 49).

This definition of success leads to what can be called the problem of model choice. Since all models are radical simplifications, even the most successful models will not fit all the known facts of a given situation. Because there is no ideal model, there often are, instead, two or more models—often quite different—that fit the facts equally well. When the evidence does not choose between rival models, "it can become very difficult to ever displace an entrenched model by a better one." "Clever and motivated ... people can fight a rear-guard battle that would make Robert E. Lee look like an amateur," quips Solow (1997: p.50). The result is that "old models never die, they just fade away" (ibid).

In principle, *more* data can make the problem of model choice less acute. Then the question is "merely" whether data good enough to meaningfully discriminate among rival models can be had, given resource constraints and disciplinary incentives. A deeper problem, emphasized by Lipsey (2001) and other scholars, arises in the modeling approach itself. Economic modeling entails regarding all differences between the actual model in use and the "true" model as "noise;" that is, the modeling approach entails seeing all omitted explanatory variables as having no *non-random* influence on the variable being explained.

John Sutton's cogent monograph (2000) explains this problem using Alfred Marshall's analogy of the tides. Tides are affected by two forces, the gravitational force of the sun and moon, which can be modeled with precision, and meteorological factors, which are famously difficult to predict. Fortunately, the meteorological factors are of secondary importance, so tides can be predicted using only the gravitational forces as the explanatory variables, and treating local weather as truly random influences, having no systematic effect upon the tides (Sutton 2000: pp. 4-5). Economic modeling, which proceeds on Marshall's analogy, likewise assumes that any omitted variables only randomly perturb the model's predictions. The worry, which is more acute in complex settings, is that the omitted variables *do* systematically influence the variable being explained, but that they are "unobservable." That is, we cannot measure or proxy or otherwise control for them (Sutton 2000: p. 8). When the omitted variables do matter but cannot be measured or proxied or controlled for, Marshall's tides analogy does not hold, and more data will not be sufficient for scientific success.

Predictions about Methods in Economics

What does the future hold for "fact-driven model building"? And what will be the fate of pure theory? In 1991, pure theorists were not sanguine about the prospects for pure theory. The distinguished theorist Frank Hahn predicted the demise of deductive, axiomatic theorizing as currently practiced. "Theorizing of the 'pure' sort will become both less enjoyable and less and less possible ... It is not my contention that it will wither under the scorn of practical men or women. The reasons for the demise are all 'internal' to the theory itself.... [T]here will be an increasing realization by theorists that rather radical changes in questions and methods are required if they are to deliver, not practical, but theoretically useful results" (*Economic Journal* 1991: p. 47).

Why? Hahn argues that none of the emerging crucial questions "can be answered by the old

procedures." Complexity and multiple equilibria are the culprits. Because questions have become increasingly complex, computer simulations will be needed instead of theorems. Increasingly, "historical modes of analysis will eventually seem to be unavoidable", to pin down which path a system with multiple equilibria will actually follow.

"Instead of simple transparent axioms there looms the likelihood of psychological, sociological and historical postulates" (1991, p. 47). Our successors, says Hahn, "will have to bring to the particular problems they will study particular histories and methods capable of dealing with the complexity of the particular, such as computer simulation. Not for them the grand unifying theory of particle physics ... or the pleasures of theorem and proof. Instead the uncertain embrace of history and sociology and biology" (p. 50). It is no small irony that purely theoretical developments have underscored the importance of history to economic processes.

Decision theorist Peter Fishburn agrees with Hahn that behavioral axioms will be brought under greater scrutiny: "researchers will continue to axiomatise new models ... but the status of axiomatising will diminish. At the same time, experimental research on decision behaviour in laboratory and field will flourish. A much better understanding of risk typology, attitudes toward ambiguity, and the effects of time on preferences will emerge" (*Economic Journal* 1991: p. 29).

The distinguished econometrician E. Malinvaud sees the decline of very general theories in favor of "a richer system of theoretical models." He sees such systems as "constellations of specific models ... for dealing with some particular aspects of phenomena.... Very general models no longer suffice for the more specific questions we have to consider" (*Economic Journal* 1991: p. 67).

Not every economist views as desirable a retreat from the mathematical virtues of generality, abstraction, and logical coherence. The theorist Beth Allen (*Journal of Economic Perspectives* 2000) laments that "many theorists are now backtracking from rigor in their work." Allen argues that "economics would be better served if theorists would more often deliberately move in the direction of abstraction and generality, which is where theory can most effectively contribute to economic science" (2000: p. 145). Allen also seems unconcerned with improving the dialogue between theoretical and empirical work: "much outstanding theory is inherently untestable, but it can frequently be validated through mathematics" (2000: p. 144).

The scholars in our sample frequently believe that a firmer empirical foundation for economics is desirable, and many, including Solow and Lipsey, argue that this requires a better dialogue between models and the "facts" they are built to explain. But how will this empirical dialogue be mediated? Some predict that relatively new empirical techniques—simulation and experiment especially—will become as common as today's statistical inference from historical data.

Wiseman (*Economic Journal* 1991) predicts that current econometric techniques will disappear "or become marginalised, being displaced by the more general use of experimental methods" (1991: p. 153). Colander (*Journal of Economic Perspectives* 2000) foresees economics in 2050 as more plural in its empirical method: "In 2050 ... simulation models ... form the core of what students are taught.... Economists ... do empirical work in a wider variety of ways.... They both create data and analyze it. Experimental economics is now an extremely important way of creating data; interestingly, it only began in the late 20th century. Economists today also use natural experiments and randomized field trials to create data much more than they did earlier" (pp. 128-129).

Schmalensee's (*Economic Journal* 1991) measured discussion sees economists increasingly relying on laboratory experiments as a means for empirically reducing the theoretical indeterminancy of multiple equilibria, especially in strategic settings. "The experimental approach offers a way to circumvent serious limitations on the availability of micro-data and seems particularly well suited for testing the implications of strategic behavior. Progress in computer software and hardware seems likely to reduce the cost of experiments. Unless future research reveals that laboratory experiments have fatal flaws, I would expect them to be routinely used in a number of fields of economics" (1991: pp. 116-117).

Alvin Roth (*Economic Journal* 1991), an experimentalist, makes an even stronger case for the empirical virtues of laboratory games. Roth warns that game theory will become purely scholastic without experiments "directed primarily at testing and developing economic theory" (1991: p.108). "If we do *not* take steps in the direction of adding a solid empirical base to game theory," argues Roth, "but instead continue to rely on game theory primarily for conceptual insights ... it is likely that ... game theory will have experienced sharply diminishing returns" (ibid).

Predictions about the Scope of Economics

Having considered the future of economic method, what can we forecast about the scope of economics, the breadth of its topical interests? Prewar economics was best defined not by its method but by its interests, a loose collection of fields within its purview—government finance, railroads, utility regulation, money and banking, industrial relations, etc. Postwar economics, in contrast, is better defined by its method and ideas, models built upon maximization and equilibrium. At the same time, the very generality of modern economics has not only unified procedures in the sub-fields of economics, it has also influenced adjoining disciplines. The spread of economic method and ideas has led to the charge that economics has imperial designs on adjoining disciplines.

The imperialism charge has some merit, at least circumstantially. In the last 30-40 years, economics has influenced, sometimes considerably, Law (Law & Economics), History (Cliometrics), Politics (Public Choice and positive political economy) and Sociology (rational choice theory), to say nothing of Finance. But in other respects, the charge does not hold up.

Law & Economics, and one its founders, Nobel Laureate Ronald Coase, provides an illustration. Coase has proved extremely influential in Law. But Coase claims he never meant to influence legal scholarship. In making an argument aimed at economists, he ultimately changed the way legal scholars regard the notion of cause. He argued (in "The Problem of Social Cost" *Journal of Law and Economics* 1960) that external costs—harms incurred by parties external to an economic exchange—are *jointly* caused. The common law traditionally assigned legal liability for harms by assuming causation is one-way—railroad sparks *cause* fires in nearby crops, so railroads should be made liable. Coase insisted that, in the absence of flammable crops planted close to the rail bed, there is no harm. Thus, farmers also cause the harm—it takes two to tort. And if "cause" in the traditional, one-way sense is no longer legally decisive, the door is open to other adjudicative criteria for deciding who should be made to bear social costs. Economists of course suggest efficiency as a criterion: it may be socially cheaper to plant crops that won't burn, or to relocate the crops.

Whatever its influence on legal scholarship, economics did not intend to colonize Law. If the Law and Economics movement is evidence of economics imperialism, it is a peculiar kind—accidental imperialism. Whether or not economics has been imperialist, accidentally or otherwise, we predict that the discipline will not abandon its traditional (prewar) areas of interest, nor will it retreat from those sister disciplines it has more recently influenced. At the same time, we think that economics will come to be seen as less imperialist.

The first reason for this forecast is mere trend extrapolation. The last century has witnessed dramatic changes in economic method and ideas, but exhibits a striking continuity of research interests. "There has been," claims Milton Friedman, "little change in the major issues occupying the attention of economists; they are much the same as ... Adam Smith dealt with" (*Economic Journal* 1991: p. 37). Leonard (in Roger Backhouse and Jeff Biddle (eds), *Toward a History of Applied Economics, History of Political Economy*, Supplement to Vol. 32, 2000) finds that the debate over legal minimum wages, a topic in Anglo-American economics for over 150 years, shows a striking continuity in the issues of interest and the policy positions taken. The future is likely to resemble the past in this respect: there will be changes in the tools of economic analysis and the nature and quality of empirical evidence, but the issues will remain much the same.

What is likely to change are the motives behind the interest of economics in adjoining disciplines. Some of the past generation's outward expansion in economic methods and ideas can rightly be regarded as analytical tools in search of fresh research applications. We predict that the relationship between economics and its sister disciplines will move closer to one of mutually beneficial trade. Economics will look to nearby disciplines less for new applications and more for new ideas. And, as with all mutually beneficial exchange, the idea of foreignness will be eroded: some of the current disciplinary boundaries will be effaced and redrawn.

Part of the reason economics is likely to increasingly look outside the discipline for inspiration is sociological: like other invaders, economists abroad are likely to go native, as Schmalensee (*Economic Journal* 1991) notes. But much of the outward-looking impetus comes from within economics itself: the canonical ideas of late 20th-century economics have, particularly in the last 10-15 years, come under increasing theoretical and empirical strain.

Predictions about Economics' Canonical Ideas

Nobel laureate Gary Becker wrote in 1976 (in *The Economic Approach to Human Behavior*) that "the combined assumptions of maximizing behavior, market equilibrium and stable preferences, used relentlessly and unflinchingly, form the heart of the economic approach as I see it" (1976: p. 5). Becker adds stable preferences to maximization and equilibrium because it facilitates dynamic maximization, determining an optimal *series* of choices over time. All three assumptions are being weakened, a trend that we are sure will continue.

Criticism of the maximization hypothesis as unrealistic is longstanding. Nobody believes that ordinary human beings are computational prodigies who routinely use sophisticated mathematics to make decisions. Economists traditionally reply that it is *as if* agents decide by setting up and solving constrained maximization problems. Imagine a consumer who operates in a world with two goods, x_1 and x_2, who has a budget of I, and who faces prices of p_1 and p_2. Assume that the consumer spends fraction a of her budget on the first good, and fraction (1-a) on the second good.

The consumer's decision can be seen as applying a rule of thumb—always spend aI on good 1. Her decision can also be presented *as if* it were the solution to a constrained optimization problem: maximizing the utility function $x_1^{a}\, x_2^{1-a}$ subject to the constraints presented by her limited budget and prices, $I = p_1\, x_1 + p_2\, x_2$. It is *as if* the consumer who applies a spending rule of thumb solves a constrained utility maximization problem. (The example is taken from Rubinstein 1998: p. 10). Since both decision rules yield identical results, maximization can be seen as a useful fiction.

Herbert Simon, a Nobel Laureate, has long argued that the maximization hypothesis, and the *as if* defense, present two problems. First, there are good *economic* reasons against the maximization hypothesis: cognitive resources are scarce, as is decision-making time. Economists, who locate scarcity at the very center of economic reasoning, should not assume cognitive free lunches. Second, the *as if* method begs the scientific question of how real individuals actually *do* go about making decisions.

Half a century after Simon first made this argument, economics has begun to take notice. Beginning a few years after Becker's manifesto, a small group of economists—who have adopted the name of "behavioral economics"—became convinced that the weight of experimental and other empirical evidence was disconfirming of the maximization hypothesis. Psychologists Amos Tversky, Daniel Kahnemann and others offered evidence that *homo sapiens* does not much resemble his idealized maximizing *homo economicus* cousin—*homo sapiens* makes systemic errors in his decisions, relies on rules of thumb instead of calculation, reverses choices in response to different framing of the same question, and discounts the near future more steeply than the remote future.

Behavioral economics tries to capture these and other empirically documented departures from idealized rationality, what Richard Thaler (in *Journal of Economic Perspectives* 2000), godfather of behavioral economics, characterizes as *homo economicus* losing IQ. The overarching modeling principle is what Simon called bounded rationality: the view that cognitive and time scarcity make solving for optima impossible or too costly, so that agents have incentives to look for alternative methods of making choices—"heuristics", rules of thumb, etc. A major theoretical focus is on how cognitively constrained individuals decide how to decide (Plott, *Economic Journal* 1991: p. 91).

One ironic unexpected outcome is that economic theorists, currently remote from behavioral economics, may increasingly turn to the more practical but mathematically demanding problems of bounded rationality. One example involves methods for determining good choices where the best-possible (that is, "optimal") choice is not computable, as in traveling-salesman-type problems. For concreteness, consider a traveling salesman who must map out a route which includes visits to N cities. The optimal solution involves minimizing travel time. But the number of possible routes grows so quickly as N increases that the optimization problem becomes effectively unsolvable. With N cities to visit, there will be $N!$ distinct possible alternative routes. For instance, if $N = 30$, $N!$ is approximately $2.65{*}10^{32}$. Evaluating that number of alternatives would keep any current supercomputer busy for eons. (The example is from a 2001 paper by Xavier Gabaix and David

Laibson, forthcoming in I. Broca and J. Carillo eds., *Collected Essays in Psychology and Economics*).

A second prong of the behavioral economics program concerns maximizing over time—a setting where choice and its consequences may be separate in time. The issues here are the stability of one's preferences as one matures, and uncertainty about how to characterize an unknown future. The Beckerian program assumes stable preferences, which, among other things, is tantamount to assuming unbounded willpower. On the behavioral view, even when real human beings know what is best, they will sometimes fail to choose it, for lack of sufficient willpower.

If, in the morning, you prefer not to eat dessert after dinner, then, with stable preferences, you will still prefer no dessert rather than ice cream after dinner. You will not be tempted. How then to explain the enormous self-control industry? Billions are spent on fat farms, diet plans, smoke-ending clinics, drug rehabilitation, and fitness trainers. Moreover, there are less formal self-control devices, such as buying cigarettes by the pack rather than the carton, avoiding streets with taverns, keeping sweets out of the house, and locating the alarm clock across the room. Behavioral economists explore theoretical assumptions consistent with temptation. Most current work explains temptation via the assumption that rates of impatience (time preference) are not constant but decrease with the duration of the wait. Many alternative ways of weakening the stable-preferences assumption remain to be explored.

Because maximization generally requires it, economists ordinarily treat uncertainty only as risk. In risky situations, agents are presumed to know the relative likelihoods and payoffs of all possible outcomes, as with a roulette wheel. In risky settings, economic agents don't know what will happen, but they do know everything that could happen. True uncertainty, a more serious kind of indeterminacy, arises when agents lack a complete probabilistic picture of the future.

The analysis of the unknown remains an important frontier in economics. Some regard the roulette-wheel conception—which tames uncertainty into risk—as misbegotten. Wiseman, for example defines the future as "unknowledge," that which "contain events which we cannot foresee" (*Economic Journal* 1991: p. 152). Less critical observers, like Turnovsky, agree that "our analytical treatment of uncertainty is pretty primitive. Typically it is represented by probability distributions, the relevant characteristics of which (means, variances, etc.) are assumed to be known to agents in the economy. By any standard this is a restrictive representation of the issue" (*Economic Journal,* 1991: p. 145).

The future thus presents two forms of uncertainty—uncertainty regarding what will happen, and uncertainty as to how one's future welfare might be affected by each possible outcome. The two forms of uncertainty are complementary and reinforcing. Consider the choice to smoke. First, there is uncertainty about what will happen—for example, the likelihood and timing of negative health consequences. Second, there may be uncertainty as to how one's future welfare will be affected by these possible future events. One might, for example, suspect that one's own preferences are likely to change with age: the typical 40-year-old does not value activities, goods and experiences the same way he did at age 20. Or future preferences might be affected by previous consumption of a habit-forming good, such as tobacco. When there is uncertainty as to how one's future self will be affected by future events which are themselves uncertain, the theoretical temptation to revert to stable preferences is understandable. The experimentalist Charles Plott agrees that "the nature of individual choice of process, a set of rules and institutions that will operate when one's preferences are different from those that exist at the time of choice, will … be a perplexing challenge" (Plott, *Economic Journal,* 1991, p. 91).

The challenge is more than one of theoretical tractability. Mutable preferences go to the heart of the policy enterprise of comparing the welfare of individuals before and after a policy change (such as, say, an increase in cigarette excise taxes). If self "one" has different preferences than future self "two," *which self's* welfare should policy recognize, and on what basis? We hazard no prediction on how the analysis of this crucial problem will evolve, but we will wager that, in the next 10-20 years, the Nobel Prize will be awarded to a behavioral economist.

Finally, the trajectory of behavioral economics tells us something about the relationship between theory and evidence in economics. For years, economists ignored the psychologists' experimental findings of departures from the maximizing paradigm. Richard Thaler's work, which emphasized these empirical anomalies, was once dismissed as mere anecdote. Abandoning the canonical principles was criticized as entailing a descent into chaotic ad-hockery; a fearful if non-specific disease that one might find in, say, the salons of sociology (Kreps *Daedalus* 1997: p. 74). Eventually though, the weight of the evidence proved disconfirming enough to induce some economists to risk ad hockery, and explore theoretical alternatives. That Thaler's work also turned out to have concrete testable implications in finance also helped. Ten years later, Alvin Roth's forecast looks prescient: "experimental economics ... will play an important role in helping game theory bridge the gap between the study of ideally rational behavior and the study of actual behavior" (*Economic Journal* 1991: p. 107).

Thaler and confreres do not intend to overthrow economic method. The behavioral economists believe only that their behavioral postulates offer a better explanation for the data than does the maximization hypothesis, which Thaler predicts will eventually come to be seen as a special case. Behavioral economists believe that people are purposive and forward looking, but imperfect, and that their departures from perfection lead to interesting and sometimes unexpected consequences. In our final section, we consider one such consequence, what we call "revenge of the norms."

Revenge of the Norms

American economics and American sociology share common descent. When the fields diverged around the First World War, among the several causes was a different conception of human action. Jon Elster (*Journal of Economic Perspectives* Fall 1989) calls it one of the deepest conceptual cleavages in social science: that between *homo economicus,* a creature who is individualistic, purposeful and forward looking, and *homo sociologus,* who is social, conventional and sometimes myopic. Thus could James Duesenberry say 40 years ago: "Economics is all about how people make choices; sociology is all about why they don't have any choices to make" (in *Demographic and Economic Change*, National Bureau of Economic Research, 1960, p. 233).

As 20th-century economic theory converged on the maximization-equilibrium-stable preferences paradigm, it became increasingly silent on social structures. Institutions such as laws, norms, and conventions were ordinarily treated as exogenous, beyond analysis. The reason was simple: maximizing agents with complete information have little or no use for laws, norms and conventions. The upshot is that economics came to be seen as somehow opposed to explaining legal rules, social norms and conventions.

But then a funny thing happened. Institutions reappeared. The causes are multifarious and uncoordinated. We suggest that the different ways in which researchers have relaxed the canonical principles has, intentionally and as a byproduct, helped revive the importance of laws, norms and conventions in economic analysis. We offer four examples.

New Institutional economists, for example, explicitly model legal property and contract rights. Because there is uncertainty, contracts that specify every possible future contingency are impossible, and it is this incompleteness of contracts that creates a rationale for contract law enforcement. In that other former world of maximizing agents with complete information, contracts will be perfectly complete, and contract law is superfluous.

American economics and American sociology share common descent, the fields diverged around the First World War.

Laboratory games show that experimental subjects are concerned with fairness—who gets what—and are willing to spend their own money to enforce norms of fairness. While there is nothing in the canonical principles that requires egoism, self-interested action has nonetheless been a commonplace assumption of late 20th-century economics, and fairness, or "bounded self-interest," has emerged as one of the research areas of behavioral economics.

Elinor Ostrom and others study common-property resource settings which have a prisoner's-dilemma structure, that is, payoff-maximizing individual choices lead to collectively inferior results (a.k.a. the tragedy of the commons). Ostrom finds in experiments and in field studies that some communities successfully evolve and enforce norms against opportunistic behavior.

Game theory has proven to be an unexpected source of insight into the nature and function of conventions. It is no coincidence that game theory is a place where the weaknesses of both canonical assumptions—maximization and equilibrium—have been exposed. The key problem is that of multiple equilibria.

John Nash shared a Nobel Prize for his dissertation's proof that in non-cooperative games an equilibrium exists. But even if we can expect players to select Nash equilibria, which of the multiple Nash equilibria will they pick? In the 1970s and 1980s, an equilibrium selection literature sought ways to reduce the number of Nash equilibria. But this Nash refinements literature still hewed closely to game theory's traditional emphasis on wholly deductive approaches: what ideally rational players, given only a complete description of the game, must deduce.

Paradoxically, this Nash refinements literature produced too many solution concepts—how is the agent to choose among multiple *theories* of equilibrium selection? In response, in the 1990s some game theorists moved away from these wholly deductive approaches, instead going toward evolutionary models. In evolutionary models, adapted from theoretical biology, players are boundedly rational: they have incomplete information, limited memory and simple conceptions of how others are likely to behave. The process of convergence to an equilibrium is dynamic. Players grope towards an equilibrium, more pushed by a changing environment, than pulled by their own deductive prowess.

One upshot is that social conventions have become relevant again. Traditional game theory ignored conventions as superfluous: a uniquely rational solution obviates the need for conventions, so players who observe conventions cannot be ideally rational, and, conversely, should it prove rational to follow a convention, then the claim of a uniquely rational solution must be false.

The hyper-rational agent of traditional game theory—who can rely upon deduction alone—fails at some rudimentary tasks of coordination that *homo sapiens* manages rather well. Consider a coordination game, where two randomly paired traders can use one of two currencies, *gold* or *silver*. If both play *gold*, each gets a payoff of one; if both play *silver*, each gets a payoff of one. If they fail to coordinate, each gets zero. Because there are multiple Nash equilibria (e.g., both play *gold,* both play *silver*), the superhuman agent of classical game theory cannot decide what to do.

But real people can. They coordinate by recourse to convention. They trade in gold (or silver), drive on the right (or left) side of the road, determine price by haggling (or by posting or by auction), use standard-form contracts, etc. Conventions enable coordination of expectations when deduction by itself is insufficient.

It has long been known that the coordinating function of conventions is valuable. What we now better understand is that there is necessity behind their virtue. It is *because* real people are cognitively constrained, that they, unlike their idealized cousins, have incentives to look for and learn conventions that can make them better off.

The revival of economics' interest in laws, norms and conventions is by itself not enough, of course, to reunite *homo economicus* and *homo sociologus*, who were separated at birth. But the revenge of the norms in economics is a scientifically welcome effect of the ongoing process whereby economics reduces the IQ of *homo economicus*, thereby sometimes making him smarter.

SUGGESTED FURTHER READINGS

The *Economic Journal* January 1991 contains short articles by twenty-two economists discussing the future of economics. Among the especially interesting ones are those by Baumol, Buchanan, Friedman, Pencavel, Plott, Roth and Schmalensee.

The *Journal of Economic Perspectives* Winter 2000 contains a Symposium "Forecasts for the Future of Economics." Of particular interest are the articles by Colander and Thaler.

Lipsey, Richard. "Successes and Failures in the Transformation of Economics," *Journal of Economic Methodology* June 2001, pp. 169-201.

Solow, Robert. "How Did Economics Get That Way, and What Way Did It Get?" *Daedalus*, 126, Winter 1997, pp. 39-58.

Sutton, John. *Marshall's Tendencies: What Can Economists Know?* Cambridge, Mass.: MIT Press, 2000.

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