Journal of Economic Perspectives—Volume 14, Number 1—Winter 2000—Pages 143–150

The Future of Microeconomic Theory

Beth Allen

While I wish to avoid attempting to define economic theory—perhaps it's like pornography, in that you know it when you see it—nevertheless it seems appropriate to begin my discussion by considering what makes good theory. I will then present some lower-risk predictable predictions about the future development of a number of research areas in microeconomic theory, along with one higher-risk prediction.

Good theory starts from a good question and does not cheat by adding enough conditions so as to assume the answer. Important research questions, even in "highbrow" theory, can almost always be explained in words to a well-trained research economist who is not a theorist and can usually also be explained to a good undergraduate major, a scholar from a different discipline, a business person or lawyer, or the educated public. A good question should instantly impress one as being significant or fascinating and it should exhibit a strong likelihood of leading to additional important discoveries. Perhaps the litmus test is whether one's instinct is to blurt out: "Why didn't I think of that?" In this sense, theory does not differ from other fields of economics. Good theory is derived from sensible long-term research goals and strives to take at least a small step toward some vision. It is a creative endeavor where innovation is desirable, even if the new ideas and methods may cause initial discomfort. Surprising conclusions can be wonderful. Finally, successful theory frequently leads to explanations that validate one's common sense, at least in retrospect. It sometimes leads to a beautiful and elegant model; when this happens, research is very satisfying indeed.

Moreover, research projects in which the risks and potential rewards are both

■ *Beth Allen is the Carlson Professor of Economics, University of Minnesota, and Adjunct Consultant to the Research Department, Federal Reserve Bank of Minneapolis, both in Minneapolis, Minnesota.*

high tend to contribute the most toward important new advances in economic theory (or any scientific field). This is not to devalue the worth of the huge volume of more pedestrian research, especially if it is carefully performed and useful, but I'm convinced that major significant and exciting strides are associated with high risk research. Funding agencies should be more cognizant of the benefits that accrue from riskier projects because such work is less likely to find sufficient private support, but more likely to provide a genuine public good.

In the area of methodology, I choose not to rehash the positive/normative distinction or the debate about testability in economics. In my opinion, these issues are discussed in economics classes more than they deserve to be at this stage in the development of economics, although the emphasis might have been warranted many decades ago—that is, before I was a student. Frequently an article or research project integrates both positive and normative aspects; one must first understand how things work before being able to suggest improvements. Moreover, I think that the old criterion of making a prediction and then testing it empirically has been overrated as a scientific standard. In economics it's even less relevant, because one can't always set up a properly controlled experiment or obtain the requisite data. Even if these obstacles can be overcome, empirical studies in economics seldom provide clean tests of a specific hypothesis, but rather tend to intermix the phenomena of interest with the specifics of the model and its maintained assumptions. Much outstanding theory is inherently untestable, but it can frequently be validated through mathematics, as discussed below.

On a similar note, I am pleased that the seemingly endless disputes on the role of mathematics in economics have largely ceased. Perhaps my circle of contacts is limited, but my impression is that a consensus has developed in the economics profession that one is justified in using whatever mathematics (or statistics or other formal tools) is necessary to formulate and solve a worthwhile problem effectively. Needed techniques cannot be criticized as "too much," although unnecessarily fancy and complicated methodologies which serve more to show off the author's skills than to advance understanding of a genuine economic problem should continue to be discouraged by the norms in our discipline.

Despite the hard-won silence on appropriateness of mathematics, I fear that many theorists are now backtracking from rigor in their work. The guilty ones are frequently young members of our profession. Perhaps this alarming trend reflects the heightened publish or perish pressures faced by new entrants to the academic job market during the last decade, but surely sloppiness is a strategy that is inconsistent with solid scientific accomplishment. (I hesitate to insert an exception for true geniuses, since most people think they're better than average and economists—or researchers in general—are probably more arrogant than most people.) In fields of economics other than microeconomic theory, I perceive ongoing tension between a group that demands rigor and a group that finds rigor to be intrinsically not worth the time. Obviously my sympathies lie with the first coalition, because I don't believe the word "applied" contains any connotations of "unscientific" and (to reiterate briefly an old standard argument) mathematical discipline prevents one from making both obvious and subtle mistakes in the analysis, mistakes that might not be caught by one's intuition and that could generate further faulty research. Without rigor, the author and the reader simply cannot evaluate whether a result is right or wrong.

Yet another disturbing trend in economic theory is that many researchers are choosing to step back from genuine theory. When they have a good idea or an interesting example that illustrates a new phenomenon, they fail to push it reasonably to its logical conclusion. The result is a cute article that contains some nice ideas pointing out something that's potentially exciting and important, but the reader has no clue about the possible generality or limits of the analysis because only an example or a highly-specific closed-form parametric model has been presented. This issue is distinct from my complaint above about the absence of rigor; a little example can be carefully analyzed so that all claims about it are true, but this is less valuable when the analysis is incomplete, albeit correct. My former colleague Dave Cass is fond of arguing that there's no clear distinction between an example and a model or theorem because the latter category also depends on certain assumptions, including the maintained hypotheses such as constrained optimization. While this point is well taken, I'm suggesting here 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.

It's hard to discuss research trends without considering interdisciplinary work. However, I confess that until recently, I had not found interactions with most other social scientists to be particularly fruitful, with the exception of those who are already themselves interdisciplinary or engaged in formal theoretical research (for example, in political science, law, or accounting). My opinion has shifted, however, as certain interdisciplinary connections have developed. The "new" political economy has emerged as a promising field. Finance has had a huge influence in economic theory, stimulating much of our large and important literature on economies with uncertainty and asymmetric information, including the subtopics of rational expectations and contract theory. A newer trend is the influence of results from operations research and computer science on topics in economic theory such as complexity, parallel processing of information, stochastic versus deterministic systems, and computational techniques.

I predict that biology and engineering will be the interdisciplinary influences of the future. For biology—or, more precisely, cognitive neuroscience—the link is the relation between biological bases of behavior and rational choice (Dickhaut et al., 1999; Grether, 1999). The potential new connections from engineering to economics, which will be discussed in more detail later in this paper, operate through the importance of technology. Finally, a list of interdisciplinary influences would be incomplete without mention of experimental economics (which is grounded in statistics and all of the experimental laboratory sciences) and the overall importance of fundamental tools from mathematics and statistics (including probability theory, stochastic processes, and statistical decision theory).

A more local version of interdisciplinary work is emerging within economics: the traditional distinction between microeconomic theory and macroeconomic theory is disappearing. The line has been blurred by the view that macroeconomics should be based on microeconomic foundations and general equilibrium theory. I anticipate that this positive trend will accelerate. Dynamic economic theory is a blend of microeconomics and macroeconomics, which offers an interesting and important area for future research.

Some Predictable Predictions

Rather than presenting a laundry list of research topics for the coming years or an exhaustive description of current research that has important unfinished extensions, I shall focus in this section on two main topics: game theory and information, not necessarily in order of importance.

The introduction of game theory into economics has a long and bumpy history, starting (implicitly) with Cournot (1838) in the 19th century. A large and sometimes technical literature developed in the 1960s and 1970s, centering on the search for strategic foundations for competitive equilibrium. This work was based on cooperative games, defined to be those in which groups of players (or coalitions) can communicate and form binding agreements with each other. This contrasts with noncooperative games, in which the only thing an individual player can do is to pick a strategy based on what he or she knows about the game. (Communication may be part of a strategy, but agreements are not enforceable if players decide to deviate, except possibly through the device of punishment strategies. Note that cooperation can arise in equilibrium in certain noncooperative games.)

By the 1980s, the focus had shifted to noncooperative game theoretic models of strategic behavior, which were then advocated as the only part of game theory that could be valuable in economics. The argument was based on the claim (more recently shown to be incorrect) that only noncooperative games could feature asymmetric information, the observation that cooperative games do not capture actual strategies very well as they're defined in terms of feasible payoffs to various coalitions, and the fact that noncooperative games could seemingly use Nash equilibrium exclusively as their sole solution concept, whereas the list of solution concepts in cooperative game theory was long and growing. The third supposed advantage of the noncooperative model was quickly eliminated as the realization that one frequently faced a plethora of Nash equilibria displaying widely varying qualitative features (that is, efficient and inefficient equilibria, equilibria in pure and mixed strategies, and nontrivial equilibria in games in which every player doing nothing persisted as an equilibrium) gave rise to well over a hundred distinct refinements of Nash equilibria appearing in journal articles. A conclusion which emerged from this work is that noncooperative games display not only the same problem of having too many possible solution concepts, but also have the additional problem that these noncooperative equilibria are fragile; they are extremely sensitive to slight variations in the assumptions. I interpret the seminal work of Kohlberg and Mertens (1986) on strategic stability to indicate that a satisfactory refinement is impossible and to imply that the elucidation of specific refinements, even if defined axiomatically, for particular economic problems cannot progress beyond the status of being a long list of examples from which no systematic general conclusions can be drawn.

While some recent research on computational and empirical aspects of Nash equilibrium is interesting, my impression is that attempts to validate Nash equilibrium have generally failed due to the robustness problem mentioned above. This tells me that the future implications of noncooperative game theory are limited, even though such strategic considerations have had a fundamental, pervasive, and largely positive impact on the way we do economic theory and, in fact, on the way economists think. Note that game theory is increasingly included in the undergraduate curriculum for nonmajors as well as majors in economics.

My personal picks for interesting research areas in game theory that are likely to be important for future economic theory include network games and games that are both cooperative and noncooperative. Network games or games on graphs provide models of strategic local interactions. Such games exhibit different possibilities for strategic behavior and different equilibria or solutions than games without the network feature. Indeed, in cooperative theory, a player in a network game may communicate or belong to a coalition (and hence make binding agreements) with only those players to whom the given player is connected according to the graph. For the noncooperative version of network games, the key idea is that a player's payoff depends only on the actions of nearby players. These networks can reflect social relationships, locations, communication patterns, and so forth.[1] An instance of burgeoning importance concerns the Internet, which is rapidly becoming a significant part of U.S. economic activity, as e-mail, Internet-based commerce, Internet-based business services, and global enterprises expand. Mixtures of both cooperative and noncooperative game theory are relevant for the noncooperative foundations of cooperative game theory, including the possibilities for noncooperative implementation of various cooperative solution concepts. As introduced by Zhao (1992), hybrid games are games which contain both cooperative and noncooperative stages; they can be used to model, for example, simultaneous cooperation within firms and competition among firms or, more generally, a strategic version of "island economies" as the term is used in macroeconomic theory.[2] Finally, I include coalition formation within this category as an important continuing area of investigation. To date, game theory has been unable, in general, to explain or predict which coalitions will emerge when a game is played. This surprising gap needs to be closed, because the concept of a coalition provides a general way to model

[1] Work on communication games, as in Forges (1986) and Myerson (1986), seems related in spirit to network games.

[2] See Phelps (1970), Lucas (1972), Barro (1980), and Prescott and Rios-Rull (1992).

various economic institutions such as firms, countries, cities, customs unions, and so on.

My second predictable prediction concerns the economics of information. Many open questions remain about both individual behavior and equilibrium in the presence of asymmetric information. Some of these questions focus on incentives, but I refuse to reduce the economics of information and uncertainty to the study of incentive compatibility constraints. One of my pet open questions in this area concerns the exploration of why markets are incomplete, which in turn can lead to endogenous explanations of market structure. A bigger question is to incorporate information in a fully endogenous way into microeconomic theory and game theory. While progress has been made during the last four decades (compare Chapter 7 of Debreu (1959) and Radner (1968) to Allen (1990)), we are unfortunately still very far from a satisfactory general model.

An Unpredictable Prediction

I believe that economic theory will and should devote more attention to technology issues in the coming decades. One motivation for pointing to this area comes from the importance of technology in explaining economic growth.[3] A second reason is that this area of economic theory has been neglected, at least since the era of research on satisficing and the behavioral theory of the firm. An exception is the area of intellectual property rights, where a recent burst of enthusiasm has followed earlier fads such as patent races and research joint ventures.

It's useful to divide the economic theory of the firm into six interrelated themes: 1) the definition of the firm and technology itself; 2) product and process innovation and R&D policy; 3) the evolution of individual firms as economic institutions (for example, Holmes and Schmitz, 1995; Lucas, 1978); 4) financial aspects of individual firm behavior and of asset markets in equilibrium; 5) managerial control, contracting, and incentives; and 6) strategic behavior. Research topics such as firm formation, networks, and product portfolios deserve more attention from researchers trained in economic theory. Further work should explore the allocation of R&D effort among various projects; Allen (1991) offers one attempt in this direction.

However, the most glaring gap is in the economic treatment of technology itself, where little change has occurred in economic theory during the last half-century. The basic standard treatment still follows the outline set forth by Debreu (1959), in which a firm's technological possibilities are specified by a production set. With the exception of consideration of economies of scope, contestability, and sustainability, economists haven't made progress in ascertaining which assumptions

[3] A range of approaches can be found in Schumpeter (1950), Romer (1990), Mokyr (1990), and Prescott (1998).

are appropriate for production sets. For example, microeconomic theory doesn't even recognize Henry Ford's assembly line, beyond stories about economies and diseconomies of scale leading to suitable shapes for cost functions. Yet the manufacturing sectors of modern economies have been changing rapidly. The business press has publicized the importance of mass customization, knowledge workers, computerization and networks in the workplace, just-in-time inventory controls, product life cycles, environmental concerns such as recycling and re-manufacture, and much more. New materials have appeared and new processes for both batch and continuous production have been developed. Computer-aided design and computer-assisted manufacturing have become common throughout most industries in developed countries. Perhaps our concept of technology needs to be redefined to go beyond standard production sets, or perhaps stronger assumptions are needed. Either way, economic theory must reflect these real-world advances.

In addressing this topic, I see vast potential for economists to learn from engineers. On the other side of many campuses, engineers are studying product selection and technology choice in a unified fashion, termed "product development" based on "design for manufacture" considerations. In our universities, unfortunately very little "technology transfer" has occurred in either direction.

More generally, the growing numbers of management of technology programs, which teach how to manage high technology enterprises, provide a prime example of new interdisciplinary connections. These programs usually lead to a certificate at the M.S./M.B.A. level for a case-study and project-based curriculum oriented toward individuals with interests and backgrounds in both business and engineering, computer science, or physical science. I'm acquainted with such programs at MIT, Cornell, Minnesota, Berkeley, and Stanford, but I'm sure there are others. Based on the influence that economists and economic research have earned in traditional areas of management science such as finance, I would expect economics to play a larger role in this growing area. This offers exciting opportunities for cross-fertilization between research and teaching during the coming decades.

■ *Preparation of this manuscript was supported by the National Science Foundation through grants SBR97-53139 and DMI98-16144, the Curtis Carlson Chair in Economics at the University of Minnesota, the Office of the Vice President for Research at the University of Minnesota, and the Federal Reserve Bank of Minneapolis. The views expressed herein are those of the author and not necessarily those of the Federal Reserve Bank of Minneapolis or the Federal Reserve System.*

References

Allen, Beth. 1990. "Information as an Economic Commodity." *American Economic Review* (Papers and Proceedings). 80:2, pp. 268–73.

Allen, Beth. 1991. "Choosing R&D Projects: An Informational Approach." *American Economic Review* (Papers and Proceedings). 81:2, pp. 257–61.

Barro, Robert J. 1980. "A Capital Market in an Equilibrium Business Cycle Model." *Econometrica.* 48:6, pp. 1393–1417.

Cournot, Augustin. 1838. *Recherches sur les Principes Mathématiques de la Théorie des Richesses.* Paris: Hachette.

Debreu, Gerard. 1959. *Theory of Value.* New Haven: Yale University Press.

Dickhaut, John, Kip Smith, Kevin McCabe, and Nicole Peck. 1999. "Inferred Brain Function in the Formation of Allocations in an English Auction." Preprint, Department of Accounting, Carlson School of Management, University of Minnesota, Minneapolis.

Forges, Françoise. 1986. "An Approach to Communications Equilibria." *Econometrica.* 54:6, pp. 1375–1386.

Grether, David M. 1999. "Physiological Bases for Economic Decisions." Presentation at Caltech on May 12, 1999 of joint work by John Allman, David M. Grether, Charles R. Plott, and Marty Sereno.

Holmes, Thomas J. and James A. Schmitz, Jr. 1995. "On the Turnover of Business Firms and Business Managers." *Journal of Political Economy.* 103:5, pp. 1005–1038.

Kohlberg, Elon and Jean-François Mertens. 1986. "On the Strategic Stability of Equilibria." *Econometrica.* 54:5, pp. 1003–1037.

Lucas, Robert E., Jr. 1972. "Expectations and the Neutrality of Money." *Journal of Economic Theory.* 4:1, pp. 103–24.

Lucas, Robert E., Jr. 1978. "On the Size Distribution of Business Firms." *Bell Journal of Economics.* 9:2, pp. 508–23.

Mokyr, Joel. 1990. *The Lever of Riches: Technological Creativity and Economic Progress.* New York: Oxford University Press.

Myerson, Roger B. 1986. "Multistage Games with Communication." *Econometrica.* 54:2, pp. 323–58.

Phelps, Edmund S. 1970. "Introduction" *to Microeconomic Foundations of Employment and Inflation Theory.* New York: Norton.

Prescott, Edward C. 1998. "Needed: A Theory of Total Factor Productivity." *International Economic Review.* 39:3, pp. 525–51.

Prescott, Edward C. and Jose-Victor Rios-Rull. 1992. "Classical Competitive Analysis of Economies with Islands." *Journal of Economic Theory.* 57:1, pp. 73–98.

Radner, Roy. 1968. "Competitive Equilibrium under Uncertainty." *Econometrica.* 36:1, pp. 31–58.

Romer, Paul. 1990. "Endogenous Technological Change." *Journal of Political Economy.* 98:5 Part 2, pp. S71–S102.

Schumpeter, Joseph. 1950. *Capitalism, Socialism, and Democracy.* 3rd edition. New York: Harper and Row.

Zhao, Jingang. 1992. "The Hybrid Solutions of an N-Person Game." *Games and Economic Behavior.* 4:1, pp. 145–60.

This article has been cited by:

1. Seung Han Yoo. 2013. An alternative proof for the existence of Radner equilibria. *Mathematical Social Sciences* . [CrossRef]
2. Carol Horton Tremblay, Victor Tremblay. 2010. The Neglect of Monotone Comparative Statics Methods. *The Journal of Economic Education* **41**:2, 177-193. [CrossRef]
3. Jingang Zhao. 2009. Estimating Merging Costs by Merger Preconditions. *Theory and Decision* **66**:4, 373-399. [CrossRef]
4. Sheila C. Dow. 2007. VARIETY OF METHODOLOGICAL APPROACH IN ECONOMICS. *Journal of Economic Surveys* **21**:3, 447-465. [CrossRef]
5. Sandra Braman. 2006. The micro- and macroeconomics of information. *Annual Review of Information Science and Technology* **40**:1, 3-52. [CrossRef]
6. KJELL HAUSKEN, JOHN F. MOXNES. 2005. THE DYNAMICS OF BILATERAL EXCHANGE AND DIVISION OF LABOR. *International Journal of Modern Physics C* **16**:01, 117-137. [CrossRef]
7. Emilio Fontela. 2002. F rom the wealth of nations to the wealth of the world. *foresight* **4**:1, 6-12. [CrossRef]