The Complexity Revolution and the Future of Economics

by

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The Complexity Revolution and the Future of Economics

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In a recent article, Robert Solow (1997), paraphrasing Oscar Wilde, described modern economics as "the overeducated in pursuit of the unknowable." In a previous article on the future of economics (Colander 1999) I developed that theme, but I also argued that economics was headed for a quite different future, one in which economics would become "the appropriately educated in pursuit of the knowable."[1] In this paper I expand upon those ideas, explaining where I see the process of change now, and how I see it changing the way we do and teach economics in the future.

The Process of Change

To think about the future of economics one must have a theory of how and why the study of economics changes. Heterodox economists, often implicitly, see that process of change as occurring through an outside revolution, as mainstream economists see their mistaken ways and change their views to a new reality. In this view change comes from the outside—ideally from heterodox economist's views being accepted. I don't see it that way. Most of the change in economics has come about from the inside, from young professors at top schools who start doing economics in a different way than was previously done. These changes occur because (1) technological changes in analytic and computing methods open up new avenues of study, and (2) because the "low hanging fruit" from previous approaches and methods have already been picked.[2]

How much of this change is allowed, and how it works its way through the profession, is a complicated process that I have explored in a forthcoming book, *The Changing Face of Economics*. (Colander, Holt and Rosser, forthcoming) In it we interview individuals on the cutting edge of change in the profession. What we find is that most of the change takes place in a slow evolutionary process that relies on elite individuals within the profession being open to change, but that the actual change takes place through the replicator dynamics of the profession. By that I mean that most economists do variations of what they were taught to do, and do not change much. Instead, the key to understanding change is the choices new graduate students are making about dissertation topics. These choices are tied to technology, to the analytic methods that they come to graduate school with, and to the analytic methods that they learn in graduate school. In my view the profession changes not because of radical changes in existing

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[1] Obviously I cannot be sure what Solow meant by his comment. My interpretation of what he mean is that economics at the turn of the millennium set too high goals for itself in trying to understand the deep theory of the economy, and that perhaps it would do better to set lower goals, finding usable relationships among variables, and concentrating on analysis that accepted the data limitations faced by economists.

[2] The incentives directing the choices are built into the institutions of the economic profession. I have explored these incentives in Klamer and Colander (1990) and Colander (1991).

economist's research, although some of that does occur, but instead through an evolutionary process brought about by the hiring and retirement process.

Graduate schools usually have a variety of different approaches represented in their faculty that are broader than what is often described as the normal orthodoxy, but which do not include many approaches that are defined as heterodox. Graduate students are attracted to those professors using approaches that seem dynamic and likely to result in publications and advancement. Over time there is a subtle change in the professors to which graduate students are attracted; older professors, who are using older analytic technology, get fewer graduate students; younger professors, who are using newer and more advanced techniques, get more. This creates a dynamic toward different, and more and more advanced techniques becoming the norm. As they do the selection committees look for new students who are better trained in the new analytic techniques being emphasized, which furthers the process of change.

As time passes, younger, differently trained, economists replace older economists, and the average image of what economics is and how one does economics changes. Since the profession replaces itself every 35 years or so, I estimate the underlying rate of change from this evolutionary process at about 3% per year. However, even that rate of change may be an over-estimate of the degree of change in the initial stages of a cycle of change, because most students choose to work with established professors in established methodologies; they do so because the newer methodologies and techniques are risky. Initially only a few risk-preferrers choose that path. So, at the beginning of a cycle of change, the rate of change toward a new acceptable approach is smaller than that 3%, probably closer to 1%. However, at some point a critical mass of work is accumulated, a shift point occurs, the new approach becomes the hot approach, and students flock toward it. At that time the rate of change increases to greater than 3%.

Because of this process, economics is becoming increasingly technical, and will continue to do so. Students are better trained in mathematics, statistics, and analytic methods. Computing power has increased, so that economists now coming into the field approach problems in different ways than did earlier economists. This increase in the technical nature of the field has sometimes been associated with formalism, and for a while in the late 20th century it was, but the modern technical developments have actually allowed a movement away from formalism, and toward a more applied mathematical approach.

This move toward more technical, but less formal, work is driven by increasing computing power. With computer power doubling every 18 months, the need to rely on analytic solutions decreases and the ability to extract information from data increases. Both of these effects reduce the value of analytic deductive theory. One can get one's insights from the data and from simulations, reducing one's reliance on the deductive theory that characterized formalism. Because of the predicted increase in computing power I see modern economics becoming more and more technical, and less and less limited by deductive formalism. Eventually, economists will have virtual economic simulations in which they can study alternative policies. These virtual economies will form the centerpiece of economist's tool kits. But that is far in the future. Now, we are taking only the initial steps away from our previous deductive approach.

Moving Away from the Holy Trinity

This movement away from deductive analytics is probably best seen in the way in which younger economists treat the holy trinity assumptions of rationality, greed and equilibrium. These assumptions were the foundations of the deductive analytic approach, and were previously treated as sacrosanct. Changing them meant giving up one's foundation of theory. Modern economics is slowly moving away from the holy trinity, and toward a broader foundation of economic theory of purposeful behavior, enlightened self-interest and sustainability.

The changes that are occurring can be seen in a variety of theoretical work, such as work in behavioral economics, evolutionary game theory, agent based modeling, experimental economics, and new institutional economics.[3] Indeed, as I have argued elsewhere (Colander, Holt and Rosser, forthcoming) much of the work that is considered cutting edge theoretical work falls into the category of moving away from the holy trinity.

One can see the movement in the allocation of recent awards in economics. For example, Daniel Kahneman and Vernon Smith recently won a Nobel Prize for their work in behavioral and experimental economics and Mat Rabin won the John Bates Clark medal for work on behavioral economics. Because of these changes today one would no longer describe modern economics as neoclassical economics. (Colander 2000a) I do not want to overstate the degree of change that is currently taking place in the profession; one sees only slight change in the work of most existing economists. But, because of my view of the process of change that I described above, I see these small changes as an indicator of much larger future changes, although those changes will likely occur in a series of sudden jumps, rather than in a smooth progression.

To make predictions about how these changes are altering the field of economics requires one to make decisions on what new assumptions and techniques will be chosen, and speed up the evolutionary process, looking at changes in generations, not in decades. Thus, I argue that the small steps that we are currently taking in modifying the assumptions of theory portend major changes in the future for how economics, and economic policy, is thought about. To consider just one example: theorists such as Jean Tirole (Tirole forthcoming), following up on the work of Thomas Schelling, are now considering how individuals struggle to restrict their own behavior. In doing so the theorists are accepting that an individual's actions may not in some broader sense reflect what the individual truly wants to do. That change, if adopted more generally, has enormous implications for change in applied policy issues; for example it can justify a whole range of taxes or restrictions on behavior, which from our current theory, would be unjustifiable.

Where the Changes Are Heading

In this paper my interest is not so much in the particular changes that are taking place, but in the overall effect of the sum total of them, and in the direction that I see those changes taking

[3] That is close to happening in behavioral economics in certain fields such as finance. As Richard Thaler has said, once, people asked what was behavioral finance; now people ask what other type of finance is there? A leading indicator of the changes that are occurring, one looks at the hiring priorities of top schools, and the needs their hiring departments see. In the early 2000s behavioral economics is seen as a hiring priority; experimental economics is not yet a totally accepted hiring priority, and agent based modeling is hardly on the horizon[3]

economics and economics teaching in the future. My thesis is that the changes involve a major shift in the underlying vision of what economists study, and how they study it. Specifically, I see the changes leading from a vision that sees *economics as the study of infinitely bright agents in information rich environments to a vision of economics as the study of reasonably bright individuals in information poor environments.*

Another way of describing my thesis is that the vision of the economy will evolve from its previous vision of highly complex, "simple system" to a highly complex "complex system."[4] Simple systems, no matter how complex, are reducible to a low dimensional set of equations, making it possible to model the system analytically. A complex system is not so reducible, and must be represented in another fashion—through simulation, or through insights gained with replicator dynamics. One never has a full analysis of the entire complex system.

Simple and complex systems differ in their micro foundations. Simple systems can be studied from micro foundations alone. Complex systems involve emergent properties, and cannot be understood from an analysis of the elements of the components of that system. There can still be micro foundations, but the micro foundations of complex systems are contextual, and can only be understood in reference to the existing system. Such complex systems are built up in path dependent stages, making individual optimization within such systems history and institution specific. This means that its institutional structure is central to understanding complex systems, and that any assumed rationality must involve some boundedness.[5]

The acceptance of this complexity vision of the economy involves a shift in economics far more fundamental than anything associated with the movements away from the holy trinity that the profession has made so far. But by moving away from the holy trinity economics is making the first step toward such a new vision.[6]

Understanding the Nature of the Change

Jokes about the economics profession are often revealing of the self-image that the profession has of itself. One joke that is often told to make fun of economists' deductive and non-practical tendencies is the can opener joke. In it a physicist and a chemists offer practical solutions to a problem of opening a can on a desert island, while the economist offers a useless solution--to assume a can opener.[7] That joke is not very complementary of economists and it provoked a less well-known joke that portrays economics in a better light. The joke is the following:

[4] For a discussion of what is meant my complex system see Ayung (2000)

[5] These ideas are developed in Colander (2000b).

[6] Of course the simplicity view has not always been the view of economics and thus the movement toward complexity will be a movement back to earlier writers, including Smith, Marshall, and Hayek. See Colander (2000c) for a discussion of the complexity in the history of economic thought, and Colander (forthcoming-a) for a discussion of how economics moved from a vision of an economy as a complex system to a vision of the economy as a complex simple system.

[7] The joke is so well known that I do not repeat it here, but those who do not know it can find it at www.aeaweb.org/RFE/Neat/JokJokAboEco.html.

A physicist, an engineer, and an economist are given a watch, a string, and a ball and are told that the person who can best measure the height of a building will get into a Scientific Hall of Fame. The physicist ties the ball to the string and hangs it down from the roof. Using the stopwatch, he calculates the length of time it takes the pendulum to swing from side to side. From that information he estimates the height of the building. The engineer takes the ball and drops it off the top. He then uses the stopwatch to determine how long it takes to fall, and estimates the height of the building accordingly. The economist, however, wins the place in the Hall of Fame by taking the stopwatch, trading it for the building plans with a guard in the building, and simply reading the height of the building from the blueprints.

This joke, obviously made up by an economist, shows both the benefits of trade and the importance of economic theory. That theory provides a blueprint of how the economy operates, and thus is to be guarded at all costs once found. It also shows that economist's assumption that the economy is a complex "simple" system, because those are the only systems for which one can find a complete set of blueprints.

The problems with this story from a complexity point of view are the assumptions that a set of blueprints exists, or that the building of the economy actually followed that set of blueprints if they did exist. The complexity vision sees the economy as emergent from a set of simple decisions in a way that no one previously pictured. Thus the complexity addendum to this story, which Robert Bassman suggested to me in private discussions, is that when the building took place, the builders made adjustments to the plans, which they never marked down on the blueprints. The economist reading from the blueprints got the wrong answer.

The questioning of the holy trinity can be seen as a movement away from a search for the blueprints of the economic system, and toward a search for understanding a system in which the blueprints are missing or nonexistent. Consider rationality. In order to achieve a blueprint of the economy strong rationality must be assumed, where individuals have information about all other's actions, and can determine what they will do given that information. The models one derives given these strong assumptions are justifiable because they provide the blueprint for the economy—once we have that blueprint we can proceed to discussions of practical issues. Behavioral economics is a direct challenge to that belief—it involves a different sense of theory and of rationality; a behavioral economist looks at what people do, and builds in those observations into his or her assumptions about behavior in his or her models. Behavioral economics is designed for economists operating without blueprints.

The "simple" approach relies on theory, uses empirical observation to test the theory, and then builds policy analysis around that "empirically tested" theory. The "complexity" approach relies on empirical observation, builds theory around those observations, and then builds policy around the resultant "empirically-determined" theory.[8] The type of rationality assumed is a key difference in the two approaches. Both assume rationality—all models of economics must assume some type of rationality—but there is a difference in the type of rationality and the level of information assumed.

8 I have called the resultant applied policy "muddling through" approach to policy to be contrasted with the economics of control approach to policy in the "simple" economy. (Brock and Colander, forthcoming a, b)

The work done at CeNDEF (http://www.fee.uva.nl/cendef) is an example of the approach I have in mind that will become the dominant approach in the future. Researchers there are combining new and old strategies to address fundamental questions. For example their theoretical work is calibrated to reproduce many features of real world data, but is based on heterogeneous agents with differing degrees of rationality, rather than on homogeneous agents. Their choice of assumptions is further governed by experimental and econometric work using field data. They study how changing the degree (e.g., the "dial") of rationality creates dynamical patterns in their artificial economies, which are then compared to dynamical patterns observed in actual economies. They use complexity tools such as bifurcation theory to study these pattern-generating mechanisms analytically as well as computationally.

The Technical Nature of the Economics of the Future

Prior to recent technological developments in nonlinear dynamics, chaos theory, complexity theory, and in computing power that allows researchers to gain insight into systems without analytics, anyone (such as Ronald Coase, Douglass North, or Oliver Williamson) who felt the economy was complex, was forced to take a heuristic approach. That heuristic approach was not consistent with the scientific vision that economics had of itself. The formal alternative to that approach was the general equilibrium theory such as seen in the work of Gerard Debreu. At the time this formal approach was developed, using heuristics to explore the complexity vision was reasonable because in the complexity vision even the most technical approach at the time was far too simple to achieve much insight if the economy was truly complex.

The difficulty for heuristic analysis in the profession is that it tends to be nonreproducible. It is dependent on the researcher having original insights and the personality to make others take those insights seriously. Few graduate students, even top ones, have those abilities. Most take an existing technique and apply it.[9]Technical work is far more reproducible; it exhibits significant increasing returns to scale. For that reason I believe that the non-technical work of North, Williamson, or Coase is not the future of economics. Instead the future of economics is increasingly technical work that is founded on the vision that the economy is a complex system.

Again, I want to emphasize that the technical future I see is not an extension of the past. The nature of that technical work will change from highly technical pure mathematical work to highly technical applied mathematical work. The pure mathematical approach that I believe is in decline follows in the tradition of Hilbert—it is technical in the sense that it is deductive pure mathematics and attempts to establish an axiomatic foundation for the field. The economics that was "in" in the 1960s and 1970s was closely tied to this approach—the Arrow/Debreu proof of

[9] For example, one of the reasons Milton Friedman had many followers in macro because he offered students the chance to do money demand and permanent income studies using data from a variety of different countries and newly developed econometric techniques. Similarly, one of the reasons Paul Samuleson had many students because he offered students a chance to develop one of the many models that he had structured. One of the reasons Ken Bolding and Abba Lerner had few graduate students because they did not offer students a set of dissertation topics that were the application of a fairly clear technique to a slightly different set of problems.

general equilibrium and the extensive work that followed in that Arrow/Debreu tradition are examples.[10]

A pure axiomatic approach attempts to start with a minimal number of assumptions and arrive at as many conclusions as possible from those assumptions. As economics developed its core assumptions in the 1950s, the holy trinity set of assumptions--greed, rationality, and equilibrium--came to be accepted as the pillars upon which theory was to be based. There were obviously many differences in how these three pillars were used, but in the pure theory of economics they were well specified, and the Walrasian general equilibrium program (called that even though Walras likely would have disagreed with significant portions of it) made them central to its goals. It asked such questions as: Can we prove existence and stability of equilibrium given the specification of these assumptions? This axiomatic approach is a deductive approach that starts with first principles and builds up a theory from which policy implications are drawn. Then, and only then, are those implications empirically tested.

This axiomatic approach requires parsimony in assumptions. Because of the intricate way in which assumptions are tied to empirical observations and policy implications, a slight change in the specification of core assumptions can change implications drastically. Thus, once the initial assumptions are chosen, they became highly entrenched and almost unchangeable.

It was this axiomatic approach that a number of us were reacting against in the 1980s when we started our campaign to change economics. (Klamer and Colander (1990), Colander and Brenner (1992)) However, we weren't quite the rebels that we seemed. In fact, in that campaign we were swimming very much with the current, which is why our work led to the establishment of the COGEE commission in the U.S. and why there was a decreased ranking of the axiomatic approach by the economics profession. While the axiomatic approach remains today, it is, in my view, far less dominant than it was. In the future of economics that I see axiomatic theory is no longer the central approach to be supplemented by applied and empirical work. In the future, the relationship will be the other way around: axiomatic work supplements applied and empirical work.

The first step away from that axiomatic approach is currently taking place as the pillars of the axiomatic approach have become far more flexible, which means that there are no absolute deductive implications that follow from core theory. As the former axiomatic foundations of economics are abandoned, economists are turning away from pure mathematics and toward applied mathematics. The approach of applied mathematics to studying a subject is fundamentally different than the approach used in the pure mathematical approach. *In the applied mathematics approach, mathematics is not the foundation of the theory but is simply a tool to be used to aid one's intuition and applied policy work*.

The applied mathematical approach is, at its core, an empirical approach in which intuition guides one's thinking. Mathematics and statistics are used as an extension of the brain

[10] This axiomatic approach follows a tradition in economics that goes back to David Ricardo (but not to Adam Smith or John Stuart Mill). See Weintraub (2002) for an interesting discussion of these issues.

to aid in the analysis. The work does not attempt to provide a deductive foundation to economics, but instead serves as a tool for reasoning and pulling information out of data.

This change from the axiomatic approach to applied mathematical approach is symbolized by two conferences held nearly a decade apart at the Santa Fe Institute. The first, held in the mid 1980s, generated a book entitled *The Economy as a Complex Evolving System* (Anderson, Arrow, and Pines, 1988). Waldrop (1992) reported that this conference featured a set of largely mainstream economists and defenders of general equilibrium orthodoxy, assembled by Kenneth Arrow, and a set of physicists assembled by others. At that first conference the economists mostly attempted to defend their axiomatic approach, facing sharp challenges and ridicule from the physicists for holding relatively simplistic views.

The second conference held in the mid 1990s saw a very different outcome and atmosphere than the first. (Arthur, Durlauf, and Lane, 1997) No longer were mainstream economists defensively adhering to general equilibrium orthodoxy. Now they were using methods adopted from biologists and physicists, many suggested at the earlier conference, in innovative ways. They were much more open to complex economic analysis.

These two Santa Fe conferences are representative of the change that occurred throughout the profession during this time. It was as if the ideas planted by earlier researchers in many areas, such as experimental economics, behavioral economics, and nonlinear dynamics, were taking root. Today the mainstream of the profession has accepted many of the methods and approaches that are associated with the complexity, applied mathematics, approach.

Changes In Economic Policy Analysis

The change in the approach to theorizing will be supplemented by a change in the approach to applied policy. Currently, the textbooks teach an applied policy approach that follows from the axiomatic approach to theory. It focuses on efficiency to the exclusion of other goals. Given appropriate assumptions, the economy will arrive at an efficient outcome. If there are externalities government action is necessary to internalize those externalities; textbook economics policy discussions focus on policies designed to guide the economy to a Pareto optimal position. Efficiency, interpreted as maximizing output independent of the distribution of that output, is currently the central focus of textbook policy models.

How we got to that point is an interesting story of its own. It begins with the philosophical approach to policy in the Grand Tradition associated with Smith and Mill. In that tradition laissez faire was supported for a variety of reasons—achieving efficiency was one of them, but not necessarily the most important. In the late 1800s, economists saw a smaller role for economists in policy, operating in their capacity as an economist, than did earlier economists. Both J.N. Keynes and Alfred Marshall focused on limited applicability of pure theory for policy, and suggested that applied policy should be considered an art, outside of both positive and normative economics, rather than a science.

Pigou backed away from this differentiation between art and positive economics, and attempted to provide a seamless flow from positive theory to policy precepts. Inherent in his model, however, was a material welfare approach to utility theory, an approach in which

economics was only a subset of analysis that was to go into policy, the subset being "that part of social welfare which can be brought, directly or indirectly, into relation with the measuring-rod of money."[11] Pigou's work began the movement to modern welfare economics, but it was a quite different welfare economics than we know today, for example, it had economists supporting progressive taxation on the basis of economic theory. The reason Pigou could support such policies was that he took for granted that there was diminishing marginal utility and that a redistribution toward low wages earners would increase social welfare.

The next step in the progression to modern welfare economics was Lionel Robbins' change in the interpretation of utility from a material welfare interpretation of utility, to an ordinalist interpretation of utility--what Pareto called ophelimity. The material welfare interpretation of utility referred to usefulness, was determinable by introspection, and was comparable across averages of individuals. Ophelimity referred to satisfaction of desire; it was not determinable, and was not comparable among individuals. Material welfare economists focused on utility; ordinalist neoclassicals focused on ophelimity. That shift made economics apply to all goods, not just to material welfare that involved a set of generally accepted, but unprovable, assumptions about the nature and comparability of utility functions. This change in approach redirected economics to a policy focus on efficiency to the exclusion of other goals. Other goals involved subjective reasoning and should not be part of positive economics.

Lerner's *Economics of Control* (1944) integrated Pigou's arguments with Robbins interpretation of utility, and placed them into a general, rather than a partial, equilibrium framework. In doing so Lerner established the framework for policy that still is central to the textbook presentation of economic policy, although he attempted to maintain a bias toward redistribution by his uncertainty approach to utility.[12] That framework has been much refined since then, with the new welfare economics and the new new welfare economics, but the central elements of Lerner's economics of control framework, minus his attempt to provide a rational for redistribution, remains the central policy vision taught in the economics texts.[13]

The textbook framework for policy analysis centers on efficiency and deviations from optimality because of externalities, and leads economists to think of micro policy, and of the role of economists in the policy process, within that framework. In actual fact, much micro economic analysis takes place with little regard to this framework, and is primarily an analysis of relations among variables and common sense theory, but the structural foundations for policy, and the framework within which economists think of policy remains within Lerner's economics of control framework. The movement away from the holy trinity is a movement away from the economics of control foundation for applied policy, and any deductive underpinnings for economic policy. It forces applied policy back into J.N Keynes' art.

The complexity vision of the economy is inconsistent with that economics of control framework, and thus if it is accepted it will mean a major difference in how economic policy is thought about. The policy approach consistent with complexity is a muddling through approach,

[11] See Cooter and Rappaport (1984) for a discussion of the material welfare approach.
[12] I discuss this development in much more detail in Colander (2003)
[13] This approach is challenged by economists such as Amaytya Sen (1999), but his work generally does not make it into the principles or even intermediate textbooks.

which is more closely related to the work of Smith, Marshall, Hayek, Coase, and Sen than it is to the Lerner tradition. I call it a muddling through approach to policy, because in a complex environment the best policy makers can do is to muddle through.

To understand what I mean by a muddling through approach, consider the building of the beautiful and amazing medieval cathedrals. That building did not rely on knowledge of scientific laws to guide the building, but instead relied on accumulated rules of thumb of what worked and what didn't. The building proceeded by trial and error. Different methods of construction would be pushed to the limit until a cathedral caved in somewhere, and then the rules of thumb would change. As the stored knowledge increased, the cathedrals became more grandiose, even without a specific understanding of the laws underlying them. That came much later. Muddling through policy follows that same approach. It is conducting policy without a full knowledge of the general laws of the economy, if there are any. What you can find, at best, are general rules of thumb for how things have worked in the past, and possibly some exploitable patterns.[14] Muddling through is not building without rules; it is simply building without an ultimate set of blueprints, which makes the rules far more tentative and cautionary.

In muddling through economic reasoning is directed by an educated common sense, and what Tom Schelling has called the "vicarious problem solving" approach. In it one informally models the situation assuming agents "operate in a purposeful manner, aware of their values and alert to their opportunities." Using this approach the researcher figures out what an agent might do by imagining him or herself in the person's position, as best he understands that position, and decides what that person will likely do given that person's aims, values, objectives, and constraints. (Schelling 2003) It is a type of armchair theorizing that most economists do.

But there are two differences. The first is that in muddling through this armchair theorizing is only the beginning of the analysis. It is the exploratory work that then will be supplemented by a variety of highly technical work, which will provide a foundation for the temporary solution to the problem one works out. This work might include field studies, agent based modeling, statistical data analysis and a variety of other techniques that might shed light on the issue. The second difference is that the assumptions about the agents will reflect how actual agents operate, and not any predetermined sense of rationality. Thus, the agents being modeled will be characterized by one's understanding of oneself, and insights from psychology.

Initially, the changes in policy analysis associated with the complexity revolution will come slowly and will be appended to existing thinking. Thus, the first set of policy proposal changes that are coming from behavioral economics involve slight addendums to standard economic results. These changes are acquiring the name benign paternalism (Benjamin and Laibson forthcoming) or libertarian paternalism. (Sunnstein and Thaler forthcoming) In this policy work one uses the insights coming from behavioral work in economics to modify the way in which policy is implemented. For example, one of the insights of behavioral work is that

[14] Now, even in a muddling through approach searching for a set of architectural plans can make sense for indeed they might exist. Thus, I would expect that in the future a few individuals will continue to search for them; abstract theory based on pure math has a role to play in the future. But it is only one strategy in the process, not a strategy to put all ones marbles in. The majority of the applied policy work will be about solving particular problems with whatever technical tools are available to them.

preferences are often ill formed. This fact means that small, seemingly innocuous, differences in the institutional environment, such as in how a choice is presented to an individual, play important roles in outcomes of policies. Libertarian paternalism involves structuring choices in a way that lead to results that the policy maker believes in best for the individual.

An example that advocates of this policy use is the structure of savings plans in which individuals must choose whether they want to automatically save or not.[15] If the policy maker structures the program with the default option being that the agent saves, approximately 80% choose saving; if he or she structures the default option as one in which the agent does not save only 30% choose to save. If the paternalistic policy maker believes saving is good, he or she structures the program so that the default option is saving. In doing so the individual's consumer sovereignty is not being violated, because he or she is choosing whether he or she wants to save, and may change at will. But by taking advantage of insights from psychology and structuring saving as the default option, the policy maker is guiding that choice to the one that the policy maker believes is best for the person.

The Slippery Slope

Libertarian paternalism seems like it involves only a small change in policy implications, and that it can be added as an addendum to standard welfare arguments of economics. In my view, that is not the case. Accepting the psychological assumptions upon which it is based undermines standard welfare theory, and thus cannot be appended to it. Instead, the implications for future change in policy analysis of accepting the implications of psychological insights are substantial. There is no reason for the policy maker to stop at libertarian policies. Accepting psychology's insight and giving up the rationality and greed foundation for policy means accepting that people's actions do not necessarily reflect what they would "really" want. Psychology shows that individual's choices are influenced by a variety of factors and can be directed in many ways, (an insight that has not gone unnoticed by many real world firms.) Thus, based on standard economic theory without the rationality pillar, there is no reason to stop at libertarian policies. If one accepts that policy makers have some insight into what is good for individuals separate from what they actually choose, a premise that is the basis of libertarian paternalism, then there is nothing in existing standard economic theory to state that one should not go further. For example, why not design policies that take into account individual's tendency to exhibit hyperbolic discounting, and design policies to restrict immediate choice, by guiding individuals toward precommitment against immediate gratification? Such policies would get significant support among liberal economists.

One can easily go further. Once one accepts that people's actions do not necessarily reflect what they really want, there is no theoretical reason within the economics of control framework to restrict individual behavior to get people to do what is good for them. For example, Robert Frank (1999) argues that a set of goods, which could be called relational goods, are primarily desired because others have them, which means that individual's welfare from a variety of luxury goods is determined by what one has relative to others. In that case, a policy of taxing luxuries can bring in revenue to the government and actually improve social welfare.

[15] In the U.S. these are called 401k plans.

Extending this line of reasoning, and assuming that advances in neuropsychology give us a much better sense of individual psychology, from a society's point of view, there may well be a determinable optimal set of tastes, and policy can be devoted to achieving that optimal set of tastes in order to optimize social welfare.

Economists as a group, even liberal ones, would, I suspect, be very much against such paternalistic policies. It is in fundamental opposition to the grand liberalist tradition of economics. The public, however, would probably be far less concerned since economists are usually much more hesitant about paternalistic policies than is the general public. My point is not that economics should support paternalist policies; my point is that, in principle, given that one accepts a behavioral foundation of economics, that hesitancy to accept paternalistic policies is not based upon deductive theory, since the underlying model that grounded that view has been eliminated when one gave up rationality. Within the new model of endogenous tastes, agents can be made better off, even in their own minds, by government paternalistic actions, because agent's actions do not reveal their true desires. Thus, the end result of giving up the holy trinity and adopting a behavioral foundation for economics is a much more complicated set of policy arguments, where right and wrong policy will be harder to characterize, and alternative explanations of economists' fear of paternalism will become part of the policy analysis.[16] Policy analysis will require muddling through as best one can using the technical tools available.

The More Distant Future

The above discussion has focused on the near term future, and issues that I believe will likely be in debate over the coming decade or two. Let me conclude the paper with some brief discussion of the longer-term future of the profession, and whether an economics profession will survive its movement away from the holy trinity. I predict that it will not, at least in the structure that we know it. The reason is that as economics moves away from its holy trinity assumptions, more and more cross specialization will occur. New hybrid fields will develop: psychoeconomics, neuroeconomics, socioeconomics, bioeconomics, and a variety of others. The training, and tools of each will differ, pulling the profession apart. Without the holy trinity of assumptions holding it together, the profession will ultimately loose it coherence as a single field. It will exist, but as loose associations of different approaches, such as what one finds in the field of psychology today.

At the same time that research specialties will be pulling the profession apart, so too will the policy applications, because they will be each institution specific. New, specific policy subfields, such as health economics, macro-forecasting economics, and forensic economics will increase in importance. What will hold these various branches together will no longer be an adherence to the holy trinity in approaching problems, but instead a shared set of applied mathematical tools such as game theory, statistical methods, and experimental methods. But these methods transcend disciplines, and will likely be shared by an increasing portion of other social scientists. Without assumptions and methods to differentiate economics from the other social sciences the study of social issues will become more and more transdisciplinary.

[16] In the muddling through approach that I have been advocating (Brock and Colander (2000) an important limitation on policy is policy makers ability to understand the effects of any policy in a complex system.

Ultimately, there will no longer be psychologists, sociologists and economists, but simply social scientists, who can be divided up in a variety of ways that are impossible to predict.

So what I am predicting is that there will be a redefinition of the boundaries between economics, and other social sciences. As that happens economics work will become more specialized as different fields become separate fields in their own right, and are no longer taught under the general "economics" umbrella. For example, macro will become integrated with complex systems study, and will be seen as a fundamentally different field from health economics, which in turn will be seen as a different field from, say public finance. It will become less specialized because the new sub-fields in economics will cross current disciplinary boundaries, with the training in the various social science and related fields such as psychology, and applied mathematics, becoming intertwined.

My second prediction concerns the nature of modeling that will likely predominate in the future. Behavioral economics, which involves a challenge to the rationality and greed assumptions, is currently having the biggest impact on economics. But that, in my view, is simply a precursor of a larger change in method and analysis that will follow. That larger change involves the third pillar of economics—equilibrium. Accepting a behavioral foundation of economics requires one to give up equilibrium because the interactions become too complex to analytically solve for equilibrium. To overcome this problem economists are now developing agent-based models, in which researchers grow a model of the economy. They will create virtual economies, in which virtual agents are endowed with behavioral characteristics that will become more and more similar to real world agents.[17] These models require no analytic specification of equilibrium, simply a specification of the behavioral characteristics of agents. Model simulation is relied upon to determine what likely basins of attractions will be.

Work on such models is currently being done in a number of areas. To give a sense of what is to come, consider the work being done in finance. There economists have created models in which agents choose strategies from a set of strategies similar to those followed by individuals on the street. Through multiple computer runs insight is gained about how such a system operates. The system has no equilibrium and each run may be different, but one can get a probabilistic sense of what will happen by repeated simulation. The results of that simulation are then calibrated to real world data to determine the probabilistic accuracy of the simulation.[18]

These agent-based models are still in their infancy, but in my view they will become central to how economic is done in the future. As long as the computing power continues to double every 18 months, deriving information from agent based models will become less and less expensive, and will eventually become more and more important as a tool of policy makers when testing implications of certain policies.[19] Ultimately, a set of computer simulation models, which embody the essential observations of the experimental and empirical data, will form the

[17] See Robert Axtell and Josh Epstein (1996) and Robert Axelrod (1997) for an early attempt at such a model.
[18] See Blake LeBaron, et al (1999) for examples and discussion.
[19] When I say that these agent based models will become the primary tool of policy makers, I am not suggesting that they will operate in lieu of other models. Behavioral insights endowed into the agents will still come from experimental work, and calibration of the models to real world data through statistical means will still be necessary.

theoretical basis of each of these the various new fields that have evolved out of what was once economics, and those models will be supplemented by a study of statistical methods to extract information from data, and a study of the institutions specific to each sub field.

Concluding Comments

Fields of study are often presented to students as static. The hypothesis of this paper is that economics is anything but static, and is composed of many different strains that are continually changing. Ultimately, it is the analytic and computing technology that will determine how this change occurs and the approach to research that social scientists will follow. Because technology is changing, significant changes are likely for economics in the future.

These changes will show up in research and in field courses first; I do not see them occurring any time quickly in the textbooks. The reason is that the principles course is itself marked by some of the same complexity. From a complexity point of view, slowness is probably for the best. The reason is that the principles, and even the intermediate textbooks, are not written for future economists; they are written for future citizens and businesspeople. For all its problems with serving as a vision for economic theorizing, the current efficiency textbook model being taught serves these students well.[20] The undergraduate economics course is designed to add value to the understanding of these normal students, and the current structure does that. True, it doesn't prepare them to be scientists, or even to have a sense of what real science is, but almost none will not go on to be scientists; they will go into business, where the lessons they currently learn in principles of economics—that there are opportunity costs to every decision and that there is no such thing as a free lunch--pay high dividends. This leads me to believe that the movement to a new economics, which I believe will occur, may also undermine one of the primary roles economics teaching currently plays today in the university curriculum. What will replace it, I do not know; as with all change, there are both costs and benefits.

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[20] This structure of the micro portion of the principles course goes back to Alfred Marshall and his Principles of Economics. Marshall's Principles was written for what might be called normal students. He assumed that the best students went into math, the second best into physics or philosophy and the others—the normal students-- would take trypos in other areas such as economics.

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