

The future of economics: the appropriately educated in pursuit of the knowable

David Colander*

This paper argues that, currently, significant change is taking place in economics because (1) technological changes in analytic and computing methods are opening up new avenues of study, and (2) the ‘low hanging fruit’ from previous approaches and methods have already been picked. It offers a vision of the future of economics that sees economists focusing less on the study of infinitely bright agents operating in information rich environments and more on the study of reasonably bright individuals operating in information-poor environments. Agent-based models and computer analysis of data will increase in importance, and deductive analytics will decrease in importance.

Key words: Agent-based model, Complexity, Vision, Muddling through
JEL classifications: A1, A2, B2

1. Introduction

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.

2. The process of change

To think about the future of economics, one must have a story of how and why the study of economics changes. The aspect of change that I shall focus on here involves

Manuscript received 20 October 2003; final version received 24 September 2004.

Address for correspondence: Middlebury College, 215 Munroe Hall, Middlebury VT, 05753 USA; email: colander@middlebury.edu

* Middlebury College.

¹ Obviously, I cannot be sure what Solow meant by his comment. My interpretation of what he meant 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.

forces that are inherent in economics' current institutional structure.¹ I emphasise these forces because, given the economic profession's current institutional structure, and its emphasis on technique in graduate education and publication, these forces are likely to be highly significant in determining the future path of the economic profession.

How much change is allowed, and how changes work their way through the profession is a complicated process that I have explored in a book, *The Changing Face of Economics* (Colander *et al.*, 2004) and here I shall simply outline the argument made there. In that book, we interview individuals on the cutting edge of change in the profession, and argue that, currently, significant change is taking place in economics because (1) technological changes in analytic and computing methods are opening up new avenues of study, and (2) the 'low hanging fruit' from previous approaches and methods have already been picked.² These changes have not been widely noticed because they are evolutionary, not revolutionary; they occur as older university teachers, using older approaches, leave the profession and are replaced by younger teachers using newer approaches.

Briefly, the process of change that I am focusing on is the following: 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.³ Graduate students are attracted to those teachers using approaches that seem dynamic and likely to result in publications and advancement. Over time there is a subtle change in the teachers to whom graduate students are attracted. Older teachers, who are using older analytic technology, get fewer graduate students; younger teachers, who are using newer and more advanced techniques, get more. This creates a dynamic toward different, and more and more advanced, techniques. As that happens, the selection committees choosing new university teachers look for job applicants who are better trained in the new analytic techniques being emphasised, 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

¹ Clearly, there are many other aspects of change, such as how real-world events transpire, or the success of existing explanations at predicting what events will occur. The aspect that I am discussing here works in combination with these.

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

³ One sees few heterodox economists at top schools, but one does see variations of orthodoxy, and there is a fine line between acceptable and unacceptable deviation in thought from the mainstream. Those economists who have crossed that line are defined as heterodox; those who have not are defined as innovative. While heterodox economists can influence the profession by winning over a majority of economists to their views, given the sociology of the profession, that is very difficult, and to the degree they do succeed in convincing people, they move back into orthodoxy. Heterodox economists are more likely to influence the profession indirectly as a catalyst of change, by pointing out problems with the orthodox views, getting mainstream economists thinking about the problems with the current approach, and suggesting new ways of approaching issues. Thus, many of the changes in economic thinking that I am predicting are changes that have been advocated by heterodox economists for years. The role that heterodox economists serve as a catalyst is important, but usually unsung, since when the developments actually take place within the mainstream, usually heterodox economists' catalytic role in emphasising these issues is not mentioned by the younger researchers (who either are unaware of the history of the idea, or have been taught that it is unimportant since it was part of a different conversation.) Why this occurs is intricately related to the sociological nature of the profession and is beyond the scope of this paper.

of change from this evolutionary process at about 3% per year.¹ 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 teachers in established methodologies; they do so because the newer methodologies and techniques are risky. Initially, only a few risk-preferrers choose the newer path. So, at the beginning of a cycle of change, the rate of change toward a new acceptable approach is lower 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 towards it. At that time, the rate of change increases to more than 3%.²

One of the outcomes of this process is that economics is becoming increasingly technical, and will continue to do so. Incoming students are better trained in mathematics, statistics and analytic methods.³ 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 twentieth century it was, but the modern technical developments have actually allowed a movement away from formalism, and toward a more applied mathematical approach.⁴

This move toward more technical, but less formal, work is, in part, driven by increasing computing power. As computing power increases, the need to rely on analytic solutions to problems decreases, and the ability to extract information from data increases. Both of these effects reduce the value of analytic deductive theory. One can get more of one's insights from the data and from simulations, reducing one's reliance on the deductive theory that characterised formalism. Because of the predicted increase in computing power, I see modern economics becoming more and more technical, but less and less limited by deductive formalism. This clearly does not mean that deductive formalism will disappear; it will simply lose the central role it has now, and will be seen as another method of considering issues that might be important in certain instances. Ultimately, as Velupillai (2003) argues, 'the tiresome dichotomy between induction and deduction . . . may well get cremated in economics, once and forever' (Velupillai, (2003, p. 22).

As Foley (2000) argues, in the longer-run future, more and more economists will employ agent-based computational models, such as described by Judd and Tesfatsion (2005), to study alternative policies. These agent-based models will be calibrated to the real economy and will provide a virtual economy, in which researchers can test how alternative policies will work in that virtual economy in the same way that engineers today test designs by creating a virtual environment in which they virtually design

¹ These estimates assume a constant rate of changes in analytic and computing technology occurring outside the profession.

² Clearly, the speed of this change will be affected by real-world events, and the success, or lack of success, of existing theories, and a complete story of change in the profession will have to integrate these. But the process of change I am describing is one that works even if no other changes occur.

³ In a survey of graduate students at top schools that I recently conducted, I found that, even though today the level of mathematics required is greater than that required 15 years ago, students found mathematics significantly less stressful than they did in the earlier survey that Arjo Klammer and I did (Klammer and Colander, 1990).

⁴ As Velupillai (2003) points out, this change is, in part reflecting significant changes occurring within the mathematics profession. Most economists are unaware of these changes, but they are nonetheless affected by them.

a prototype before building an actual prototype. But such a movement to agent-based modelling is far in the future. Now, we are taking only the initial steps away from our previous deductive approach.

3. 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.¹ 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 mainstream economics is slowly moving away from that holy trinity, and towards a broader foundation of economic theory of purposeful behaviour, enlightened self-interest and sustainability.

The need to move away from this narrower conception of individuals has been recognised by economists for a long time, although, previously, any economist who pushed the envelope on these assumptions was quickly classified as heterodox, with the result that his or her work was placed outside the mainstream. An example is Herbert Simon’s work, which in many ways provides a map for the direction in which I am predicting economics will evolve. Simon was neglected by large parts of the mainstream profession until he won the Nobel Prize in 1978, and even then his work had little impact. Today, however, his work and approach are beginning to be considered more carefully, as the profession catches up with his vision of the way in which one can understand the economy.² Numerous other economists classified as heterodox have been making similar arguments. What is happening today is that previous views considered heterodox are moving into the mainstream, as the analytic and computing technology is allowing younger researchers to develop these ideas in ways that will lead to institutional advancement.

The changes that are occurring can be seen in a variety of theoretical work, such as work in behavioural economics, evolutionary game theory, agent-based modelling, experimental economics and new institutional economics.³ Indeed, 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 this movement in the allocation of recent awards in economics. For example, in 2002 Daniel Kahneman and Vernon Smith won a Nobel Prize for their work in behavioural and experimental economics and Matt Rabin won the John Bates Clark medal for work on behavioural economics. Then, in 2003, Robert Engle and Clive Granger won the Nobel Prize for work in advanced statistical methods relevant

¹ I have been interviewing graduate students at top graduate schools in the US as I update my book *The Making of an Economist*. While individual schools differ, the interviews show much more openness to the broader trinity than Arjo Klamer and I found in our previous study (Klamer and Colander, 1990).

² In the previously mentioned study of the changing face of economics, Herbert Simon and Thomas Schelling came up repeatedly in our interviews as visionaries far ahead of their times. For introductions to their work, see, for instance, Simon (1996) or Schelling (1978).

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

for pulling information from time series data and the applications of that work. 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 described above, I see these small changes as an indicator of much larger future changes, although those changes will probably 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 over generations, not years. 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 and Roland Benabou (2003), following up on the work of Thomas Schelling, are now considering how individuals struggle to restrict their own behaviour. 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 behaviour, which, from our current theory, would be unjustifiable.

4. 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 their sum total, and in the direction that I see those changes taking economics and economics teaching in the future. My thesis is that the changes involve a major shift in the underlying vision of what mainstream 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 a highly complex 'simple system' to a highly complex 'complex system'.¹ Simple and complex systems differ in their micro foundations. Simple systems can be studied from micro foundations alone. Complex systems are too complicated to study from micro foundations and involve emergent properties that could not have been predicted through deductive means. Complex systems 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

¹ Complexity is a complex concept, and provokes significant discussion of what is meant by complexity. (The difficulty in defining the concept is discussed in the contributions in Colander (2000B, 2000C).) The nature of complexity that I am focusing on here is similar to the distinction Tony Lawson (2003) makes between an open system and a closed system. An open system relates to a complex system and a closed system relates to a simple system. The similarity between the views of Lawson (who is generally seen as a heterodox economist) and the views of cutting-edge mainstream economists, whom we interviewed in *The Changing Face of Economics*, is another indication of how the mainstream of the profession is moving closer to views that previously were considered heterodox.

complex systems are built up in path-dependent stages, making individual optimisation within such systems history, and institution, specific. This means that their institutional structure is central to understanding complex systems, and that any assumed rationality must involve some boundedness.¹

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.²

5. Understanding the nature of the change

Jokes about the economics profession are often revealing of the self-image that the profession has. 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 chemist 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.³ That joke is not very complimentary to economists, and it provoked a less well-known joke that portrays economics in a better light. The joke is the following:

A physicist, an engineer and an economist are given a stopwatch, a string and a ball and are told that the person who can best measure the height of a particular 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 the ball 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 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. The assumption made by the joke is that theory provides a blueprint of how the economy operates and, once found, is to be guarded at all costs. It also shows that economists' assumption is that the economy, while highly complex, is not complex in the technical sense of the word, since 'simple' systems are the only systems for which one can hope to find a complete set of blueprints.

If the economy is complex, the joke breaks down, because no set of blueprints of a complex system can 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 Basmann suggested to me in private discussions, is that when construction started on the building, 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.

¹ These ideas are developed in Colander (2000B).

² 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.

³ 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.

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, non-existent or misleading. Consider rationality. In order to achieve a blueprint of the economy, there is a strong push to assume a precise form of rationality in order to make the models analytically tractable, even though one knows that real-world rationality is anything but precise. 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 where precise rationality does not hold. Behavioural economics is a direct challenge to that belief—it involves a different sense of theory and of rationality; a behavioural economist looks at what people do, and builds those observations into his or her assumptions about behaviour in his or her models. Behavioural 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.¹ The type of rationality assumed is a key difference between 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.

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 dynamic patterns in their artificial economies, which are then compared to dynamic patterns observed in actual economies. They use complexity tools such as bifurcation theory to study these pattern-generating mechanisms analytically as well as computationally.

6. The technical nature of the economics of the future

Prior to recent technological developments in non-linear dynamics, chaos theory, complexity theory and in computing power (that allows researchers to gain insight into systems ‘scientifically’ without a highly formal analytic base), 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 is 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

¹ I have called the resultant applied policy the ‘muddling through’ approach to policy, to be contrasted with the economics of control approach to policy in the ‘simple’ economy (Colander, 2004).

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 non-reproducible. 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.¹ 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 emphasise 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 a Bourbakian tradition—it is technical in the sense that it 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 general equilibrium and the extensive work that followed in that Arrow/Debreu tradition are examples.²

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.³ 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 programme (called that even though Walras probably would have disagreed with significant portions of it) made them central to its goals. It asked such questions as: Can we prove the 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 were not quite the rebels that we

¹ For example, one of the reasons Milton Friedman had many followers in macro is that 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 Samuelson had many students was that he offered students a chance to develop one of the many models that he had structured.

² This axiomatic approach follows a tradition in economics that goes back to David Ricardo (but not to Adam Smith or John Stuart Mill). See Velupillai (2003) and Weintraub (2002) for interesting discussions of these issues.

³ Greed is not a formal pillar of the argument, but it is necessary to draw policy implications because, with highly interdependent utility functions, it is difficult to draw any policy conclusions from the model formally.

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 Commission on Graduate Economic Education (COGEE) commission in the US, 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 from 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 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 an axiomatic approach to an applied mathematical approach is symbolised 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 *et al.*, 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 from the first (Arthur *et al.*, 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, behavioural economics, and non-linear 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.

7. Changes in economic policy analysis

The change in the approach to theorising will be supplemented by a change in the approach to applied policy. Currently, the textbooks present 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 internalise those externalities. Textbook economic policy discussions focus on policies designed to guide the economy to a Pareto optimal position.

Students immediately recognise the limitations of this analysis, but, as Duncan Foley points out, the standard theory is ‘justified by telling students, in effect: Look. The only way you’re going to think your way through difficult problems is with these abstract models. True, we start with oversimplified models, but they will become richer and richer as you advance’. He further points out that ‘the complexity point of view undermines the standard economic approach at a fundamental level. It takes away the promised path . . . Available mathematical tools thus become a Procrustean bed, rather than a path to understanding’ (Foley, 2000, p. 170).

As the acceptance of the economy’s complexity undermines the standard textbook approach to policy, which might be called the economics of control approach, the blueprint for policy provided by the textbook model is also undermined. Applied policy no longer follows from theory, but instead follows from a much more complicated set of reasoning that goes far beyond economics.¹ It places applied policy in a ‘muddling through’ framework, rather than the ‘economics of control’ framework in which current textbook presentation places applied policy.

To understand what I mean by a muddling through approach, consider the building of the beautiful and amazing mediaeval cathedrals. That construction did not rely on knowledge of axiomatically derived scientific laws to guide the building, but instead relied on informally derived accumulated rules of thumb of what worked and what did not. Over time, these informal rules became codified and served as guideposts about what could and could not be done. As architects attempted to span larger areas, they would use these rules combined with their common sense, pushing the rules to their limits. At various times, the rules would be pushed beyond the limit, and a part of the cathedral would cave in, providing future builders with more insight as to where the limit of the rules was. As the stored knowledge increased, the cathedrals became more grandiose, even without a specific understanding of the formal laws underlying them. The formal laws 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.² 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

¹ This separation of applied policy from theory has a long history in economics, and can be seen in J. N. Keynes’s (1890) separation of the art of economics (applied policy) from the science of economics (positive economics), but that distinction has been lost in modern discussions of applied policy.

² Now, even in a muddling through approach, searching for a set of architectural plans can make sense, for indeed they might exist for parts of the economy. Thus, I would expect that, in the future, a few individuals will continue to search for them; abstract theory based on pure maths has a role to play in the future. But it is only one strategy in the process, not a strategy to put all one’s marbles in. The majority of the applied policy work will be about solving particular problems with whatever technical tools are available.

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 or she 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 theorising that most economists do.

But there are two differences. The first is that, in muddling through, this armchair theorising 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 modelling, 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 modelled will be characterised by one’s understanding of oneself, and insights from psychology.¹

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 behavioural economics involve slight addenda to standard economic results. These changes are acquiring the name ‘benign paternalism’ (Benjamin and Laibson, 2003) or libertarian paternalism (Sunstein and Thaler, 2003). In this policy work one uses the insights coming from behavioural work in economics to modify the way in which policy is implemented.

Eventually, however, integrating behavioural economic issues into policy precepts will involve major changes in the scope of policy analysis. 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 behaviour 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 an 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. 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 (determined by some value system) in order to optimise social welfare.

8. The more distant future

The above discussion has focused on the near-term future, and issues that I believe are likely to be in debate over the coming decade or two. Let me conclude the paper with a 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

¹ This prediction is very similar to the prediction of Velupillai (2003), who sees a possible movement to what he calls a natural historian’s approach.

moves away from its holy trinity assumptions, more and more cross-specialisation will occur. New hybrid fields will develop: psychoeconomics, neuroeconomics, socio-economics, bioeconomics and a variety of others. The training and the tools of each will differ, pulling the profession apart. Without the holy trinity of assumptions holding it together, the profession will ultimately lose its 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 specific to each institution. 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 applied game theory of the Schelling type, statistical methods and experimental methods. But these methods transcend disciplines, and are likely to 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. 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 specialised 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 specialised 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 modelling that is likely to predominate in the future. Behavioural 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 behavioural foundation of economics requires one to give up equilibrium because the interactions become too complex to solve analytically for equilibrium. There are two ways in which the profession could resolve the problem of intractability: one is to move to the natural historian's method and be content with the natural historian's method of classifying and systematising particular intractable accounting schemes, as suggested by Velupillai (2003, p. 22). That approach, which I believe is very reasonable, is, however, not consistent with the current nature of the profession. I believe it is much more likely to try to overcome the intractability problem by moving strongly into the development of agent-based models, in which researchers grow a model of the economy. I see future economists creating virtual economies, in which virtual agents are endowed with behavioural characteristics that will become more and more similar to those of

real-world agents.¹ These models require no analytic specification of equilibrium, simply a specification of the behavioural characteristics of agents.

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.²

These agent-based models are still in their infancy but, in my view, they will become central to how economics is done in the future. As computing power continues to grow, 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.³ Ultimately, a set of computer simulation models, which embody the essential observations of the experimental and empirical data, will form the theoretical basis of each of 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. These simulations will not provide truth or a blueprint for policy. Such a blueprint is impossible for a complex system. They will simply be aids to help in solving problems, as all models must be.

Whether these agent-based models will actually aid in solving problems is still unknown; we are a long way from deriving any useful results from them, and there are many methodological and practical problems with developing useful models. So my point is not that these models will provide an appropriate foundation of policy analysis; my point is simply that they are the approach to understanding that I believe the economics profession will follow.

9. 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 at all quickly in the beginning and intermediate 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

¹ See Robert Axtell and Josh Epstein (1996) and Robert Axelrod (1997) for an early attempt at such a model.

² See Blake LeBaron *et al.* (1999) for examples and discussion.

³ 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. Behavioural insights endowed upon the agents will still come from experimental work, and calibration of the models to real-world data through statistical means will still be necessary.

principles, and even the intermediate, textbooks, at least in the US, are not written for future economists; they are written for future citizens and business people.¹ For all its problems with serving as a vision for economic theorising, the current efficiency textbook model being taught serves these students well, and it is not clear that the current lessons it teaches would survive a movement in the texts to a complexity foundation.² The undergraduate economics course is designed to add value to the understanding of these normal students, and its the current structure does that. True, it does not prepare them to be economic scientists, or even to have a sense of what real science is, but almost none will 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 in the university curriculum. What will replace it, I do not know; as with all change, there are both costs and benefits.

Bibliography

- Anderson, P. W., Arrow, K. J. and Pines, D. (eds) 1988. *The Economy as an Evolving Complex System*, Redwood City, CA, Addison-Wesley
- Arthur, W. B., Durlauf, S. N. and Lane, D. A. (eds) 1997. *The Economy as an Evolving Complex System II*, Redwood City, CA, Addison-Wesley
- Axelrod, R. 1997. *The Complexity of Cooperation, Agent Based Models of Competition and Collaboration*, Princeton, NJ, Princeton University Press
- Axtell, R. and Epstein, J. 1996. *Growing Artificial Societies from the Bottom Up*, Cambridge, MA, MIT Press
- Benjamin, D. and Laibson, D. 2003. Good policies for bad governments: behavioral political economy, paper presented at the Boston Fed Conference on Behavioral Economics. June
- Colander, D. 1991. *Why Aren't Economists as Important as Garbagemen?* Armonk, NY, Sharpe
- Colander, D. 1999. New millennium economics, how did it get this way, and what way is it? *Journal of Economic Perspectives*, vol. 14, no. 1
- Colander, D. 2000A. The death of neoclassical economics, *Journal of the History of Economic Thought*, June
- Colander, D. (ed.) 2000B. *Complexity and the History of Economic Thought*, New York, Routledge
- Colander, D. (ed.) 2000C. *The Complexity Vision and the Teaching of Economics*, Cheltenham, Edward Elgar
- Colander, D. 2004. Muddling through and policy analysis, *New Zealand Economic Papers*
- Colander, D. and Brenner, R. (eds) 1992. *Educating Economists*, Ann Arbor, University of Michigan Press
- Colander, D., Holt, R. and Rosser, B. Forthcoming. *The Changing Face of Economics*, Lansing, MI, University of Michigan Press
- Foley, D. 2000 Complexity and economic education, in Colander, D. (ed.), *The Complexity Vision and the Teaching of Economics*, Cheltenham, Edward Elgar

¹ The large majority of economics majors in the US have no intention of going on to graduate work in economics; the largest group of majors is in liberal arts colleges that do not have business programmes, and are what might be called business majors in disguise.

² 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, and he saw economics studies as combining economic and business studies (Groenewegen, 1995, p. 556).

- Frank, R. 1999. *Luxury Fever: Why Money Fails to Satisfy in an Era of Excess*, New York, The Free Press
- Groenewegen, P. 1995. *A Soaring Eagle: Alfred Marshall 1842–1924*, Cheltenham, Edward Elgar
- Judd, K. and Tesfatsion, L. (eds) 2005. *Handbook of Computational Economics II: Agent-Based Models*, The Netherlands, North Holland
- Keynes, J. N. 1891. *The Scope and Method of Political Economy*, London, Macmillan
- Klamer, A. and Colander, D. 1990. *The Making of an Economist*, Boulder, CO, Westview Press
- Lawson, A. 2003. *Reorienting Economics*, New York, Routledge
- LeBaron, B. and Abu-Mostafa, Y. S., Lo, A. W. and Weigend, A. S. (eds) 1999. *Computational Finance*, Cambridge, MA, MIT Press
- Schelling, T. 1978. *Micromotives and Macrobehavior*, New York, W. W. Norton
- Schelling, T. 2003. Foreword, in Heckelman, J. and Coates, D. (eds), *Collective Choice: Essays in Honor of Mancur Olson*, Berlin, Springer
- Simon, H. 1996. *The Sciences of the Artificial*, 3rd edn, Cambridge, MA, MIT Press
- Solow, R. 1997. How did economics get that way and what way is it? *Daedalus*, (Winter)
- Thaler, R. and Sunstein, C. R. 2003. Libertarian Paternalism, *American Economic Review*, vol. 93, no. 2, 175–9
- Tirole, J. and Benabou, R. 2003. Self knowledge and self regulation: an economic approach, in Brocas, I. and Carillo, J. (eds), *The Psychology of Economic Design*, Oxford, Oxford University Press
- Velupillai, K. V. 2003. The unreasonable ineffectiveness of mathematics in economics, paper presented to the Cambridge Conference on the Future of Economics. Cambridge, UK, June
- Waldrop, M. M. 1992. *Complexity: The Emerging Science at the Edge of Order and Chaos*, New York, Simon & Schuster
- Weintraub, E. R. 2002. *How Economics Became a Mathematical Science*, Durham, NC, Duke University Press