

Fixing ideas: how research is constrained by mandated formalism

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The puzzle: why do so many economists in principle acknowledge the importance of creative destruction, and yet in practice give so little attention to creative destruction in what they teach and what they research? The answer lies, in part, in the difficulty of obtaining what is viewed as 'hard' evidence in support of some of the central claims. For example, one such claim is that new products contribute more to consumer well-being than price competition on old products. The only kind of evidence accepted by much of the profession is the testing of econometric hypotheses generated from formal models. The sort of evidence found in persuasive sources such as DeLong's 'Cornucopia' consists of historical examples and raw time series. I argue that in the short run, a more pluralistic methodology would be better, and that in the long run, we should seek to understand which methods work best under which circumstances.

Keywords: methodology; evidence; Schumpeter; pluralism

JEL Codes: B25; B41; B52; O30

The puzzle

Schumpeter's central claim in *Capitalism, Socialism and Democracy* is that creative destruction is the essential fact about capitalism (Schumpeter 1950). I believe that the claim is true and important (see Diamond 2006). The initial puzzle that stimulated the current paper was this: if creative destruction is the essential fact about capitalism, then why do so many economists act as though it were not the essential fact?

Accepting creative destruction means accepting a couple of propositions:

- (1) New products have generally lengthened and improved life.
- (2) Dynamic, leap-frog, competition helps us understand the development of new products, while the standard static, equilibrium-based, price competition has little to say about the development of new products.¹

The first proposition is partly descriptive and partly normative, while the second is wholly descriptive.

In recent times one of the most persuasive cases that creative destruction is the essential fact about capitalism can be found in Bradford DeLong's 'Cornucopia' (2000) which is intended as a draft of an early chapter of his much-anticipated history of United States economic growth. Part of 'Cornucopia' shows, through numerical examples, graphs and tables, that using standard measures, growth was substantial in the 1800s and even more substantial in the 1900s. Then he critiques the standard measures by showing us that they fail to capture most of the growth due to the new products that arise from

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Schumpeterian creative destruction, a point that also has been extensively emphasized in an illuminating paper by Nordhaus (1997).

In the core part of his paper, DeLong argues that the vast majority of us would prefer the bundle of goods available to us in 2000 to the bundle of goods that was apt to have been available to us in 1900. The case can be put most strongly for medical advances. DeLong, for instance notes that without the medical advances of the twentieth century, he would not have survived beyond infancy. Robert Fogel has made a strong case (Fogel 2004) that the enormous gains in longevity and health over the last two centuries have in part been made possible by the technological advances in capitalist economies. Moreover, he sees the potential for continuing substantial gains (Fogel 2005).

Notice that most of the evidence in 'Cornucopia' consists of time-series measurements of what might be called 'raw' data. And a little of the evidence in 'Cornucopia' consists in, dare I say it, something akin to introspection: contemplate the bundle of goods in 1900 and in 2000, and tell us, if your feet could time travel, in which direction would they walk?

I have shown elsewhere (Diamond 2007, in press) that Schumpeter's work is receiving growing recognition from economists, both generally, in terms of citations, and specifically, in terms of several important mainstream economists. Mainstream economists such as Stigler, Becker and Krugman, write favorably about the process of creative destruction, when they are grappling with concrete, practical policy issues; and yet they do not incorporate creative destruction in their textbook treatments of microeconomics (see Diamond 2007). Their ambivalence, I suggest, reflects an ambivalence in the profession as a whole. Many economists, if pressed, will acknowledge the importance of the entrepreneur in the economy, and may also acknowledge that something of Schumpeter's creative destruction captures what goes on. But having so acknowledged, they proceed in their research, and their teaching, and their policy advice, to act as if entrepreneurs, and new product innovation, were not essential facts about capitalism.

This is not just some sort of academic puzzle: if we fail to focus on what is most essential about capitalism, we may lose what is most essential. And if Schumpeter, DeLong and I are right, that means lives that are shorter, nastier and more brutish.

To solve the puzzle, it may be useful to examine an earlier episode in the history of economics when something similar seemed to have happened. From about 1820 until about 1870, the economics profession was entranced by the first great mathematical model in the profession's history: David Ricardo's corn model of the British economy.² The model was beautiful and internally consistent, and predicted that in the long term the economy would stagnate, and laborers would be stuck with subsistence wages. But it became increasingly clear in Britain in the 1800s that the economy was growing, not stagnating, and that labor was doing much better than just subsisting.³

Some economists (Schumpeter, for example), view this period as representing the nadir of the economics profession.⁴ Most economists at the time continued to endorse, and teach, some version of the Ricardian corn model, even as the implication of long-term stagnation became increasingly dissonant with the facts of economic growth.⁵

How did it happen? It happened because economists allowed themselves to become so entranced with the beauty of theory that they closed their eyes to what was actually happening in the world of business and policy. Schumpeter described this failing as the 'Ricardian vice'.⁶

And I think we are in the process of repeating the mistake. Schumpeter was right that the most 'essential fact' about capitalism is the process of creative destruction. But because the most important aspects of creative destruction generally have not been

formulated in a beautiful formal theory, we economists neglect the process of creative destruction in our teaching and in our policy advice.

The concerns of the conscientious

No less a mathematical economist than Gerard Debreu⁷ has expressed concerns about the growing mathematization of economics:

In the past two decades, economic theory has been carried away further by a seemingly irresistible current that can be explained only partly by the intellectual successes of its mathematization.

Essential to an attempt at a fuller explanation are the values imprinted on an economist by his study of mathematics. When a theorist who has been so typed judges his scholarly work, those values do not play a silent role; they may play a decisive role. The very choice of the questions to which he tries to find answers is influenced by his mathematical background. Thus, the danger is ever present that the part of economics will become secondary, if not marginal, in that judgment. (Debreu 1991, p. 5)

In contrast to the mathematicians, there is another group of economists who entered the profession because they thought a greater knowledge of economics could improve the world. Klamer and Colander have documented how these economists often become cynical by the mathematical game-playing that they learn in graduate school. (See Klamer and Colander 1990; and Diamond 1993)

Alexander Rosenberg (1992) has plausibly argued that the economics profession can go in whatever direction it wants. But if it chooses to follow the mathematicians, it will become increasingly irrelevant and ignored. And some other profession will arise that aims to seriously address policy issues. Why? Because the broader world needs to have policy issues addressed.⁸

Generosity requires that one grant that some economists who care deeply about policy issues remain sincerely convinced that mathematics is the sole path to enlightenment. But it is clear that among the economists who care about policy relevance, and who initially thought the path to truth lay in the direction of mathematical rigor, there are some who are finding themselves drawn in a different direction.

For example, there are economists who stick with the standard modeling assumptions and methods, in their academic work, and in their pedagogy. But when they come to policy advice, they draw from a much wider range of argument and evidence. Possible examples would include economists who have contributed to economic theory, but also have written on practical policy issues, e.g. Stigler, Becker, and Krugman.⁹ Also included would be economists who have contributed to theory, and have taken positions with the government in which they play active roles in evaluating and promoting policies. Possible examples would include Stiglitz and Lazear.

A paper by the former head of the President's Council of Economic Advisors, Ed Lazear, is significant for what it says near the end about economists forgetting facts, because the facts do not fit into current theory:

Researchers have begun to make jobs rather than individuals the unit of analysis. This change of focus can illuminate new issues and provide answers to questions that were once posed and forgotten. The questions were forgotten not for lack of importance, but for lack of theoretical frame-works. The theory is now developed and awaits confirmation in the data. (Lazear 1995, p. 263)

It is useful and important that Lazear highlights that the economics profession currently forgets facts that do not fit into theoretical frameworks. Although Lazear does not choose

to do so, one could proceed from this observation to admonish the profession that it is wrong to forget facts, even if the facts do not fit into the current theoretical framework. One could so argue both because such facts may be crucial for the eventual improvement of theory, but also, more importantly, because focusing on such facts may be necessary in order to advocate the best policies.

We may also add the names of Lawrence Summers and N. Gregory Mankiw, who, although differing in political party affiliation, share both substantial academic credentials, and significant stints as applied economists in government. Each of them has written that the last couple of decades of highly formal macroeconomic models have not produced results of much use to macroeconomic policy makers (Summers 1991; Mankiw 2006).¹⁰

Other economists, who seem contented with current methods, may be harboring reservations that they have yet to express. For example, for most of their careers, there was little evidence that Sherwin Rosen and Zvi Griliches had reservations about the dominant methods and assumptions. And yet each of them toward the end of their lives expressed reservations: Rosen reservedly, Griliches with passion.

For example, in one of his last (1997) papers,¹¹ Rosen suggested that Austrian economists might have something to offer neoclassical economists. What they specifically had to offer was that they had continued to talk about some important phenomena that neoclassical economists seldom talked about: innovation, disequilibrium and entrepreneurship.

Much of what Rosen says with bemused and guarded puzzlement, Zvi Griliches says with a hotter passion, in the final chapter of the book that he wrote during the year in which he was dying of cancer. Griliches applies to economists (2000, pp. 3–4), the famous story of the drunk who looks for his missing car keys under the lamp-post, even though he lost them somewhere in the dark, because under the lamp-post is where the light is.¹² Similarly, he suggests, economists write papers about equilibrium economics, because that is what we understand, even if the important questions lie in the darker regions of innovation and entrepreneurship (Griliches 2000, pp. 88–90; and see Diamond 2004).

Good deathbed advice is a valuable thing; and it would be ungrateful to wish that the advisor had himself more fully followed his own advice during the peak of his career. It is better to be wise late, than to never be wise; but it would be better still to be wise early and often. A third group of economists includes those who have shown that they can play the mathematical game, but who, in various degrees and ways, openly criticize it. These economists would include Deirdre McCloskey, Richard Nelson and Thomas Mayer.¹³

The claim that formalism is needed ‘to fix ideas’

During a plenary session, at the International Schumpeter Society meetings near Nice in France, I heard an exchange between three distinguished Schumpeterian economists that gave me insight into what might be wrong with the currently dominant economic methodology.¹⁴ (The economists were Richard Nelson, Philippe Aghion and William Baumol) Nelson suggested that there was too much reliance on mathematical models, and Philippe Aghion passionately responded that mathematical models were needed ‘to fix ideas’.

Frequently, the phrase is used to justify making convenient, but unrealistic assumptions, in a mathematical model, in order to allow results to be derived. So, for example, one might assume constant returns to scale, or a log-linear functional form. The implication seems to be that using the phrase ‘to fix ideas’ excuses one from justifying the assumptions, either in terms of their realism, or in terms of saying why they are superior to

other assumptions that might be made. Often, what happens next is that the model is then used to derive stylized facts that have already been shown to correspond with what is generally known or believed.

What exactly is accomplished by such activities? One might imagine that the credibility of the model is first established by its ability to imply well-known stylized facts, and then it is used to infer unknown, novel facts. And then the economist goes out and does some independent testing, and finds that the facts are true. But usually there are no truly novel implications, and when there are, there are usually mixed empirical results (assuming that there are any empirical results at all).

Aghion is not arguing that mathematical modeling is one method, among many, that can be tried to see how fruitful it is in different situations. He seems to be arguing that it is the *only* sound method for doing economics. For instance, Easterly's rich and convincing work (eg. 2002, 2005) is criticized for not being based on formal modeling. Aghion seems to be giving broadly two reasons. The first is the view that you need to have models to be clear and precise. The second is the view that mathematical modeling is a 'discipline'.

On the view that math is needed for clarity and precision, at least two points can be made. One is that, as Marshall and Stigler¹⁵ pointed out a long time ago, mathematics can be just as clear, and just as obfuscating, as prose. A second is that the primary goal is not actually conceptual precision;¹⁶ the primary goal is useful truth.

What of the view, then, that math is needed because it imposes a 'discipline' on the economist? This view rests on two assumptions, both false. It assumes a labor theory of value: the supposed dictum 'if it's hard, it's true; if it's easy, it's false' has many counter-examples. It also assumes that math is hard, and other methods are easy. But this too, can be false. Collecting accurate data can be extraordinarily hard, and can require enormous 'discipline'.¹⁷ So can the construction of a careful, accurate and insightful case study. Conversely, applying a well-worn mathematical apparatus to some slightly different stylized facts may not necessarily require a high level of effort, concentration or 'discipline'.

Consider a concrete example, from one of the 'hard' sciences: chemistry. Several years ago, I spent some time studying the polywater episode (Diamond 1988b). Polywater was alleged to be a form of water with novel empirical characteristics (e.g. boiling and freezing temperatures different from those of ordinary water). Theoreticians rushed in with formal models to 'explain' the phenomenon. Eventually the novel characteristics were found to be due to ordinary water leaching quartz from the sides of test tubes. The theoreticians, perhaps with a quick blush, moved on. One of them took time to note, however, that we should not put too much emphasis on the model he had published in *Science*, since it had been 'concocted one evening while watching TV, between commercials, with tongue in cheek'.¹⁸ Research that is formal is not necessarily disciplined, and is not necessarily sound.

The word 'fix' in the phrase 'fix ideas' is illuminating in unintended ways. Because most economists have mastered the mathematics of only certain sorts of economic situations (equilibrium situations), if good work in economics requires mathematical modeling, they will focus on issues that can be modeled with equilibrium models, regardless of whether these are the issues that matter empirically, or matter for policy making. So, in effect, the exclusive emphasis on mathematical modeling has served to 'fix' ideas, in the sense of gluing them down and making them stationary. Fixing ideas in this way leaves economic science in a bit of a 'fix' (meaning 'predicament'), because, as a result, the 'fix is in' (meaning 'an improper fixing').

There is a sense, however, in which it is indeed desirable to 'fix ideas', though not in the sense in which economists use the phrase. We ought to try to fix ideas in the sense of

'repairing' them, e.g. by joining Stigler in acknowledging that the concept of competition is expected to evolve (Stigler 1957); and by joining Schumpeter in acknowledging that the most fruitful evolution is toward something like the process of 'creative destruction'.

The five steps of the mandated formal method

Mathematics has been used in a variety of ways in economics.¹⁹ Solow's view (1997) of the proper way, explained and accepted in Goldfarb and Ratner (2009) probably represents the dominant ideal of the profession today. In a key passage, quoted by Goldfarb and Ratner, Solow sees theorizing as a kind of noble game.²⁰ We observe some stylized fact, and see if we are clever enough to devise a model that implies it.

But besides providing evidence of our cleverness, how does such a game help us to better understand how the world works? If, as physicist Richard Feynman plausibly argues (1999), the purpose of scientific theories is primarily to help us understand how the world works, then we would judge theories, not by their display of mathematical prowess, but by the breadth and importance, and novelty, of their empirical implications.²¹

But most contemporary economic theorists do not seem to be developing theories primarily with the goal of increasing the breadth, importance and novelty of their empirical implications. In theoretical work, we instead judge the theories based on the consistency, elegance, sophistication and novelty of the mathematics. In empirical work, the theory (model) section is judged by similar criteria (though with lower expectations), and is a required component of the paper, even when its connection to the empirical section is tenuous.

Here are what I hypothesize to be the steps in the method for most 'empirical' papers in major economics journals.

- (1) The economist comes up with a behavioral hypothesis. The source may be conversation with peers, an article in the *Wall Street Journal*, reading case studies, personal observation, etc.
- (2) The economist searches through the portfolio of currently available models to find one that most easily can be tweaked to imply the behavioral hypothesis.
- (3) Any unrealistic implications of the model are excused on the grounds that such assumptions are necessary in order to 'fix ideas' or to make the model 'mathematically tractable' or to make the model imply specific results (as opposed to just laying out an almost exhaustive list of possibilities).
- (4) The economist tweaks the model, by making the modifications and appropriate assumptions, so that the model implies the behavioral hypothesis.
- (5) The economist seeks data and econometric models to systematically test the behavioral hypothesis.

Many theoretical papers follow the same method, except that they drop step 5.

Some of the practitioners of applied fields in economics have expressed modest, or full-fledged, worry about the sterility of this method. For example, within the field of labor economics, in a survey of contract theory, Don Parsons expressed modest worry:

The empirical analysis of employment contracting has only begun. I suspect that much more empirical work is now necessary if progress in this area is not to degenerate into the relatively un insightful enumeration of the theoretical possibilities. (Parsons 1986, p. 843)

I elsewhere (Diamond, 2008a) briefly discuss the implicit contract literature on academic tenure as one example where the degeneration may have occurred.

Similarly, but more strongly than Parsons, Sam Peltzman expresses full-fledged worry about the procedure as it is applied in the field of industrial organization:²²

By suitably permuting and combining the problems and assumptions, new models can be produced almost *ad libitum*. Indeed the production of new models and tidying up of old ones seem to be major goals of this research enterprise. The uninitiated observer faced with this long march of models soon begins groping for motivation to stay to the end of the parade. (Peltzman 1991, p. 207)

Recent reports by graduate students at distinguished graduate schools also support the plausibility of my account of how models are frequently used. The first passage is from an MIT graduate student:

There is a sentiment here from people that I have talked to that if they want a model we can give them a model. We sit down and make up a model, and we play with it until it gives us the empirical result that we find, even though you could have just as easily have written down a model that would have given a different result. We have even had seminar speakers say, 'Oh, I worked on a model that predicts it; of course I could have written down a model that predicts the opposite, but why would I do that?' In general, you can write down a model that predicts just about anything. (Colander 2007, p. 241)

The second passage is from a Chicago graduate student:

When a professor here writes a paper, of course he has his view of the results he wants to get, and then he designs a model to get this result. For example, trade must make countries better off. So I do a model in which trade makes countries better off. The theory is really, really coherent in this aspect. But the theory seems to be an instrument; the beliefs are there before. They put their views into the models. It is not only Chicago; it is most places. (Colander 2007, p. 241)

A rent-seeking account of why formalism is mandated, and whether there is hope for change

If I am roughly right in laying out the steps of the mandated method, then the question is: why do we always have to do steps 2 through 4, when we often could just as well jump from step 1 to step 5? Why do we always need to do steps 2 through 4 in order to usefully add to our knowledge about the world?

Karl Popper (1959) may have been right to distinguish the context of discovery from the context of justification. Most famously, Kepler had a semi-mystical source in the 'perfect solids' for his hypothesis that the orbit of planets was elliptical.²³ Kepler's hypothesis was fruitful, not because of its source, but because of soundness of its implications. If mathematical modeling was indeed always superior to other means of generating hypotheses, then it would not be necessary for its advocates to mandate it. The superiority would soon enough be revealed from the open competition of the method with alternative methods for generating hypotheses.

If fruitfulness in generating sound hypotheses does not explain the growing mathematization of economics, what is the explanation? A small part of the explanation may be that there is a glut in the market for mathematicians: more mathematics PhDs are supplied than are demanded. Some of these PhDs find their way into economics, where they are free to do mathematics, with only a perfunctory need to worry about the applicability of the mathematics to empirical and applied issues. Another small part of the explanation may be that there actually still are some economists who sincerely believe that mathematical modeling is the only method that can be fruitful.

But I believe, more generally, that there is another, and larger, group of technical economists who have not given the issue much thought, but who support the method

because they personally are rather good at doing steps 2 through 4, and hence, they personally benefit if the profession lauds and rewards those proficient at steps 2 through 4.

If skill at mathematics is a screen of competence (like a medical license for physicians), then those who possess the skill will benefit in maintaining it as a screen, since they will then be differentially rewarded, relative to their less mathematically talented peers.²⁴

This might be called a 'rent-seeking' account of economic methodology.²⁵ Let me give you a example that I believe increases the plausibility of this account. Milton Friedman (1981; see also Friedman 1994) wrote a *Newsweek* column many years ago that caused a firestorm of anger among his colleagues in the economics profession. Friedman's argument was that, in general, the government is not going to do a good job of identifying the best and most productively innovative economists. In particular, he argued that economics funding by the National Science Foundation (NSF) had made the economics profession more mathematical than was appropriate.

Even his 'Chicago' colleagues, who were otherwise inclined to be sympathetic to his work, were appalled: Robert Lucas (1981) wrote against Friedman in the *New York Times*, and Zvi Griliches spoke against him before Congress (see Griliches 1994).

Economic methodologists often understandably complain that our work is ignored by the rest of the profession. Sometimes, this may be partially due to our giving too much attention to philosophical minutiae. But other times, it may be because following our recommendations would go against the self-interest of most of those who are powerful in the profession.

If I am right about the rent-seeking account, is there any hope for change? What hope there is comes from two sources. Klamer and Colander have documented that many young economists enter the profession, at least in part, in order to make the world a better place. Maybe some of these, to some extent, could be rallied to produce work that hues more closely to their original ideal, even at the price of fewer plaudits and rewards. Secondly, as Alexander Rosenberg and others have noted, policy makers need sound economic advice. One would hope and expect that eventually resources external to the profession (non-university-endowment resources²⁶) might have an effect on the method of economics (or at least of the method of a policy discipline that replaces economics).

Thomas Mayer bolsters our hope for change, when he points out (1993, p. 13) that the profession changed once before, in a way that seemingly conflicted with the self-interest of the powerful in the profession. This occurred in the 1930s and 1940s, when many of the non-mathematical leaders of the economics profession hired, promoted, praised and published the papers of the incoming generation of mathematical economists. Schumpeter himself is a prime example of what Mayer was talking about. Though not possessing substantial mathematical tools himself, he seems to have sincerely believed in the potential fruitfulness of mathematical methods, and so he promoted the increased use of mathematics within economics, most notably by helping to found the Econometric Society, and by working hard to promote the early career of his young student Paul Samuelson (see several of the letters referenced in Diamond 2007).

Pluralism: the short-run alternative

If we are going to answer the question of which method is better than which other method, we are going to have to have some answer to the question: better for what?²⁷ There is a long, and deep, literature on the question of the aims of science. In the context of this paper, I will simply lay out my own position, which is that most consumers of science, and

many scientists themselves, and a respectable number of philosophers of science, have found it plausible that one of the main aims of science is to provide knowledge that is useful in affecting the world.

In its purest form, the view of science that emphasizes the practical predictive efficacy of knowledge is sometimes called 'instrumentalism'.²⁸ One brief, clear, plausible defense of this sort of view can be found in Stephen Toulmin's *The Philosophy of Science*. In the specific context of economic methodology, Wade Hands (2001, pp. 236–239) has laid out a careful, useful and nuanced account of some main varieties of instrumentalism. Even more specifically, Yuichi Shionoya has argued (1990) that a form of instrumentalism is what best characterizes Joseph Schumpeter's own view of the aim of economic science.

I would claim that an open-minded reading of the history of economic thought would reveal that instrumental usefulness has not been the exclusive monopoly of only one method. Until we can develop, and empirically ground, a good meta-theory of the circumstances in which various methods are likely to prove superior, the most progressive position is to adopt some form of methodological pluralism.²⁹

It may be reassuring that many enlightened and successful economists have adopted just such a position. These would include Frank Knight,³⁰ Milton Friedman,³¹ Deirdre McCloskey, and, I think, William Baumol. (See Baumol's brief comment at the end of Appendix 1 to Diamond 2009)

The long-run, to-be-hoped-for alternative

In their business best-seller, *The Innovator's Solution* (2003), Harvard professor Clayton Christensen, and co-author Raynor, argue that a wholesale reformulation of management theory is needed. They criticize much past management research for the common practice of stating a dictum, and then cherry-picking some examples that seem to fit the dictum. So, some best-selling business books (e.g. Collins and Porras, *Built to Last*, 1994) argue that firms should stick to their historical core competencies, while some other best-selling business books (e.g. Foster and Kaplan, *Creative Destruction*, 2001) argue that firms must strike out in new directions, and innovate to survive. Christensen and Raynor suggest that what is needed is a more general account that tells the business executives the circumstances under which the first dictum holds, and the circumstances under which the second dictum holds.

Similarly, in the long run, we need to try to do for methodology what Christensen and Raynor are seeking to do for management theory: it would be useful to practicing economists if we could learn something about the circumstances under which one method is most likely to work, and the different circumstances under which some other method is likely to work. This approach to methodology is in the spirit of the proposal by Larry Laudan, and co-authors, to empirically test scientific methodologies on the basis of how successful they have been at achieving scientific progress (Laudan et al. 1986; and Donovan, Laudan, and Laudan 1988). Schmaus' paper (1996) also provides a useful elaboration of the Laudan research agenda.

By way of illustration of the approach, I will sketch three examples of how this might be done – one from economics, and two from astronomy.³²

In economics, I elsewhere (Diamond 2008b) use standard price theory tools of analysis to suggest that as the price of computing falls, and it becomes cheaper to perform large-scale econometric analysis, it would make sense for the mix of methods to have a greater intensity of econometric research.

Similarly, in astronomy, Chris Anderson explains (2006, pp. 58–62) how the advent of the inexpensive, computer-guided small telescopes with good-enough optics has increased the appropriate role of amateurs in astronomy; and as a result has made the discipline more empirical. He makes the case that only the growth in amateurs made possible ‘one of the greatest astronomical discoveries of the twentieth century’ (p. 60). Theory had predicted that when a star exploded, the first empirical evidence would be a substantial increase in the stream of neutrinos, to be followed a few hours later by the first visible light. On 23 February 1987, the neutrino rush was observed. Only with the work of the amateur observers was it possible to have enough ‘eyes in the sky’ to observe the subsequent ‘splash of light’.

Another example, related partly to astronomy, has been suggested by Timothy Eastman, who has been a program director in the field of plasma science for NASA and the NSF. He is suggesting (2005, 2006) that in some areas of science, new technologies that have allowed the collection, storage and analysis of huge observational datasets increase the plausibility of a new observational-inductive methodological framework. Eastman’s speculations may have broader application to fields such as economics where some have observed a growing interest in data-mining techniques.

In the future, it would be useful to collect additional examples of this sort, in the hope, eventually, of learning what generalizations can be reached about which methods are appropriate in which circumstances. This is an exciting agenda for those of us who join Thomas Mayer (1994) in believing that economic methodology can and should make important contributions to the actual practice and advance of economics.

Conclusion

When Galileo wanted to convince the counts and prelates of Florence and Rome that his telescope could tell them something about the stars, he first had to convince them that the telescope delivered an accurate picture of what they already knew, by having them point the telescope at the familiar statue in the distant piazza.

About 30 years ago NASA launched a probe to learn whether there is life on Mars. The probe landed, and performed its tests. On the basis of these tests, the NASA scientists concluded that there was no life on Mars (see Begley 2006).

Now, recently, someone thought to apply the same technology, and the same tests, here on earth. They performed the tests, and did the analysis, and concluded that there is no life on earth (see Begley 2006).

So should we reject our observation that there is indeed life on earth, or should we reject the equipment and the tests?

Now NASA used very sophisticated equipment, and few of us know fully all the details of how it works. But if the sophisticated equipment tells you something you know is false, do you reject what you know, or do you reject the equipment?

One hundred and fifty years ago, the corn model told economists that they would observe economic stagnation. Any yet they observed growth. So should they have rejected the observation of economic growth, or should they have rejected the corn model?

Today, the mandated formalist method leads us to discount the kinds of evidence that indicate that creative destruction is the essential fact about capitalism. So should we reject creative destruction or should we reject the mandate of the formalist method?

Acknowledgements

I am especially grateful for detailed comments from Wade Hands and Matthias Klaes. I also appreciate the comments of two anonymous referees. Chan H. Cho and Miaomiao Yu provided

research assistance. An earlier version of the paper was presented at the 'Theory and Evidence in Economics' International Network for Economic Method session at the Allied Social Sciences Association meetings, in Chicago, on 6 January 2007. I am grateful to Roger Backhouse for organizing the session, and to Matthias Klaes for chairing it.

Notes

1. Backhouse (2004) has drawn our attention to Robert Lucas's defense of equilibrium models that allow for dynamic implications. Even with this caveat, it remains true that such dynamic equilibrium models are not illuminating on the fundamental issues of entrepreneurship and innovation, e.g. which policies best encourage, or allow, innovation?
2. Hollander (1977) provides extensive documentation for the long dominance of the Ricardian model in the economics profession.
3. See e.g. Hartwell (1961).
4. In referring to the years 1820–1870, Schumpeter once commented: 'In those years economics touched the low ebb of achievement and prestige' (1952, p. 570).
5. See Blaug (1956). De Marchi (1970) argues that Blaug did not sufficiently appreciate the Ricardians' response that the observed economic growth was a short-term aberration that was not inconsistent with the long-term prediction of stagnation. In the end, however, Blaug's central message is mainly supported by de Marchi (p. 275): 'With each successive edition of the *Principles*, ... Mill unquestioningly extended the short run required for the effects of technical progress in agriculture to work themselves out beyond Ricardo's period of twenty-five years. With each edition, therefore, Blaug's charge of evasion begins to look more plausible'.
6. Schumpeter defined the 'Ricardian vice' as: '... an excellent theory that can never be refuted and lacks nothing save sense' (1954, p. 473).
7. Debreu was a member both of Berkeley's economics department and its mathematics department. Elsewhere (Diamond 1988a) I have presented citation evidence indicating that Debreu's own work may not have been very rich in empirical implications.
8. For a sympathetic summary and evaluation of Rosenberg's book, see Diamond (1996a).
9. The examples of Stigler, Becker and Krugman, are discussed at somewhat greater length in Diamond (2007).
10. In the abstract of his paper (1991, p. 129), Summers says: 'It is argued that formal econometric work, where elaborate technique is used to apply theory to data or isolate the direction of causal relationships when they are not obvious *a priori*, virtually always fails. The only empirical research that has contributed to thinking about substantive issues and the development of economics is pragmatic empirical work, based on methodological principles directly opposed to those that have become fashionable in recent years'. Although Mankiw remains hopeful that the research on formal models may eventually yield a practical payoff, he admits that there is no present evidence for such a payoff (p. 44): 'New classical and new Keynesian research has had little impact on practical macroeconomists who are charged with the messy task of conducting actual monetary and fiscal policy. It has also had little impact on what teachers tell future voters about macroeconomic policy when they enter the undergraduate classroom. From the standpoint of macroeconomic engineering, the work of the past several decades looks like an unfortunate wrong turn'.
11. Sherwin Rosen died on 21 March 2001.
12. Darby and Zucker (2003, p. 1) cite Milton Friedman as having told the story in his Economics 331 class in 1967.
13. Deirdre McCloskey has suggested that I add Frank Hahn to the list.
14. In an appendix to Diamond (2009), I have done my best to transcribe the key passages in this exchange.
15. See Stigler's (1949) essay 'The Mathematical Method in Economics'. (One puzzle is why Stigler did not have his 1949 essay reprinted in any of his several later collections). A similar position is expressed in this passage from a 1947 letter from Stigler to Arthur Burns, writing of 'economists': 'They can reason fallaciously, with or without mathematics' (Stigler in Hammond and Hammond 2006, p. 69).
16. Thomas Mayer (1993) has extensively and persuasively argued that precision does not equal truth, and that truth and relevance are more important than precision. Also relevant is Klaes'

paper (2006) in which he presents plausible argument and example to support the view that novel concepts need not be precise, to be scientifically useful and progressive.

17. Oskar Morgenstern, whose mathematical credentials include co-inventing game theory with John von Neumann (1944), discusses (1965) the entertaining example of the 1950 Census in which there was found to be a 'surprising number of widowed fourteen year-old boys' (p. 40). Supporting my claim that non-mathematical work can be hard, Morgenstern continues (p. 40): 'The reasons for these oddities were not easily discovered, but it is convincingly shown that these were errors and that they have to be attributed (p. 41) to mistakes in punching cards which had bypassed even the severe Census controls. . . . The complete unraveling of the origin, size, and effect of errors discovered is necessarily complicated and can only be performed by highly experienced statisticians'.
18. F.J. Donahoe as quoted in Diamond (1988b, p. 185).
19. I use 'theory' in the broadest sense of any abstract construct, intended, at some level, to explain something in the world. Since roughly 1950, all mainstream economic theory has been mathematical, though to varying degrees, and sometimes in varying ways. Substantial research has focused on some of these ways. Weintraub (2002), for example, has focused on the history of general equilibrium theory, Giocoli (2003) has focused on game theory, and Morgan (1990) on econometrics. The ways in which all of these varieties have been implemented share a common characteristic: the belief that more is not only better, but is actually a *sine qua non* for contributing to the advance of knowledge. It is this common belief, rather than the distinctions, that is relevant in the present discussion.
20. Blaug (2003) provides a plausible history of how in economics in the 1950s, the mathematically complex model came to be viewed as an end in itself.
21. Something like this view of theories is not without supporters among modern economists. For instance, of his four precepts, Williamson (2009) places most emphasis on his fourth precept, the insistence upon 'predictions and empirical testing'. Williamson also provides us an important related observation from Milton Friedman, sent in an email during the last year of his life: 'I believe in every area where I feel I have had some influence it has occurred less because of the pure analysis than it has because of the empirical evidence that I have been able to organize'.
22. Concrete examples are harder to dismiss than abstract methodological claims. So one of the especially valuable aspects of Peltzman's full-fledged worrying about the current procedure for doing research in industrial organization is his rather detailed specific example. Peltzman was reviewing a prestigious two-volume *Handbook of Industrial Organization* (Schmalensee and Willig 1989). His specific example (pp. 206–207) focuses on one of the chapters from those volumes. If space permitted, I would reproduce here much of Peltzman's somewhat lengthy example.
23. See e.g. Field (1988).
24. McCloskey (1985, pp. 69–72) has an insightful discussion of Samuelson's *Foundations of Economic Analysis* (1947), which is often seen as a key document in the increased mathematization of the economics profession. An especially relevant passage (p. 70): 'Samuelson's skill at mathematics in the eyes of his readers, an impression nurtured at every turn, is itself an important and persuasive argument. On good grounds, he presents himself as an authority. That the mathematics is sometimes pointless, as here, is beside the point. Being able to do such a difficult thing (so it would have seemed to the typical economics reader in 1947) is warrant of expertise'.
25. Rent-seeking accounts of the behavior of scientists are discussed in Diamond (1996b, pp. 16–17). Grubel and Boland (1986), in particular, developed a rent-seeking explanation of the increasing emphasis on mathematics in economics.
26. Adam Smith has a great discussion of the deleterious effects of university endowments on the productivity of academics (1976, pp. 758–764).
27. A clear and credible discussion of this issue can be found in Schmaus (1996).
28. Since Milton Friedman's classic methodology article (1953) has been interpreted (Boland 1979) as a version of instrumentalism, and since I elsewhere highly praise Milton Friedman, it may be worth noting that my endorsement of Toulmin's instrumentalism does not imply endorsement of all of Friedman's explicit account (most notably, what Samuelson and others call the 'F-twist').
29. For several, generally sympathetic, analyses of pluralism as an economic methodology, consult the essays in Salanti and Screpanti (1997). An earlier brief defense of pluralism (under the label

'eclecticism') can be found in Hausman (1989). Also, see Wible's summary and evaluation (2000) of philosopher of science Nicholas Rescher's defense of pluralism.

30. For an account of Knight's pluralism, see Hands (1997).
31. A substantial literature exists on Friedman's methodology as represented by his explicit writings on methodology, and as represented by his actual practice of economics. It is especially appropriate to call Friedman a pluralist in regard to his actual practice of economics. A useful discussion of these issues, and references to some of the literature, can be found in Colander (1995).
32. Adam Smith (see Diamond 1986), and many others, have taken astronomy to be paradigmatic of the sciences, so it is appropriate that two of the main illustrations of the method of a science changing due to a change in circumstances, apply to astronomy.

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