Fixing Ideas:

What Counts as Good Evidence that Creative Destruction is the Essential Fact about Capitalism?

Short running title: Fixing Ideas

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Abstract

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, on 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.

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The Puzzle

Schumpeter's central claim in *Capitalism, Socialism and Democracy* is that creative destruction is the essential fact about capitalism. I believe that the claim is true and important (see Diamond 2006b). The central puzzle of this paper is 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 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 20th century, he would not have survived beyond infancy. Robert Fogel has made a strong case (Fogel 2004) that the enormous gain 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 2006a, 2006c) 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, and 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 2006a). 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 and DeLong, and I are right, that means lives that are shorter, nastier, and more brutish.

Past as Prologue: The Ricardian Corn Model

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 Debreu,[7] 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. (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.[10] 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; Mandiw 2006).[11]

I say "apparently" contented, because we do not know what they are thinking to themselves, and what reservations they may yet express. For example, for most of there 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, and Griliches, with passion.

I remember hearing Sherwin Rosen speak to a good-sized auditorium of technical economists at a plenary session of the Fifth World Congress of the Econometric Society in 1985.[12] Rosen was proceeding in his typically bemused style, when he suggested to the stunned audience that they might benefit from re-reading

Alfred Marshall. I remember him saying that there are some things in Marshall that we don't talk about any more, but that we **should** still talk about. I specifically remember him mentioning that Marshall had said that the success of the institution of contract depended on the correct expectation of a certain level of ethical behavior among the participants in the economy. I wish I could remember the specifics better, but Rosen's auditors were visibly dismayed, and I supposed that they were thinking something like: 'read Marshall?, here was the sad sight of a once proud theoretician, going soft and senile.'

Several years later (1997), in one of his last papers,[13] Rosen was even more scandalous in suggesting that Austrian economists might have something to offer neo-classical economists. What they specifically had to offer was that they had continued to talk about some important phenomena that neo-classical economists seldom talked about: innovation, disequilibrium, and entrepreneurship.[14]

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.[15] 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.[16]

A Nice Epiphany

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. (I have done my best to transcribe the key passages in this exchange, which I have reproduced in Appendix 1 of this paper.) The economists were Richard Nelson, Phillipe Aghion and William Baumol. Nelson suggested that there was too much reliance on mathematical models, and Phillipe 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, e.g., 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. (Some preliminary information on economists' use of the phrase "fix ideas" is provided in Appendix 2 of this paper.)

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).

So is anything accomplished? Well, one accomplishment is that the economist demonstrates that they are mathematically well-educated enough, and clever enough, to produce the model.

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 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 Stigler[17] 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;[18] 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.'[19] 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., different boiling and freezing temperatures 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."[20] 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, irregardless 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.’

More on the Method

I was at a dissertation seminar once, at Chicago where a conscientious PhD candidate was presenting his dissertation to Gary Becker. I don’t remember the exact area that the student was working on, but I do remember that it was one where it was common for the mathematical models to assume that people live forever. To differentiate his model from the earlier norm, the student said “I assume life is finite.”

Gary Becker, as was his frequent habit, was leaning back in his chair with his hands behind his head, looking in the direction of the ceiling with an expression of intense concentration. Without cracking a smile, Becker drawled: "Not *too* bad an assumption." The student looked stressed; the five or six others in the room looked puzzled; and I broke out laughing.[21]

The student probably felt hurt at being gently mocked, because he was proud that he had made his model more realistic. But the mocking was probably (and appropriately) more directed at the profession, than at the student.

Here are what I hypothesize to be the steps in the method used 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 current 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, (such as infinite life) are excused on the grounds that such assumptions are necessary in order to "fix ideas" or make the model "mathematically tractable" or to make the model make specific implications (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 uninsightful enumeration of the theoretical possibilities. (p. 843)

I elsewhere (Diamond 2008) 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. (p. 207)

Why the Method 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.[24] 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.[25]

This might be called a "rent-seeking" account of economic methodology.[26] Let me give you a sentimental[27] example that I believe increases the plausibility of this account. Milton Friedman 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 wrote against Friedman in the *New York Times*,[28] and Zvi Griliches spoke against him before congress.

Not too long after Friedman's article came out, I praised it during one of the sessions of a Liberty Fund colloquium held in California. After the session, a very distinguished economist came up to me, and started talking about the Friedman article in a very irritated and animated manner. He said that what Friedman wrote in the article, might be true, but he shouldn't have written it in a public forum.[29] He said that within the NSF, the physicists have always been opposed to funding economics, and that Friedman's article gave the physicists just the ammunition they needed. I remember distinctly that after this conversation, the distinguished economist got into his very large and very expensive car and drove off. To the cynical, it may also be worth mentioning that this economist had received very substantial funding from the NSF.

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. Secondarily, 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 resources[30]) 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 nonmathematical 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 in Appendices 3 and 4 of this paper.)

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?[31] 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 effecting the world. This view of science is sometimes called "instrumentalism." 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.[32]

It may be reassuring that many enlightened and successful economists have adopted just such a position. These would include Frank Knight,[33] Milton Friedman,[34] Deirdre McCloskey, and, I think, William Baumol. (See Baumol's brief comment at the end of Appendix 1 of this paper.)

Schumpeter Probably Would Be on Board

It is not logically necessary, but is aesthetically pleasing, that the methodology most appropriate for evaluating and applying creative destruction, is the methodology that Schumpeter himself advocated. Of course Schumpeter's views on many subjects changed over time, as we would expect from an open-minded scholar who is willing to learn.[35] On the question of method, Schumpeter, throughout his career, was a fairly consistent advocate of some form of methodological pluralism.

At first glance, this view may seem unlikely, since earlier in his career, Schumpeter had helped found the Econometric Society,[36] and Samuelson jabs at the neo-Austrians by sarcastically saying the Schumpeter "was not a good Austrian" in part because he lusted "after mathematical economics" (Samuelson, 1981, p. 4). But Samuelson concludes (p. 4) that on-balance: "From the beginning his methodology took the eclectic road of good sense."[37]

The superficial appearance of Schumpeter having shifted from support of economic mathematics to support of economic history, may be due to his vocally defending whichever branch of a balanced methodology was currently under attack. In the Schumpeter archives at Harvard, one can find several letters from Schumpeter to colleagues, trying to support the career of the young Paul Samuelson. Schumpeter defends Samuelson as a brilliant mathematician, and fears that, given the current state of the profession, Samuelson might not be able to find a job. (I have included some key excerpts from a few of these letters, in Appendix 4.) Early on, the practice of mathematical methods received fragile support, so Schumpeter defended mathematical methods.

Later, the practice of economic history received fragile support, so Schumpeter defended economic history. In the year before his death, in his remarks before the National Bureau of Economic Research (NBER) business cycles conference, Schumpeter defends the use of economic history to a surprised[38] audience that is already pre-disposed to value only mathematically sophisticated econometric analyses.

It has been seriously suggested, by at least one major economist,[39] that these remarks must be excused as the ramblings of a mind in terminal decline. But Samuelson, tells us that Schumpeter's parting remarks must be taken seriously:

> Was this the ranting of a decaying arteriosclerotic (sic) mind, poised two months from extinction? My evidence is against that. The terminal Schumpeter was lucid and witty and often wise. (Samuelson 2003, p. 465)

One need not rely only on Samuelson, to reject he view that end-of-life senility brought Schumpeter to his embarrassing defense of the study of economic history. For he had defended economic history a full seven years earlier, in a 1942 letter:[40]

> I have been primarily a theorist all my life and feel quite uncomfortable in having to preach the historian's faith. Yet I have arrived at the conclusion that theoretical equipment, if uncomplemented by a thorough grounding in the history of the economic process, is worse than no theory at all. (reprinted in Swedberg 1991, pp. 229-230)

Additional evidence can be found in some of the letters excerpted in Appendix 3 of this paper. Further evidence also can be found in Schumpeter's subtitle, to what he hoped and believed would be his most important work. He subtitles his 1939 *Business Cycles* (p. iii) as: "A Theoretical, Historical, and Statistical Analysis of the Capitalist Process."

In the end, one can plausibly label Schumpeter's position as "pluralism" or "eclectic," or one may prefer methodologist Fritz Machlup's description (1951, p. 95) of the position as "methodological tolerance":

> When others reiterated their bigoted patter, Schumpeter could not help coming back with his own message, which urged methodological tolerance and was intolerant only of illiteracy and intolerance itself. (Machlup, p. 95)

The Long-Run, To-Be-Hoped-For Alternative

In their business-best-seller, *The Innovtator'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.[41]

By way of illustration, I will sketch three examples of how this might be done---

one from economics, and two from astronomy.[42]

In economics, I elsewhere (Diamond 2006d) 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 February 23, 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 data sets, increase the plausibility of a new observational-inductive methodological framework.[43] 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?

150 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 mathematical 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 mathematical method?

Appendix 1:

Exchange Between Richard Nelson, Phillipe Aghion and William Baumol on Formal Methods[44]

June 21, 2006, at the end of Plenary Session 1, in the Agora Applon auditorium.

(video clip 1483)

Richard Nelson

Your[45] exposition and the style of what you were proposing was so radically different than Phillipe's that I was surprised that somewhere near the middle, you labeled the kind of study that you were proposing really ought to be done, and I entirely agree with you, as in a sense preliminary to formal modeling, and to axiomitization. And I wonder why you think that's preliminary to formal modeling, and to axiomitization. Another view of this is that the bulk of the relevant, deep, and policy useful knowledge that economists have is not encapsulated in formal models, but rather in the form of theory that Adam Smith started and runs through Marshall, and runs through Schumpeter, and that formal models are in a sense an aid to thinking about those richer types of theoretical formulations as contrasted with being the heart of the understanding in themselves. That certainly is my belief. But I'm curious about what your reaction is to that.

Phillipe Aghion:

So, Dick, it is great that you are here. I mean, as I was starting to say, my objective was not to explain the whole world, and the history, it was just to say, to come up with policy design, OK? And you ask me why modeling can help. Well you know, for me it helps sometimes (to) **fix ideas**. I will give you example, OK? Education. OK. You had the Lucas approach to education, human capital accumulation, and it was leading nowhere. And then I found a beautiful model by Nelson and Phelps. And then I, and then I got it. And then I said, yea, that's good, now and I can understand why the stock of human capital matters, because it's complementary to the... (sic). Of course once you do the model, the idea becomes obvious. (sic). .

The model is like . . .; my mother was in fashion, you know. She would do dresses. You needed what you call a "patron". How do you call it, in English; a "patron" in English? "A pattern." Once the dress is done, you can take the pattern out; you have the dress, you see? But here you need a pattern. And Nelson and Phelps was mine for education, OK? And I realized that why is it. But the problem is that when Kruger (?) and Linda (?), when they try to do a regression based on your model, we just always see the data, like Easterly, no effect. And that's why I try to think, well but maybe because they do not interact, because they do not decompose education. It was . . . , but you see, I needed a model to think about it. Once the model is there, you can think about it. And you can tell the idea to your grandmother, if you still have one, or your mother, if you still have one. And then you run; you can look at (the) regression, because the regression gives you an order of magnitude; you want to know the sort of thing, the first order and second order effects. You see the modeling. And you know it very well.

[There may be a few words, or even sentences, missing here]

(video clip 1484) It's a way we have to do the patterns; you see then we can take it out and tell a story. But it helps **fix the ideas**; it's a discipline; it used to be a discipline we impose on ourselves; now it's true that you can come up with regressions. George Langrees (?) will tell you that economic theory is useless; you just come up with regression to run.

I think he's wrong. I think a serious applied econometrician; he is a very serious guy; but I think people like Blundell who are the top micro-econometricians know very well that you need a dialogue between modeling and econometrics. It's very useful at least for kind of thing I've been doing (?), maybe for history. You get a lot out of it. I mean I think you don't do without. But this dialogue has proved to be very important. And now if you look at the economists who go out on the job market, they are both good applied theorists and they learn econometrics.

That's why they take longer to get their PhD. And that's an evolution; it's the way the profession is evolving. And you know it; and you know it very well. That's why I was surprised by your question.

[Someone may have said something between the end of Aghion, and the beginning of Baumol's comment. I do not remember anything, but I am not sure.]

(video clip 1485)

William Baumol

I think the fact is that each one of the methods I've seen: purely econometric, macroeconomic, microeconomic, has something to contribute and the worst thing we can do is taking potshots at one another because you are using a different model from mine.

* In the transcript I have added bold font to indicate the two passages where Aghion uses the words "fix ideas" in his discussion. A question mark in parentheses means that I am unsure that I correctly heard the preceding word.

Appendix 2:

The Beginnings of an Empirical Analysis of the Use of the Phrase "Fix Ideas" in Economics

The earliest source of something like the concept of fixing ideas might be found in David Hume's *A Treatise of Human Nature*.[46] Somewhat later, the American pragmatist philosopher C.S. Peirce, wrote of "the fixation of belief."[47]

Since all articles included within JSTOR,[48] are fully text-searchable, the database makes it possible to exhaustively track and analyze the use of words and phrases in economics, insofar as the economics profession is represented by the journals included.[49] More broadly, the first article in the complete JSTOR database to use the exact phrase "fix ideas" was Hopkinson's 1887 article in physics.[50]

Within economics, as of October 23, 2006, the archives of journals listed in JSTOR resulted in 273 entries in which the phrase "fix ideas" appeared. (An entry was usually a full scale article, but could also be a note, or a book review, or rarely, any other material that appears in the journal (e.g., reports of meetings, "back matter", etc.)

Table 1 lists the economics journals participating in JSTOR, and the number of times the exact phrase "fix ideas" is used in each journal. Table 2 lists each of the years since the first use of "fix ideas" in a JSTOR economics journal (1928), up through the last year in which the phrase is used in the journals (2003). Most journals in JSTOR have a "moving wall" of recent years that are currently excluded from

JSTOR. Roughly 40% of the journals have a five year moving wall, and roughly 30% of the journals have a three year moving wall. As a result, the counts for the years after the year 2000, are very incomplete, and should not receive much attention.

Table 3 lists the 12 economists who used the phrase three or more times in JSTOR economics journals. The earliest economist to use the phrase is A.L. Bowley in 1928.

Two economists who used the phrase were not expected, and so may be worth an explicit mention. Joseph Schumpeter (1946, p. 196, footnote 2) uses the phrase in a footnote of an article on Keynes. He is discussing Lange's formalization of what Schumpeter describes as a Keynes verbal "model." In the footnote, Schumpeter is discussing the functional form assumed in Lange's version:

> The exact form of it is not unique, however. Nor are the possible forms all equivalent. But since we cannot enter into this here we shall, in order to fix ideas, adopt the one presented by O. Lange in *Economica*, February 1938. I understand that it was approved by Lord Keynes, though I learned this not without experiencing some surprise.

The other unexpected article using the exact phrase "fix ideas" was co-authored by Deirdre McCloskey and Stephen Ziliak (1996). In a section discussing estimation of purchasing power parity regressions, they list some generic possible econometric problems that could occur in such regressions, and then they say (p. 98): "But to fix ideas suppose that all the usual econometric problems have been solved."

Table 1: Number of Articles Using "Fix Ideas", by Journal

Journal Name	# of Articles Using "fix ideas"
American Economic Review	**32**
American Economic Association Quarterly	0
Publications of the American Economic Association	0
American Journal of Agricultural Economics	2
American Journal of Economics and Sociology	0
Annals of the American Academy of Political and Social Science	0
Brookings Papers on Economic Activity	0
Brookings Papers on Economic Activity. Microeconomics	1
Business History Review	0
Bulletin of the Business Historical Society	0
Canadian Journal of Economics / Revue canadienne d'Economique	**11**
Canadian Journal of Economics and Political Science / Revue canadienne d'Economique et de Science politique	0
Contributions to Canadian Economics	0
Canadian Journal of Political Science / Revue canadienne de science politique	0
Canadian Journal of Economics and Political Science / Revue canadienne d'Economique et de Science politique	0
Contributions to Canadian Economics	0
Canadian Public Policy	2
Desarrollo Económico	0
Econometric Theory	5
Econometrica	**59**
Economic Development and Cultural Change	0
Economic Geography	1
Economic History Review	0
Economic Journal	**9**
Economic Policy	3
Economica	3
Illinois Agricultural Economics	0
Industrial and Labor Relations Review	3
International Economic Review	7
International Journal of Health Care Finance and Economics	0
Journal of Applied Economics	4
Journal of Business & Economics Statistics	6
Journal of Economic Education	0
Journal of Economic History	1
Journal of Economic Literature	**9**
Journal of Economic Abstracts	0
Journal of Economic Perspectives	2
Journal of Farm Economics	1
Journal of Human Resources	4
Journal of Industrial Economics	6

Journal of Insurance	0
Journal of Labor Economics	**8**
Journal of Law and Economics	5
Journal of Law, Economics, & Organization	1
Journal of Legal Studies	1
Journal of Money, Credit and Banking	4
Journal of Political Economy	**13**
Journal of Risk and Insurance	1
Journal of Insurance	0
Journal of the American Association of University Teachers of Insurance	0
Proceedings of the Annual Meeting (American Association of University Teachers of Insurance)	0
Land Economics	1
Journal of Land & Public Utility Economics	0
North Central Journal of Agricultural Economics	0
Oxford Economic Papers	**9**
Publications of the American Economic Association	0
Quarterly Journal of Economics	**14**
RAND Journal of Economics	**8**
Bell Journal of Economics	0
Bell Journal of Economics and Management Science	1
Review of Agricultural Economics	0
Review of Economic Studies	**23**
Review of Economics and Statistics	5
Revue économique	0
Scandinavian Journal of Economics	6
Swedish Journal of Economics	0
Ekonomisk Tidskrift	0
Southern Economic Journal	1
Supreme Court Economic Review	0
Total Articles	272

* A journal listed in bold font is one of the 11 journals that published eight or more articles that used the phrase “fix ideas.”

Table 2: Number of Articles Using "Fix Ideas", by Year*

Year	# articles using "fix ideas"	Year	# articles using "fix ideas"	Year	# articles using "fix ideas"
1928	1	1954	1	1979	2
1929	0	1955	0	1980	5
1930	0	1956	1	1981	3
1931	0	1957	2	1982	2
1932	0	1958	0	1983	7
1933	0	1959	0	1984	10
1934	0	1960	0	1985	7
1935	2	1961	0	1986	4
1936	0	1962	0	1987	7
1937	1	1963	3	1988	8
1938	0	1964	0	1989	17
1939	1	1965	0	1990	9
1940	0	1966	0	1991	10
1941	0	1967	1	1992	10
1942	0	1968	0	1993	9
1943	1	1969	1	1994	14
1944	0	1970	2	1995	14
1945	0	1971	2	1996	16
1946	1	1972	1	1997	10
1947	1	1973	3	1998	17
1948	0	1974	5	1999	15
1949	1	1975	1	2000	12
1950	3	1976	3	2001	11
1951	0	1977	1	2002	7
1952	0	1978	2	2003	5
1953	0				

* Any apparent decline in the years 2001-2003 is an artifact of the data, since many journals do not allow general JSTOR access during the last five complete years. (So, for many journals, the last year of inclusion in JSTOR, as of this writing, was the year 2000.)

Table 3: Number of Articles Using "Fix Ideas", by Economist

Rank	Economist Name	# of Articles Using "fix ideas"
1	Phillips, Peter	7
2	Linbeck, Assar	6
3	Fisher, Franklin	4
4	Maggi, Giovanni	4
5	Deaton, Angus	3
6	Goldberger, Arthur	3
7	Hahn, Frank	3
8	Heckman, James	3
9	Persson, Torsten	3
10	Schweizer, Urs	3
11	Topel, Robert	3
12	Ungern-Sternberg, Thomas	3

Appendix 3:

Some Informal Comments of Schumpeter on Method from His Correspondence in the Harvard Archives

The letters quoted in Appendices 3 and 4 all can be found in the Schumpeter collection at the Harvard Archives, and roughly are from the last decade and a half of Schumpeter's life. A few of the letters from this period, also mainly from the Harvard Archives, have been reproduced in Appendix III of Swedberg's (1991) biography of Schumpeter. Of the letters referred to in my Appendices 3 and 4 here, only the letter to Wright was included in the Swedberg appendix. [51]

Schumpeter wrote a letter responding to the managing editor of *Econometrica*'s request for advice on a dispute presumably related to a paper submission by Smithies and a referee report by Burgess. When Schumpeter mentions "his sneer" he is referring to Burgess's sneer:

> . . ., everyone could be expected to see that the working out of exact models that will fit real life is of necessity a slow process which can proceed only gradually. Hence his sneer at writers who deal with an invented world as an excuse for equations is quite uncalled for. The world of physical science is also

an invented one.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Mr. Dickson H. Leavens (regarding an *Econometrica*-related dispute between Burgess and Smithies) (November 11, 1939), in HUG(FP) 4.8 Box 3 Folder "K, L" Harvard University Archives.

Schumpeter in 1939 wrote a letter that reads mainly as a guide to how to read his *Business Cycles* with the most gain, and the least pain (e.g., which chapters could be skipped without missing the most important evidence and lines of argument). Apropos methodology, there is a key sentence:

> . . . my analysis of the capitalistic process (the business cycles are really only the outer form of it) rests equally on a theoretical schema, on statistical observation and on historical material.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Mr. Herbert G. Gatterer (regarding Schumpeter's *Business Cycles* book) (November 21, 1939), in HUG(FP) 4.8 Box 3 Folder "G" Harvard University Archives.

In the following excerpt from a letter on his *Business Cycles* book, when Schumpeter refers to “World War” he almost certainly is referring to the first World War:

> Allow me to repeat, . . ., what I have stressed all along, viz., that in dealing with an evolutionary process, going on in historic time, one cannot expect any given conceptual model to fit reality indefinitely. I have a strong conviction of the substantive truths of my view of the capitalist process, say, up to the World War. But its application to the post-war time is another matter. And although such evidence as I was able to assemble seemed to me to justify the statement that the same process still asserted itself, there must unavoidably be much room for honest difference of opinion about that and hence also about the economics and sociology of the New Deal.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Mr. Algernon Lee (regarding Schumpeter’s *Business Cycles* book) (February 23, 1940), in HUG(FP) 4.8 Box 3 Folder “K, L” Harvard University Archives.

Having in 1940 been President of the Econometric Society (Swedberg, p. 117) , Schumpeter in 1941 sent a letter to the Rockefeller Foundation begging them to subsidize *Econometrica*, in spite of their normal policy against subsidizing journals. Here are a couple of methodologically relevant excerpts:

> The Econometric Society is a strictly scientific body and has, so I believe I may say, done good work in a limited but very important field. None of us would of course like economics to be confined to that sector which is amenable to exact method in the mathematical sense. But every one of us more or less recognizes that, within its proper limits, this line of advance does merit support as much as any other. . . .
>
> . . .
>
> This then is the situation. Let me repeat that I believe the case to be a special one. The success of a purely scientific venture has been destroyed by external circumstances. That venture serves a special but nevertheless very real need which is felt widely especially among the younger generation of economists. It would really be a pity to let it die.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Mr. Joseph H. Willits (regarding Schumpeter's request for a subsidy for a Rockefeller Foundation grant to *Econometrica*) (May 27, 1941), in HUG(FP) 4.8 Box 3 Folder "W" Harvard University Archives.

Schumpeter makes some methodological comments in a letter to economist David McCord Wright:

Concerning the “tremendous” stock of actual knowledge which you are good enough to credit me, I can give a very simple piece of advice: never miss an opportunity to add to it, and furthermore choose your leisure-hour reading so as to add to the historical part of it, and the stock will automatically grow beyond your own expectations.

I experienced a moment of real pleasure when I read your brief reference to your own family history. This is, indeed, the one thing in my theoretical (so far as it is not purely technical) writing on which I pride myself; it is all seen, and in this sense there is nothing in my structures that has not a living piece of reality behind it. This is not an advantage in every respect. It makes, for instance, my theories so refractory to mathematical formulations. They can never be so cut and dried as Keynes’ schema is; but there are compensating advantages, and one of them is that so many people have told me, as you have done: “Yes, that is so. I know that from my own experience and observation.” Your family seems to be a particularly typical case.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Professor David McCord Wright (regarding Schumpeter’s responding to a letter from Wright) (December 6, 1943), in HUG(FP) 4.8 Box 3 Folder “1943 Dec. – 1944 Jan.” Harvard University Archives. [The McCord letter has been reprinted on pp. 230-231 of Swedberg (1991); the portion excerpted above occurs entirely on p. 230.]

Appendix 4:

Schumpeter's Support for Samuelson, from His Correspondence in the Harvard Archives[52]

The following brief letter was written by Schumpeter, presumably in response to hearing of Samuelson receiving an extension of his Social Science Training fellowship:

> I have been delighted to hear that Mr. Samuelson's application has been granted. I can only repeat that he is certainly a most capable youngster.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Mr. Richard D. Schryck (regarding Samuelson) (March 9, 1936), in HUG(FP) 4.8 Box 2 Folder "S" Harvard University Archives.

In this letter excerpt, Schumpeter mentions that Samuelson has just handed him a paper which he considered "an extremely good piece of work":

> That paper will not be easy to place for I am positive that the majority of economists won't understand it and when the time comes Samuelson himself won't be easy to place, for neither his talent nor his achievement is of the kind

> readily saleable to heads of economic departments.
>
> I am worrying about that already because I really think that it is to the interest of science that that youngster should have space to develop in and to fill to the full the measure of his gifts and his intellectual ambition.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Mr. John E. Pomfret (regarding Samuelson) (November 10, 1936), in HUG(FP) 4.8 Box 2 Folder "S" Harvard University Archives.

Here is the body of a letter that Schumpeter wrote to Dean Birkhoff on behalf of Paul Samuelson's being named a "junior fellow" of Harvard's Society of Fellows. Note especially, Schumpeter's worry that because of Samuelson's mathematical method "he will not be very acceptable to the common run of economists":

> My department has recommended Mr. Paul Samuelson to the Society of Fellows. May I draw your attention to the recommendation and suggest that you look at the man who is a rough diamond and does not obtrude himself? I am positive that he is the most gifted graduate we have had these many years.
>
> He came to us from Chicago on a Social Science Training fellowship in September, 1935 which was extended for the current year. That he did very

well in his studies and that at the end of his last academic year passed his generals with the utmost ease is the least of his achievements. But he not only stood out in discussion, in fact discussed with any professor on the footing of perfect intellectual equality, but also completed two highly original papers which will be published soon.

He has the additional claim that, owing to the mathematical turn of his mind, he will not be very acceptable to the common run of economists and that, unless he gets that fellowship, he will be forced to deviate from the path he has cut out for himself and to accept injurious compromise. The Society I know is not very favorably disposed to economists and to theoretical economists of the mathematical type least of all.

This is an additional reason for bothering you with the matter, for surely you, if anyone, can be expected to extend a helping hand to gifted young men who devote their energies to the thorny task of making an exact science of economics. I have also written to Lawrence Henderson.

Papers of Joseph A. Schumpeter. General Correspondence. Letter, Schumpeter to Dean George D. Birkhoff (regarding Samuelson) (February 1, 1937), in HUG(FP) 4.8 Box 2 Folder "B" Harvard University Archives.

(Note: In the letter above, I have corrected a typo that appeared on the onion-skin carbon copy which had "diamond" as "diamong.")

Footnotes

[*]On the use of the "fix ideas" phrase, I have benefited from the comments of Kevin D. Hoover, Eric Schliesser, and John Aldrich. Chan H. Cho and Miaomiao Yu provided research assistance.

[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-give 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 1996.

[9] The examples of Stigler, Becker, and Krugman, are discussed at somewhat greater length in Diamond (2006a).

[10] A paper (Lazear, 2005) by the current 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:

> (p. 260) Human capital theory is primarily a supply-side approach that focuses on the characteristics and skills of the individual workers. It pays far less attention to the environments in which workers work. As such, the human capital framework has led researchers to focus on one class of questions, but to ignore others. Specifically, little attention has been paid to the jobs in which workers are employed.

> (p. 263) The fact that some jobs and some job characteristics are more likely to lead to promotions than other jobs is not surprising. But the analysis suggests that other ways of thinking about wage determination, namely, through job selection, may have been unduly ignored in the past.

> . . .
>
> 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.

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.

[11] 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.”

[12] I remember a large auditorium-like venue for the session, but could not remember any other session details, so I dug out a copy of the program for the meetings. The only plenary session participation listed for Rosen, was his serving as a discussant for Oliver Hart and Bengt Holmstrom’s “Theory of Contracts” paper, which was delivered on August 19, 1985 (see: “Program . . .,” 1986, p. 471). In searching through Rosen’s publications, I cannot find any evidence that his comment was ever published. In an email response (email dated Nov. 19, 2006) to my inquiry, Bengt Holmstrom has replied: “I know that his comment was not published.”

[13] Sherwin Rosen died on March 21, 2001.

[14] Another of Rosen’s minor acts of methodological delinquency, this time in his role as co-editor of the *Journal of Political Economy*, was his asking me to review Alexander Rosenberg’s criticism of the economics profession for drifting further and further into irrelevant mathematical puzzle-solving. My review was basically favorable to Rosenberg, and my memory is that Rosen was quite content with my review.

[15] Darby and Zucker (2003, p. 1) cite Milton Friedman as having told the story in his Economics 331 class in 1967.

[16] Deirdre McCloskey has suggested that I add Frank Hahn to the list.

[17] See Stigler's essay "The Mathematical Method in Economics." (1949) One puzzle is why Stigler did not have this essay reprinted in any of his several later collections.

[18] 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.

[19] 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."

[20] F.J. Donahoe as quoted in Diamond 1988b, p. 185

[21] Becker at the time was intense, rarely smiled, and was far from an overt jokester. But if you were around him often enough, and shared some of his view of the world, you realized he had a well-developed, wry, and deadpan, sense of humor.

[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* (1989). His specific example is from one of the chapters from those volumes. To show that there is meat on the bones of my argument, I reproduce much of Peltzman's somewhat lengthy example here:

> (p. 206) The tone of this review suggests a skepticism about the marginal value of the kind of theoretical work that has come to populate the frontier of the field. . . .
>
> The main reason for my skepticism is the seeming inability of the recent theory to lead to any powerful generalization. This is especially true in the area of game theory, where this problem seems beyond remediation. The impression one tends to come away with from many of the theory chapters in the *Handbook* is of an almost interminable series of special cases. The conclusions drawn from these cases tend to be very sensitive to the way problems are defined and to the assumptions that follow. Fudenberg and Tirole's chapter on noncooperative game theory provides an important example because game theory plays so prominent a role throughout the *Handbook*. In reading (p. 207) this chapter, I was struck by the variety of questions that arise in formulating and solving game-theoretic models---questions whose answers can crucially

affect results. Here is a nonexhaustive list gleaned just from this chapter: (1) How many players are there? (2) Who moves first? (3) Who remembers what (e.g., are there information lags)? (4) Who knows what (e.g., is knowledge common or private)? (5) When do they know it? (See also question 3 above.) (6) Who can communicate with whom and when? (7) What is the probability of any outcome? (8) How reasonable are the players? (9) Is choice once for all or subject to change over time? If subject to change, in how many periods? (10) How long is each period? (11) What is the discount rate? (12) How long do the players live? (13) How do players today respond to past play (e.g., do players develop reputations)? (14) Does an equilibrium exist? (15) Is equilibrium coalition proof? (See question 1 above.) (16) Is equilibrium robust to changes in assumptions? (17) How are deviations punished? (18) Is there a continuum of reactions or a discrete number? (19) Are the players' reaction functions smooth or discontinuous? (20) What does player A believe about B's objective function and vice versa?

Of course, a particular game-theoretic model will not have to answer all these questions, but it will typically have to answer a nontrivial subset.

There then follows the passage I quoted in the main body of the paper.

[23] See, e.g., Field, 1988.

[24] Of course, not all of those with powerful mathematical skills, enter economics as an excuse to do mathematics on the sly. Steve Landsburg has a PhD in mathematics, and yet has applied his formidable intellect to empirical and policy puzzles (e.g., Landsburg

1993), although he still has the mathematician's inclination to view puzzle-solving as fun in its own right, irregardless of any practical implications. See, e.g., his enthusiastic investigation (Landsburg, 2002) of why some people just stand on escalators, while others actively climb them. Another example might be Salim Rashid. Although not a mathematics PhD, his early work (Rashid 1986) made the highly technical contribution of showing that standard results could be derived from a set of less restrictive assumptions. Now he advocates (Quddus and Rashid 1994) using much simpler tools, to address much more practical problems.

[25] 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."

[26] Rentseeking accounts of the behavior of scientists are discussed in Diamond (1996, pp. 16-17). Grubel and Boland (1986), in particular, developed a rent-seeking explanation of the increasing emphasis on mathematics in economics.

[27] It is sentimental because it involves the recently departed Milton Friedman, one of my professional and personal heroes.

[28] I remember mentioning my disappointment that Lucas had written contra Friedman, and Stigler gave me his cynical smile, and said that I should have expected that Lucas, and the rest of the profession, would defend NSF funding.

[29] Most of the conversation I remember in broad terms, but specifically, I remember he said something very close to: 'Friedman shouldn't air the profession's dirty laundry in public.'

[30] Adam Smith has a great discussion of the deleterious effects of university endowments on the productivity of academics (1976, pp. 758-764).

[31] A clear and credible discussion of this issue can be found in Schmaus (1996).

[32] 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.

[33] For an account of Knight's pluralism, see Hands (1997).

[34] 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).

[35] For a useful discussion of the evolution of Schumpeter's views on method, see Kesting (2006).

[36] But Tinbergen (1951) argued that in his own practice, Schumpeter's method of research was more descriptive and eclectic than one might suppose, if the supposition was based on Schumpeter's having been a founder of the Econometric Society.

[37] In a much later commentary, Samuelson continues to describe Schumpeter's methodology as "eclectic" (2003, p. 465). (In the 2003 passage, there is a typo, in that the word is spelled "electic.")

[38] "Imagine then the surprise that greeted his 1949 NBER statement, . . ." (Samuelson, 2003, p. 465)

[39] I am still trying to document where I have read this. Samuelson himself (1951, p. 49) seems to imply the possibility that old age may have been a factor: "Only in the last year of his life did Schumpeter express before the National Bureau of Economic Research Conference on Business Cycles . . . " But in a footnote on the following page (p. 50), he says ". . ., I do not think we have to invoke old age as an explanation for this uncharacteristic performance. He loved to oppose the popular side; and in the Cowles Commission, I respectfully suggest, he met a faith not less fervent than his own, thereby reversing completely his usual motivation." (See also: Swedberg, 1991, pp. 175-176.)

[40] The passage quoted, has been excerpted from a letter from Schumpeter to Miss Edna Lonegan, dated February 16, 1942, stored in the Schumpeter archives at Harvard, and reprinted in Swedberg (1991, pp. 229-230).

[41] This method is in the spirit of the proposal by Larry Lauden, and co-authors, to empirically test scientific methodologies on the basis of how successful they have been

at achieving scientific progress (see: Lauden, Donovan, Lauden, et al, 1986; and Donovan, Lauden and Lauden, 1988). Schmaus' paper (1996), mentioned earlier, also provides a useful elaboration of the Lauden research agenda.

[42] 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.

[43] Here is Eastman's more fleshed-out summary of his position, lest my summary in the body of the paper be so vague and abstract as to make Eastman's position appear more quixotic than it is.

> . . . , a third methodological framework has begun to emerge, especially in those fields such as geophysics and space science where direct testing of certain initial conditions or core hypotheses is difficult, if not impossible, but where gigabyte to petabyte datasets have emerged. This new *observational-inductive* mode of inference now shows promise in the wake of advances in high-performance computing, rich data sets, high-speed sensor systems, and multi-dimensional, multi-scale modeling. For the first time in the 400 years since Francis Bacon introduced induction, new developments in computer-aided knowledge discovery (data mining, pattern recognition, artificial intelligence) enable observational-inductive inferences, which are observation-driven, focus on causal implication, and can minimize theory-dependent assumptions. The recent discovery of T-dwarf stars by data mining massive datasets demonstrates the potential of such data-driven inductive, knowledge discovery methods. The

hypothetical-deductive/inductive and observational-inductive frameworks are complementary and synergistic; however, reduction in theory dependence through applying observational-inductive inference may be the only way to break the logjam. (Eastman, 2005, p. 1)

[44] The attempted draft of a transcript is made from three brief low-resolution video clips, taken in fairly close succession with one another in the main auditorium at the International Schumpeter Society (ISS) meetings in Sophia-Antipolis (near Nice) where the plenary sessions were held. (The clips can be viewed at: http://artdiamond.com/Nice_ISS_2006_06_21/) The ellipses (three dots) represent pauses made by the speaker, or when the speaker moves on to a new sentence before fully finishing the current one. Words in parentheses may have been spoken (and would normally be used in the phrase), but I could not hear them, if they were there, in the clip. I, naturally, had more trouble transcribing Aghion's statements than Nelson's and Baumol's. I had special trouble with several proper names. When I was making a guess at what name I was hearing, I would put a question mark in parentheses after the guess. One name that I am sure of, is "Easterly" which refers to William Easterly. The reference is probably to an Easterly article in *The Handbook of Economic Growth*, that is consistent with the approach presented most notably in his *The Elusive Quest for Economic Growth*.

[45] Nelson's question appears to have been addressed, not to Philippe Aghion, but to the other speaker in session, who was Robert Boyer. Boyer's topic had been "How do Economists Deal with Radical Innovations?" which owed much to Boyer (2001b) which

has been translated into English (2001a). Phillipe Aghion's topic had been "The Case for Schumpeterian Growth Theory."

[46] I am grateful to Eric Schliesser for this suggestion. In one passage, Hume says "it may be proper to fix some general rules" (This can be found in 1.3.15.2 and can be found online at: http://etext.library.adelaide.edu.au/h/hume/david/h92t/chapter28.html)

[47] Roger Backhouse (1994) has applied Peirce's account of fixation of beliefs to the case of fixation of economic beliefs.

[48] "JSTOR" stands for "Journal Storage."

[49] Becker and Knudsen (2005) provide some precedent for the use of JSTOR for bibliometric purposes.

[50] The version of JSTOR that I had access to at the University of Nebraska at Omaha (UNO) included (I believe) the complete set of economics journals, and almost, but not quite, all of the journals in other disciplines. I base my conclusion about the economics journals on comparisons of the economics journal list on UNO's library web site, with the list on the official JSTOR web site.

[51] The original German edition of März's book on Schumpeter, which appeared in English in 1991, included an appendix of a few personal letters written by Schumpeter during his years in Germany. The publishers of the English edition of the book, without explanation, decided to omit this appendix (1991, p. vi).

[52] A word of advice for those who seek to use the Schumpeter archives at Harvard: alphabetical folders usually contain letters alphabetized by the last name of the person

to whom Schumpeter was writing. But sometimes, alternatively, they contain letters alphabetized by the last name of the person Schumpeter was writing **about**. So, for example, letters of support for Samuelson, written to a variety of people, were often filed in the "S" folder.

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