

Review Essay

On Reviewing *Machine Dreams*: Zoomed-in vs. zoomed-out

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Philip Mirowski, *Machine Dreams, Economics Becomes a Cyborg Science*. Cambridge: Cambridge University Press, 2002. Pp. xiv + 655.

When I set out to review this book I began reading the many review essays about Phillip Mirowski's *Machine Dreams: Economics Becomes a Cyborg Science*, I was immediately reminded of the old story about the seven blind men and the elephant. The one who touched its side thought it was a wall; the one who touched its leg thought it was a column holding up the roof; the one touching an ear thought it was a chapatti; and so on.

That this book has been so extensively reviewed seems to be an interesting fact that by itself needs to be examined. While one could categorize the various reviews by noting which parts they focused on – since almost every reviewer chose a different aspect of *Machine Dreams (MD)* – I will instead categorize them by their seeming purpose as book reviews. My experience with reviews suggests to me that there are three major purposes for a book review: (1) a simple report of the contents of the book that someone could read to learn what is in the book, (2) a launch pad to present the reviewer's own views on the topics covered in the book, and of course, (3) a means to criticize the author's views. Also, one could distinguish between audiences for the book as well as the audiences for the review and how a review might be tailored for each. As we will see in the case of *MD*, the audience could be foes of neoclassical economics or true-believing neoclassical model builders or maybe complete outsiders. Additionally, given the broad spectrum of the arguments in *MD*, there seems to be two different ways of looking at those arguments. Some reviewers cannot see the trees for the forest and others cannot see the forest for the trees. I found it particularly interesting that those who seemed to like the book zoomed out to see the forest while those who were critical tended only to zoom in on specific tree details. It was also interesting that one reviewer who liked the book thought it should have been shorter while another who liked it thought it should be longer. The only thoroughly critical reviewer was particularly zoomed in and caused me to remember Harry Truman's famous retort: "stuck pigs always squeal".

THE BOOK

Machine Dreams is the third in a series examining the history of modern economics. The first was the 1989 book, *More Heat than Light: Economics as Social Physics, Physics as Nature's Economics*, which discussed the early neoclassical economists and their attempts in the late nineteenth century to imitate the physicists of the day – accusing them of physics envy. *MD* is about how modern economics developed from efforts of economists to help the American war efforts during World War II. The basic idea is that the high-tech mathematical tools developed and used during the war were put to work creating a new economic methodology dominated by the mathematics of what became known later as Operations Research. The evolution of Operations Research was driven mostly by the eventual development of the computer. The main

characteristic of this evolution in economics was the marrying of two methodological ideas: (a) the core notion of neoclassical theory – namely, that all social and economic events can be explained as the consequences of decisions made by individuals who are motivated solely to choose the best of their available options; with (b) the idea that such individuals can be modeled as rational machines. This marriage amounts to a mechanized rational-choice model which takes methodological individualism for granted. Mirowski calls this “cyborg science”.

In the two decades before the war, mainstream economics was not dominated by a rational-choice model. Instead, the dominant methodology was what today is called “institutionalism”. By this methodology, one would see culture and social institutions playing a determining role in the progress and explanation of an economy. At the time, there were a few “young turks” who thought that, in effect, Adam Smith’s “invisible hand” could not be denied. Moreover, in this regard, they thought that Leon Walras’ nineteenth-century general equilibrium model was a more fruitful and relevant basis for economic methodology than institutionalism. The basic Walrasian idea is to see the state of an economy as being the result of every individual simultaneously but autonomously and independently choosing their best option. And since all are choosing their best, nobody has a reason to change – hence the economy is seen to be in a state of “general equilibrium”. The proponents of this Walrasian equilibrium methodology spent most of their ink exploring the mathematical requirements for modeling such a general equilibrium.¹ In all cases the mathematical technique involved solving a set of simultaneous equations representing the necessary calculus conditions for maximization – one equation for each individual in the economy. A few proponents worried over the knowledge requirements of the individual decision makers being modeled since if perfect knowledge is required for the state of equilibrium to be reached, such a methodology might not be so fruitful as first thought (see Giocoli 2003). But, alas, before this problem could be conquered, the war intervened.

After the war, prompted in some cases by the work of John von Neumann and Oscar Morgenstern (1944), economists eventually turned to game theory since it dropped the obviously unrealistic assumption of autonomous and independent decision makers. Game theory is amenable to the recognition that in most situations, what is the best choice of one individual depends on what other individuals choose to do. A state of general equilibrium in this case is less demanding than is a state of Walrasian equilibrium. For Walras, the state of equilibrium requires every decision maker to be choosing his or her *one* best of all possible options. For game theory, the state of equilibrium is a matter of compatibility between the players. What the optimum choice is for one individual depends on what that individual thinks other players will choose to

¹ The central problem was to prove the mathematical existence of a set of prices that would allow all markets to clear and hence allow all decision makers to be choosing their respective optimum option simultaneously. Walras thought this was a simple matter of counting the number of equations and unknowns such that if these numbers are equal, the proof is obtained. Unfortunately, this is neither necessary nor sufficient (see Boland 1992, Chapter 4). Abraham Wald (1951) did provide a proof but then it was recognized that the real world involves markets that exist at many points of time – just because today’s market for machines is in equilibrium (i.e., demand equals supply today), if the machines are to be used to produce goods to be sold at later points in time, today’s demand will depend on future needs that cannot be known today. So, the next problem for mathematical economists was to prove that there exists a set of prices that clears today’s markets and all future markets. That is, a hamburger produced today is different from a hamburger produced next week – hence we should treat next week’s market for hamburgers as a different market with a possibly different market clearing price. This problem was eventually overcome by Kenneth Arrow and Gerard Debreu (1954) who provided a proof of existence for this multi-period general equilibrium theory. Unfortunately, their theoretical model and proof still leave room to complain about the knowledge requirements for the individual decision makers that plagued the pre-war Walrasian theorists.

do – in particular, an individual’s optimum choice will depend on what that individual thinks opposing players think would be their optimum choice. Since in game theory all players are constrained by the (mathematical) structure and rules of the game, it is possible to mathematically determine what joint options form feasible equilibria. It was the mathematician John Nash (portrayed in the 2001 movie “*A Brilliant Mind*”) who provided the needed mathematical analysis to define a feasible equilibrium concept based on the structure of the assumed game. If one assumes that a current social event is the outcome of decisions made in such a game, one does not need to know much about the players if one can identify the Nash equilibrium situation that will be chosen by the players.

It should be noted that any Walrasian equilibrium analysis also involves a (mathematical) structure² that determines the outcome so long as the decision makers make optimum choices. What is important with a game-theory-based explanation is that one’s interdependent optimum choice need not be the exact option one might make if one did not have to depend on what others choose. A state of equilibrium in game theory requires only that every player having no reason to change – not that their choice be the best for all possible opponents as presumed in Walrasian analysis. Interestingly, the question raised in the 1930s concerning the knowledge requirements for an equilibrium still has not been overcome – all that has been overcome are the limitations of the notion that every individual is able to independently choose his or her best option. Now we are supposed to think that anyone’s optimum depends on what others do.³

So, game theory which pervades modern economics is just another mathematical technique for modeling a state of equilibrium – but it is a technique that does not require knowledge of a huge number of equations representing the decision making process for everyone in a possibly large economy. Modeling the interdependent decisions of just two individuals is far more manageable. Moreover, the mathematics of game theory is far more tractable than the mathematics necessary for the use of Walrasian general equilibrium theory.⁴

While Mirowski focuses on the ideas of key figures such as von Neumann, his main task is to elucidate the role of the military establishment in the development of modern economics. That is, *MD* is concerned mostly with the post-war sociology of the economics establishment and in particular how and why game theory and its computerized manifestations including Operations Research – which is the paradigm of cyborg science – managed to overcome the well established but not mathematics-focused institutional economics. Mirowski painstakingly documents how the war-time researchers put all of their development work on Operations Research to good use by hijacking the economics profession. These researchers were able to do this because they found ready financial support in some key military organizations such as the Office of Naval Research and even the U.S. Air Force.⁵

² The structure is that formed by all of the equations which in turn are what logically follows from the model builder’s assumptions about the production functions and utility functions that constrain the decision makers. If they are all making optimum choices, the necessary conditions for that optimization form the equations necessary for such optimization.

³ Note that it is still presumed that while the decisions are interdependent, they can still be autonomous so long as the players are able to understand the structure and rules of the game.

⁴ For an explanation of the mathematical advantages of game theory, see Boland (2003a, Chapter 4).

⁵ To see this one need only consult some of the volumes of collected papers on mathematical economics (e.g., Newman 1968).

THE REVIEWS

In what follows, I will be using Mirowski's book as a case study of the methodology of reviewing books. I will review the major reviews of *MD* with an eye on the question of why most reviewers get it and in particular why the one severely critical one does not. I will devote a little more time here to examining that one critical review since my background is similar to that of the critical reviewer.

Positive reviews but different parts

Mirowski ... raises his colleagues' blood pressures because his work has both transcended and transformed the subdiscipline, and few are intellectually flexible enough to enjoy rethinking accepted ideas. ...

The heat of Mirowski's rhetoric is ratcheted upwards as he takes on figures like game theorist Kenneth Binmore, Nobelist Robert Solow, and *New York Times* journalist Sylvia Nasar of "A Beautiful Mind" film fame. ... Overall, this is the single most important totalizing narrative of the history of economics that we have had in the last twenty years. Important does not however mean that it will be loved.

E. Roy Weintraub 2004, 419, 421–2

I hope this book will be widely read by economists, historians of economics, and economic methodologists. Philip Mirowski is an accomplished historian who has written an original and remarkable account of the economics of the last half century well-embedded in contemporary Science Studies scholarship.... his history is quite complex, with advances and reversals, mixed and unclear developments, different often imperfectly communicating levels, and dead ends and false paths throughout. There are of course points in this history at which reasonable scholars will disagree with Mirowski. Indeed, the book contains literally thousands of historical claims many of which will no doubt be the subject of further investigation. This, then, is a very thought-provoking book that deserves to be taken seriously. One might begin reading it with the following question in mind: why is it that the half century of postwar economics in which economics has achieved greater influence on society than ever before has only now acquired its first attempt at something on the order of a complete history?

John Davis 2004, 483 & 488

Philip Mirowski's thesis regarding the advent and timeliness of a computational economics that acknowledges and proceeds from the emerging algorithmic structure of increasing parts of economic and social life deserves the serious attention of economists and methodologists of all allegiances. *Machine Dreams* is breaking new ground in evolutionary economics by proposing an algorithmic vision of the economy, bound to turn into a classic of algorithmic evolutionary economics. ... But a disingenuous observer might well regard this as an exercise in the tradition of good old arm chair economics.

Matthias Klaes 2004, 497

Mirowski steadfastly holds on to his champion von Neumann, and particularly to his Cyborg project, he makes clear in no uncertain terms his contempt for the Walrasian school.

Till Gruene 2004, 696

Mirowski does persuade the reader both of how deeply implicated in military funded research most top-flight economists were on the one hand, and the degree to which that connection was effaced or erased in the main academic journals on the other. Often, work that has its origins in some military appropriations, budgeting or strategy problem would morph into a mainstream paper that made no mention of those origins.... *Machine Dreams* is [a] controversial reinterpretation of the history of post-war American economics, blended with a trenchant critique of formal microeconomic theory, wrapped up in a wide-ranging narrative rich in detail and (it seems to me) filled with score-settling

asides, that finally culminates in the triumphant return of the repressed and a theoretical program for the future. Oh, and all presented in a prose style that constantly threatens to spin out of control. The book gets inside your head, but it's hard to know how to assess it.

Kieran Healy 2002

MD is infuriating as much as it is fascinating ... Mirowski's theory regenerates system theories that conceive of social organization as a self-regulating system evolving by itself, shaping people to fit its design rather than being shaped by them. ... Has Mirowski replaced a naive and theoretically unsustainable assumption about markets for a naive and theoretically unsustainable assumption about the rationality of human beings?

Yuval Yonay 2004, 621 & 627

My judgment is that it could have been trimmed to 400 pages with little loss of intellectual content. It's not that it's repetitive, it's just that there is too much cleverness, obscure references to popular culture, and *recherché* puns ("Previews of Cunning Abstractions" "blastoplasts from the past", "blikkrieg", "send in the clones"). Some may find this amusing, and at one level it is. But as a reviewer trying to extract the essence of the argument, I found it distracting ... when you do know more about a subject, you may well be frustrated in different ways. ... Reading this book has made me much more aware of the "machinic hum" of cyborg science, aware that people are getting funding to pursue studies in complexity theory, artificial intelligence, and a whole variety of other endeavours. As scholars, however, we must pick and choose our intellectual investments carefully. While I share Mirowski's dissatisfaction with much of economics as it has been practiced in recent years, I also remain sceptical about the likely payoffs to many cyborg enthusiasts.

Alexander J. Field 2003, 616 & 622

Of the definitely positive reviews, Roy Weintraub offers what I would think is a real book review. He actually tells the readers what is in each chapter of *MD* without beating one single horse or pachyderm yet still giving a positive spin. John Davis chooses to focus his positive review on famous players, in particular, Norbert Wiener, John von Neumann and John Nash. Matthias Klaes focuses on the sociology of scientific knowledge (SSK) in his positive review. A short positive review by Till Gruene focuses just on von Neumann. The sociologist, Kieran Healy focuses his positive weblog review on the idea of a cyborg science.

Another sociologist, Yuval Yonay, seems more concerned with fifteen things in *MD* each of which requires a book by itself (RABBI). Accordingly for Yonay then, the book should be much longer than its 667 pages. But, economist Alexander Field – who focuses on Mirowski's writing style – thinks it should be much shorter. Obviously, you cannot please everyone.

A long review that takes both sides

Mirowski's achievement is more than praiseworthy. His overall thesis, though highly controversial, is so well defended that it is simply impossible to dismiss or ignore it.

Nicola Giocoli forthcoming, §1

Nicola Giocoli has provided a very long review that is positive when it zooms out to focus on the role of the Operations Research aspects of the post-war history and is negative when it zooms in to focus on trees such as von Neumann.

Short reviews with little focus or purpose

Mirowski writes in an exciting, highly metaphorical style. However, this can be frustrating because it makes it hard, at certain points, to be sure of the precise claims being made. Clearly, the U.S. Air Force funded much economic research, but the important question is the extent to which

economists were influenced by the source of their funding. Given that those involved will typically deny any connection, this needs to be argued much more carefully.

Roger E. Backhouse 2003, 770

Mirowski's is a grand theme: how the economic thought that shaped our world was forged in a 'Great transformation' (p. 159)—the emergence of the cyborg sciences—that also upended how we talk about life, work, and everything in between. Its thesis is profound. But Mirowski's language is often distracting. Some of the puns are enjoyable and precise ('Blipkrieg', p. 177), but his writing is unreadable in places and displays an unwelcome cyborg influence.

Jon Agar 2003, 401

von Neumann envisioned that automata theory would be useful not as a tool for replicating the mental processes of individuals, but to explain the functioning of social institutions. In his concluding chapter, Mirowski endorses just such a vision of economics, though he does not show why this 'cyborg' approach would be better than the sensible blend of deductive theory and institutional analysis that characterised the work of the classical economists and the marginalist school up to the middle of the last century.

Mirowski has exposed some important but neglected themes in the postwar history of economic theory. His dense, unwieldy book provides a fertile starting point for debate and further research.

Gary Mongiovi 2004, F347

Now, there are some short reviews, of course.⁶ The positive one by Roger Backhouse and the slightly negative one by Jon Agar are both too short to have any focus. I am not sure what the purpose is for such short reviews. In some cases its shortness is the implied backhanded negative comment.⁷ However, neither of these seems to be such a negative statement. Gary Mongiovi's short review does seem to have a focus on von Neumann but it is too short to contribute much to the discussion.

Book reviews as launch pads

Mirowski has established his main point: much of the elaboration of general equilibrium by the Cowles economists aimed to reply to Mises. ... These models have no relevance to the real world: they give us an economics "that never was, on sea or land." Our author might have noted that Mises knew this perfectly well. ... Once more Mirowski's economists spurned the Austrian approach: it was not "scientific" enough to suit them. Hayek presumed to write about economics without using high-powered mathematics.

David Gordon 2002

two issues raised here, the larger issue of asociality of modern economics and the smaller issue of "duplicity" of economists. ... On a first reading, e.g. mine, one might think that Mirowski simply has the details wrong. Wald (1950) employed von Neumann's minimax decision procedure in a statistical context and demonstrated that minimax estimation is Bayesian with the least favorable distribution as prior. Thus, minimax is rather more pessimistic than the expected utility alternative. But Mirowski is, I think now, not using "paranoid" as synonym for "pessimistic," he is talking about solipsistic madness.

David M. Levy 2004, 424

Mirowski moves on to ... projecting his own ascerbity [*sic*] onto his witnesses and perpetuating rather than elucidating misunderstandings.

Dave Taylor 2003, 523

⁶ I will not be discussing the type of short review one can find on Amazon.com since they are much too difficult to assess given that they are not edited by anyone.

⁷ Mark Blaug's review of my 1997 book in the *Economic Journal* is a perfect example of this.

For an academic like me, it is easy to see why any prospective reviewer might question the benefits of spending time reviewing a book as few universities reward or recognize reviews in their tenure or salary assessments. But of course, if one could use the review as a launch pad for one's own views, then the time might be well spent. So, we can find such a review with David Gordon's review of *MD* that spends most of its ink promoting Ludwig von Mises. David Levy seems more interested in promoting his own touchy-feely views of Adam Smith and David Hume as well as underlining Mirowski's economics of the sociology of economics.⁸ Similarly, Dave Taylor goes beyond complaining about style to show that he is most interested in promoting his own ideas concerning the informational aspects of cybernetics.

And now for something completely critical

Mirowski writes well. He has also read widely, and uncovered sources that are potentially very valuable. But his obsession with cyborg science makes his book worthless as a piece of history.... Not only do I think Mirowski's history is bad, I think his whole line of criticism of modern economic thought lacks either understanding or coherence. I cannot even make out what he would like to put in its place.... So how come the non-computable models of physics work? The reason is that they provide mathematically tractable approximations to the way the universe really is.... we need to elaborate the Arrow-Debreu model so it approximates better whatever the real-world analogue of Scarf's algorithm happens to be in any particular context. Whether the elaboration turns out to be computable or not matters not in the least. If you think it does, consistency also demands that you advocate abandoning the use of calculus and most other mathematical tools in physics.

Kenneth Binmore 2004, 478 & 480

Let us now turn to examining Ken Binmore's critical review. I met Binmore at a dinner given to honour him after a talk he gave to the economics department at Simon Fraser University. During the meal, I naively mentioned Mirowski's name and received a rather unexpected blast. Now that I have read Ken's review of Mirowski's book, I think I understand. And as expected, Harry Truman's sense of humour seems now to be even more relevant. Let us see if we can get past the pig pen to see if there is something worthwhile in the critical review.

According to Binmore, the basic theme of *MD* is "that economics has fallen prey to an invasion of evil cyborgs" and about this Binmore claims that *MD* "explicitly identifies anyone who has worked in artificial intelligence, general equilibrium theory or neoclassical economics, Operations Research, experimental economics, bounded rationality or cognitive psychology, and especially game theory" are all potential targets for Mirowski. Binmore's frequent complaint is that Mirowski is guilty of "misrepresenting my views". He is particularly concerned with Mirowski's allegedly misrepresenting Binmore's views of von Neumann. And to set the record straight, he says (479) that:

"My understanding is that Von Neumann held the view that the definition of a Nash equilibrium only gives reasons for not playing something that is not a Nash equilibrium, but that a proper solution concept should give positive reasons why it should be played. I think this remains a sound criticism for an eductive solution concept, but the huge success of the Nash equilibrium idea lies in the fact that it ... also works in an evolutive context."

⁸ The economics of the sociology of the economics profession was a significant aspect of the argument provided in Grubel and Boland (1986) to explain why when surveyed members of the American Economic Association thought there was too much mathematics displayed in economics journals but the editors of the journals seemed to be ignoring their readers. Our explanation supports much of the findings Mirowski reports in *MD* and discussed in David Levy's review.

It is not clear that Mirowski actually disagrees with this. But, it appears that how one views John Nash is also a problem for Binmore. In this regard he goes on to say:

“In brief, *contra* Mirowski, Nash’s theorem is indeed a natural generalization of the minimax theorem, but it had a revolutionary effect because Nash’s stripped-down version of the equilibrium notion allowed a whole bunch of problems to be put on the back burner while economists used game theory to reinvent their subject.”

As expected, Binmore asks “Did the military connection influence later workers?” and as expected he denies being influenced. This might seem to be a direct refutation of any claim that economics of the 1950s and 60s was influenced by the fact that many of the early works in mathematical economics were financed by the Office of Naval Research or the U.S. Air Force. It might have been except that Binmore fails to point out that he did not study economics as an undergraduate or as a graduate student. He studied mathematics at the Imperial College of the University of London where he received his degrees. He was a professor of mathematics for some time – long before he discovered the mathematics of economics.

From my perspective, I cannot see how one can deny the influence of “cyborg science” in modern economics. My perspective is that of a PhD student during the early 1960s in a program explicitly designed to train us to be mathematical economists. It was a program that was financed by the National Defense Education Act. Looking at the textbooks we used I can report the influence was explicit.⁹ But, of course, we did not see ourselves engaged in doing the military establishment’s work but nevertheless the explicit purpose of the NDEA fellowships which financed many of us was to support the development of mathematical economics exactly along the lines identified by Mirowski (see the 1958 federal act: PL85-864).¹⁰ We were taught that what Mirowski calls cyborg science is the essential methodology for economics. We certainly were not told about institutional economics – let alone that it was the mainstream of economics in the 1930s.¹¹ Instead, we were led to believe that what we were being taught was directly connected to the work of Leon Walras by way of John Hicks¹² and Paul Samuelson¹³.

And finally, since Mirowski seems to be advocating that Walrasian equilibrium solutions should be computable, Binmore says (480) that:

“Whether the elaboration turns out to be computable or not matters not in the least. If you think it does, consistency also demands that you advocate abandoning the use of calculus and most other mathematical tools in physics.”

The first sentence of this quotation is surprising given that, in his famous 1987–88 article on modeling rational players, Binmore advocated what he called an “eductive process” whereby we

⁹ For examples, consider Koopmans (1951) which contains several papers supported either by the Office of Naval Research or by the U.S. Air Force; and Dorfman, Samuelson and Solow (1958) which was commissioned by the RAND Corporation that is in turn connected with the Air Force.

¹⁰ A copy of this is available as www.the-aps.org/publications/tphys/legacy/1959/issue2/58.pdf.

¹¹ Malcolm Rutherford (2000) discusses the dominance of institutional economics in the 1930s.

¹² See Hicks’ famous book, *Value and Capital*, which was first published in 1939 and was intentionally promoting Walrasian general equilibrium analysis. This book was a standard text in the programs that were promoting mathematical economics in the 1960s.

¹³ See Samuelson’s PhD thesis (1965) which was published first in 1947 and was the textbook for those of us studying mathematical economics in the 1960s and, in my case, teaching mathematical economics in the 1970s. See also his (1967) article where he explains how he thinks Walrasian general equilibrium theory to be superior to Marshallian partial equilibrium analysis (which today is the theory and methodology still taught to introductory economics students).

are to envisage players “as potentially very complex machines” (1987, 185). In particular, he said each player is to be considered a “Turing machine” which he says is “Essentially ... a computing device” (1987, 204). But now, in defense of his new position against necessarily computable equilibria, Binmore complains that Mirowski did not pay enough respect to Herbert Scarf’s “computable algorithm for finding *approximate* Walrasian equilibria” (2004, 480 (emphasis added)).¹⁴ Now, really, this is silly. If, in accordance with Mirowski, one is advocating that whenever we use an equilibrium state to *explain* economic variables, our equilibrium *must* be computable (if for no other reason, so that our explanation is testable), then it is exactly approximationism that one is rejecting.¹⁵

While one might see the computability question as a redux of the late Nineteenth-Century dispute among mathematicians over constructive proof,¹⁶ at root here is the typical presumption by mathematical economists that we are all instrumentalists and thus the truth status of our models and explanations does not matter.¹⁷ This is understandable since mathematicians are taught that it does not matter what you assume so long as you do not make mistakes in logic. *Contra* Binmore, for many of us the truth status of one’s models and assumptions matter and moreover, tractability is less important than truth status – and an approximation is just that, an approximation. And there are prominent economic theorists who think no approximation is an acceptable substitute for the real – computable – thing.¹⁸ Simply stated, an approximate equilibrium that is not the exact computable equilibrium is a false equilibrium. Surely the pachyderm agrees.

ON THE METHODOLOGY OF REVIEWING BOOKS

So, the obvious question to ask is: why do journals publish book reviews? Is it to make the readers aware of the existence of recently published books? If so, surely the publication of an annotated list of newly published books would be sufficient. Is it to provide readers with limited time or budget a summary of the book? I could understand this but it is rarely found. Who among academics would be willing to offer such a menial service with such a limited payoff? Obviously, writing a book review with the purpose of criticizing the book would seem to be a

¹⁴ Presumably, Binmore is referring to Scarf (1969) or (1973).

¹⁵ And, to avoid question-begging, the equilibrium of such an explanation must also be unique and this is also assured by the equilibrium being computable. For more about the importance of uniqueness and its role in explanations, see Chapter 5 of my (1986) or Chapter 6 of my (1989).

¹⁶ The debate concerned the use of indirect proofs which invoked infinity or infinitesimals. One suspects that for some the concern was that the use of infinity might support deists but for most it was the questionable use of indirect proof. In particular, it was the questionable use of the excluded middle of ordinary Aristotelean logic which is needed for indirect proof. Their solution to all of this was to require that all proofs be constructive allowing only the “original intuition” of counting. For more about this so-called “intuitionism” see Meschkowski (1965, Chapter 7).

¹⁷ For a discussion of the relationship between instrumentalism and mathematical economics, see Boland (2003b) and Hands (2003).

¹⁸ In regard to approximation in economic theory it is interesting that the Dean of mathematical economics, Paul Samuelson, seems to reject the kind of approximation that Binmore is advocating. In Samuelson’s *Foundations* (1965, 173) he says:

“A recurring source of difficulty ... which goes back as far as Marshall is the practice of introducing certain mathematical relations as alleged “approximations.” These are presented as being valid in the neighborhood of a point of equilibrium. Even if such a relationship be admissible ..., precisely because it can be applied to any and all properly continuous functions it is devoid of meaningful significance.”

For more on the shortcomings of approximationism, see Boland (2003a, Chapters 11,12 and 18).

more worthwhile endeavor. We surely can find many such reviews – some of them unfair criticisms, though.

Critical reviews, nevertheless, would seem to many of us to be worth publishing but the possibility of unfairness needs to be systematically addressed. The reviews of *MD* by Binmore, Davis and Klaes were part of a symposium published by the *Journal of Economic Methodology*. And as such, Mirowski was given the opportunity to respond. This is the obvious way to deal with the possibility of unfairness but, unfortunately, most journal editors would be unwilling to regularly use so much of their space for such review symposia.

Perhaps there is now a way around the space limitations given the widespread availability of the internet. The time is now ripe for all journals that publish book reviews to create *and edit* web pages where there can be opportunities for reviews that zoom out and those that zoom in and, more important, for *every* author of a book for which they have published one or more reviews can be given an opportunity to respond to all critical reviews. Perhaps these journals can even encourage publishers to sponsor or advertise on those pages. It's an idea.

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