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IN DEFENCE OF SERIOUS ECONOMICS: A REVIEW OF TERENCE HUTCHISON; *CHANGING AIMS IN ECONOMICS*

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Abstract

Hutchison's *Changing Aims in Economics* is a stirring defence of traditional economics against both the dominant formalism and against McCloskey's attack. He argues that economists have largely abandoned the traditional aim of economics, to ameliorate the condition of mankind, and that this represents a wrong moral choice. This review essentially agrees with Hutchison, but criticizes a number of his arguments, and presents alternative arguments for his position.

Key words: Hutchison, formalism, positivism, new conversation, McCloskey

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IN DEFENCE OF SERIOUS ECONOMICS:

A Review of Terence Hutchison Changing Aims in Economics,

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This is a passionate book, an angry book. And appropriately so. One should not react with indifference when one sees one's field go off in the wrong direction. In 1938 Hutchison wrote a classic indictment of economics for its failure to formulate testable hypotheses and then to test them. In subsequent years he had the gratification of seeing economics shift in his direction as a pervasive enthusiasm for positivism influenced to some extent what economists did, and to a greater extent what they said they did. But in the last few decades positivism has been in retreat before an advancing army of formalists brandishing the sharp axes of mathematics. In recent years it has also been attacked with very different weapons by McCloskey, Klamer and their associates. Hutchison's book is a spirited response to both of these approaches, and thus provides a follow-up to his defense of falsificationism during the 1970s and 1980s (Hutchison, 1972, 1988).

Before looking at the contents of the book a few words about its form. It is an expanded version of Hutchison's 1990 Hennipman lecture, and hence is unusually brief, a hundred and three short pages of text followed by forty-five pages of endnotes, all in large print. Some of these endnotes are long. One, a respone to McCloskey, is eleven plus pages and is substantive enough to deserve publication as an independent paper. In effect there are two books here, the text for readers who only want the main argument, and the notes for those who want to follow the argument more closely. By giving the reader such a choice Hutchison makes a virtue of the deplorable habit of not putting the notes on the appropriate page of the text. It is the only possible justification for using endnotes instead of footnotes.

There are ten short chapters. The first two point out the strong policy orientation of economists prior to 1945. The third discusses the increased emphasis on abstraction after WW. II. As Chapter 4 illustrates, this tendency was criticized by many leading economists. But it won out, for reasons Hutchison explains in the next chapter. In Chapter 6 he then argues that this increased abstraction has not generated greater understanding. The following two chapters are the most important in the book, for here Hutchison talks about the aims of economics, and also criticizes the prevailing anti-positivism and the attitude that "anything goes". Chapter 9 points out that most economists hold nonacademic jobs in which forecasting is a major task, so that the claim of some academics, e. g. McCloskey, that we cannot forecast must be wrong. The next chapter is a plea to continue our attempts to predict. The final chapter is a postscript devoted to a special 1991 issue of the Economic Journal in which many leading economists tried to predict the future of economics. Hutchison reads these predictions as justifying the hope that economics will return to dealing with reality. Thus he covers an immense amount of material in a few pages, and obviously had to leave many issues unexplored. I will use this as an excuse for presenting some alternative views which are congruent with Hutchison's.

Turning to the specifics, Hutchison points out that before we can debate the virtues of competing methodologies we must decide what we want economics to accomplish. It is here that Hutchison's disagreement with the formalistic tendencies of contemporary economics is centered. Hutchison does not claim that there is anything wrong with the way formalists proceed to build abstract models if their purpose is merely to show the logical implications of certain axioms. Rather, his charge is that the formalist who now dominate academic economics aim at the wrong target. The task of economists is to provide advice that will alleviate the condition of mankind, and not to develop elegant theorems that lack applicability to the real world. Economists are not writers of science fiction who are free to describe imaginary worlds.

Hutchison's criticism is therefore an ethical criticism, a criticism of ends rather than of means. Economists like to avoid moral judgments, but we cannot avoid making such judgments about what economists should do. The way we generally avoid moral judgments is to use the markets' judgment instead of our own. But in this case that is not a palatable alternative. What is the market whose judgment we should use? Is it the judgment of firms and governments that employ economists? As Hutchison points out, these employers are unhappy with the formalistic training provided by our graduate schools. Moreover, some data cited by Hutchison suggest that many more people seem to be employed as economists than hold advanced degrees in economics; maybe what we offer does not meet the market test. That us not so surprising since there is no effective market mechanism for translating the wishes of nonacademic employers of economists into changes in our product. Suppose that firms prefer informally trained to formally trained graduates. What will happen? Dead-End University, which has difficulty in placing its graduates and hence in attracting graduate students, may decide to adjust to the market's preferences. By contrast, Status University, which is mainly concerned with placing its students into academic slots at other prestigious universities will not respond to pressures of the nonacademic market. Nor will it be concerned that Dead-End will compete away its best students. That is not to deny that the market works to <u>some</u> extent; it can change the nature of what is taught at Dead-End. But even there a principal/ agent problem impedes it. The Dead-End faculty does not like to see itself as acting very differently from the Status faculty, and the market does not provide a strong enough incentive to overcome this dislike. If my graduate students get better jobs that is fine, but it does not put much money into where it counts, into my pocket.

If the market for nonacademic economists cannot exercise appropriate control, how about the market for academic economists? Yes, in principle, that market can. But does the self-interest of agents in this market induce them to act in the general interest? It does encourage academic economists to do work that other academic economists consider prestigious, but what determines the type of work that is prestigious? Prestigious work could well be work that is interesting because it is technically "sweet", even though it is of no practical use. The probability that prestige will be accorded to technical virtuosity and not to practical usefulness has increased with the mathematization of economics, because that has attracted into economics just the type of person who cherishes technical sweetness. Hence, it may take a moral decision to work on problems that are important but mundane. No wonder that much of Hutchison's book strikes a moral tone.

If neither academic nor nonacademic employers provide the disciplining functions of a market, can graduate students do so. No, they can not. Hutchison cites Colander and Klamer's data on the dissatisfaction that graduate students at leading American Universities feel with the direction of their training. But as long as a degree from Status leads to a much better job than a degree from Dead-End, students have strong incentive to grin and bear it.

Given the weakness of the market mechanism Hutchison is right in setting out the choice of methodologies as a problem with a moral dimension. At first glance the implication seems obvious; work on practical problems. But matters are more complex. First, a pedantic point: we need to quantify before we can decide. Even someone like myself, who agrees with the statement sometimes attributed to Einstein, that "elegance is for tailors", would prefer a substantial enhancement of the elegance of economic theory over an enhancement of its policy relevance that raises aggregate GDP by only ten cents a year.

I make this seemingly trivial point because Hutchison gives the impression that elegance and refinement of theory do not matter at all, that they are just toys economists play with. He argues that since economic research is financed by the public, we must give the public what it thinks it is paying for, which is relevant rather than elegant economics. But how do we know that the public is not willing to pay for <u>some</u> amount of elegant but useless theory? It is willing to pay for the teaching of art history and Latin poetry, so why not for the teaching of general equilibrium theory?

But how much impractical theory does the public want? That is hard to say. One possible argument is that economists tend to over-emphasize the social utility of having a theory that is elegant and therefore intellectually pleasing. Hence the public wants less formalism than we do. But there is a counter-argument. One would also expect economists to exaggerate the practical benefits that economics can provide. Given how little notice the public takes of our policy advice, perhaps it believes that we are just as usefully occupied in playing intellectual games as in trying to solve the world's problems. We don't <u>really</u> know. All the same, I must admit that I am just playing the devil's advocate. I believe that - at the margin - Hutchison is right, that we are abusing our trust by giving the public much more elegance and much less relevance than it wants. As G.B. Shaw once remarked, every profession is a conspiracy against the public. Veblen's distinction between the instinct of workmanship and the pecuniary instinct is relevant here, though Veblen, failed to realize how dysfunctional the instinct of workmanship can be, and hence greatly underestimated the social value of the pecuniary instinct.

Moreover, many formalists may believe, rightly or wrongly that formalist theory has substantial practical value in an indirect way, an issue discussed below. Furthermore, one of the great advantages of academic life is the right to work within broad limits on the topics one finds most congenial. To ask formalists to work instead on practical problems that do not interest them is to ask a lot. Many of them might not have become academic economists if they had thought that they would have to work on problems that do not interest them. Finally, formalism is not just a disease of economics, but pervades much of intellectual life, from abstract art to mathematics, and hence its practitioners may be forgiven for not considering themselves unusually lacking in moral fibre.

But enough of indifference curves, we also have to look at budget constraints. How much relevance do we surrender when we allocate more effort to elegance? Hutchison believes that it is a substantial amount. But, again to play devil's advocate, perhaps elegance - and the rigor that goes along with it - enhances the practical usefulness of economics. After all, a theory that is wrong usually does little good, and formalist claim, rightly or wrongly, that the rigor of formal analysis guards against errors. Moreover, formalists can argue that their work has important practical implications. Thus new classicals can point to the Lucas critique and to the policy invariance proposition as examples of important policy implications. Frank Hahn has argued that general equilibrium theory shows that the "invisible hand" proposition is invalid. Hutchison, like Coddington (1975) before him, has little difficulty in countering Hahn's contention, but that leaves the more general point that formalism may have practical by-products. There are two answers to this. One is that while formalist analysis <u>may</u> generate practical knowledge, we are more likely to generate such knowledge if we aim for it directly. Formalism is not like pure research in the natural sciences. When physicists look for new particles, they are searching for something that is not of immediate use, but they are looking for something that increases our understanding of physical reality. By contrast, when an economist develops a more parsimonious proof of a certain proposition, her results, however laudatory, do not generate or explain any new facts.

Hutchison also argues that formalist economics tells us little about the real world because the quest for formalism has made academic economists develop models that for mathematical tractability require bizarre assumptions, so that these models are totally unrealistic. A fervor for hyper-abstraction dominates economics. Hutchison makes this argument in general terms rather than by citing examples. That is not surprising. Not only would citing a sufficiently large number of examples from various fields of economics tax the breadth of even such a distinguished scholar as Hutchison, but also it would require a much large book than the one Hutchison wrote. Even so, his reliance on broad generalizations, while understandable, is still a weakness of this book. Hutchison has to count on his charges resonating in the reader's own perception of the literature, and hence he will not shake the faith of many formalists.

Moreover, formalists are likely to reply that abstraction is in the nature of science, and indeed of all thought. Hutchison readily disposes of this argument by pointing out that while some abstraction is good, too much is bad. Hutchison relates the appropriate degree of abstraction to the issue of the realism of assumptions. Formalists are likely to respond to Hutchison's criticism of their unrealistic assumptions with the argument that the realism of assumptions is irrelevant and to cite Friedman in support. But as Hutchison rightly points out, they should not call upon Friedman, because Friedman insisted on the testing of theories by their implications, an injunction that formalists do not take very seriously. Friedman wrote his essay in good part as a criticism of formalism.

Hutchison rejects Friedman's strictures against testing by the realism of assumptions because we have so few possibilities of testing our theories that we cannot afford to give up any of them. But desirable as it is to increase the opportunities for testing, there is a problem here that Hutchison ignores, a problem that is the basis of Friedman's argument. How can we know whether an assumption passes the test? No assumption is perfectly correct, and whether we say that it is sufficiently accurate or not depends upon the theory for which it will be used. For example, a recent news-report stated that the Black Sea is heavily polluted because it is a closed body of water. Is the assumption that it is a closed body of water correct? It is, of course, incorrect when used by a shipper who wants to know how to get goods from Rome to Odessa. But the Straights of Marmara being very narrow, it is a good enough assumption for an ecologist concerned about the drainage of pollutants. Friedman's solution to the problem that the accuracy of an assumption depends on the theory for which it is used, is to point out that certain assumptions of a theory can also be stated as implications of that theory, and can then be used to test the theory under the label of implications. Thus, in the above example the theory that the Black Sea is polluted because of insufficient drainage does not imply that it is completely enclosed. Hence, the realism of the assumption that it is completely enclosed does not matter. By contrast, the assumption that pollutants are pouring into it is an implication of the theory, and hence can be used to test it. Testing only by implications is simply a way of ensuring that the "assumptions" by which one is testing are necessary assumptions, and not just supplemental assumptions made for ease of exposition (see Mayer, 1993a).

All the same, Hutchison is right to complain about the unrealistic assumptions that formalists make, because some of these assumptions can be reformulated as implications that would be rejected by the data. Compare, for example the two country, two goods case in international trade theory with the new classical policy-invariance proposition. Both make bizarre assumptions. But we do not object to the two country, two goods assumption because it is obvious that this assumption is not really needed, that we make it only for expository convenience, so that its unrealism does not invalidate the theory. By contrast, the policy-invariance proposition requires the assumption of rapid price flexibility, and this assumption/implication is unlikely to pass an empirical test. Hence, new classicals should test that implication, and not just rely on their GNP predictions in Sargent's words "not being obscenely at variance with the data" (Sargent, 1976, p. 233)

The use of outlandish assumptions is a special case of an important characteristic of formalism, which I have elsewhere (Mayer, 1993b) called the principle of the strongest link. It is a tendency to focus great care and attention on that part of the argument that can be formalized, and hence is probably already strong, while passing by with little attention the weaker steps in the argument, and then to act as though the whole argument were as strong as that link that has been formalized. Thus the validity of some assumption\implications may be ignored in favor of a more elegant proof of some step in the chain of the argument. A classic example is the literature on time inconsistency in monetary policy, where sophisticated mathematics is often used to "explain" why central banks follow inflationary policies. That, at least in the U.S., monetary policy has, except in the 1965-1980 period, not been unduly inflationary escapes notice.

Why has economics developed in this way? Hutchison attributes it to careerism: formalist work has great prestige and doing such work advances a young academic's career. Besides, he argues, it takes less time to write a mathematical paper than an empirical paper. But I am not sure that mathematical economists would agree that their papers take less time. I know of no evidence on this issue. Hutchison's other reason, that academics are careerists is probably correct, but not a sufficient explanation. One needs to explain why formalist papers have greater prestige. If it is fashionable for men to carry dead rats in their breast pocket, I do not need much explanation for why Mr. Smith does so. But I do want an explanation of why dead rats are fashionable. Elsewhere (Mayer, 1993b) I have argued that the prestige of formalism can be explained by its making economists look good to the public and to themselves.

Formalists are not the only ones who incur Hutchison's wrath. Much of the book is a criticism of McCloskey and the new conversationalists. Although Hutchison generally does not put it in these terms, much of his criticism focuses on the distinction between "is" and "ought". McCloskey has shown that economists are greatly influenced by rhetorical devices. He then argues that rhetorical criteria, rather than philosophical criteria, such as falsificationism, should govern economics. This accords with the current tendency of philosophers of science to focus on the descriptive pole rather than the prescriptive pole of their discipline, and to argue that whatever scientists do, well, that is what scientists should do. But while such permissiveness may be reasonable when considering physics it is dubious when applied to a much less successful field, such as economics.

In rejecting the reasoning of the new conversationalists Hutchison has to meet McCloskey's argument that philosophers have announced the death of positivism. There are two responses open to him. One is to challenge philosophers on their own ground, and the other is to challenge the applicability to economics of the philosophical criticisms of positivism. Hutchison takes the first approach, writing, "as a philosopher has recently remarked of his subject: nothing dates faster than allegations of outdatedness; to which might be added that nothing has a shorter expectation of life than a philosopher's proclamation of the 'death' of some doctrine or idea of which he disapproves." (p. 61<) All the same, it is risky to dismiss the thinking of experts in another field as a mere fad. Granted that in a field given to fads one should not put great credence in the latest enthusiasm. Hutchison is right, we should be skeptical of the current enthusiasms of a field given to fads. But still, the current thinking of specialists is the best guide we have. Alternatively Hutchison could have argued that what philosophers have to say about science in general does not necessarily apply to economics. One can argue this on two grounds. One is that economics differs in salient ways from the sciences that form the paradigms for philosophers of science. Thus, as Hausman (1992) points out, the opportunities for empirical testing are unusually sparse in economics. In addition, economics faces more pressure from ideological bias and financial interests than do most other fields. More generally, a weak science like economics may need greater discipline than more developed sciences, and positivism is a good disciplinarian. If one thinks of positivism as merely a useful heuristic rather than as the embodiment of truth, then it is legitimate for economists to stick with positivism despite its aches and pains. Besides, what economists call positivism or falsificationism is often just an attenuated and hence less vulnerable version of these doctrines. Probably only a minority of those economists who call themselves Popperians know that Popper argued that a theory is not confirmed by being successful on more and more tests. That this Popperian doctrine is open to serious criticism (see Hausman, 1992) should therefore not disconcert "Popperian" economists.

Hutchison also criticizes McCloskey for being incoherent, writing: "Right from the ... original ur-text of the New Conversationalism (McCloskey, 1985) incoherence is more or less systematic, built into the rhetorical tactics. First the obviously nebulous, shifting Aunt Sally of 'modernism' is set up and then the `modernist' stance on some vital issue - falsification or prediction - is comprehensively denounced ... Anything from one to 40 pages later, however, a somersault is performed: that is, falsification, written off as `not cogent and itself `falsified' ... is subsequently ...declared to provide `a powerful test'..." (p. 139) Similarly, McCloskey is not clear on whether factual confirmation or being part of a good story is more important for a theory. Hutchison also challenges McCloskey on factual issues, such as Keynes' success as a speculator.

All in all, Hutchison provides a powerful indictment of two very different methodological trends, all within a few pages, in a style that while sometimes not clear, makes for exciting reading. The book offers extraordinary value for the short time it takes to read it. I wish that all graduate students who are dismayed by the formalism and irrelevance of their training would read it. It might just give them the courage to resist, if not as students, then at least later on. Who knows, such resistance may succeed, for the high status of formalism is not written in the stars. It depends on what we ourselves think is good economics.

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