

# HAYEK THE FALSIFICATIONIST?

## A REFUTATION

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### I. INTRODUCTION

In *The Politics and Philosophy of Economics*, Terence Hutchison (1981) dissects the contributions of various heterodox movements in economics. Two chapters are devoted to the development of Austrian methodological thought. The writings of Friedrich A. von Hayek receive extended attention. Though Hutchison discovers “much constancy of view” in Hayek’s work, he also finds that “on some quite fundamental and very important points of methodology and philosophy, vital and critical changes in Hayek’s views can be discerned . . . which have not received the attention and appreciation which they deserve” (p. 211). The magnitude of these changes leads Hutchison to claim that, in matters methodological, there exists a Hayek I and a Hayek II.

Hayek I’s views are inferred from his work in the 1920s and 1930s on the trade cycle and the socialist calculation debate. The early Hayek is influenced by both Wieser and Mises, particularly in his claims about the indisputability of the general laws of economic science. Hayek I also advocates the use of

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equilibrium models in theorizing. Finally, the early Hayek parts company with certain other Austrians by making "some strong claims for prediction and forecasting" (pp. 210-14).

Hayek II emerges in the mid-1930s. This later Hayek is more circumspect about the use of equilibrium models. Such models should only be used if it can be shown that a tendency toward equilibrium exists. Though the existence of such a tendency is an empirical question, Hayek II notes that economists often simply assume the problem away. Hayek II also recognizes that the use of an equilibrium model necessitates some further analysis of the process of knowledge acquisition. But the most significant change in the development of the Austrian's thought is the "marked Popperian influence" revealed in his later writings. The later Hayek praises prediction, recommends testing "at every step," and emphasizes the importance of constructing economic theories that are falsifiable (pp. 214-19).

Although Hutchison is careful to support his argument with numerous citations, I believe that in certain areas he has misunderstood the exact nature of Hayek's methodological contributions. The goal of this paper is to critically evaluate Hutchison's interpretation of the development of Hayek's methodological thought.<sup>1</sup>

In section II, the methodological aspects of Hayek's *Monetary Theory and the Trade Cycle* (1932), a book chosen by Hutchison as representative of the Austrian's early thought, are examined. Hutchison's claim that the early Hayek advocated equilibrium theorizing stands undisputed, but I argue that Hayek's views on prediction and empirical work are more complicated than Hutchison indicates. Next, Hayek's 1937 "turning point" is examined. While agreeing that the publication of "Economics and Knowledge" (1948b) constituted an important turning point for Hayek, I challenge Hutchison's claim that Hayek's later work is best characterized as a move towards falsificationism. An alternative interpretation of Hayek's "new direction" is then briefly presented. In section IV, I note an anomaly in Hutchison's review of Hayek's methodology, his failure to examine the Austrian's famous 1944 essay, "Scientism and the Study of Society" (1952). A cursory examination of that methodological work suggests that Popper's influence on Hayek had yet to occur in the early 1940s. In the following section, I critically examine the major piece of evidence for the thesis that Popper greatly influenced Hayek's approach to methodology—five quotations, taken from Hayek's later methodological work and cited by Hutchison, which suggest a strong falsificationist strand in the Austrian's methodology. I argue that the quotations are taken out of context. When they are read in context, they support a very different picture from that found in Hutchison's text regarding Hayek's attitude toward the prospects for falsification in economics. The paper concludes with a suggested answer to the question; How could Hutchison have gotten Hayek so wrong?

## II. MONETARY THEORY AND THE TRADE CYCLE

In *Monetary Theory and the Trade Cycle* (1932), Hayek argues that the origin of the trade cycle is monetary. In addition to defending a Mises-Wicksell approach to the origins of the cycle, Hayek criticizes a number of alternative theories. Among those found wanting are various nonmonetary theories (all of which, Hayek claims, must violate the assumptions of the standard equilibrium model to generate a cause of the business cycle) and the quantity theory of money (which errs in focusing on changes in the aggregate price level or the neglect of changes in relative prices) (Hayek, 1932, pp. 51-135). Hayek also criticizes those who would replace theoretical explanations of the cycle with empirical descriptions of series of data; it is in this context that he discusses "empirical studies."

Citing a footnote from the book, Hutchison claims that Hayek makes some "strong claims" for the importance of prediction and forecasting in economics (Hutchinson, 1981, p. 211). Let us examine more carefully what the Austrian had to say on this topic.

Hayek begins Chapter One by noting the limitations of empirical studies in economics. Such studies cannot provide a new theoretical structure since that structure is already given by the self-adjusting equilibrium model (pp. 27-32). Nor are empirical studies very helpful for the verification of theories, except "in a negative sense."

Either statistics can demonstrate that there are phenomena which the theory does not sufficiently explain, or it is unable to discover such phenomena. It cannot be expected to confirm the theory in a positive sense.... There is no reason to be surprised, therefore, that although nearly all modern trade cycle theories use statistical material as corroboration, it is only where a given theory fails to explain all the observed phenomena that this statistical evidence can be used to judge its merits (Hayek, 1932, pp. 34-35).

Only after arguing against measurement without theory and for a weak "negative" view of testing does Hayek acknowledge a positive role for empirical studies: the pragmatic task of providing forecasts (pp. 35-36). It is curious that Hutchison chooses only this last point, which is relegated to a footnote by Hayek, as representative of the Austrian's thought. It is clear that Hayek's early methodological views did not include any inordinately "strong" claims for the importance of prediction in economics. His emphasis throughout *Monetary Theory and the Trade Cycle* is on the secondary position of empirical studies in relation to theoretical work. To acknowledge in a footnote that forecasting may play an important pragmatic role in assisting policymakers in no way alters Hayek's major point that empirical work is subsidiary to theoretical analysis in economics.

### III. HAYEK'S TURNING POINT

If Hayek simply presumed in his earlier book that any adequate economic analysis must begin from an equilibrium framework he scrutinized the concept of equilibrium in a series of articles published in the mid-1930s. Like many economists at the time,<sup>2</sup> Hayek recognized that the idea of an equilibrium is inextricably linked to whatever assumptions are being made about the state of knowledge. His most direct statement of this relationship is found in his 1937 article, "Economics and Knowledge."

Hayek begins by contrasting the concept of equilibrium as applied to an individual with the concept as applied to a competitive economic system. The meaning of the latter concept has not always been clear. Hayek defines it in terms of a compatibility of plans: "For a society, then, we *can* speak of a state of equilibrium at a point in time—but it means only that the different plans which the individuals composing it have made for action in time are mutually compatible" (1948b, p. 41). But if plans are to be compatible, correct foresight is necessary:

Correct foresight is then not, as it has sometimes been understood, a precondition which must exist in order that equilibrium may be arrived at. It is rather the defining characteristic of a state of equilibrium. Nor need foresight for this purpose be perfect in the sense that it need extend into the indefinite future or that everybody must foresee everything correctly. We should rather say that equilibrium will last so long as the anticipations prove correct and that they need to be correct only on these points which are relevant for the decisions of the individuals (1948b, p. 42).<sup>3</sup>

Hayek asks next why economists have concerned themselves with the notion of equilibrium, a concept which obviously refers to a "fictitious state" (1948b, pp. 44-45). Concern with equilibrium can only be justified if a "tendency toward equilibrium" exists. The existence of such a tendency is an empirical question that economists should explore: What conditions might bring it about, and by what process? To answer these questions, more must be known about the process by which knowledge is acquired, the kinds of knowledge that decisionmakers consider relevant, and the importance of the dispersion of knowledge. The fundamental problem confronting economists is posed by Hayek in the following way: "How can the combination of fragments of knowledge existing in different minds bring about results which, if they were to be brought about deliberately, would require a knowledge on the part of the directing mind which no single person can possess?" (1948b, p. 54).

Hutchison states that the publication of "Economics and Knowledge" in 1937 "marks a vital turning-point, or even a U-turn, in Hayek's methodological ideas, and ought to be, but has not been, recognized as marking a fundamental shift in Austrian ideas" (Hutchison, 1981, p. 215, original emphasis). The key question, of course, is whether it was a turning point towards Popperian thought.

Three pieces of evidence for such an interpretation exist. The article contains Hayek's first reference to Popper, a footnote citation of the concept of falsification. Next, Hayek's view that the existence of equilibrium is an "empirical question" suggests that he may believe that empirical studies of the process of knowledge acquisition are warranted. If this were true, it would constitute a reversal of Hayek's earlier views on the limited role of empirical work in economics, and such a reversal might be attributable to Popper's influence. Finally, Hayek states in an article written in 1964 that "Economics and Knowledge" initiated a change in the direction of his research, a change that led him away from "technical economics" and toward "questions usually regarded as philosophical" (1967c, p. 91). Was it Popper who led Hayek to turn his back on economics and embrace the study of philosophical questions?

Though it might be tempting to conclude that Popper's influence on Hayek's methodology reaches as far back as the 1930s, the evidence against such an interpretation seems compelling. Consider first the question of Hayek's footnote reference to Popper. As Hutchison notes, even though the footnote is found on the first page of the article, "the influence of Popper is not obvious in the subsequent pages" (Hutchison, 1981, p. 215). And there is good reason for this. Though both Hayek and Popper lived in Vienna in the 1920s, they did not know each other there. They first met in the 1935-1936 academic year, when Popper read an early draft of "The Poverty of Historicism" in Hayek's London School of Economics seminar (Popper, 1974, pp. 86; 169, n. 163). "Economics and Knowledge" was first presented in November 1936, and it is unlikely that Popper could have influenced Hayek's ideas in such a short time. But even more to the point, in private correspondence with the author Hayek remarks that, as he remembers, the footnote citation of Popper was inserted in the galley proofs of the article (letter, Hayek to Caldwell, of September 29, 1984). If this is true, the ideas expressed in Hayek's article cannot be attributed to Popper.

We may inquire next whether the fact that Hayek believed that the existence of a tendency towards equilibrium was an "empirical question" meant that the Austrian embraced an empirical approach to the study of equilibrium. Again the question must be answered in the negative. Indeed, in the last section of "Economics and Knowledge," Hayek directly addresses this question; his response is clear and explicit:

[I]n stressing the nature of the empirical propositions of which we must make use if the formal apparatus of equilibrium analysis is to serve for an explanation of the real world, or in emphasizing that the propositions of how people will learn, which are relevant in this connection, are of a fundamentally different nature from those of formal analysis, I do not mean to suggest that there opens here and now a wide field for empirical research. I very much doubt whether such investigation would teach us anything new (Hayek, 1948b, p. 55).<sup>4</sup>

What about Hayek's later reminiscences concerning the importance of "Economics and Knowledge"? If we examine the relevant quotation in its entirety, it is clear that the "philosophical" questions that Hayek chose to address in his later career were not Popperian questions.

...though at one time a very pure and narrow economic theorist, I was led from technical economics into all kinds of questions usually regarded as philosophical. When I look back, it seems to have all begun, nearly thirty years ago, with an essay on "Economics and Knowledge" in which I examined what seemed to me some of the central difficulties of pure economic theory. Its main conclusion was that the task of economic theory was to explain how an overall order of economic activity was achieved which utilized a large amount of knowledge which was not concentrated in any one mind but existed only as the separate knowledge of thousands or millions of different individuals. But it was still a long way from this to an adequate insight into the relations between the abstract overall order which is formed as a result of his responding, within the limits imposed upon him by those abstract rules, to the concrete particular circumstances which he encounters. It was only through a re-examination of the age-old concept of freedom under the law, the basic conception of traditional liberalism, and of the problems of the philosophy of the law which this raises, that I have reached what now seems to me a tolerably clear picture of the nature of the spontaneous order of which liberal economists have so long been talking (Hayek, 1967c, pp. 91-92).

The U-turn in Hayek's thought initiated by the publication of "Economics and Knowledge" was not a movement toward Popperian thought. But the question remains: What was the exact nature of Hayek's transformation? I have dealt with this subject at greater length in a separate article (Caldwell, 1988), and will only briefly summarize my findings here.<sup>5</sup>

In the mid-1930s Hayek became a major player in the socialist calculation debate. By that time, the socialists were on the offensive. In a provocative article originally published in 1920 (Mises, 1935), an early article, Ludwig von Mises had advanced the claim that rational calculation (e.g., solving the allocation problem) was "impossible" under a socialist regime. The socialists replied that, at least formally, the standard equilibrium model that had been developed to describe the workings of a competitive economy could just as easily be used to describe a socialist economy. For example, if competition led to the long-run equilibrium condition that product prices equal marginal costs of production, the same result could be obtained in a socialist economy in which managers are instructed to set prices at marginal costs. This point was used to defeat von Mises' claim that calculation under socialism was impossible.

The Austrian counterattack involved questioning the ability of the standard equilibrium model to capture the dynamic properties of a competitive market system. Recall that in his earlier work, Hayek had insisted that any legitimate economic explanation of the trade cycle had to presume the validity of "equilibrium economics." But in "Economics and Knowledge," he questioned for the first time the efficacy of the equilibrium concept for describing a system

in which knowledge is dispersed and subjectively-held. This questioning was to continue in later articles (e.g., Hayek 1948c, 1948d). Ultimately, Hayek was to reject the use of the concept of equilibrium and to recommend that it be replaced with the notion of a market process in which the information-coordination aspects of markets receive prominent attention. Given his earlier claim that the use of the equilibrium construct was a necessary condition for providing an *economic* explanation, it is not surprising that his rejection of the construct led him away from the study of "technical economics" and toward questions "usually regarded as philosophical."

To be fair to Hutchison, the meticulous reader can find elements of the above interpretation, though never explicitly rendered, hinted at in Hutchison's account of Hayek's U-turn. For example, after citing Hayek's reminiscences about the importance of "Economics and Knowledge," Hutchison remarks in a footnote that "Hayek's approach to fundamental problems of policy broadened out from what might be described as a narrowly 'Ricardian' to a comprehensively 'Smithian' approach" (Hutchison, 1981, p. 228, n. 17). In the text, Hutchison chooses to emphasize other ways in which Hayek's views changed, and even mentions the role of equilibrium and assumptions about knowledge among these (pp. 215-16). But looking at the chapter as a whole, there is an unmistakable stress on the influence that Popper allegedly had on Hayek's methodological work. The unwary reader would not be unjustified in concluding that the major difference between Hayek I and Hayek II was that the latter had become a falsificationist.

For some final evidence that Popper had little influence on Hayek's early methodological views, we turn next to an examination of "Scientism and the Study of Society" (1952).

#### IV. THE NEGLECTED SCIENTISM ESSAY

I begin by noting some oddities in Hutchison's treatment of Hayek's methodology. First, it is peculiar that Hutchison begins his appraisal by examining two nonmethodological writings by Hayek, his book *Monetary Theory and the Trade Cycle* and his discussion of the socialist planning debates. Next, it is strange that Hutchison chooses an article written by Hayek in 1937 as marking a methodological turning point when the next methodological article by Hayek he cites was not published until 1955. It is easy to agree with Hutchison that U-turns are seldom "clean breaks" (1981, p. 215), but how many turning-points take eighteen years to transpire? Finally, it is curious that in a treatment of Hayek's methodology Hutchison all but ignores (he gives it only a footnote citation) what is arguably Hayek's most famous and complete statement of methodological principles, "Scientism and the Study of Society."

A brief sketch of Hayek's lengthy and provocative "Scientism" essay shows that it is an important link in the chain of development of his methodological thought. In the essay Hayek contrasts Scientism, the "slavish imitation" of the methods and languages of the natural sciences, with the spirit of disinterested inquiry that characterizes true scientific study (Hayek, 1952, p. 24). He decries the objectivism, collectivism, and historicism of the scientistic approach to the investigation of social phenomena. The purpose of the social sciences is "to explain the unintended or undesigned results of the actions of many men" (1952, p. 41). Because of the complexity of social phenomena, the appropriate method to follow is individualistic and compositive: social phenomena are "built up," or composed, from the actions of the individual agents. Most important, those agents' actions are based on their subjective perceptions of particular situations. As such, the perceptions and purposes, the knowledge and expectations of individual agents are fundamental data in the social sciences. In Hayek's words, "it is probably no exaggeration to say that every important advance in economic theory in the last hundred years was a further step in the consistent application of subjectivism" (1952, p. 52). A few pages later, he links this concern with subjectivism with those problems he discussed in his earlier article, "Economics and Knowledge," namely, the dispersion of knowledge and the compatibility of expectations (1952, p. 57).

To summarize, Hayek rejects scientism, objectivism, collectivism, and historicism in his essay. He emphasizes that social phenomena are built up from the actions of many individuals, individuals whose knowledge is based on their subjective perceptions of data. And he reiterates his earlier theme that subjectively held knowledge is dispersed among many agents. These ideas are compatible with Hayek's later work in the theory of complex phenomena and, indeed, may be viewed as the bridge between his 1937 article and his later writings.

Why in a treatment of Hayek's methodology did Hutchison neglect Hayek's "Scientism" piece? There is no good answer. It may be pointed out, however, that the "Scientism" essay provides further evidence that Hayek's 1937 U-turn was not a movement toward Popperian thought. Though Hayek did not know Popper in the mid-1930s, he *was* familiar with his work by the time "Scientism" was published. Indeed, as editor of *Economica* during the war years, Hayek was responsible for publishing Popper's "The Poverty of Historicism" (1957) in that journal. Yet "Scientism and the Study of Society" contains not a single reference to Popper's work! (In contrast Popper's "Historicism" contains no less than sixteen citations of Hayek's writings.) If one wishes to maintain the strong thesis that "Economics and Knowledge" initiated Hayek's turning towards *Popperian* methodological thought, then his first explicitly methodological essay, published after Hayek had come to know of Popper's work, must be regarded as a troublesome anomaly that is perhaps best left unmentioned.

## V. HAYEK'S LATER METHODOLOGICAL WORK AS REVEALED IN HUTCHISON'S FIVE QUOTATIONS

If it is now clear that Popper had little influence on Hayek's earlier methodological writings, it is equally evident that, in his later methodological work, Hayek refers approvingly and often to Popper's ideas. To support his thesis that Hayek ultimately embraced falsificationism, Hutchison selects quotations from three of the Austrian's methodological papers, one published in the 1950s, one in the 1960s, and one in the 1970s. While his choice of articles is exemplary, I argue that Hutchison's selection of quotations does not accurately represent Hayek's methodological position. This task is best accomplished by examining the five quotations in detail.

1. Hutchison begins by asserting that all three papers show "a marked Popperian influence," which he buttresses with a quotation from Hayek on prediction and explanation. The text in Hutchison reads,

This influence is apparent in Hayek's renewed emphasis on prediction: 'Prediction and explanation are merely two aspects of the same process' [1967a, p. 9]; so that to the extent that human action is unpredictable it is inexplicable (Hutchison, 1981, p. 217).

The full quotation from Hayek is "I assume that the prejudice of certain earlier positivists against the work 'explanation' is now a thing of the past and that it may be taken for granted that prediction and explanation are merely two aspects of the same process" (Hayek, 1967a, p. 9). The passage is taken from a footnote of Hayek's; its sole purpose is to point out a terminological distinction which was quite well-accepted in the philosophy of science of the mid-1950s. Furthermore, Hayek does not make this distinction in order to say that unpredictable human action is inexplicable. Rather, it is to point out that, because different "degrees of explanation" exist in science, an implication is that different "degrees of prediction" also exist in science.

More important, Hutchison focuses on a rather minor footnote while neglecting the important discussion of explanation and prediction found in the text. In that discussion, Hayek notes that prediction is usually defined as the process by which a person moves from a small group of known antecedent conditions to a conclusion. This view of how prediction takes place in science poses no problem for certain disciplines like physics. But Hayek goes on to say that "[t]he situation is different, however, where the number of significantly interdependent variables is very large and only some of them can in practice be individually observed" (1967a, p. 8). This latter case is characteristic of the social sciences, in which "complex phenomena" are examined. Hayek asserts that the standards applied to physics are often inapplicable in the study of complex phenomena, but that this need not lead us "to despair." What is

required is “a kind of reversal of what has been described as the standard procedure of physics” (1967a, p. 9). That reversal, a reasoning from that which is known (about individual atomic agents) to that which is unknown (about the behavior of social aggregates) is characteristic of the individualistic and compositive methods espoused by Hayek in his earlier article on “Scientism.”

2. Hutchison argues that Hayek favors falsifiability. The representative passage that Hutchison selects to support his claim is, “It is certainly desirable to make our theories as falsifiable as possible.” The full quotation from Hayek actually reads,

The advance of science will thus have to proceed in two different directions: while it is certainly desirable to make our theories as falsifiable as possible, we must also push forward into fields where, as we advance, the degree of falsifiability necessarily decreases. This is the price we have to pay for an advance into the field of complex phenomena (Hayek, 1967b, p. 29).

Hayek is arguing that, in the study of complex phenomena, certain general patterns are often all that can be predicted. Such “pattern predictions” possess small empirical content and as a result, “the range of phenomena compatible with it will be wide and the possibility of falsifying it correspondingly small” (1967b, p. 29). Nonetheless, this is often the best we can do, and we should not belittle such “pattern predictions” as less than scientific. It is only in the investigation of “simple phenomena” (as in certain areas of physics) that more can be hoped for.

Hutchison’s quotation completely changes the meaning of the passage. By omitting the “while” from the sentence, Hutchison has made it appear that Hayek thinks we should *always* try to make our theories more falsifiable. Hayek actually says that we should push forward in fields where progress is accompanied by decreased falsifiability.

3. Hutchison’s next sentence reads, “Testing ‘at every step’ is also recommended [1967, p. 6]” (Hutchison, 1981, p. 217). The full quotation from Hayek actually reads:

It is, no doubt, desirable that in working out such deductive systems the conclusions would be tested against the facts at every step. We can never exclude the possibility that even the best accredited law may cease to hold under certain conditions for which it has not yet been tested. But while this possibility always exists, its likelihood in the case of a well-confirmed hypothesis is so small that we often disregard it in practice. The conclusions which we can draw from a combination of well-established hypotheses will therefore be valuable though we may not be in a position to test them (Hayek, 1967a, p. 6).

Once again, Hutchison has taken a portion of a sentence from Hayek’s work and, by ignoring the context in which it is found, has changed its meaning. But even more important, in this passage Hayek is talking about the physical

sciences! His point is that, *even* in the physical sciences, scientific procedure does not always consist of testing natural laws. Rather, even in these sciences, scientists often simply assume such laws are true, and then apply them in new areas.

4. Hutchison turns next to Hayek’s Nobel Lecture, “The Pretence of Knowledge” (1978). He quotes from a sentence intended to show Hayek’s affinity for prediction and falsifiability: “Nevertheless Hayek, surely justifiably, goes on: ‘Yet as I am anxious to repeat, we will still achieve predictions which can be falsified and which therefore are of empirical significance’” (Hutchison, 1981, p. 218).

At least, this time Hutchison acknowledges that the quotation he has selected is from a qualifying remark of Hayek’s. The sentence quoted follows a paragraph in which Hayek posits a ball game played by athletes of approximately equal skill. Part of the passage is reproduced here; it provides a graphic example of the sort of “pattern prediction” that Hayek thinks is possible in the social sciences.

If we know a few particular facts in addition to our general knowledge of the ability of the individual players, such as their states of attention, their perceptions and the state of the game, we could probably predict the outcome.... But we shall of course not be able to ascertain those facts and in consequence the result of the game will be outside the range of the scientifically predictable, however well we may know what effects particular events would have on the results of the game. This does not mean that we can make no predictions at all about the course of such a game. If we know the rules of the different games we shall, watching one, very soon know which game is being played and what kinds of actions we can expect and what kinds not....

This corresponds to what I have called earlier the mere pattern predictions to which we are increasingly confined as we penetrate from the realm in which relatively simple laws prevail into the range of phenomena where organized complexity rules (Hayek, 1978, pp. 32-33).

Thus Hayek does indeed praise prediction. But it is a very different sort of prediction from that which prevails in sciences whose subject matter consists of simple phenomena. Hayek’s whole point is to distinguish between the two types of prediction. Hutchison’s exposition does little to illuminate the issue.

5. Hutchison’s final citation of Hayek is contained in the sentence, “He reiterates, in the most emphatic general terms, the demarcation principle of Popper: ‘We cannot be grateful enough to such modern philosophers of science as Sir Karl Popper for giving us a test by which we can distinguish between what we may accept as scientific and what not’” (Hutchison, 1981, p. 218).

To show that Hutchison once again has misperceived Hayek’s position, we need go no further than the next sentence of Hayek’s article.

There are some special problems, however, in connection with those essentially complex phenomena of which social structures are so important an instance, which makes me wish

to restate in conclusion in more general terms the reasons why in these fields not only are there absolute obstacles to the prediction of specific events, but why to act as if we possessed scientific knowledge enabling us to transcend them may itself become a serious obstacle to the advance of the human intellect (Hayek, 1978, p. 32).

## VI. CONCLUSION

The purpose of this article is to challenge T.W. Hutchison's interpretation of Hayek's methodology. This is a negative exercise, an attempt at refutation. I have not attempted here the more positive and important task of systematically explicating Hayek's contributions to methodology, which is a subject that manifestly warrants attention.

But a puzzle remains. In a long and distinguished career as an historian of economic thought, Terence Hutchison has always been a careful, even judicious, scholar. But he was not very careful in his treatment of the development of Hayek's methodological thought. How might we account for this fact? Some background on the protagonists may help us to answer this question.

Those familiar with the writings of Professor Hutchison know that he has had what might well be described as a love-hate relationship with the Austrians for decades. On the positive side, Hutchison agrees with many of the Austrian criticisms of standard equilibrium theory. In particular, since 1938 Hutchison has argued that any adequate theory of choice must incorporate in a fundamental way an analysis of the role of expectations in the decision-making process. More recently, he has decried the economic profession's movement toward ever more sophisticated mathematical models, models which he feels have little relation to real-world policy problems. His views on the "crisis of abstraction" in economics would surely receive a sympathetic hearing in the Austrian camp (Hutchison, 1938, ch. 4, 1977, ch. 4).

On other matters, however, Hutchison has locked horns with the Austrians. In the course of nearly fifty years of methodological writing, Hutchison has been a consistent proponent of falsificationism. His latest writings indicate a more circumspect attitude toward the prospects for falsificationism in economics: if the Lakatosian labels were applied, one might say that Hutchison has evolved from a "naive" to a "sophisticated" methodological falsificationist. But one element of his thought has not changed: Hutchison from the outset has been a persistent critic of all forms of a priorism in economics. His (1938) *The Significance and Basic Postulates of Economic Theory* is a sustained attack on the methodological views of Lionel Robbins, and Hutchison has repeatedly criticized the a priorist "science of human action," or praxeology, developed by the Austrian Ludwig von Mises.<sup>6</sup> These two approaches to methodology, the a priorism favored by Mises and the falsificationism favored by Hutchison, are diametrically and irreconcilably opposed.

It is my contention that Hutchison misread Hayek on methodology. And I think that it is easy to understand why it happened. Given Hutchison's lifelong campaign against a priorism, and given the natural opposition of the falsificationist and a priorist approaches to methodology, it is understandable that Hutchison would choose for particular emphasis the falsificationist elements in Hayek's methodology. If he could but show that Hayek endorsed, and has long endorsed, Popperian falsificationism, Hutchison might be able to convince other Austrians to abandon their commitment to praxeology.

Though Hutchison's misreading of Hayek may be understandable, his interpretation cannot be allowed to stand unchallenged. In attempting to undercut a priorism, Hutchison distorted the true nature of Hayek's contribution. Though there are traces of falsificationism to be found in Hayek's methodological writings, they are far less important than Hutchison would have us imagine for explaining the Austrian's overall methodological vision.

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## NOTES

1. Hutchison's text could be used to support either a weak claim or a strong claim regarding Popper's influence on Hayek. The weak thesis, that Hayek frequently makes reference to some of Popper's ideas in his later methodological writings, is both true and uncontroversial. The strong thesis is that Hayek's methodological thought underwent a "U-turn," beginning sometime in the 1930s, and that Popper's influence was decisive both in the U-turn and in the subsequent development of Hayekian methodology. It is not always clear from the text whether Hutchison supports the weak thesis, the strong thesis, or some intermediary position. In any case, it is the strong thesis that I hope to refute here.

2. One of the other economists was T.W. Hutchison. In 1938, Professor Hutchison published his treatise on methodology, *The Significance and Basic Postulates of Economic Theory*. In chapter four, "The Basic Postulates of Pure Theory: Expectations, Rational Conduct, and Equilibrium," Hutchison argues that the whole notion of rational maximizing behavior, and with it the notion of equilibrium, depend crucially on the further, and empirically false, assumption of "perfect expectation." That choice actually takes place under conditions of uncertainty forms the core of his methodological critique of standard theory.

Thus Hutchison and Hayek characterized the "problem of equilibrium" in nearly the same terms in the late 1930s. Hutchison's solution to the problem, one that has advocated in varying forms for nearly half a century, is for economics to become a more empirical science. Though Hayek began with the same problem, we will see that his solution differs from that embraced by Hutchison.

3. Hayek notes that this is a "special sense" of the term, correct foresight. It is interesting to compare Hutchison's treatment of these topics (1938, pp. 100-104). Note also Hutchison's disdain for the idea of a "tendency toward equilibrium" (pp. 104-109), and that Hayek's article is quoted there.

4. On the same page, Hayek says that empirical studies can be useful for the "verification" of the "applicability" of models. This is contrasted with the mathematical exercises that constitute the "Pure Logic of Choice." This role for empirical studies differs significantly from the view that theories can be confirmed or disconfirmed by testing. Compare Hutchison (1938, pp. 113-114), where such empirical investigations are embraced as the best way forward.

5. Though my 1988 paper preceded the present one in terms of date of publication, it follows it in terms of execution. It is also logically the successor: the present paper criticizes Hutchison's interpretation of Hayek's transformation, while the other offers an alternative interpretation of the episode.

6. See Caldwell (1982, ch. 6) for the argument that Hutchison's book may be read as a point-by-point refutation of Robbins' (1935) *An Essay on the Nature and Significance of Economic Science*. Though Robbins was not a priorist, he thought that the generalizations of economics were self-evident truths, a position that is anathema to a falsificationist like Hutchison. For examples of Hutchison's distaste for a priorism, see Hutchison (1956, pp. 482-483, 1981, pp. 218-19, 223, 230, note 26). For a critique of praxeology that does not involve falsificationism, see Caldwell (1984).

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