

HAYEK AND "MODERN AUSTRIAN" METHODOLOGY

COMMENT ON A NON-REFUTING REFUTATION

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I have before me, under various titles, and in one "greatly modified" version after another, successive editions of Professor Caldwell's "Working Paper" dated 1983, 1984, 1985, and 1986. I have been invited to comment on the version dated 1986—(though this same piece has been dated by Caldwell as "1988" in various of his other recent publications about Hayek, notably one, which has happened to reach me, listed below as 1988a). In this Comment on Caldwell's "Refutation," I shall also need to refer to others of his various recent writings, such as his 1982, 1984, 1988a and 1988b.

The trouble starts with Caldwell's choice of a deliberately confrontational, "macho" title, which involves him at once in serious ambiguity and exaggeration. The term "falsificationist" does not occur anywhere in what I wrote about Hayek. The only word beginning "fals-" which I used, and that once only, was "falsifiability," which also occurs in a quotation from Norman Barry; while "falsifiable" and "falsified" occur once each in quotations from

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Hayek. Like many words ending in “-ist” and “-ism”, “falsificationist” is much too ambiguous a term on which to try to base a reasonably precise “refutation.” In fact, in his 1988a piece, Caldwell states that Hayek borrowed from Popper “the idea that a theory must be in a certain sense falsifiable to be scientific” (pp. 82-83), while also admitting, somewhat grudgingly, that Hayek “is at best a minimalist falsificationist”—(whatever, precisely “a minimalist falsificationist” amounts to). It seems, therefore, that Caldwell may have refuted his own “Refutation” before, even, he has got it republished—perhaps some kind of record. Alternatively, Caldwell is bending and stretching the highly ambiguous term “falsificationist” on the basis of which he has chosen to launch this attack (or “Refutation”). It is, of course, one of the oldest ploys in the controversial business to bend or exaggerate some argument or statement, which one feels an urge to attack, in order to make it vulnerable. Meanwhile, whatever Caldwell’s “Refutation” may or may not refute (if anything nontrivial) it certainly does not refute any proposition which I have ever stated or believed. It seems, in fact, that Caldwell may not perceive the distinction between a refutation and an assertion to the contrary.

At this point, I would simply call attention to one kind of ambiguity lurking in the package term “falsificationist.” I am not referring to the rather imprecise distinction between a “naive” and a “sophisticated” falsificationist, but to the importance of distinguishing (as Caldwell fails to do) between (a) recommending, *in the interests of clarity*, that theories be formulated in falsifiable terms; and (b) recommending that serious critical efforts be made, as far as possible, to falsify theories. Under (b) alone, there may be quite a range of possible, or reasonable attitudes.¹

Meanwhile, might I counter one of Caldwell’s strenuous attempts to make mountains out of molehills, when he refers to my “chapter as a whole” (Caldwell, 1986, p. 11), suggesting that I had written an entire chapter on Hayek. My chapter as a whole dealt with “Austrians on Philosophy and Method since Menger”; that is, from Böhm-Bawerk and Wieser to Rothbard and Kirzner. I wrote eight and a half pages (plus notes) on Hayek, while Caldwell spends about 50 percent more than that on his nonrefuting “Refutation.”

I would still venture to refer any interested reader to what I actually wrote on Hayek in my book of 1981 (pp. 210-219), and especially, also, to the following section entitled “Dilemmas of Modern Austrians.” There is only one point where I wish to make an alteration, though there is quite a lot I would like to add here (as far as restrictions on space allow) while perhaps offering a fuller account in some future work.

II

Of the new primary material and information which have emerged in the decade since I was writing, one might mention first the announcement that an entire volume of Hayek’s *Collected Works* is to be devoted to his correspondence with Popper, which began in 1937. This information will come, of course, as a fascinating surprise to most of those interested in our subject. But one cannot help wondering what has been made of this recent revelation of the length and scale of the intellectual relationship between Hayek and Popper by those “many modern Austrians” who, according to Professor Richard Langlois, have been regarding Sir Karl as “a rabid empiricist noteworthy primarily for the bad influence he exerted on Hayek” (1982, p. 77, emphasis added). The news that Hayek has so long been exposing himself to the “bad influence” of such “rabid” empiricism must surely have been somewhat disconcerting to those “Modern Austrians” who have proclaimed, or assumed, that “praxeology” is the methodology of “Modern Austrian” economics.

The second new item which emphatically calls for mention is the very remarkable reminiscence of Hayek himself, which apparently dates from 1977 but was first published in 1982. This intensely intriguing account describes the two stages of Hayek’s conversion to Popper’s refutability (or falsifiability) principle. (Caldwell does not mention this reminiscence in this “Refutation” (1986), but he quotes it, at some length, in his 1988a paper, *while stopping exactly short of the most emphatic and precise sentence*. Regarding the criticisms of Popper by Kuhn, Lakatos, and Feyerabend, Hayek rejects those of Kuhn, while conceding some “reservations” about those of the other two. Hayek insists, however, that “basically I am still a Popperian,” and goes on to state that he was a Popperian in the twenties, before the appearance of Popper’s work. He explains that his own reaction to the pretentious claims of Marxians and Freudians, that their theories were in their nature irrefutable, had been similar to that of Popper, who had been led to “the conclusion that a theory that cannot be refuted is, by definition, not scientific. When Popper stated that in detail, I just embraced his views as a statement of what I was feeling. *And that is why ever since his Logik der Forschung first came out in 1934, I have been a complete adherent to his general theory of methodology*” (Weimer and Palermo, 1982, p. 323, emphasis added).

Caldwell apparently just could not face quoting this last definitive sentence of Hayek’s reminiscence, which is so precise regarding dates. (Perhaps Caldwell wanted to protest about Hayek having “gotten Hayek so wrong” (1986, p. 31). Anyhow, very intriguing problems certainly arise as to the precise interpretation of these Hayekian recollections regarding his views in the 1920s. More recent statements in letters from Hayek certainly do not reduce these problems. Actually, however, Caldwell himself is to be congratulated on having, quite unintentionally of course, provided us with a strikingly

appropriate phrase, applicable to Hayek's account of his position at this earlier period, to the effect that Hayek was "something akin to a closet Popperian." (Needless to say, Caldwell never intended that this somewhat bizarre phrase should be applied to Hayek. He concocted the phrase "closet Popperian" so as to apply it to *my views about Hayek*, in order to ridicule them in a hit-and-run footnote, inserted for the furthering of his "flirtation"—as Professor Eugene Rotwein (1986) has so appositely described it—with his "praxeological" readers).²

As regards Hayek in the twenties, there are, however, two points to be emphasized. *First*, there seems to be not the faintest jot or trace, in *Hayek's writings before 1937*, of any methodological ideas, remotely approximating to those of Popper's *Logik der Forschung* (1934), on the subject of refutability or related questions. *Second*, from the start of his studies of economics, and of his career as an economist, Hayek at once came under very powerful methodological influences almost diametrically contradictory of Popper's ideas regarding refutability.

III

Whenever and however Hayek's phase as a "closet Popperian" may have unfolded, when, in 1921, he chose economics as his subject rather than psychology ("perhaps wrongly," as he puts it), he had as his most authoritative teacher, Friedrich Wieser (Weimer and Palermo eds., 1982, p. 288). Later in the 1920s, Hayek was to edit Wieser's collected essays, contributing a most respectful introductory tribute to his "revered teacher." The first of these essays represented Wieser's most striking methodological statement, his denunciation of the youthful Schumpeter's "positivism." The importance of this essay of Wieser has not been sufficiently recognized by 'Modern Austrians' (possibly because it has not been translated into English, and possibly because of Wieser's sympathy with socialism and social reform).³ Anyhow, Wieser was the predecessor whom Mises most closely followed with regard to the "necessity" and certainty of the results of introspection, which at times Mises also stressed with the same extreme pretentiousness.⁴ In fact, Misesian methodology is largely that of Wieser, *plus* an infusion of pseudo-Kantian dogmatism which would probably cause Kant to turn in his grave.

Hayek has also described how, for the most formative decade of his career as an economist, "the chief guide in the development of my ideas was Ludwig von Mises" (Hayek, 1984, 1). Stephan Böhm, moreover, has referred to "the intense methodological discussions in Mises' private seminar" (1989a, p. 204)—which presumably left a mark. Indeed, that Mises's ten-year guidance of Hayek's intellectual development somehow stopped just short of the hazy and flexible frontier of methodology, though conceivable, seems highly improbable

(though, of course, Hayek's methodological views never became totally, 100 percent identical with those of Mises).

Mises did not publish any full version of his methodological doctrines until 1933, with the first, previously unpublished essay of his *Grundprobleme*. The complete Misesian conceptual framework, however, was not to appear until later. In 1933, though Mises insisted on the "apodictic certainty" and a priori nature of the truths on which the science of human action rested, he did not employ the term "*praxeologie*."⁵ Anyhow, soon after the appearance of the *Grundprobleme*, Hayek published the following concise but comprehensive summary of his methodological tenets at this time:

The essential basic facts which we need for the explanation of social phenomena are part of our common experience, part of the stuff of our thinking. *In the social sciences it is the elements of the complex phenomena which are known beyond the possibility of dispute. In the natural sciences they can be at best surmised.* The existence of these elements is so much more certain than any regularities in the complex phenomena to which they give rise that it is they which constitute the truly empirical factor in the social sciences (1935, p. 11; quoted in Hutchison, 1981, pp. 213-214, emphasis added).

Professional philosophers may, of course, argue interminably about the differences between the a priori apodictic certainties of Mises, and what is "known beyond the possibility of dispute"—according to Hayek. For economists, on the other hand, insistence on significant differences in epistemological status between various claims to certainty or indisputability, whether or not "a priori," (anyway, a highly ambiguous term), may justifiably be dismissed as a largely irrelevant philosophical quibble. The extreme closeness of Hayek (1935) to Wieser and Mises (without, of course, total 100 percent identity of views) is surely obvious regarding the two fundamental points of (a) the infallibilist irrefutability of "the essential basic facts of economics"; and (b) the fundamental contrast and difference in nature between the methods and basic postulates of the social sciences and those of the natural sciences—a contrast drawn with about equally preposterous pretentiousness by Wieser, Mises, and Hayek in favour of economics as a social science. Both these basic principles set out by Hayek (1935) flatly contradict, of course, the main doctrines of Popper, including, in particular, his refutability principle. In spite of having earlier agreed with Popper in firmly rejecting Marxian and Freudian claims for the irrefutability of their doctrines, after ten years of guidance from Mises, Hayek was, in 1935, quite prepared to claim "indisputability" for his version of the basic propositions— *which extraordinary propositions, however, like Mises, he never permitted critics to appraise for their possibly trivial, vacuous, or even tautological nature, because he never gave a coherent list, or indication of what they included in relation to human knowledge.*

It has been necessary to outline some main features of Hayek's intellectual biography down to 1935 in order to demonstrate the extreme sharpness of his

methodological transformation, or U-turn, from 1935 to 1937. Regarding Hayek's methodological views in this period, where stands Caldwell? The answer is on a heap of impenetrable ambiguities, which may be outright contradictory, but which *just might* be reconcilable: (1) In 1982, while recognizing differences regarding a priorism, Caldwell assured us of "a striking similarity between the writings of, say, Ludwig von Mises or Friedrich von Hayek and the positions espoused by Robbins . . . all agreed that the fundamental axioms of economics are obvious and self-evident facts of immediate experience" (1982, pp. 103-104, emphasis added). These statements are, of course, substantially correct *regarding 1935*, though they are deprived of most of their value by Caldwell's failure to mention either (a) any citations or dates regarding Hayek, or (b) any subsequent changes in Hayek's published methodological views—a seriously misleading omission in view of the profound transformation occurring barely two years later. (2) In 1988a, however, Caldwell states his belief that "in the field of methodology Hayek is not a disciple of Mises *and never has been*" (p. 76, emphasis added). Again, it may be complained that "discipleship" is a characteristically ambiguous concept. It would seem rather a pity if Caldwell's inclination toward validity of 1982, regarding the similarity between the methodological views of Hayek and Mises (in 1935), has now been overridden by this ambiguous proposition of 1988. When Hayek stated that for ten years of his early career as an economist, Mises was his "chief guide," he did not exempt methodology. Anyhow, "disciple" or no "disciple", Hayek in 1935 published methodological views which had "a striking similarity" with those of Mises (without, of course, being totally 100 percent identical).

IV

The most serious omission from my eight-and-a-half page discussion of a decade ago was that of fuller comment on Hayek's "Economics and Knowledge" of 1937 (henceforward "EK"). Moreover, I very much regret the remark that "the influence of Popper is not obvious" in this article—though it cannot, of course, be obvious to those who have not read, *and understood*, Popper's *Logik der Forschung* (henceforward *LF*) in its original, slim, powerful form. It would be a profound mistake if my remark were assumed to lend any countenance to Caldwell's thoroughly misleading statement that the "text of this article does not show any Popperian imprint" (1988a, p. 82).

It was quite unnecessarily speculative on my part to have introduced the often problematically biographical question of "influence." It would have been, and is, quite sufficient to assert that, as between his Wieserian-Misesian pronouncement of 1935, and his "EK" article of 1937, Hayek's methodological views show a significant, and indeed decisive move in the direction of those

of Popper, whatever "influences" (from *LF* or anywhere else) may, or may not, have been at work. For Hayek has stated that "EK" was written "before I knew anything about Popper," (letter to this author, May 15, 1983), in spite of the apparent emphatic assertion in his earlier reminiscence that 'ever since his *Logik der Forschung* first came out in 1934" he had been "a complete adherent to his general theory of methodology."⁶

Hayek has also remarked that the "main intention" of "EK" was "to explain gently to Mises why I could not accept his a priorism" (personal letter, May 15, 1983). (Unfortunately, the message to Mises was imparted so "gently" that 40 to 50 years later, it had still not got through to some "Modern Austrians"). "EK," however, possesses the almost unique significance in Hayek's writings of containing a rejection—however gentle—of an idea of Mises. Certainly, this rejection of a priorism stamps 'EK' as centrally and fundamentally methodological—in contradiction to the opposite, quite untenable, suggestion of Caldwell (in his 1988b, p. 514). This break with Mises, and also with the claims about indisputability in his own 1935 article, mark an essential first step in Hayek's move toward Popper's views, perhaps promoted by a revival of his "closet Popperian" inclinations of the 1920s in favor of rejecting Marxian, Freudian (and then Misesian) claims to irrefutability.

Let us turn next to the vital footnote reference to Popper at the start of "EK," with which Hayek introduced the term "falsification" as a correction of "verification" in the text above. Hayek has explained that this footnote was added at the proof stage, from which Caldwell, with a mixture of ambiguity and non-sequitur, concludes that therefore "the ideas expressed in Hayek's article cannot be attributed to Popper" (1986, p. 7). Obviously, this follows with regard to a Popperian *influence*, if Hayek knew nothing about Popper in 1936—though Caldwell (1988a, p. 81) has stated that Hayek read *LF* in 1935. But, in an obvious sense, Caldwell's remark is the opposite of the truth. *Why on earth cannot ideas "be attributed to Popper" if Popper had been the first fully and powerfully to develop and publish these ideas? Caldwell must try to face the question as to why Hayek inserted this footnote.* Is it suggested that Hayek was merely concerned to display erudition, or to show that he was up-to-date with the literature? Or was this footnote based simply on guesswork, unsupported by a thorough grasp of the main thesis of *LF*? Hayek, presumably, inserted the footnote, in accordance with the practice of conscientious scholars, so as to indicate, at the start of his article, that some of the arguments he was putting forward ran, at important points, closely parallel with some of the principal ideas of Popper's *LF*. These ideas centered around the relationships between falsifiability, tautologies, and empirical content. Moreover, the replacement of "verification" by "falsification" was crucial for Hayek's argument, because tautologies, which he was criticizing for lack of empirical content, are, of course, verifiable, but not falsifiable.⁷

In the mid-1930s, there had been among economists considerable discussion of the nature and role of tautologies in economic theorizing. I might mention, incidentally, that my own first two publications were mainly concerned with this question (1935 and 1937a). I was, therefore, bound to be specially interested, when reading "EK" towards the middle of 1937 (having read *LF* in 1936) in the vital distinction, which Hayek drew right at the start of his article, between "the tautologies," of which formal analysis consists, and, on the other hand, "propositions which tell us anything about causation in the real world," that is, "the empirical element in economic theory—the only part which is concerned not merely with implications but with causes and effects, and which leads therefore to conclusions which, at any rate in principle, are capable, of verification,"—or, rather, "falsification."⁸ This footnote might, indeed, quite reasonably be regarded as Hayek's announcement that he had become "a falsificationist" (if one can imagine that he would ever have been ready to employ such an ambiguous word).

It is also important to realize that, for several years before 1937, much discussion had been taking place among economists not only of the distinction between tautologies and explanatory statements, but also of the other main theme of "EK," that of the concept of equilibrium and the assumption of equilibrating tendencies.⁹ Hayek's ideas on these subjects ran on similar lines to those of Johan Åkerman, who complained of some writers that: "They find the ideal of abstract description in the perfect logical circle. . . . The setting is thus *a priori* tautologous' it arrives at results which are exactly identical with the elements of thought which have been put into the argument" (1936, p. 118, quoted by Hutchison, 1937a, pp. 87).

Whenever, precisely, Hayek read *LF* between 1934 and 1937, by the time he reached page 13, he would have met with the challenging statement: "Ein empirisch—wissenschaftliches System muss an der Erfahrung scheitern können" ("It must be possible for an empirical-scientific system to be refuted by experience"). Hayek might, perhaps, have recalled that the Marxian and Freudian systems were claimed to be not thus refutable. In fact, he compared the analysis of Mises with "the pure logic of choice" (which actually is a misnomer for the logic of *omniscient or automatic choice*—which is not what most human choice resembles). Anyhow, resolved to break with Misesian claims to *a priori*, apodictic certainties, Hayek came out with the decisive summons: "To economics as an empirical science we must now turn" ("EK", p. 44).

Popper's falsifiability criterion, the emphasis on the empirical emptiness and unfalsifiability of tautologies, and his summons to the study of economics as an empirical science, were quite new themes in Hayek's writings, as was the methodological break with Mises and his vacuous "apodictic" certainties. It would certainly be difficult to conceive of a sharper methodological transformation, or U-turn, than that involved in moving from the Wieserian-

Misesian claim to indisputability of 1935, to the appeal to the Popperian falsifiability principle in 1937.

V

Professor Caldwell seems to entertain very simplistic notions as to how the ideas have developed of a thinker both prodigiously erudite and profoundly original, over an almost unprecedentedly long career, who has been engaged on an exceptionally wide range of different intellectual fronts. Caldwell seems to imply (1986, pp. 11-13) that Hayek's moves in the direction of Popper's ideas cannot have added up significantly because they took place over too long a period and have been interspersed with too many interruptions and moves in other directions. We would venture to suggest here, by way of contrast, that the kind of intellectual pilgrimage, of which Hayek's is a, or the, outstanding example, may not be comprehensible in terms of simple, rapid, continuous, unilinear progression.

In the early 1940s, Hayek turned from questions of the postulates, concepts, and criteria of economic theory toward broader comparisons of the methods of the natural and social sciences, in "The Facts of the Social Sciences" (1943) and in the mainly historical essays on "The Counter-Revolution of Science" (1961) and "Scientism and the Study of Society" (1942/1943/1944) dating from the early 1940s. Although Hayek still maintained a dualist position in comparing the methods and criteria of the natural and social sciences, *it is important to recognize that these papers contained no signs of the preposterous Wieserian-Misesian "pretences of knowledge," which had been reproduced by Hayek in 1935*, regarding (1) the certainty, or "knowledge beyond the possibility of dispute," on which economics and the social sciences are supposed to be based; or (2) regarding the distinct superiority, or greater reliability, of the basic propositions of economics, as compared with those of the natural sciences. These Wieserian-Misesian claims disappeared from Hayek's writings after 1935, as he moved further in the direction of Popper's views. (Surely, incidentally, on this much controverted issue of the similarities or differences in methods and criteria as between the natural and the social sciences, the only sensible position is somewhere in the middle, away from the extreme "pretences of knowledge").

Professor Caldwell indulges in quite misconceived complaints about my "neglect" of the essays on "The Counter-Revolution of Science." The reason for my brief treatment was the fact that *very soon, over the following decade, Hayek moved decisively away from the dualist emphasis in these essays (which therefore became obsolete as an expression of his methodological views) in a direction which brought him markedly closer to Popper (without, of course, total, 100 percent identity of views being reached)*. Hayek has explained that

“Popper’s influence on me was great on the question of the methods of the natural sciences” (letter of May 15, 1983), and he had, indeed, proceeded to reformulate his ideas in terms of the differences between more complex and less complex sciences, or studies, making this difference one of gradual degree, and abandoning the idea of a fundamental distinction in kind. The two very important papers which marked this further fundamental break with Wieserian-Misesian ideas were “Degrees of Explanation” (1955)—which Hayek describes as “little more than an elaboration of some of Popper’s ideas” (1967, p. 4n)—and “The Theory of Complex Phenomena” (1964).

Professor Stephan Böhm has recently observed that “all Austrian subjectivists are staunch exponents of methodological dualism” adding, however, “with the notable exception of Hayek.” “Notable exception,” indeed! Böhm confirms that Hayek “in his sadly neglected (among economists) later methodological work appears to narrow down the radical differentiation between the problems of the natural and social sciences to one of degrees” (1989b, pp. 65, 91). This “sad neglect,” it should be noted, has, of course, been most serious on the part of the “Modern Austrians.” Indeed, Professor Caldwell seems to have succeeded in completely blinding himself to this further fundamental departure by Hayek from Misesian doctrines.

A number of other, mainly non-Austrian writers have, however, succeeded in discerning these important later Hayekian developments. I first called attention in 1978 to “the transition, which seems to be detectable over the decades in Hayek’s methodological views, in a direction away from Mises and towards Popper” (1978, p. 841). Norman Barry (1979) also noted the Popperian component in Hayek’s methodological ideas. Since my brief treatment in 1981, a number of other writers have emphasized Hayek’s turn in the directions of Popperian views on both (a) falsifiability, and (b) differences between the natural and social sciences—for example, Böhm (1982), Butler (1983), Klant (1984), Gray (1988), and, in her very thorough and profound study, Loy (1988).¹⁰

Caldwell obviously has lots more “Refutations” to concoct. He has recently come up, however, with the assertion that Hayek had nothing important to add on methodology after the early 1940s: “‘Scientism and the Study of Society’ is Hayek’s most important methodological work . . . all the subsequent writings of Hayek on methodology virtually only deepen the themes which one finds in this essay; nothing truly important has been added in the meantime” (1988a, p. 79). Thus, because his “flirtation” with praxeology impels him to such desperate attempts to keep Hayek in line with the “dualism” of Mises, Caldwell is unable to recognize the obvious significance of Hayek’s very important later methodological writings as a move in Popper’s direction (as recently agreed by the various distinguished authorities cited above).¹¹

VI

Professor Caldwell concludes his “Refutation” with an attempt at explaining my motives (which are, of course, strictly irrelevant in assessing the validity of my eight and a half pages). According, however, to Caldwell, if I “could show that Hayek endorsed, and has long endorsed, Popperian falsificationism” (together, perhaps, with Popperian “rabid” empiricism) then I might have been able “to convince *other Austrians* to abandon their commitment to praxeology” (1986, p. 20, emphasis added). My motives, in fact, were considerably broader. I have provided a clue to them by italicizing Caldwell’s gratuitous introduction of that much misused term “Austrian.” (There was no need for the word “Austrian” at this point. Caldwell could simply have asserted that I had been trying to convince those with a commitment to “praxeology” to abandon that commitment).

My chapter as a whole was concerned to demonstrate the widely contrasting methodological views held by Austrians, as, for example, by the brothers-in-law Böhm-Bawerk and Wieser, and also, more specifically, by Mises and Hayek. Much of my motivation, provocation, or even inspiration, was provided (as I indicated in 1981, pp. 219–221) by two leading “Modern Austrians,” (or “MA’s”), Professors Murray Rothbard and Israel Kirzner. In an earlier manifesto of the “MA” movement, published shortly before I was writing my chapter (Dolan, 1976) one finds the title of the opening essay by Rothbard (1976) proclaiming “Praxeology” as “the Methodology of Austrian Economics.” This Austrian methodology is then defined by quoting precisely the very Wieserian-Misesian statement of Hayek (1935) which I discussed in section III, as though Hayek had there produced the authoritative, official statement of “MA” methodological, *and praxeological*, principles. The most fatal flaw in Rothbard’s thesis was simply, of course, his assumption that no fundamental transformation in Hayek’s methodological views took place after 1935 (in this respect, Caldwell seems to have followed Rothbard). Immediately after Rothbard’s proclamation came an essay by Kirzner, “On the Method of Austrian Economics” (1976), which opened with the following astounding claim: “One of the areas in which disagreement among Austrians may seem to be non-existent is that of methodology” (cf. Hutchison, 1981, p. 220). Kirzner could hardly have set down such an extreme claim if he had not believed that it was then widely supported by his fellow “MAs” (e.g., Rothbard). Kirzner himself, however, went on to express profound doubts—all too well-founded—regarding “Austrian” methodological harmony, and proceeded to expose a fundamental “dilemma” regarding the empirical nature of the necessary assumptions of equilibrium theory and regarding the predictability of human preferences. The point here is that Kirzner identified the base of this crucial Austrian “dilemma” *precisely in the arguments first put forward by Hayek in “EK” (1937), when he was breaking with Mises.* Kirzner offered a long,

supporting quotation from "EK" (1976, p. 48).¹³ My thesis of a U-turn by Hayek between 1935 and 1937 was, I must therefore confess, largely, though unintentionally, anticipated by Rothbard and Kirzner in 1976 and confirmed by the contrasting quotations they provided from Hayek (1935, 1937). Moreover, that is where Kirzner's fundamental Austrian "dilemma" started. Forty years later, Kirzner (1976) was warning that "the future progress of the Austrian school" required some resolution of this crucial dilemma—which has never been faced, let alone resolved (1976, p. 50).¹³

Caldwell's record on this question of the extent of methodological agreement among Austrians has proceeded from one baffling ambiguity to another. In 1982, though admitting that "many" "MAs" were "lukewarm" toward praxeology, he put forward the extraordinary expository explanation that "for ease of exposition, the terms 'praxeology' and 'Austrian methodology' will be used interchangeably," while warning that this usage "should not be taken to imply that all Austrians adhere to the praxeological position" (p. 119). Just what, might one ask, *should* this terminological hijack "be taken to imply," especially regarding Hayek? What undoubtedly follows is that Austrians who are not "praxeologists" are to be referred to as "praxeologists" for the sake of an "ease of exposition," which surely only Caldwell can discern. What, however, this peculiar expository device *does* serve is an attempt to paper over the gaping cracks of fundamental methodological disagreement between Hayek and Mises since 1937. Following Caldwell's recent switch to the view that Hayek never was methodologically a disciple of Mises, it is not clear whether "for ease of exposition" Hayek is still being called a "praxeologist"—or not. Anyhow, our conclusion is that Hayek, though in a most valid sense an Austrian (that is, by birth, education, and first language) is certainly neither a "praxeologist," nor, methodologically, a "Modern Austrian," either regarding (1) his advocacy of the falsifiability principle; or (2) his abandonment of methodological dualism as between the natural and social sciences, or (3) his views on prediction (in spite of Caldwell's desperate attempts to keep him in line with "MA" doctrines).¹⁴

May I put two concluding points? First, I would venture to offer the simple, personal suggestion to Professor Caldwell that he give this "Modern Austrian" concept a very long rest, and that, "for ease of exposition" (so to speak) he tried cutting out the word "Austrian" altogether for a year or two. Abandoning this word will surely assist him in avoiding any number of ambiguous and tendentious interpretations and questionable generalizations. Secondly and finally, I would, nevertheless, like to express, in conclusion, my considerable gratitude for having my interest reawakened in a fascinating subject which I had felt strongly inclined to let drop.

NOTES

1. See the discussion by Lawrence Boland (1989, pp. 10-11).
2. Caldwell's footnote is as follows:

Hutchison's claim that Hayek in his later methodological writings is something akin to a closet Popperian seems to me completely unfounded, providing further evidence that falsificationist eyeglasses need not improve one's vision (1984, p. 373n.).

3. On Wieser's methodological views, see Hutchison (1981, pp. 205-207, 213). Wieser is not mentioned in the index of Caldwell, (1982). Too late to discuss here, I have learnt that Stephan Böhm has written about Wieser's methodological views.

4. For Mises's Wieserian claims for introspection, see Hutchison p. 210. The treatment of Menger by Mises is especially noteworthy. Though Mises refers to Menger's "path-breaking" *Untersuchungen* he alleges that they suffer seriously from the empiricism and "psychologism" of J.S. Mill. Moreover, Mises makes the extraordinary allegation that Menger's *Untersuchungen* "do not start from modern formulations of subjectivist economics, but from the system, methodology, and logic of classical political economy" (1933, pp. 20n, 67n).

5. It is quite misleading of Caldwell to state, regarding the term "praxeological," that it "can be found in Mises's 1933 work" (1982, p. 104). The term "praxeological," or "praxeology," only occurs in a citation of a work by Slutsky, of which Mises expressed disapproval, though it may have provided him with the source of this term, adopted by himself later on. In the English translation of the *Grundprobleme* (1960) the term is introduced four items to render—though hardly to translate—such German terms as "soziologie," "praktik," and "gesellschaftslehre."

6. According to Popper, Hayek told him that it was Gottfried Haberler who called his (Hayek's) attention to *LF*. Incidentally, my most valuable encouragement and advice at that time (1937), with regard to criticizing Mises, came from Haberler—to whom I was, and am, deeply grateful.

7. See the very illuminating discussion by Boland of "Falsifiability versus Verifiability," and "Tautology versus Testability" (1989, pp. 47-48, 131-132).

8. Hayek clearly observed that tautologies cannot be falsified empirically because they do not "forbid" anything (except, of course, contradictions in terms). As Popper put it: "Not for nothing do we call the laws of nature 'laws': the more they prohibit the more they say" (*LF*, 1935, p. 73). This statement was quoted in my 1937b, (p. 651). I am delighted to find Dr. Claudia Loy (1988, p. 202) reaffirming, after half a century, the importance of Popper's aperçu. According to Popper, this may have been the first time he was quoted in English.

9. See Hicks (1933) and Myrdal (1933). Hayek himself in "EK" was following up some of his earlier contributions on intertemporal equilibrium and business fluctuations.

10. Professor Böhm (1982, p. 50) has stated: "It is important to note that Hayek's views on methodology have changed drastically since the late thirties and early forties . . . crudely put, in a direction away from Mises and towards Popper." Butler (1983, p. 137) notes that Hayek's views on the social sciences:

underwent a significant change in the early 1940s . . . He was originally of the view that the methods of the social and natural sciences were completely different. . . . However, in the meantime, Sir Karl produced a convincing explanation of the essential unity of all scientific method which forced Hayek to reconsider.

Professor J. J. Kiant has observed of Hayek, how, "influenced by Popper, he shows himself to be clearly aware of the importance of the criterion of falsifiability" (1984, p. 79). Mr. John Gray has described how Hayek:

came to adopt Popper's proposal that falsifiability be treated as a demarcation criterion of science from non-science. Again Hayek followed Popper in qualifying his earlier Austrian conviction that there is a radical dualism of method as between natural and social science. (1986, pp. 19-20).

(Incidentally, it is rather disappointing to find Mr. Gray maintaining that I have not correctly identified "Hayek's real debts to Popper," when his sole quotation from Hayek in support of his own account—very sound, as far as it goes—is identical to one of my quotations, except for his addition of an erroneous and ungrammatical ("s").

Dr. Claudia Loy (1988, pp. 15-16) gives a very precise account of how, "under the influence of Popper, Hayek, towards the end of the thirties, gradually modified his methodological views." Incidentally, Dr. Loy also remarks in a footnote on the reverse influence of Hayek on Popper, which must almost certainly have occurred, to some extent, in such a lengthy exchange. Presumably, most of Popper's rather slight acquaintance with economics and political economy came from Hayek. The question is whether the results of the influence of Hayek on Popper (apart from helping Popper's move from New Zealand to the L.S.E.) were ever of nearly the same interest and importance, considering the minor role which economics and political economy have played in Popper's interests and achievements. Certainly the evidence seems very speculative and uninteresting for Caldwell's view that "Popper has had little influence on Hayek, but Hayek has greatly influenced Popper" (1988a, p. 81).

11. In section V (1986), Caldwell picks out five of the 25 to 30 quotations in my eight-and-a-half pages on Hayek and complains that I have seriously misrepresented him. Of course, the meaning of a quotation can always be altered, however slightly, by lengthening it. Caldwell proceeds to lengthen considerably my quotations and then to complain indignantly that their meaning has been altered. The question is, of course, whether the alteration in meaning is significant and nontrivial, which Caldwell invariably fails to show. By claiming three times (pp.s 14, 15, and 16) that he is replacing my quotations by "the full quotation," Caldwell might perhaps be taken to be implying that I have omitted words or sentences in my quotations without indicating the omission. If so, this is quite untrue. Caldwell's quotations are no more *the full* quotations than are mine. They are simply longer, and I could, of course, always, relevantly or—like Caldwell—quite irrelevantly, overtrump his longer quotations by still longer ones.

*It must be noted that all five of Caldwell's complaints arise regarding the interpretation of Hayek's three later articles of 1955, 1964, and 1974. Like a number of writers quoted above, I certainly hold that these later articles are of the highest importance, and introduce modifications of fundamental significance in Hayek's views, which bring them significantly closer to those of Popper (though I renounce any speculations regarding influence). Caldwell objects to my quotations because they support this foregoing interpretation and are opposed to his view that 'nothing truly important' was added by Hayek in these later essays. Some of Caldwell's charges are obviously false (e.g., that a quotation of mine "completely" changes Hayek's meaning (p. 16), and again when Caldwell asserts to be Hayek's "whole point" what is clearly only a part of his point (p. 18). In particular, Caldwell has shown himself to be hardly qualified to pass judgment on other people's quotations by perpetrating such a seriously misleading truncation of Hayek's reminiscence as to how he became a "complete adherent" of Popper's methodology on the publication of *Logik der Forschung*.*

12. In the very lengthy passage from "EK", quoted by Kirzner, Hayek stated that his "significant point" was that it is the "assumptions that people do learn from experience, and about how they acquire knowledge, which constitute the empirical content of our propositions about what happens in the real world." From which Kirzner straightway and inevitably concluded: "If we are able to say anything about the process of equilibration . . . we shall have to rely upon the particular empirical proposition that men learn from market experience in a systematic manner. This is inconsistent with the second tenet underlying Austrian economics that there is an inherent

indeterminacy in the way by which human knowledge changes" (1976, pp. 48-9; Hayek, 1937, p. 46).

13. Dr. Claudia Loy (1988, pp. 188-90) comments very cogently on the "extreme polarities" and outright contradictions at the heart of "Modern Austrian" doctrines about expectations, equilibrium, and predictability. She describes it very politely as "astonishing" that such contradictions have not led to any reforms of Misesian claims to certainty. This condition of long-persisting contradictions between rival "apodictic certainties," without any accepted means of resolving such dilemmas, seems to point to a state of methodological bankruptcy—which certainly Caldwell's "pluralism" is incapable of relieving.

14. For a detailed and discerning analysis of Hayek's views on prediction, see Graf (1978), who also suggests that, at some points, Hayek moved significantly toward Popper's views.

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REPLY TO HUTCHISON

Bruce J. Caldwell

I

Professor Hutchison's comment contains a lengthy explication of his views on the relationship between the ideas of Hayek and Popper. In addition, he raises numerous questions about the consistency and coherence of some of my published work. In my reply, I will first try to answer his broadsides concerning my scholarship. I will then identify as best I can the differences in interpretation which still separate us.

II

Five of my publications are mentioned by Hutchison. Hutchison jumps around quite a bit in citing them, so that it may be difficult for the reader to keep straight what Caldwell was supposed to have said when. A brief description of the works which Hutchison cites may help to clarify matters.

Caldwell (1982) is my book, *Beyond Positivism*. The final draft of the book was completed during the 1981-1982 academic year, which I spent at New York University (NYU) on a postdoctoral grant to learn about Austrian economics.

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I mention this because the year at NYU led me to add some new material to the manuscript. For example, the chapter on the methodological positions of Robbins and Hutchison in the 1930s includes a discussion of the Robbins-Austrian connection.

Significantly, however, my focus in this section was not on Hayek but on Ludwig von Mises. Specifically, I undertook an extended analysis of Mises' a priorist defense of praxeology, the so-called "science of human action." I compared praxeology with Robbins' views, noting, for example, that though both he and Mises believed that the postulates of economics were somehow self-evident, only the latter characterized them as a priori true. There is no similar extended treatment of Hayek in *Beyond Positivism*. His name is mentioned in five places, always in passing. Ironically, my interest in Hayek was not awakened until late in the academic year, when Jerry O'Driscoll handed me a copy of Professor Hutchison's 1981 book, the one which contains his claims about Hayek's U-turn. By that time, *Beyond Positivism* had been sent off to the publisher.

Given its paucity of references to Hayek, why is Caldwell (1982) mentioned at all by Hutchison? It would appear that most of his citations are designed to cast doubt on either the quality of my scholarship or the consistency of my views. Thus, Hutchison (in note 3) chides Caldwell (1982) for failing to mention Hayek's teacher Wieser. This was hardly a sin, given that there was so little attention paid in the book to Hayek! In note 5, Hutchison complains about my statement that praxeology was first mentioned by Mises in a work published in 1933. I had read the English translation, where the word praxeology was used four times, but apparently in the German edition it was mentioned only once. Hutchison finds this "quite misleading," which it might have been had I discussed Mises' book in any detail. But there is no discussion of the book at all; the reference again was a passing one. When I examined praxeology, my references were to two other books by Mises, *Human Action* (1949) and *The Ultimate Foundations of Economic Science* (1962), or to the later secondary literature.

While these potshots are niggling irritations, Hutchison's next reference to my book is substantive. It also provides a vivid illustration of one of my complaints, that Hutchison at times misrepresents what others have said to suit his own interpretive purposes. At the end of his section III, Hutchison's text includes the following "quotation" from Caldwell (1982): "a striking similarity between the views of, say, Ludwig von Mises or Friedrich von Hayek and the positions espoused by Robbins... all agreed that the fundamental axioms of economics are obvious and self-evident facts of immediate experience" (1982, pp. 103-104, italics added). The quote implies that in 1982, I saw little difference among the views of Hayek, Mises, and Robbins. Hutchison uses this quote to suggest that Caldwell (1982) is inconsistent with the another quote from a later paper of mine, "in the field of methodology

Hayek is not a disciple of Mises *and never has been*" (1988a, p. 76, italics added). It is bad enough that Hutchison had to add italics twice to help produce the desired interpretive effect. But worse, his "quotation" from pp. 103-104 is constructed from two different sentences which are separated by over a full page of text! Worst of all, the point I was making in my book is in fact the exact opposite of the one that Hutchison attributes to me. My point was that, although there are many similarities in the views of Robbins, Hayek, and Robbins, *none adhered to Mises' a priorist defense of the basic axioms of economics*. This can be seen from my next sentence, which Hutchison ignores: "But all did not agree with Mises' particular (and perhaps peculiar) vision that economic science is praxeological, that the basic postulates of the discipline are necessary and unquestionable truths about the human condition; that the status of the fundamental axioms is that of synthetic statements that are a priori true" (1982, pp. 104-5). Thus, in Caldwell (1982) I *denied* that Hayek (or, for that matter, Robbins or Frank Knight, who were also mentioned) accepted Mises' a priorism.

The second quotation is also taken out of context. It is the last sentence of a paragraph which deals exclusively with the question of whether Hayek ever accepted Mises' specific variant of a priorism. In that context, my meaning is very clear. When I said that Hayek is not a disciple of Mises in terms of method, I did not mean to imply that Mises had no influence on Hayek. I was simply reiterating the point that Hayek did not accept Mises' a priorism. Hutchison obviously disagrees with this view, which is his right. But rather than developing counterarguments to it, he chose to invent a new position for me, then claim to have caught me in an inconsistency.

Let us move to Caldwell (1984), "Praxeology and Its Critics: An Appraisal." This paper extends the analysis of Mises' views undertaken in *Beyond Positivism*. The paper contains two substantive claims. The first is that the usual attacks against praxeology in the methodological literature, attacks which are based on either logical empiricist or Popperian versions of the philosophy of science, are problematical. One reason that they are problematical is that Mises expressly opposed these philosophies. But more important, my understanding of the current philosophy of science was that it successfully challenged the logical empiricist and Popperian visions. My second substantive claim was that alternative routes to the criticism of praxeology exist. In the last section of the paper, I sketched such an alternative, an internal criticism of Mises' position.

Again the question arises: Why does Hutchison even bother to mention this article? What does it have to do with Hayek and Popper? Actually, Hutchison explicitly cites it only once (in his note 2). In the accompanying text he suggests that Eugene Rotwein's (1986) characterization of the article as a "flirtation with praxeology" is an accurate rendering. Later in his comment, in his discussion of what might have motivated my position, my flirtation with praxeology

comes up again. Hutchison suggests that my hope was to “paper over the gaping cracks of fundamental methodological disagreement between Hayek and Mises since 1937,” and further mentions “Caldwell’s desperate attempts” to keep Hayek in line with the doctrines of the “Modern Austrians,” the term he uses for Israel Kirzner and Murray Rothbard.

Hutchison thus represents me as someone who is trying to portray the Austrians as having closed ranks behind praxeology. In fact, this is the opposite of the position which I took, not only in *Beyond Positivism*, (p. 137, note 45, which begins “The claim that many contemporary Austrians do not embrace the tenets of praxeology is based on conversations I had with a number of Austrian economists ...”) but also in the 1984 article, whose opening reads:

The Austrian approach to methodology has never been monolithic. Two recent studies show that since Menger’s time Austrians have differed, at times dramatically, in their views on methodology (White, 1977; Hutchison 1981 [!]). This diversity continues to be evidenced in the writings of modern day Austrians (1984, p. 363).

Should any questions remain about my motivation, perhaps the following statement will clear them up. For what it is worth, I think that Mises’ a priorist defense of praxeology was a double mistake. My gut feeling is that the doctrine is mistaken epistemologically (though thus far there have been precious few demonstrations, as opposed to assertions, of this in the methodological literature in economics). I also think it was a mistake for so many American Austrians to have spent so much time trying to defend it. When I wrote the 1984 article, I was trying to convince some of the Austrians I had met at NYU to give up their attachment to a priorism. However, my understanding of developments within the philosophy of science led me to believe that the old arguments against a priorism were no longer effective. Taking these problems in philosophy seriously, I attempted to construct an alternative critique of Mises’ position.

We may now move to my two principle works on Hayek, Caldwell (1992): “Hayek the Falsificationist?: A Refutation,” and Caldwell (1988b): “Hayek’s Transformation.” These two began as one paper, but its length grew in subsequent drafts (all of these were sent to Professor Hutchison, hence his opening complaint); I finally decided to split it into two articles. The first is published here for the first time: though accepted for publication in 1986, with 1988 given as the anticipated year of publication, problems encountered by the editor prevented it from being published on time.

The two articles are clearly related. In the first, I dispute Hutchison’s characterization of Hayek’s “U-turn” in the 1930s and of Popper’s subsequent influence on him. In the second, I offer my own version of Hayek’s transformation. Very briefly, I argue there that Hayek changed from an economist who insisted that economics make use of equilibrium theory, to a

broader social theorist who denied that equilibrium theorizing sheds any light on the central problem of the social sciences, that of social coordination. The ideas contained in these two papers form the core of my arguments against Hutchison. I expected that in his comment Hutchison would try to answer the objections I had raised against his version and to attack the accuracy of my alternative account.

Hutchison does, of course, respond to my criticisms of his version in his comment. But remarkably, he refers to my (1988b) paper only once, in section IV, where he leaves the impression that I deny that Hayek’s 1937 “Economics and Knowledge” article contains any criticism of Mises. (Recall Hutchison’s desire to picture me as an apologist for Mises.)

Again, this is a direct misreading. I spend three paragraphs in the middle of the article explicitly discussing how “Economics and Knowledge” marks Hayek’s break with Mises. The first of these paragraphs begins as follows:

Hayek states that the equilibrium of the individual follows a priori from the Pure Logic of Choice. Does this mean that the Hayek of “Economics and Knowledge” was a Misesian? Paradoxically, though Hayek unselfconsciously utilizes the Misesian Pure Logic of Choice in the paper, the case can be made that this article marks Hayek’s first real *break* with his mentor (1988b, p. 528, italics in the original).

Contrary to Hutchison’s incorrect portrayal of my views, I do believe that Hayek expresses disagreement with Mises in “Economics and Knowledge.” Where I differ from Hutchison is over the *grounds* of the disagreement. Briefly put, I believe that even though Hayek repeatedly uses the term “a priori” (even in papers published in the 1940s, I might now add) in characterizing the axioms of economic theory, he never uses the phrase in the way that Mises does. This is hinted at in Hayek’s article when he asserts that it is wrong to try to draw conclusions about social coordination from the axioms, something which Mises felt was permissible. In any case, instead of discussing these legitimate and interesting differences in our interpretations of Hayek’s break with Mises, Hutchison ignores my position and focuses on an unrelated footnote.

The last article cited by Hutchison is Caldwell (1988a), “La Méthodologie de Hayek: Description, Evaluation et Interrogations.” This was a conference piece, written for a colloquium on Hayek’s work held in Montreal in January, 1988. In this paper, a number of episodes in the development of Hayek’s methodological thought are quickly summarized. After each, I offer an evaluation of his position, then note a number of questions which remain in the literature. The stated purpose of the paper is to offer a broad survey of Hayek’s methodological thought and to stimulate interest in Hayekian scholarship. I do *not* provide any extensive justification or defense of the claims I make. Indeed, I twice tell the reader that I am providing an intentionally provocative reading in order to stimulate discussion. None of this comes across

in Hutchison's account. Hutchison latches onto two of my assertions: that little of importance was added to Hayek's methodology after the publication of his "Scientism" (1952) essay, and that at least as much evidence exists for the claim that Popper is a Hayekian as for the claim that Hayek is a Popperian.

Now though these claims are provocative, they are not outrageous: there is evidence for each of them. Much of Hayek's later methodological work concerned the theory of complex phenomena and the use of the compositive method, both of which were anticipated in "Scientism and the Study of Society" (1952). Also, in his social science writings (which Hutchison ignores), Popper advocates the method of situational analysis, which he describes in his autobiography as "an attempt to generalize the method of economic theory [*marginal utility theory*] so as to become applicable to the other theoretical social sciences" (Popper, 1974, p. 93, italics in the original). If I am right that Hayek is a minimalist falsificationist, and if Popper modeled his philosophy of social science on economics, then the claim that Popper is a Hayekian rather than vice versa is at least plausible. Nonetheless, at this point these two assertions are best viewed as bold conjectures, hypotheses to be tested as work in the Hayek and Popper archives proceeds.

I finally must respond to Hutchison's complaint that I somehow misled readers of my (1988a) paper by leaving out a sentence from one of Hayek's reminiscences. Hayek's missing sentence reads, "And that is why ever since his *Logik der Forschung* first came out in 1934, I have been a complete adherent to his [Popper's] general theory of methodology" (Weimer and Palermo, 1982, p. 323). As Hutchison recognizes, the reminiscence is problematical concerning the dates. But surely it is clear from the quotation that Hayek considers himself a Popperian.

In my paper, I list a number of problems which my account of the Hayek-Popper relationship faces. That Hayek claims to be a Popperian is mentioned explicitly in the list, as follows, "Not least of all, Hayek self-reports that he is a Popperian (e.g., Weimer, 1982, p. 323)." I then go on to argue that, *despite* Hayek's claim, his methodological views differ quite dramatically on a number of key issues from those of Popper. These differences are evident in work published by Hayek in the 1940s (even Hutchison admits that the "Scientism" essay of 1942-1944 show a "dualist emphasis"), and I argue that even in his later work, Hayek's views often depart from those of Popper, and that his endorsements of Popper's positions are typically heavily qualified (Hutchison disagrees here).

Why, then, does Hayek represent himself as a Popperian? I use his quote to suggest that Hayek and Popper shared the same enemies (i.e., Marxists and Freudians), so that Hayek simply "embraced his [Popper's] views as a statement of what I was feeling" (Weimer and Palermo, 1982, p. 323). I certainly was not trying to *hide* the fact that Hayek reports himself to be a Popperian. Just the opposite, I was trying to *explain* it, since it poses a problem for my position.

Hutchison's italicizing of the sentence and his remark that "Caldwell apparently just couldn't face quoting this last definitive sentence of Hayek's reminiscence, which is so precise regarding dates" implies, of course, that I was trying to engage in a cover up.

I apologize to the reader for engaging in the tedious exercise of responding at such length to Hutchison's claims concerning the consistency of my views. I felt compelled to do so to protest Hutchison's dissembling account of my position. Legitimate differences in opinion separate us, and I have tried to be clear in my published work as to what those differences are. I wish that Hutchison had focused more of his attention on these differences, and on the arguments which underlie them. Instead, he sought to portray my position as full of ambiguities and inconsistencies, a portrayal which seems to me to border on willful and systematic distortion.

A final point: at the end of his section V, Hutchison lists a number of scholars, all of whom he claims agree with his views. All of them acknowledge that Popper had some general influence on Hayek, and I do, too. But not all of them agree with Hutchison's specific interpretation. Indeed, Gray (1986) spends two pages (pp. 18-19) explicitly criticizing Hutchison's (1981) account. And Boehm (1989), a paper listed in Hutchison's bibliography, includes the following footnote discussion of Hutchison's views of Hayek's "Economics and Knowledge" (1948b).

Hutchison attributes to it the catalytic role of a "U-Turn" in Hayek's evolution of thought, away from an a priori conception of economics grounded in apodictic certainty that he was supposed to have taken over from his mentor, Mises, to a hypothetico-deductive method based on conjectures and refutations in the Popperian spirit. Briefly, there seems to be little warrant for a periodization of Hayek's career in those terms. First, since among the Austrians Mises was the sole advocate of praxeology, there was nothing for Hayek to escape from in that respect; and secondly, in his philosophy of social science Popper is arguably more indebted to Hayek than vice versa (Boehm, 1989, p. 204).

In this brief paragraph, Boehm takes issue with three of Hutchison's claims: on the nature of Hayek's "U-turn," on Hayek's acceptance of Mises' views, and on who gained more in the Popper-Hayek exchange of ideas. It is simply incredible that Hutchison lists Boehm and Gray as among those who agree with him.

III

The reader can certainly be forgiven for losing track by now of who believes what concerning the Hayek-Popper relationship. To clarify matters, here is a brief summary of the points of accord and of disagreement between Hutchison and Caldwell, as I understand them.

1. Both of us agree that Hayek reports to be a Popperian. Both presumably also agree that Hayek's refusal to publicly criticize the views of his friends (these include both Mises and Popper, whose positions are diametrically opposed) makes it quite difficult to discern his true position.

2. Both agree that Hayek's 1937 "Economics and Knowledge" (1948b) contains a veiled rebuke of Mises' ideas.

3. We disagree about the nature of the rebuke. Hutchison claims it was a denial of the two "Wieser-Mises theses": that the axioms of economics are a priori true or irrefutable, and that the natural and social sciences follow different methods. Caldwell claims that Hayek rejected the Misesian belief that the movement from statements concerning individual equilibrium (where a priorist reasoning was applicable) to statements concerning social equilibrium (where questions of expectations became important) was unproblematical.

It should be added that both interpretations face the same difficulty: How are they to make sense of Hayek's apparent adherence to the two "Wieser-Mises theses" into the 1940s, not just in the "Scientism" essay, but also in such pieces as his 1943 paper, "The Facts of the Social Sciences" (1948c)? Hutchison tries to finesse the issue by claiming that no breaks are ever clean breaks (though this, of course, undermines his claim in section III that Hayek experienced a methodological transformation of "extreme sharpness" between 1935 and 1937). Caldwell, who argues that Hayek never followed the Misesian line concerning the a priori status of the axioms, tries to dodge it by claiming that Hayek's usage of such terms as "a priori" is different from that of Mises.

4. Hutchison asserts and Caldwell denies that Hayek's 1937 article marks a methodological "U-Turn" away from Mises and toward Popper. Caldwell offers an alternative account of the change in Hayek's research direction in the paper "Hayek's Transformation" (1988b). Hutchison has not commented on this alternative.

5. Hutchison asserts that there is evidence of an early (meaning post-1937 but prior to the 1950s) Popperian influence on Hayek's methodological writings. Caldwell asserts that no such influence is discernable, and that in fact much of Hayek's early work is incompatible with Popperian principles concerning the unity of science, the importance of empirical work, and so on.

6. Both agree that there was a later Popperian influence. But we disagree about the extent of the influence. For Hutchison, the later Hayek came to accept Popperian principles pretty fully; in any case, nowhere does he identify any differences of opinion separating the two. My position is as follows:

I acknowledge that Hayek accepts two of Popper's ideas: that science follows a hypothetico-deductive method, and that falsifiability provides a demarcation criterion between science and nonscience. This implies that Hayek accepts that the differences between the natural and the social sciences are differences of degree, which is one of Hutchison's key claims. However, if one reads Hayek's writings on complex phenomena, it is clear that he believes the "differences

in degree" to be serious ones, and crucially, that they *concern the ability of the social sciences to produce falsifiable theories*. Thus, though economics is a science (it meets Popper's demarcation criterion), it is also a field for which progress comes at the cost of producing theories which are less falsifiable.

So, is the later Hayek a Popperian, or something else? I focused on Hayek's qualifications and, as a result, I interpreted him as only a minimalist falsificationist. Other interpretations are clearly possible. But I thought it was strange for Hutchison to simply *ignore* the qualifications in his (1981) portrayal of "Hayek II," so I called him on it in section V of "Hayek the Falsificationist?." This exchange has given me no reason to change any of the views expressed in that paper.

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