Journal of Economic Methodology,
Vol. 16, No. 3, September 2009, 315–324

Routledge
Taylor & Francis Group

A skirmish in the Popper Wars: Hutchison versus Caldwell on Hayek, Popper, Mises, and methodology

Bruce Caldwell*

Director, Centre for the History of Political Economy; and Professor, Department of Economics, Duke University

The paper is a reminiscence of T.W. Hutchison by way of a retrospective view of our debate over the relationship between the ideas of Karl Popper, F. A. Hayek, and Ludwig von Mises on methodology. Our dispute was part of a larger debate over the relevance of Popper's thought for economic methodology. Its place within the larger debate is also explored.

Keywords: T.W. Hutchison; F.A. Hayek; Karl Popper; Ludwig von Mises; methodology; a priorism; falsificationism; positivism

The 1992 volume of *Research in the History of Economic Thought and Methodology* carried a paper I wrote titled 'Hayek the Falsificationist? A Refutation', which was followed by a lengthy comment by T.W. Hutchison, to which I added a brief reply (Caldwell 1992a, b; Hutchison 1992). The basic points at dispute had to do with the evolution of F.A. Hayek's methodological thought, and in particular of the influence of the ideas of Ludwig von Mises and Karl Popper on Hayek's position. In his book *The Politics and Philosophy of Economics: Marxians, Keynesians and Austrians* (1981) Hutchison had posited the existence of a Hayek I and a Hayek II in terms of methodology. Hayek I drew heavily on the writings of Friedrich von Wieser (who had been Hayek's major professor) and Ludwig von Mises (who had been his mentor). Hayek II was more closely aligned with Karl Popper's thought. For Hutchison, 1937 was a critical date for marking the change from Hayek I to Hayek II. That was the year that Hayek published 'Economics and Knowledge' (Hayek [1937] 1948), a paper that he later would describe as a 'gentle attempt to persuade Mises to give up the a priori claim' (Hayek 1983, p. 58), and also one that he would later claim was significant for putting him on a new research path.[1]

My position was that Hutchison had misidentified the change that began to appear in Hayek's work following the 1937 paper. I argued that Hayek had never endorsed Mises' a priorism, that his debt to Popper only began to show up in the 1950s, and that even then he had reservations about the role of falsifiability in economics: while acknowledging that it served as a demarcation criterion, Hayek also insisted that progress in fields that study complex phenomena (like economics) typically involves positing theories that are less falsifiable. Furthermore, I thought that the (in my mind, long) process of transformation that had begun with the 1937 paper was mischaracterized by Hutchison. The most important changes in my mind were Hayek's turn away from a reliance on equilibrium theory and toward market process analysis in characterizing markets, and his

*Email: bruce.caldwell@duke.edu

ISSN 1350-178X print/ISSN 1469-9427 online

DOI: 10.1080/13501780903129306
http://www.informaworld.com

transformation from 'narrow economic theorist' to broad social theorist. For those interested in the details of the discussion, I provided a succinct and I hope accurate description of each of our positions, and the evidence each of us adduced for our versions, at the end of my 'Reply' (Caldwell 1992b, pp. 40–41).

An exchange like ours is, of course, the sort of interpretative debate that is the common currency of historians of economic thought. And as is often the case in such matters, there was evidence on both sides. I would further submit that Hayek himself bears a considerable amount of the blame for the profusion of conflicting pieces of evidence. He frequently managed to say things that could support either argument. Thus despite his explicit claim in correspondence with both Hutchison and me that he had never been an a priorist, Hayek also defended the use of a priorism in certain circumstances both in those letters, as well as in publications subsequent to his 1937 paper (see e.g. Hayek [1943] 1948, pp. 67–68; 1952, pp. 165–172). And though there is little evidence of any Popperian influence in his work until the 1950s, and even then there remains considerable question as to exactly what Hayek's debt may have been to Popper (and vice versa, on which see Caldwell (2006)), one must still square this with Hayek's claim that ever since reading Popper's *Logic of Scientific Discovery* in 1934 he had been a 'complete adherent' of Popper's methodology (Hayek 1982, p. 323). I continue to believe in the position that I defended, but for those of you who might not feel that that is a compelling reason for you to do so, the best I can do is simply to refer you to the debate itself, so that you may make your own judgment.

If you do so, however, you may be somewhat taken aback by the vehemence of the final exchange. There are accusations of selective quotations, of willful misreading of texts, of sloppy work, of inconsistencies, of hidden agendas. I have never, before or since, been involved in such an intense and vitriolic back and forth. And I must admit that while it is difficult and even somewhat embarrassing to reread the papers after all these years, at the same time doing so brings back some of the intense feelings that I can remember having had at the time the exchange took place.

In this paper I will trace how I think it happened that we came to write such impassioned lines. Evidently I am telling only one side of the story, and given that it is a story of a fairly bombastic academic encounter, some may question my ability to provide an accurate accounting. But I figure that one side is better than none, and that in any event the reader will make up her own mind as to the fairness of my presentation. And given that in the end the whole affair appears to have been simply a minor skirmish in the larger Popper wars, perhaps it will suffice.

Prelude – a harbinger at the Homestead

I first encountered the work of T.W. Hutchison while I was working on my dissertation, a manuscript that I finished in fall 1978 and which carried the title 'Recent Methodological Thought in Economics: Survey and Critique from a Philosophy of Science Perspective'. Hutchison was unique among the protagonists whose ideas were examined in that he was involved in not one but two methodological debates. There was his famous one with Fritz Machlup in the pages of the *Southern Economic Journal* in the 1950s that John Hart will appraise. But it also seemed to me that his 1938 book, *The Significance and Basic Postulates of Economic Theory* ([1938] 1960), though not explicitly naming Lionel Robbins as a target, could be read as a critique of the views that Robbins had expressed in his own *An Essay on the Nature and Significance of Economic Science* (1935).

The reader can well imagine my delight when I had the opportunity to meet Hutchison in October 1979. The venue was a Liberty Fund Symposium held at the Homestead Resort in Virginia and titled 'The Methodology of the Social Sciences and the Future of Freedom: An Exploration into Basic Issues'. On the first night Hutchison's then recently published book, *Knowledge and Ignorance in Economics* (1977), was taken up for discussion. David Levy and Larry Moss were his discussants, and they offered a number of criticisms of the book (though I also recall that Moss included some magic tricks along with his comment, which lightened the mood somewhat). When they were done Hutchison responded. My dissertation advisor Vincent Tarascio was there, and when Hutchison was finished Vince commented to me *sotto voce* about how deftly and effectively Hutchison had disarmed his interlocutors with his self-effacing humor and qualifying remarks – 'He is a master – look how their criticisms run off of him like water off a duck's back'.[2]

The next day after lunch I spotted Hutchison going for a walk and I asked if I might accompany him. He graciously invited me along. I remember two things from that walk along the Homestead golf course. First, he responded to my inquiry about whether his 1938 book was aimed at Robbins with a smile and the words, 'of course'. That was heartening. Second, he was an exceedingly strenuous walker – by walk's end I was completely worn out by a man 40 years my senior. Hutchison and I were on the same plane to the Washington DC airport following the flight, and he inscribed my copy of his book as a parting gesture.

Hutchison in *Beyond Positivism*

In 1982 I published *Beyond Positivism: Economic Methodology in the Twentieth Century* (Caldwell [1982] 1994), a book based on my dissertation. One of my major findings was that 'positivism' – a term that I used loosely to denote a number of doctrines within twentieth-century philosophy of science, including logical positivism, logical empiricism, operationalism, and Popper's falsificationism – had been thoroughly and effectively criticized by philosophers.[3] I drew from this the straightforward conclusion that the frequent invocation of falsificationist principles by methodologists of economics had been illegitimate. This critique put me in direct conflict with two prominent advocates of Popper's thought, Mark Blaug and Terence Hutchison.

Two chapters of *Beyond Positivism* were devoted to a detailed discussion of Hutchison's work. In 'Robbins versus Hutchison – The Introduction of Positivism in Economic Methodology', I credited Hutchison with introducing positivist themes into methodological discourse in economics (a rather dubious honor in a book titled *Beyond Positivism,* to be sure), but also argued that he had made a mistake in his claim that the propositions of pure theory necessarily lacked empirical content. The second half of the chapter contained a section on Austrian methodology, and there I argued that results within the philosophy of science had eviscerated many of the usual falsificationist criticisms of the Austrians, and I then made the case for alternative forms of criticism. In the next chapter, titled 'Hutchison versus Machlup – On Indirect Testing and the Rationality Postulate', I argued that Machlup's emphasis on indirect testing seemed more in line with contemporaneous philosophy of science than did Hutchison's 'ultra-empiricism'.

Given the amount of criticism he came in for in my book, and my evocation of a boxing match in my chapter titles, Hutchison may well have thought that I was out to get him. If he didn't then, he almost certainly would a little later, when I challenged his interpretation of what he called Hayek's methodological 'U-turn' (Hutchison 1981, p. 215).

Hayek the falsificationist?

I had finished writing *Beyond Positivism* while I was on a post-doc at NYU, and about a month before I left to return to North Carolina, Jerry O'Driscoll put Hutchison's 1981 book into my hands. The Austrians at NYU and I had been talking a lot about methodology that past year, though Mises had been the principal focus, not Hayek. Even so, it seemed strange to me that anyone could go from being a Misesian to being a Popperian (the two views were just too far apart), but that is what Hutchison had claimed had happened to Hayek. So once I got back I began working on a paper on the subject. I completed 'Hayek the Falsificationist? A Rebuttal to Hutchison on Hayek's Methodology' in spring of 1983, and sent Hutchison a copy of it in May.

I received two handwritten letters in reply. The first, dated 15 May 1983, was three pages (six sides) long. In the first part of the letter, he offered some general comments on *Beyond Positivism*, a copy of which I had had sent to him on its publication. In the course of his comments, he offered some interesting insights into the rationale for his own views. The reader should note that Hutchison's own lines about the 'unjustifiably pretentious claims' of 'pseudo-Keynesians' could very easily have been written by Hayek.

> Yes: I favour some *fairly* strict code of falsificationism. This is based on a *political* value-judgement as to the kind of scientific community I think economists should comprise. Of course, I can only state my own values and suggest likely consequences of their being neglected. To some extent you are right in saying that they stem from the war, or, rather, the experience of Nazism. But they have been reinforced by the performance of (especially) British economists, mostly pseudo-Keynesians, in the '50s and '60s. My book of 1968 was regarded as an amusing polemic. But it was primarily intended by me as a serious methodological tract. By quite unjustifiably pretentious claims, many economists were claiming an influence over policy, and political power, to which, on any scientific or disciplined basis, they are quite unentitled.
>
> Just because I think economists *might* be entitled to exercise, and might *beneficially* exercise, a *much* more modest influence, I would like to see a code of discipline widely observed by economists.
>
> (Almost) my sole criticism of your book was that you attacked the discipline of falsificationism without putting any other in its place. In one of your papers you say you do not espouse 'Anything Goes.' Good. But what precisely does *not* 'go' – in your book? I don't find it easy to say. I also believe that my political values, or a thorough-going libertarianism, *require* a fallibilist, anti-dogmatic *methodology*, or *epistemology*. This is an essential component of Popperian falsificationism which provides the necessary philosophical basis for opposing dogmatic *in*fallibilism ('apodictic certainties' etc. etc.).

He did not like my paper on 'Hayek the Falsificationist'. And this time there were few qualifying remarks: he felt he had been totally misrepresented.

> ... your paper on Hayek is to me completely unacceptable. You again and again attribute words to me which I have never used and would not want to use.
>
> You repeatedly stretch my words unacceptably in order to have something to shoot at.
>
> I quoted Hayek at some length: you quote very little of *my* actual words – hardly any.
>
> ... I nowhere contend, or state, that '*Hayek's new direction is best characterized as falsificationist*'. I *reject* this formulation which you have invented for me!
>
> Surely you must see that there is a crucial difference between such propositions as these and simply saying that certain papers of Hayek 'show a marked Popperian influence,' or that he 'also puts great emphasis on falsifiability'. Incidentally, why did Hayek dedicate his 1967 volume to Popper? (Letter, Hutchison to Caldwell, 15 May 1983)[4]

Hutchison included with his own a copy of a letter he had received from Hayek in 1981. He sent the copy of the letter because he felt that it confirmed his claim that Mises and Hayek differed on methodology. (I felt that it confirmed my own view that Hayek had never been a Misesian!) In any event, Hutchison wrote on the top of the copy in bold red ink: 'Private Letter – Not for Quotation or Citation in any Form!' Now that both Hayek and Hutchison are dead, and that the letters are available to scholars in Hayek's archives, I feel that prohibition is no longer reasonable: the letter is reprinted in the Appendix to this article.[5]

Only five days later, on 20 May 1983, Hutchison sent me a second letter. The occasion for writing so soon was that he had himself just received another letter from Hayek, again commenting on Hutchison's treatment of him in the 1981 book. (This one also appears in the Appendix.) If one compares the two letters, it looks like Hayek may have forgotten some of what he had written in his 1981 letter, for he repeats that he was in 'Economics and Knowledge' gently trying to tell Mises that he did not accept his a priorism. On this point – that the 1936 piece was a gentle rebuke of Misesian a priorism – Hutchison and I agreed. Where we disagreed was whether Hayek had *ever* (that is, prior to 1936) accepted a priorism. Hutchison claimed that Hayek's writings revealed a Misesian–Wieserian influence in terms of methodology. I read Hayek's statement in his first letter '*I* was *never* an a priorist' as a clear denial of it. In retrospect, it seems clear that we both could have been right: there could have been some influence not having to do with a priorism.

Anyway, in June 1983 I responded to Hutchison's two letters with a lengthy one of my own. I will quote here the parts of it that I think get at some significant differences between us:

> [The] paper was due to my sincere belief that you misread Hayek, that though Hayek was greatly influenced by Popper, he takes a substantially different approach to methodology than does Popper. … It may well be true that I overstated my case and [I] intend to read through all of the texts very carefully once more before beginning my next draft. If I have misquoted you, I assure you I will change any such references.
>
> … I do believe, sincerely, that we are all Popperians now. But I see the distinctions between what parts of Popper I would accept and what parts Hayek, Boland, Blaug, Hutchison, Friedman, etc. etc. would accept as crucial. This is the key to our disagreement, I think. Little is gained by my saying that the differences are crucial, and others saying that they are matters of degree. What is needed, I think, is a careful categorizing of the differences. (Letter, Caldwell to Hutchison, 1 June 1983)

I believe that the two portions of the letters quoted above do much to explain why our final exchange was so vitriolic. Hutchison felt that I had misrepresented him, that in an attempt to get recognition for myself I was taking unfair potshots at him. As I had promised him, I went back and reread all of the texts before I wrote my next draft. I found that Hutchison's text was indeed full of qualifying remarks.[6] As it had been for his discussants at the Homestead, it was hard to pin him down.

For my part, it was very important to get things straight. As I noted in my letter, a number of different economists were talking about Popper in the early 1980s, but each seemed to take different things from him.[7] And it also seemed to me that it was precisely those places where Hayek differed from Popper that were of most crucial importance for understanding the limits of economics. The limits of economics would become a major theme in my later work on Hayek, so I did not want to let the problem go.

The paper went through a variety of drafts, and at one point in 1984 I even sent a copy to Hayek. I was delighted when he responded by thanking me for clearing up 'Hutchison's misunderstandings'! (Hayek's letter to me is included in the Appendix.) I sent a copy of Hayek's letter to Hutchison, who responded by pointing out, quite correctly, that Hayek was not particularly clear about exactly what misunderstanding I had supposedly cleared up.[8] So much for going to the horse's mouth for clarification!

In any event, the paper was finally accepted for publication in 1986, with an expected publication date of 1988. But that was not to be. Warren Samuels, the editor of *Research in the History of Economic Thought and Methodology* where 'Hayek the Falsificationist' was to be published, had sent letters to both Hutchison and Hayek asking for their comments on the paper. Hutchison said he would wait until Hayek's had come in, but Hayek never replied.

The paper got filed away, and that, together with various other publication difficulties too tedious to mention, explains why a paper accepted in 1986 did not appear until 1992.

The rapid denouement of the Popper wars

Meanwhile, things were moving fast in the methodology community. Throughout the 1980s debates about the applicability of Popper's falsificationist methodology for economics had increased in number and intensity. In a key paper, Wade Hands argued that there were actually two Poppers. Popper$_n$ was the falsificationist, the well-known Popper of bold conjectures and severe tests, who wrote with the natural sciences (hence the n) in mind. The less well-known Popper$_s$ wrote with the social sciences in mind. Popper$_s$ employed the method of situational logic and the rationality principle in explaining social phenomena. Crucially, there existed a tension, if not an outright inconsistency, in the prescriptions offered by the two Poppers:

> The fact remains that the rationality principle is not actually subjected to the severe testing necessary to demarcate scientific theories from metaphysical theories. Thus, by strictly Popperian standards, explanations which include the rationality principle (necessary or not) are as close to metaphysical explanations as they are to scientific explanations. (Hands 1985, pp. 88–89)

Hands' article implied that economists had been borrowing from the wrong Popper. Even though this argument drew a nearly immediate response from Mark Blaug (Blaug 1985), it was clear that the traditional defenders of Popper were on the defensive.

In December 1985 there was a conference in Amsterdam to honor the retirement of the Dutch methodologist Joop Klant, and papers critical of Popper were in evidence – though there was also one by Hutchison, aptly titled 'The Case for Falsification' (Hutchison 1988, in De Marchi 1988). New voices arguing for new directions were also heard at the conference: for example, Deirdre McCloskey and Arjo Klamer, for whom the standard debates were quite beside the point, argued forcefully that more careful attention be paid to rhetoric.[9] As the decade turned, criticisms of falsificationism multiplied (e.g. Caldwell 1991; Hands 1993; Hausman 1992; Mäki 1993). The title of a collection of readings edited by Neil De Marchi indicates the direction in which the tide was turning: *Post-Popperian Methodology of Economics: Recovering Practice* (De Marchi 1992).

In short, by the time my exchange with Hutchison came out, the discourse had already begun rather radically to change. The research annual in which our papers were published did not have the circulation of the new journals that had begun springing up in the 1980s, so few would have even read it, and perhaps just as well. In the larger scheme of things, it was a minor engagement, little more than a skirmish, at the end of the Popper wars.

The debt I owe to T.W. Hutchison

Still, it is hard not to regret the exchange. This is not only because both of us come off rather badly, which we do. I also regret it because it obscures the real areas of agreement that actually exist between Terence Hutchison, Friedrich Hayek and me regarding the ubiquity of ignorance in economics, and of the resultant modest role that is to be played by economists. In our views on the limits of economic science, both Hutchison and Hayek made arguments that I completely accept.

If one were to try to get something positive out of the episode, perhaps the whole debate can be thought of as a case study on the difficulties of historical interpretation. I was naive to think that the task of disentangling what men like Popper and Hayek really thought about certain issues, or to assess who might have influenced whom, could be accomplished without too much controversy. There are few brute facts in intellectual history, and all of them are always open to alternative interpretations. The letters from Hayek that are included in the Appendix are a testimony to that.

One thing that is very clear to me is that my exchange with Terence Hutchison played a critical role in determining the direction of my own research over the ensuing decades. Responding to Hutchison's book is what started me on Hayek, and ultimately led me to consider writing a book about the methodological thought of the Austrian economist. In addition, wrestling with Hutchison and with Mark Blaug about Popper led me to try to clarify exactly what Popper had to offer economists (Caldwell 1991), in the course of which I began to correspond with Bill Bartley, who was to have been both Popper's and Hayek's biographer. This led me to Stephen Kresge, Bartley's successor as the General Editor of *The Collected Works of F.A. Hayek*. Soon thereafter I became an editor of two of the volumes in the series, and ultimately succeeded Kresge as the General Editor.

So I will here offer my belated but heartfelt thanks to T.W. Hutchison, a man whose work more than anyone else's set me on a new research path, a person with whom I sometimes disagreed but also shared much common ground. On those issues on which we did disagree, he was a worthy adversary if ever there was one.

As a final tribute I offer here some notes on Hutchison that I composed soon after I attended the Workshop in Political Economy at Duke University on 26 March 1984. Hutchison gave a paper there on John Stuart Mill's methodology, one which challenged claims that had been made by the Workshop host, Neil De Marchi, in an earlier paper.

> He is broad and rectangular, not thin but *solid*, and in general appearance looks most like Jim Buchanan.
>
> His full hair is gray, and slicked down – giving him a bit of a schoolboy look, since his brown suit looks a little small on him and he not entirely comfortable in it.
>
> He is tan, wears black horn-rimmed glasses for the presentation, which he often removes and closes up when looking up to make a point.
>
> There is a slight shake in his hands, and under one eye he has some dark spots, like little warts, like my sixth grade teacher Miss Weser – these are the most notable signs of age. He walks somewhat stiffly, his upper body not moving – he turns from the waist rather than turning his head.
>
> He is a fascinating speaker. His voice is resonant, it goes up and down, speeds and slows (like Lachmann), to make his case. In answering De Marchi, he will repeat De Marchi's case, say yes, perhaps this could be, and maybe this could be, about six times on each point – to say, of *course*, we *cannot rule out* such a *possibility*, no matter how *remote*, but, respectfully, I rather think *not*. He is delightful. Neil can see what he is doing of course and smiles throughout. How enjoyable.
>
> I take my leave afterwards. He is a great man.

Notes

1. '… 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" …' (Hayek [1964] 1967, p. 91).
2. Some of the others who attended the symposium were Jim Buchanan, Geoff Brennan, Brian Loasby, Paul Heyne, Karen Vaughn, Roger Garrison, Stephen Littlechild and E.G. West. As a newly minted PhD, I had no idea how esteemed the group was that had been assembled that October at the Homestead.
3. Some people dislike the term 'falsificationism', but it was such a convenient label for Popper's position that it became widely used in the methodological literature in economics, as in, e.g. Blaug (1980, pp. 10ff.); Hands (1985, pp. 83 ff.).
4. I thank Robert Hutchison for granting permission to quote from his father's correspondence.

5. All letters from F.A. Hayek appear with the permission of the Hayek estate. As he had requested, I did not quote either of Hayek's two letters to Hutchison during our published exchanges.
6. For this reason, the opening footnote of my 1992a paper reads, '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 writing, 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 in both 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'.
7. One of these, Larry Boland, thought no economists had gotten Popper right: in Boland (1982, pp. 172–173), for example, he labeled Blaug's approach 'Conventionalist Pseudo-Popper'!
8. This was why I never cited Hayek's statement about my clearing up Hutchison's misunderstandings in my published exchange with Hutchison.
9. I hope that Deirdre will not find it too catty if I share here my comment to Hutchison in a letter that I sent to him before the conference: 'I thought it was nice of Neil to invite McCloskey – that way, if our disagreements become too vehement, we can be brought back to order when he starts in on rhetoric – the old common enemy routine!' Caldwell to Hutchison, 27 March 1985.

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Appendix. Letters from F.A. Hayek to T.W. Hutchison and B. Caldwell

November 26, 1981

Dear Hutchison,

I had for some time intended to write to tell you how much I liked and admired your *Politics and Philosophy of Economics* but too much travel and trying to finish a major book seem to prevent my ever studying it systematically from cover to cover as I wished to do at first. So after a half an hour spent on some to me specially interesting parts I want to express at least the main point. You are of course perfectly right that my 1936 lecture on 'Economics and Knowledge' marks the beginning of my independent thinking as an economist. But its chief point I had seen a little earlier and you will find some of the basic ideas already in my essays in *Collectivist Economic Planning*. But the main intention of my lecture was to explain gently to Mises why I could not accept his a priorism. Curiously enough, Mises, who did not readily accept criticism from his juniors, accepted my argument but insisted it was not incompatible with his view which, by implication, he restricted to what I called the Logic of Choice or the Economic Calculus. I left it at that, but I did want to say that *I* was *never* an a priorist, though I would still insist that *part* of the essential knowledge of the economist or the social theorist generally is derived from his given familiarity with the processes of human thinking.

With best regards, sincerely,
F.A. Hayek

May 15, 1983

Dear Hutchison,

I have recently at last been able to read at least the sections of your new book dealing with me and had intended to write. I have however stupidly lent it to a student before doing so and must now try to write before my memory fades completely, but without being able to refer to the text. There is some misunderstanding in the emphasis on the complete break in my thinking, although I may be partly responsible for it. I had never accepted Mises' a priorism and when I speak of man's knowledge of his own thinking I mean of course only our knowledge of the general manner in which men think, not knowledge of what they know or think at a particular moment. This I still believe to be true. My 1936 essay was written and the lecture delivered before I knew anything about Popper and the reference to 'falsification' was inserted in the proofs. As it is evident from certain passages in my essays on *Collectivist Economic Planning*, I had arrived at a definite hypothetico-deductive view before I knew of Popper's existence, but of course then embraced the philosophical justification of it which I could never have provided. Popper's influence on me was great on the question of the methods of the natural sciences – that they did not do what most of the physicists claimed they did and urged us to imitate. You will probably find more evidence of my pre-Popper thinking already in my 1933

Inaugural lecture in *Economica* of which I have at the moment however not even a copy at hand – I never reprinted it, because I was aware that some parts of it were muddled. Certainly 1936 was the time when I first saw my distinctive approach in full clarity – but at the time I felt it that I was merely at last able to say clearly what I had always believed – and to explain gently to Mises why I could not ACCEPT HIS A PRIORISM. Curiously enough, Mises, who was so sensitive to criticism from younger men, praised that article without seeming to be fully aware how much it was in conflict with his views.

There was yet another much less important point which I wanted to take up with you, but which I do not now remember but which I may rediscover, when (if I ever) I get the book back. Perhaps there may still occur an opportunity to discuss these questions.

With best regards, sincerely yours,
F.A. Hayek

29 September 1984

Dear Professor Caldwell

Although your kind letter of July 10 was promptly forwarded to me at my vacation address, the accompanying typescript had in accordance with my instructions to await my return which has been unusually delayed. I have now at last been able to read your article and greatly enjoyed it and am very grateful to you for clearing up Professor Hutchison's misunderstandings. I entirely agree with you. Though I very gratefully accepted Popper's justification of the hypothetico-deductive procedure which I have long been following but could not have adequately justified – I am not a trained philosopher – I am sure he had no influence on my method in economics – certainly not yet in 1936 when I had only recently become acquainted with his *Logik der Forschung*. Indeed I clearly remember that I only at the last moment remembered that 'verification' was no longer adequate and I believe only in the proofs inserted the footnote in which I refer for the first time to Popper, whom at that time I did not know personally. I am most grateful to him for having made clear to me that the natural scientists did not really follow the inductive methods which they claimed to observe and urged us to imitate, and I have much sympathy with his recent distinction of the 'three worlds,' though cannot quite accept it. We became close friends in the few years we were together at the London School of Economics, but I would not wish to say which of the two of us had had the greater influence on the other (Not for quotation!)

I still have not read your book though I am expecting the copy which I bought last week in London.

I assume you have already noticed yourself that on the sixth page of your typescript, line three, 'theoretical' ought to be 'philosophical,' and that the emphasis mentioned as added in the last line is in fact missing.

With repeated thanks, very sincerely yours,
F.A. Hayek