

CAHIERS D'ÉPISTÉMOLOGIE

Publications du
Groupe de Recherche en Épistémologie Comparée
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Hayek's Popperian Critique of the Keynesian Methodology

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Cahier n° 9707

237^{ième} numéro



Université du Québec à Montréal

<http://www.philo.uqam.ca>

"S'il devait un jour n'être plus possible pour les observateurs scientifiques de s'entendre au sujet des énoncés de base, cela équivaudrait à l'échec du langage comme moyen de communication universel. Cela équivaudrait à une nouvelle "Tour de Babel", la découverte scientifique s'en trouverait réduite à une absurdité. Dans cette nouvelle Babel le haut édifice de la science tomberait bientôt en ruines."

Karl Popper, *La Logique de la découverte scientifique*, section 29.

"But, even if there is agreement about 'basic' statements, if there is no agreement whatsoever about how to appraise scientific achievement relative to this 'empirical basis', would not the soaring edifice of science equally soon lie in ruins? No doubt it would."

Imre Lakatos, "Popper on Demarcation and Induction", in *The Methodology of Scientific Research Programmes*, p.145.

Couverture: La Tour de Babel (Bruegel, 1563)
Bureau des graphistes de l'UQAM

Cette publication, la deux cent trente-septième de la série, a été rendue possible grâce à la contribution financière du *Fonds pour la Formation de Chercheurs et l'Aide à la Recherche* du Québec ainsi que du *Programme d'Aide à la Recherche et à la Création* de l'UQAM.

Tirage: 50 exemplaires

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Dépôt légal - 2^e trimestre 1997
Bibliothèque Nationale du Québec
Bibliothèque Nationale du Canada
ISSN 0228-7080
ISBN 2-89449-037-2

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This paper was prepared for the 1997 meeting of the *History of Economics Society* to be held at the College of Charleston (Charleston, South Carolina, USA) June 20-23, 1997. The session will be chaired by Prof. Paul B. Trescott (Southern Illinois University, Carbondale, IL) and the commentators will be Prof. James C.W. Ahikpor (California State University, Hayward, CA), Prof. Michael S. Lawlor (Wake Forest University, Winston-Salem, NC), and Prof. Maria Cristina Marcuzzo (Universita La Sapienza, Roma).

Those who are interested in both J.M. Keynes and F.A. Hayek have found the last several years to be fertile ones for research. Indeed, the number of publications using various approaches to analyze, discuss and closely examine the works of each has been very large. Moreover, we now possess a considerable store of historical research results concerning the relations between these two giants of economic thought. This paper is concerned with appraising the methodological dimension of the Keynes/Hayek intellectual relationship.

As Bruce Caldwell has recently made clear, one of the important reasons given by Hayek for not endorsing the Keynesian theoretical vision (a doctrine with roots in the *General Theory* but which goes beyond it to become a school) is of a methodological nature. For this reason, it would be a mistake to claim that Hayek's and Keynes's ideas generally converged, even if though it remains the case that they come together at a number of points.¹

I would nonetheless like to show that taking this divergence into account forces us to reopen the debate which Bruce Caldwell and Terence Hutchison² held a few years ago over the meaning of and the reasons for what Caldwell has called the "transformation of Hayek"³. The reason for this is that we must admit, regardless of what Caldwell says, that the methodology employed by Hayek to oppose Keynesianism is from one end to the other inspired by Karl Popper's philosophy of science, that is to say, by anti-inductivist falsificationism. Or at least, this is what I hope to establish.

But before criticizing Caldwell's interpretation, we first ought to thank him. Obviously, one cannot ignore the fact that Bruce Caldwell has explicitly called our attention to the important divergence between the methodological conceptions of Hayek and of Keynes.⁴ In fact, this is surely one of the important and interesting elements of his recent contribution to the history of the intellectual relationship between Keynes and Hayek.

¹ See Dostaler 1990, 1991 and 1996.

² See Caldwell 1992a and 1992b, and Hutchison 1992.

³ See Caldwell 1988.

⁴ See Caldwell 1995.

Caldwell's insight is that certain formal characteristics of the *General Theory* greatly contributed to its rapid adoption by the economic profession as the theory of the day (Caldwell 1995, p. 33). Right away, the "static comparative" framework adopted by Keynes seemed very rigorous and well-defined to the rising generation of economists, whereas Hayek's theory had a more dynamic feel and seemed immune, unlike Keynes's, to all mathematical treatment.

Quoting "The Hayek Story" by John Hicks, Caldwell reminds us that during the thirties, many attempts were made to translate the Hayekian analysis into mathematical language, but always in vain (Caldwell 1995, p. 33, n. 67). By way of contrast, Hicks had no trouble mathematizing Keynes's model, which he did as early as 1937 in an article for *Econometrica*.⁵

However, I believe that his approach to this fundamental divergence of view between Keynes and Hayek causes Caldwell to contribute, for all practical purposes, not so much to rendering it a commonplace idea but rather to a misappraisal of the ins and outs of Hayek's own work. Caldwell's objective is to provide a satisfactory response to the following question: "Why, after all, did Hayek never write a review or a critical appraisal of Keynes's *General Theory*?" In the course of his attempt to provide a rigorously documented and systematically argued response to this question, Caldwell advances four reasons which might explain the situation. Of these, only the fourth and last is of a methodological nature, properly speaking.⁶ Caldwell writes indeed:

Hayek disagreed with Keynes on both theory and policy. But it was Keynes's methodological approach, specifically his use of aggregates, that Hayek came to view in retrospect as being his opponent's most dangerous contribution. ... Aggregates mask the movement of relative prices, and relative price movements are the central foci of Austrian theory. (Caldwell 1995, pp. 42-43.)

⁵ See Hicks 1937.

⁶ Caldwell suggests, first, that Hayek might have feared that Keynes would change his mind, and that his critique would therefore fall flat. Second, Hayek, who was just coming out of a lively polemical debate with the socialists as well as with Piero Sraffa, might have thought it was time he turned toward more essential tasks. And finally, third, by once more attacking Keynes, Hayek feared, according to Caldwell, that he might lose some support in his political and ideological fight against the advocates of the Welfare State.

So Caldwell identifies in a way that is both pertinent and to the point a general critique which Hayek addressed to the Keynesian approach. But, *pace* Caldwell, it is difficult to imagine this critique as providing Hayek with a motive for refusing to write a critical study of the *General Theory*.⁷ After all, to Caldwell's question, " Why did Hayek fail to write a review of *The General Theory*?", one can hardly respond: "Because Hayek disapproved of Keynes on a crucial methodological point, that is, on the reference to statistical aggregates." This disapproval should, on the contrary, have strongly inclined Hayek towards writing such a review.

Let us allow that Caldwell doesn't really believe that the methodological motive alleged by Hayek, and to which I shall return below, is the main reason for Hayek's abstention, for he adds: "What doesn't ring quite true in Hayek's claim is that he was vaguely becoming aware of this difference over methodology in the 1930s", since according to Caldwell "[the] opposition to the use of statistical aggregates had long been a methodological principle among Austrians" (Caldwell 1995, p. 43). The really methodological argument, of which Hayek was just discovering the importance in the mid-thirties, was, Caldwell thinks, that he "began to lose faith in the 'equilibrium theory' portrayal of the market mechanism" (Caldwell 1995, p. 43). It is this point which Caldwell argues and documents in detail in his 1988 article entitled "Hayek's Transformation". Without wanting at this time to contest Caldwell's well-handled analysis of this point, I would nonetheless like to show that opposition to the quantitative or statistical (in fact, econometric) approach was more important to Hayek's negative evaluation of the Keynesian doctrine than Caldwell wants to admit.

I need first to render more credible the methodological motive which Hayek offers against the very idea of reviewing Keynes's *General Theory*, a motive which Caldwell first accepts, but then rejects as unwarranted. I conjecture for my part that one ought to see in this motive an important — indeed, key — element in the turn made by Hayek in the 1930s. I wish to emphasize that this methodological argument operates in and is crucial to Hayek's critical

⁷ We know that, in a letter to Keynes, Hayek assured himself that his remarks and requests for clarification would receive a positive response if written for publication in the *Economic Journal*. See Caldwell 1995, p. 40.

rejection of *The General Theory*. More than that, I wish to argue, against Caldwell in particular, that this methodological argument is proof of the dependence of Hayek's thought on Karl Popper's falsificationist philosophy, since one can clearly establish that the methodological critique Hayek advances against the Keynesian doctrine is from beginning to end coloured by Popperian anti-verificationism and anti-inductivism.

Let us be clear from the start that the fact that Hayek never published a formal review of Keynes's *General Theory* does not mean that one cannot find a critique of Keynesianism in his work. It is a matter of knowing where to look. For example, one can find the main lines of this critique in Hayek's Nobel Prize Lecture of December 1974. Whereas, in 1936, when *The General Theory* was published, Hayek was more concerned with restructuring his ideas than with writing a review of this monumental work, the situation was very different in 1974. "Keynesian politics" had clearly started to take off and to produce their perverse effects — effects anticipated by Hayek some forty years earlier. But it is patently clear that the summarily presented critique of 1974, in a speech intended for a specific occasion, connects with some of Hayek's most basic lines of thought. It brings out, among others, the main points of the arguments formulated in a series of articles dating back to 1942-1944.⁸ Indeed, it touches on the main lines of what Hayek considered central and unavoidable epistemological concerns from the mid-1930s on, and which were first articulated in "Economics and Knowledge".⁹

The struggle against what Hayek called "the scientistic prejudice" in the social sciences constituted for him the most central epistemological and methodological task. This is no doubt why Hayek make it the theme of his "Nobel Memorial Lecture" (Stockholm, 11 December, 1974), which was published under the title "The Pretence of Knowledge" (Hayek 1974). In this lecture, far from wanting to diminish the genuine merits of authentic scientific work, Hayek right away submits for our consideration the idea that one must protect the reputation of science by refusing to be taken in by a mere pastiche masquerading as genuine scientific knowledge. From

⁸ "Science and the Study of Society", Part I, *Economica*, n.s., 9:35 (1942), pp. 267-291; Part II: 10:37 (1943), pp. 34-63; Part III: 11:41 (1944), pp. 27-54. Reprinted as part I of Hayek 1952.

⁹ The text published in 1937 (*Economica* 4:13 pp. 33-54; reprinted in Hayek 1948, pp. 33-56) was presented for the first time as a paper in November 1936 (See Caldwell 1992a, p. 5).

this point of view it is extremely important, according to Hayek, to clearly point out the fundamental difference between the kind of knowledge provided by the physical sciences and the much more modest kind which a social science like Economics is able to provide.

Hayek is even more radical than this: he goes so far as to claim that it is impossible to begin building an adequate body of knowledge in the social sciences, and in particular in Economics, without first having taken a stand on matters of general epistemology, that is, without having laid the foundation for a general as well as scientific theory of knowledge, one which identifies the inherent limitations of each kind of knowledge. Considering this stand, it is not surprising that Hayek should have been so extremely interested in the ideas of Karl Popper, and this from the moment that they become known.¹⁰ After all, for Hayek it was not only necessary that epistemological questions be explicitly integrated into the theoretical realm of preoccupation peculiar to the social sciences; Hayek was, moreover, convinced that Popper had managed to provide us with a criterion (Hayek says a "test") permitting the demarcation of theoretical statements that could be considered genuinely scientific from those that could not be.¹¹

Hayek's primordial methodological objective in his Nobel Prize acceptance speech was to explain the bitter and indisputable failure, symptomatic of economists, to come up with scientific theories that would guide the action of economic agents in such a way that this action would not be socially deleterious and would not, for example, create a situation of combined inflation and unemployment (stagflation). According to Hayek, this clear failure on the part of economists is the direct result of their stubborn insistence on imitating at all costs the methods that have been so spectacularly successful in the natural sciences. This failure is thus the consequence of a scientific prejudice on the part of theoreticians of economic science.¹²

Even though this argument was rejected by Popper (for whom scientism is more the imitation of the method one *thinks is* that of the natural sciences), it nonetheless seems that at the

¹⁰ Popper's *Logik der Forshung* was published in Vienna in December 1934 but with the copyright year 1935. Hayek got acquainted with this book in 1935.

¹¹ This affirmation is located at the heart of the Nobel Prize Lecture. See Hayek 1974, p. 274.

¹² "It seems to me that this failure of the economists to guide policy more successfully is closely connected with their propensity to imitate as closely as possible the procedures of the brilliantly successful physical sciences — and attempt which in our field may lead to outright error." (Hayek 1974, p. 266)

level of scientific methodology we can see here an area of agreement between Hayek and Popper. Indeed, we know that for Popper the idea that theoretical science aims definitively to confirm (i.e., to verify) the laws of nature stems from a poor understanding of the kind of knowledge science really makes available to us. Here Hayek seems to be following in Popper's footsteps, at least in the social sciences, since he also rejects the confirmationist objectives of his contemporary economist fellows. The latter prefer, if we are to believe Hayek, a quantifiably verifiable but inadequate theory to a theory that might well be true but which is not, as such, statistically verifiable. Hayek claims that when it comes to specific problems such as inflation and unemployment, problems which different economic theories approach in radically different ways and for which they provide completely different and opposing explanations, the Keynesian model has generally been preferred over other possible explanations precisely because it has been thought to be statistically confirmed.¹³ What justifies the argument that the Keynesian doctrine is incorrect, following Hayek, is in particular the methodological norm in the name of which many economists claim it to be empirically justified. This norm requires that only theories which are confirmable on the basis of what one can precisely observe, that is, on the basis of that which can be calculated exactly, may be considered scientifically acceptable theories. But this norm, for Hayek as for Popper, is erroneous and as such quite unacceptable.

The effective range of Hayek's argument is not always clearly grasped. One would misinterpret his theory if one placed Hayek on the side of those who think that the social sciences are not sciences *in the same sense* that the physical sciences are. If one has him say that since economic theories are not confirmable, we must admit that they can never give us access to *the same kind of* knowledge as that provides by physics, one fails to understand him. Even though economists' explanations are not able to be "confirmed", according to Hayek, this does not mean that he is ready to admit into Economics non-empirical theories, that is, theories that have no relation to observable facts. What Hayek will not accept is that the criterion for demarcating an

¹³ "[W]e find the curious situation that the [Keynesian] theory, which is comparatively best confirmed by statistics because it happens to be the only one which can be tested quantitatively, is nevertheless false. Yet it is widely accepted only because the explanation earlier regarded as true, and which I still regard as true, cannot *by its very nature* be tested by statistics." (Hayek 1984, p. 7).

empirical economic theory from a non-empirical economic theory is statistical confirmability, and this is the key to his argument. The only logical-methodological criterion for judging whether a theory is scientific to which we must submit, following Hayek, whether in Economics or, for that matter, in physics, is whether it is refutable (or falsifiable)¹⁴, and not its possible inductive or statistical confirmability.

Following Hayek, we must give preference to a theory which is true though it makes possible no precisely quantifiable prediction, over a theory which can be quantitatively tested or statistically confirmed but which, in view of the very phenomena which are to be explained, must be considered defective or even plainly false. Now, since the social sciences take as their object what Hayek calls "essentially complex" situations, that is, phenomena which cannot really be explained unless we refer to models allowing for a very large number of variables, or again to calculations which help us to grasp only the abstract form of the phenomena in play but for which the concrete parameters remain inaccessible, to demand that they conform to canons of precision drawn from the natural sciences, where it is experimentally possible to rigorously control the explanatory factors, is to cause them irreparable harm, or at least, to demand of them something which they are unable to give.

This analysis allows us to share something essential in Caldwell's point of view. He writes in effect:

The Second World War and the subsequent history of many of the Western powers seemed to many economists to provide confirmation of the Keynesian propositions. Increased public expenditures during the war, the equivalent of expansionary fiscal policy, yielded no immediate dire consequences. Indeed, after a brief round of postwar inflation, a boom began that was sustained with only mild inflationary or recessionary interruptions until the 1970s. The postwar growth experience seemed the best evidence that, in Keynes' words, "The right remedy for the trade cycle is not to be found in abolishing booms and thus keeping us permanently in a quasi-slump; but in abolishing slumps and thus keeping us permanently in a quasi-boom." [Keynes, *The General Theory*, op. cit., p. 322] By the mid-1960s, much of the American economic community felt that the business cycle could be (and perhaps had been) vanquished, that a permanent state of quasi-boom was indeed achievable. It was not

¹⁴ "... in the sense that it might be proved false." (Hayek 1974, p. 269)

until the 1970s that the costs of the policy began to become evident, and some of Hayek's more ominous prognostications started to materialize. (Caldwell 1995, p. 32).

Like Caldwell, I hold that what explains the Keynesian doctrine's rapid rise in popularity after its publication in 1936, and the fact that it completely overshadowed the Hayekian doctrine, is not so much the fact that Hayek's audience were not up on the technical details of the theory of capital as the fact that the Hayekian analysis was completely alien to the statistical approach sought out by State managers more and more preoccupied with getting national finances into better condition so that they could better calculate and anticipate the GNP and its presumed rate of growth. As Caldwell writes, "Hayek's general worldview was antagonistic towards such aggregative statistical work; he was bypassed by the econometrics caravan, as well" (Caldwell 1995, p. 33). But, contrary to Caldwell's, my analysis makes it clear that both Hayek's anti-positivism as well as his adoption of the refutationist, anti-inductivist methodology of Karl Popper, certainly constitutes one of his important motives for rejecting the Keynesian doctrine.

But if this is the case, then, as a consequence of this, we cannot accept Caldwell's conclusion that there is no relationship between Popper's philosophy of science and the methodological and epistemological doctrines Hayek began to hold and articulate during the 1930s. Indeed we now find ourselves overlapping with a different matter which has recently been the occasion of an interesting and intriguing controversy between these two, which took place between 1988 and 1992. To put it in very broad terms, Caldwell thinks he has dramatically refuted the narrative offered by Terence Hutchison to explain the transformation of Hayek's thought in the mid-1930s. In 1988, Caldwell put forward his version of the "transformation of Hayek" during the 1930s (see Caldwell 1988, 1992a, 1992b) — a version which contests the reading made by Hutchison of Hayek's writings from this period. In 1981, Hutchison (see Hutchison 1981, Chapter 7) thinks that he has established beyond any doubt that "Hayek's 'U-turn' was chiefly methodological in nature, consisting of an abandonment of Misesian a priorism and a turning towards Popperian falsificationism." (This is Caldwell's formulation: see Caldwell 1988, p. 514, n.1.)

Caldwell, on the other hand, thinks that he has established that the debate over the socialist calculus is the most important episode for the understanding of the transformation of Hayek's thought. Caldwell's opinion is that in the course of this crucial episode, Hayek came to understand two things that would cause a general reversal of his thought a few years later (after the publication of *The Pure Theory of Capital*) — a definitive abandoning, for all practical purposes, of any research in economic theory properly speaking, in favour of philosophical, historical and juridical-sociological-political analyses.¹⁵ On the one hand, he became conscious of the centrality of the question of the economic coordination of individuals, a coordination which occurs (according to him) through an intrinsically dispersed and non-totalisable distribution, which for this reason cannot be centralized in an organ of political management, and of the knowledge transmitted by market prices. And on the other hand, he became aware of the intrinsic deficiency of the static model of general equilibrium for analyzing the process by which economic agents are coordinated.

Pushing his analysis still further and radicalizing his critique of Hutchison, Caldwell published in 1992¹⁶ a proper "refutation" of Hutchison's thesis according to which it was the adoption of the falsificationist methodology of Karl Popper which brought about a major transformation in Hayek. Even though Caldwell grants to Hutchison that "Economics and Knowledge" marks an important turning point in Hayek's thought, Caldwell "challenge[s] Hutchison's claim that Hayek's later work is best characterized as a move towards falsificationism" (Caldwell 1992a, p. 2).

Referring himself to "Scientism and Study of Society", which appeared in three parts in *Economica* between 1942 and 1944, Caldwell submit that there is a kind of "anomaly" in Hutchison's interpretation of Hayek's methodology, since "[a] cursory examination of that

¹⁵ Hayek wrote in 1965 that "Economics and Knowledge" marked the turning point in his research, since he was to subsequently abandon the technical problems of economic theory for the kinds of questions normally thought of as philosophical (See Hayek 1965, p. 91; "Kinds of Rationalism" in *Studies in Philosophy, Politics and Economics*. Chicago: University of Chicago Press, 1978, pp. 82-95, p. 91; cited by Caldwell in Caldwell 1992a, p. 5).

¹⁶ The text, however, dates back to 1988, that is, to the same year as "Hayek's Transformation"; indeed, Caldwell cites this article (See Caldwell 1988, p. 539) referring the reader to a forthcoming article (reference noted 1988b).

methodological work suggests that Popper's influence on Hayek had yet to occur in the early 1940s" (ibid., p. 2). If Popper's influence was not evident in Hayek until after 1940, and also to a much less significant degree than Hutchison claims, then it would at least be incongruous to claim that it was under Popper's influence, in the mid-1930s, that Hayek radically modified his economic thought.

I shall not dispute the argument, which is sufficiently documented by Caldwell, to the effect that the ideas presented by Hayek in "Economics and Knowledge" cannot have been formulated under the "markedly discernible influence" of Popper.¹⁷ Nor will I dispute here the fact that the essay "Scientism and the Study of Society" carries no tangible and observable mark of any major or minor "influence" of Karl Popper on his friend Hayek, even if I don't think this to be quite true on historical grounds.

That said, Caldwell shares with Hutchison a presumption of historians of ideas according to which one can employ a rigorous documentary proof to establish a *causal* relation (an "influence") between thinkers or-for this is the only evidence we have as historians of ideas and philosophers-between texts. Let me just say that I do not share this historiographical conception. Such one-way causal relations are usually destined to remain with no real proof, since the history of ideas is not an experimental science. At the most, we might be able to establish coincidences between dates of writing or of publication, convergences of views between authors; we might be able to observe sequences of argumentation in the course of which certain theoretical concepts may have been modified; we might be able to notice relations of interlocution within which two or more researchers were led to form a network for the exchange of ideas, to propose points of view to one another, to put forward critiques of contrary points of view, and indeed to recognize the deficiencies of specific arguments or the validity of specific conceptual constructs. On this evidential basis, we might be able to propose the legitimacy of certain explanations to the detriment of others.

¹⁷ Caldwell insists that "the ideas expressed in Hayek's article cannot be attributed to Popper." (Caldwell 1992a, p.5).

If, instead of looking for "causal influences" by Popper on Hayek (and also by Hayek on Popper), we were to look in the work of each for convergences of ideas and points of view, we would be able to recognize that over the course the 1930s a new preoccupation begins to play in Hayek's mind that follows falsificationism down the line — since, as we have seen, Hayek supports his rejection of the methodological foundations of Keynesianism through a Popperian critique of inductivism and confirmationism. While we might not find a "causal influence" of Popper on Hayek, we should at least recognize a comfortable philosophical collusion between these two theoreticians. Far from closing the debate about the connections to draw and not to draw between the systems of thought of these two, the Caldwell/Hutchison debate has merely staked out the terrain. Others must now venture in and re-explore it.

For a large number of interrelated intellectual events took place in the 1930s, and a complete, convincing analysis of the relationships between Hayek and Keynes, on the one hand, and between Hayek and Popper, on the other hand, should be able to show the connections between these diverse elements. The debate concerning the socialist calculus in which Hayek was involved at the beginning of the 1930s, his discovery of Popper's *Logik der Forschung* in 1935, the discussions between Keynes and Hayek, the polemic with Sraffa, Keynes's publication of *The General Theory* in 1936, Hayek's "Economics and Knowledge" lecture in November 1936, Popper's first presentation of the theses of *The Poverty of Historicism* in Brussels in December 1936, the meeting between Hayek and Popper at the LSE at the beginning of 1937 on the occasion of a paper by Popper on that same text — all these events are to be interconnected. Once this network has been constructed, it becomes obvious that over this same period, Hayek became receptive to Popper's methodological ideas (and the reverse is also true). And it is obvious that if this adoption of the falsificationist perspective by Hayek counts toward his rejection of Keynesianism, even if the documentary proof of this "influence" was not to be provided by Hayek until much later, then we must recognize that it is, contrary to Bruce Caldwell's claim, in the middle of the 1930s that this relationship was set up and not later.

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