Did F. A. Hayek Embrace Popperian Falsificationism? A Critical Comment About Certain Theses of Popper, Duhem and Austrian Methodology

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Resumen

La perspectiva metodológica de Hayek a la hora de investigar el ciclo comercial se acercaba más al apriorismo praxeológico que al falsificacionismo popperiano. Una consideración de la tesis de Duhem destaca el hecho de que, incluso desde una perspectiva metodológica común, el falsificacionismo es más problemático de lo que se suele pensar. A pesar de que las líneas de argumentación praxeológicas y comunes rechazan el énfasis popperiano en la falsificación por distintos motivos y desde un fondo distinto, las perspectivas del falsificacionismo en la metodología económica no parecen ser nada prometedoras.

Palabras clave

Metodología general; Metodología austriaca; Falsificacionismo; Popper; Hayek; Duhem; Argumento duhemiano; Prueba de teorías; Significado e interpretación de resultados econométricos; Correlación y causalidad;

Abstract

The author of this article argues that Hayek´s methodological outlook at the time he engaged in business cycle research was actually closer to praxeological apriorism than to Popperian falsificationism. A consideration of the Duhem thesis highlights the fact that even from a mainstream methodological perspective falsificationism is more problematic than is often realized. Even if the praxeological and mainstream lines of argumentation reject the Popperian emphasis on falsification for different reasons and from a different background, the prospects for falsificationism in economic methodology seem rather bleak.

Key words

General methodology; Austrian methodology; falsificationism; Popper; Hayek; Duhem; Duhemian Argument; Testing of Theories; Meaning and Interpretation of Econometric Results; Correlation and Causality;

Clasificación JEL: B20, C10, B23, A12, E32, B53, B40

1. Introduction

In his remarkable intellectual biography of F. A. von Hayek, Hans Jörg Hennecke (2000, 83) contends that F. A. Hayek, at the time he engaged in business cycle research and in particular when he wrote *Monetary Theory and the Trade Cycle* (Hayek [1933] 1966), had independently reached a falsificationist methodological position.

A closer reading and analysis of the text of the first chapter of Hayek´s *Monetary Theory and the Trade Cycle* (Hayek [1933] 1966) reveals that Hayek was not defending the view, commonly ascribed to Popper, that theoretical propositions are (sometimes) falsified, or at least, should conceivably be falsifiable, on the basis of statistical or empirical evidence.[1]

A few methodological issues considered relevant in the context of a critical examination of Popper´s falsificationism are discussed first.

2. A reminder: Popper´s falsificationism

The philosopher K. R. Popper made a career and became famous on the basis of his rejection of induction and his elaborate defence of the thesis that a hypothesis is only scientific if it is capable of being proved false by observation. The doctrine of falsifiability asserts that the criterion of the scientific status of a theory is its falsifiability or refutability, not its verifiability or confirmability.

Popper´s proposal of falsifiability as a criterion of demarcation was first launched in his *The Logic of Scientific Discovery*. ([1959] 1980) This proposal was based upon an *asymmetry* between verifiability and falsifiability. This asymmetry results from the logical form of universal statements since these are never derivable from singular statements, but can be contradicted by singular statements. Consequently it is possible by means of purely deductive inferences – with the help of the *modus tollens* of classical logic – to argue from the truth of singular statements to the falsity of universal statements. Such an argument to the falsity of universal statements is the only strictly deductive kind of inference that proceeds, as it were, in the “inductive direction”; that is, from singular to universal statements. (Popper [1959] 1980, ch. I)

Thus according to Popper, there is no induction, because there is no way of deducing universal statements from singular statements. His own approach seemed to offer the advantage that it formulated a critical method for science which proceeded through trial and the correction of error. Therefore “testability is falsifiability”. (Popper [1963] 2002, 48) Falsifiability by potential negative instances is claimed to play a distinguished role to the exclusion of inductive supportability or probabilistic confirmability by positive instances.

It was in fact soon recognized that Popper´s falsificationist methodology raised several problems which were never completely solved by Popper himself. Among these the issues raised by the Duhemian argument, which is considered further, remain most challenging to falsificationists.[2]

To summarize, Popper´s methodological position can be characterized as a variant of *methodological monism*. Methodological monism amounts to the claim that scientific explanation and prediction are always of one and the same logical structure, in the sciences of nature no less than in the sciences of human action and society.

A clear statement of Popper´s methodological monism can be found in his *The Poverty of Historicism* in a section entitled "The Unity of Method". (Popper [1957] 1994, 130 ff.) Popper does not deny that there may be some differences between the methods of the theoretical sciences of nature and of society. According to Popper, however, the methods in the two fields are fundamentally the same. The methods always consist in offering deductive causal explanations, and in testing them by way of predictions. This method of hypothesis as it is often called does not achieve absolute certainty for any of the scientific statements which it tests; rather, these statements always retain the character of tentative hypotheses, even though their character of tentativeness may cease to be obvious after they have passed a great number of severe tests. (Popper [1957] 1994, 131)

Popper provided an excellent summary of his position in footnote:

"The opposition here pointed out, between *deductivism* and *inductivism*, corresponds in some respects to the classical distinction between *rationalism* and *empiricism*: Descartes was a deductivist, since he conceived all sciences as deductive systems, while the English empiricists, from Bacon on, all conceived the sciences as collecting observations from which generalizations are obtained by induction. But Descartes believed that the principles, the premisses of the deductive systems, must be secure and self-evident – 'clear and distinct'. They are based upon the insight of reason. (They are synthetic and *a priori* valid, in Kantian language.) As opposed to this, I conceive them as tentative conjectures, or hypotheses."(Popper [1957] 1994, 131 footnote 2)

Inductivists have generally remained unimpressed by Popper´s contention that science is deductive rather than inductive. Popper´s approach has been attacked by advocates of the objective Bayesian view who, following Cox (1961), point out that probability theory is inductive logic, and vice versa, and that the laws of probability are laws of inference. According to this view, it is not the function of induction to tell us which predictions are right; it is only when inductive inferences are wrong that new things about the real world will be learned. (see Jaynes 2003 *passim*; also Garrett 1989) It has also been attacked by subjective Bayesians such as Howson and Urbach (2006) who argue that much of standard statistical practice, which is implicitly based on methodological falsificationism, should be abandoned.[3]

With respect to economic methodology, it has been observed that economists rarely play the falsificationist game consistently. (Hoover 2005, 12) Several authors have considered that no scientifically significant proposition has ever been decided on the basis of a statistical test and reject the falsificationist strategy altogether. (Summers 1991; Keuzenkamp and Magnus 1995) Where Blaug (1992) has called for a redoubled commitment to "serious" falsificationism, Keuzenkamp (2000) considers that both on the positive level, and on the normative level, the Popperian emphasis on falsification has done the reputation of econometrics much harm.

As regards the causes of the non-falsifiability of theories in economics, attention has been drawn to the logical properties of the theory, in particular the use of parametric constants which are not numerical but algebraic magnitudes and of which it is assumed that they are in fact variable (Klant 1984, 155); and to the inapplicability of a "constancy principle" (Hoppe 2006); or simply to the absence of constants in economics (Hicks 1979, 39; also Boland 1998).

3. The Duhemian argument against falsificationism

One of the most compelling cases against the possibility of the unambiguous falsification of individual scientific hypotheses has been based on a set of ideas associated with the French physicist and historian of science, Pierre Duhem. (Duhem [1914] 1991)

At the core of the thesis are two related ideas: *non-separability*, meaning that the empirical claims of hypotheses arise from conjunctions of hypotheses and background knowledge rather than from individual hypotheses taken in isolation; and *unfocused refutation*, in the sense that anomalous empirical evidence implies falsity somewhere in the conjunction of hypotheses and background knowledge under consideration, rather than necessarily implying that any particular hypothesis is false.[4]

Pierre Duhem made the sound and useful point that the falsification of a scientific prediction is a highly ambiguous item of information. For if a group of scientific theories and auxiliary hypotheses T_1, T_2, ..., T_n collectively yield a prediction P that *fails* to be realized, then what we have is just the following pair of facts:

(1) $(T_1 \& T_2 \& \ldots \& T_n) \Rightarrow P$

(2) $-P$

And from this we can conclude: $-(T_1 \& T_2 \& \ldots \& T_n)$. All that we have is that something is wrong somewhere within the family: T_1, T_2, …,T_n. But we have no idea what is amiss; we can make no particular imputation of fault. The lesson is straightforward. When things go wrong with a prediction to which various theories contribute, we cannot tell specifically where to attribute the blame.

Pierre Duhem formulated his thesis in the context of a philosophical reflection about the essential features of the experimental method in physics. Even in physics, he argued, the testing of theories is a great deal more complicated than the uncritical observer might imagine; "crucial experiments" are impossible.

From the perspective of a correct understanding of the methodology of the sciences of human action, it is not at all obvious that the economist will confront the Duhem problem in his or her attempts to appraise economic theories. There are *prima facie* no reasons to believe that econometric modelling may serve as a substitute for the experimental method, which will allow the economist to conduct "crucial experiments" on the basis of which it can be expected to be possible to decisively refute and reject a particular theory, and to validate a different, rival theory or hypothesis.[5]

Nevertheless empirical economists who advocate the use of econometric modelling methods will confront a Duhem problem of sorts, if only in virtue of the impossibility to satisfy the unspecified *ceteris paribus* condition to which all economic predictions are subject.[6]

This circumstance is related to the fact that economics – and macroeconomics in particular – have as their object of study systems that are not amenable to effective closure. No matter how many variables the macroeconomist includes in an *explanans* set, an indefinitely large number of potentially relevant variables are known to be left out.[7] When a prediction turns out to be false, it might at first seem as if the situation as regards the general laws used in making it is indeterminate: it seems that it cannot be known with certainty whether one or all of the general laws have been disconfirmed or whether the *ceteris paribus* condition has not been fulfilled. In general, however, the confidence of economists in the simplifications and *ceteris paribus* assumptions will be much lower than their confidence in the basic laws, and thus the more likely explanation for the apparent disconfirmation will usually be a failure of the simplifications and *ceteris paribus* assumptions. This observation has led one author to conclude that economics is an "inexact and separate science" since "it becomes almost impossible to learn from experience". (Hausman 1992a, 307)[8]

The praxeologist is confident, however, that this sad conclusion regarding the inexact nature of economic science can be avoided.[9]

Not surprisingly, Popper has argued against the Duhem thesis. A critical examination of Popper´s critique is instructive. According to Popper, Pierre Duhem, in his famous criticism of crucial experiments, succeeds in showing that crucial experiments can never *establish* a theory. He fails to show, Popper contends, that they cannot *refute* it. (Popper [1963] 2002, 150 footnote 26)

A first argument (Popper [1963] 2002, 324) is that, in axiomatized systems, counterexamples can be found by the practice of independence proofs, that is, proofs which show that certain axioms of an axiomatic system cannot be derived from the rest. The more simple of these proofs consist in the discovery of a model which satisfies all of the axioms except the one whose independence is to be shown. For this one axiom – and therefore for the theory as a whole – the model constitutes a counterexample. This point fails because Duhem is not referring to purely axiomatic systems, but to scientific theories in which theories are in some way linked to observational evidence. A second challenge (ibid. 151) is that scientists can take background knowledge and auxiliary assumptions as given, and regard anomalous evidence as refuting one or other of the hypotheses which are the targets for testing. The suggestion is that if we take each of the two theories between which the crucial experiment is to decide *together* with all this background knowledge, then we decide between two systems which differ *only* over the two theories which are at stake. This argument fails because the “refutation” would still remain inconclusive in that the fault may lie in the background knowledge or auxiliary hypotheses taken as given. (Duhem [1914] 1991, 216–18)[10]

A third challenge is that scientists do invoke good reasons for changing specific components of their theoretical systems when confronted by refutations. (Popper 1983, 187 ff.) Thus, from the normative viewpoint, Lakatos reminds us that the sophisticated falsificationist will allow *any* part of the body of science to be replaced *but* only on the condition that it is replaced in a ‘progressive’ way, so that the replacement successfully anticipates novel facts. (Lakatos 1978, 99) This is not denied by the Duhem–Quine thesis. The point is that such refutations *cannot logically force one* to give up one component of the theoretical system rather than another. These reasons of good sense do not impose themselves with the same implacable rigor that the prescriptions of logic do. (Duhem [1914] 1991, 217)

4. The praxeological critique of falsificationism

The precepts of Austrian methodology allow to sidestep Hausman´s sad conclusion that since empirical methods do not allow the economist to conduct "crucial experiments" in order to sift correct theories from false ones, it is not possible to learn from experience in economics.

Neoclassical economists still by and large accept the positivistic thesis to the effect that no non-trivial part of economic theory could be of a *synthetic a priori* nature, thus implicitly or explicitly embracing a variant of the analytic-synthetic dichotomy. The propositions of economics, according to this view, are inductive hypotheses, and the method of economics consists in the building of testable models, selection among which is effected, at least in principle, on the basis of relative predictive strength. Realism, according to this view, falls out of account as a criterion of theory selection. Austrian economists, in contrast, and while they do not contest the relevance of empirical and applied work, at first attempt to grasp conceptually the basic categories in which the science of economics has its roots.[11]

Austrians make a distinction between conception (theory) and understanding (history), while recognizing that historical understanding is the vital goal for which the theoretical construct of economics is to be employed. The entire purpose of theory is to aid the act of historical interpretation.

Thus from an Austrian viewpoint historical facts cannot be used to "test" the truth of the theory. Economic theories *cannot* be "tested" by historical or statistical fact. These historical facts are complex and cannot, like the controlled and isolable physical facts of the scientific laboratory, be used to test theory. There are always many causal factors impinging on each other to form historical facts. Only causal theories derived *a priori* to these facts can be used to isolate and identify the causal strands.[12]

The essence of Austrian methodology was briefly and brilliantly summarized by M. N. Rothbard in the Introduction to his *America´s Great Depression.* (Rothbard [1963] 1975) Considering how to gauge the results of economic policies, M. N. Rothbard refers to the circumstance that the "facts" will always underdetermine theory choice. He wrote:

"Suppose a theory asserts that a certain policy will cure a depression. The government, obedient to the theory, puts the policy into effect. The depression is not cured. The critics and advocates of the theory now leap to the fore with interpretations. The critics say that failure proves the theory incorrect. The advocates say that the government erred in not pursuing the theory boldly enough, and that what is needed is stronger measures in the same direction. Now the point is that *empirically there is no possible way of deciding between them*. Where is the empirical "test" to resolve the debate? How can the government rationally decide upon its next step? Clearly, the only possible way of resolving the issue is in the realm of pure theory – by examining the conflicting premises and chains of reasoning."(Rothbard [1963] 1975, 4-5)

Elsewhere Prof. Rothbard further clarifies:

"This methodology begins with the conviction that while the economist, unlike the physicist, cannot test his hypotheses in controlled experiments, he is, in another sense, in a *better* position than the physicist. For while the physicist is certain of his empirical laws but tentative and uncertain of his explanatory

generalizations, the economist is in the opposite position. He begins, not with detailed, quantitative, empirical regularities, but with broad explanatory generalizations. These fundamental premises he knows with certainty; they have the status of apodictic axioms, on which he can build deductively with confidence.”
(Rothbard 1979, 34)

As one author has pointed out, the praxeological and Duhem-Quine positions possess the same implication for the idea of the “testability” of theoretical propositions in scientific work. (Boettke 1998, 538) As regards economics, this contention must be qualified, however.

First, it can be pointed out that theoretical propositions in economics are unambiguously tested, namely in the praxeological thought experiment and in the process of ratiocination. As Prof. Rothbard aptly clarifies:

“The nature of the evidence on which the praxeological axiom rests is, moreover, fundamentally similar to that accepted by the self-proclaimed empiricists. To them, the laboratory experiment is evidence because the sensory experience involved in it is available to each observer; the experience becomes “evident” to all. Logical proof is in this sense similar; for the knowledge that B follows from A becomes evident to all who care to follow the demonstration. In the same way, the fact of human action and of purposive choice also becomes evident to each person who bothers to contemplate it; it is just as evident as the direct sense experience of the laboratory." (ibid. 36-37)

Thus logical proof is no less evidence than the direct sensory experience of the laboratory experiment. Valid (or correct) praxeological reasoning transmits *truth* from the premises to the conclusion. One of the basic tools for the deduction of the logical implications of the axiom of human action is the use of the *Gedankenexperiment*, or "mental experiment". The *Gedankenexperiment* is the economic theorist´s substitute for the natural scientist´s controlled laboratory experiment. Since the relevant variables of the social world cannot actually be held constant, the economist holds them constant in his imagination. Using the tool of verbal logic, he mentally investigates the causal influence of one variable on another.

Furthermore, if there exists any superficial analogy between the insights provided by the Duhem-Quine thesis on the one hand and praxeological methodological insights on the other, it is rather to be found in the context of that other major discipline dealing with human beings, viz history. This discipline examines the applicability and relevance of particular theoretical propositions in particular historical contexts.

As Ludwig von Mises had already pointed out, the economic historian is confronted with a somewhat analogous problem which is related to the use of *judgments of relevance* in historical research and their inevitability.

The course of history is determined by the actions of individuals and by the effects of these actions. The actions are determined by the value judgments of the acting individuals, that is, the ends which they were eager to attain, and by the means which they applied for the attainment of these ends. The choice of the means is an outcome of the whole body of technological knowledge of the acting individuals. (Mises 1998, 49) It belongs to the preliminary work to be achieved by the historian to establish the facts that people were motivated by definite value judgments and aimed at definite ends. Then understanding must appraise the effects and the intensity of the effects brought about by an action; it must deal with the relevance of each motive and each action. (ibid. 55) To every historical factor understanding tries to assign its relevance. (ibid. 57)

The historian can enumerate all the factors which cooperated in bringing about a known effect and all the factors which worked against them and may have resulted in delaying and mitigating the final outcome. But he cannot, except by understanding, assign to each of *n* factors its role in producing the effect *P*. Understanding is in the realm of history the equivalent, as it were, of quantitative analysis and measurement. (ibid. 56) In the realm of physical and chemical events there exist (or, at least, it is generally assumed that there exist) constant relations between magnitudes, and man is capable of discovering these constants with a reasonable degree of precision by means of laboratory experiments. No such constant relations exist in the field of human action. (ibid. 55)

Mises´s most important conclusion with respect to historical understanding was that it “can never produce results which must be accepted by all men.”(Mises 1998, 57) Two historians who fully agree with regard to the teachings of the non-historical sciences and with regard to the establishment of the facts as far as they can be established without recourse to the understanding of relevance, may disagree in their understanding of the relevance of these facts. They may fully agree in establishing that the factors *a*, *b*, and *c* worked together in producing the effect *P*; nonetheless they can widely disagree with regard to the relevance of the respective contributions of *a*, *b*, and *c* to the final outcome. As far as understanding aims at assigning its relevance to each factor, it is open to the influence of subjective judgments. Of course, these are not judgments of value, they do not express preferences of the historian. They are jugdments of relevance.

5. A closer look at Hayek´s view

Hans Jörg Hennecke (2000, 83) contends that a prefiguration of Popper´s falsificationism can be found in Hayeks´s assertion that "[i]t is therefore only in a negative sense that it is possible to verify theory by statistics." (Hayek [1933] 1966, p. 34)

In the passage immediately preceding the previously quoted statement, Hayek writes:

"It might be shown, for instance, by statistical investigation that a general rise in prices is followed by an expansion of production, and a general fall in prices by a diminution of production; but this would not necessarily mean that theory should regard the movement of price as an independent cause of movements of production. So long as a theory could explain the regular occurrence of this parallelism in any other way, it could not be disproved by statistics, even if it maintained that the connection between the two phenomena was of a precisely opposite nature.” (Hayek ibid., pp. 33–4)

Hayek further wrote: “Even as a means of verification, the statistical examination of the cycles has only a very limited value for Trade Cycle theory. For the latter–as for any other economic theory– there are only two criteria of correctness. Firstly, it must be deduced with unexceptionable logic from the fundamental notions of the theoretical system; and secondly, it must explain by a purely deductive method those phenomena with all their peculiarities which we observe in the actual cycles. Such a theory could only be ‘false’ either through an inadequacy in its logic or because the phenomena which it explains do not correspond with the observed facts. If, however, the theory is logically sound, and if it leads to an explanation of the given phenomena as a necessary consequence of these general conditions of economic activity, then the best that statistical investigation can do is to show that there still remains an unexplained residue of processes. It could never prove that the determining relationships are of a different character from those maintained by the theory.” (Hayek ibid., pp. 32–3)

In footnote Hayek quotes from Pigou´s *Industrial Fluctuations* what has become the textbook proposition that "correlation does not imply causation" (with the corollary that "the absence of correlation does not imply the absence of causation"):

"The absence of statistical correlation between a given series of changes and industrial fluctuations does not by itself disprove–and its presence does not prove–that these changes are causes of the fluctuations."(Hayek ibid., p. 31)

It is not entirely useless to rehearse this otherwise well-known textbook truth, however. An economist of the caliber of Milton Friedman has on occasion declared the Austrian theory of the business cycle "wrong" on the basis of a supposed absence of any observed statistical correlation between the amplitude of expansions and the amplitude of the succeeding recessions (considered at a chosen, in particular too high a level of aggregation), which Dr. Milton Friedman considers "decisive refutation of von Mises". (Hammond 1992, p. 102; also 1996, *passim*)

Besides the fact that working at too high a level of aggregation may actually tend to conceal rather than to reveal the most relevant relationships (Garrison 2001, 224 ff.), in fact statistical studies will indeed tend to establish the *applicability* (or the absence thereof) of a particular theory in a particular historical context:

"(…) very complicated statistical investigations are needed to ascertain whether these circumstances whose presence indicates the applicability of theoretical conclusions were in fact operative."(Hayek ibid., p. 37)

According to Hayek the use of statistical studies is thus rather limited:

"*A priori* we cannot expect from statistics anything more than the stimulus provided by the indication of new problems." (Hayek ibid., p. 31)

It remains true that "[o]ften statistical analysis may detect phenomena which have, as yet, no theoretical explanation, and which therefore necessitate either an extension of theoretical speculation or a search for new determining conditions."(Hayek ibid., p. 37)

There thus seem to be no compelling reasons for Austrians to reject econometric methods *per se*, provided the aspirations (and pretences) with respect to the possible accomplishments of these methods are appropriately tempered. The requirement that econometric analysis should attempt to lead to the refutation (or the falsification) of established theories clearly reflects a too ambitious aspiration, and this will inevitably tend to damage the credibility of the whole econometric enterprise. Econometricians tend to search for adequate empirical representations of particular data. If econometricians are able to deliver useful approximations to empirical data, they achieve a major accomplishment.[13]

Economists may disagree, for instance, about whether there was any significant credit expansion going on during a particular historical period, say, the (late) 1920s. Only under conditions of such credit expansion is the Austrian theory of the business cycle deemed applicable. Austrians will thus point out that there is a crucial difference between contending that the empirical evidence with respect to this historical episode *refutes* (or *falsifies*) the theory of the business cycle on the one hand (which is deemed methodologically impossible), and the contention that this evidence substantiates the claim that the theory *does not apply* to the facts of this period (or that certain facts of this period are not explained by the theory), on the other hand (which is deemed methodologically possible but in casu factually false).[14] Statistical investigations can indeed inform us about the applicability (or the absence thereof) of previously derived theoretical propositions. For the

Austrian economist, investigations of a conceptual, theoretical nature on the one hand and applied (statistical, historical…) research on the other thus always remain largely distinct cognitive acts.[15]

6. Conclusion

The suggestion that Hayek, at the time he wrote *Monetary Theory and the Trade Cycle*, had independently arrived at a methodological position much akin to Popperian falsificationism cannot withstand critical analysis. The methodological view outlined by Hayek in the first chapter of *Monetary Theory and the Trade Cycle* is actually more akin to the Misesian or praxeological view regarding the epistemological status of theoretical propositions. Even from the perspective of mainstream methodology, however, falsificationism remains more problematic than is often realized in view of the issues raised by the Duhem thesis, which falsificationists haven´t resolved satisfactorily.[16] Even if it remains true that both lines of argumentation reject the Popperian emphasis on falsification for different reasons and from a different background, the prospects for falsificationism in economic methodology seem rather bleak.

Notes

[1] We are here only concerned with the methodological views Hayek expressed as an economist, that is, the views he expressed at the time he engaged in business cycle research. There can be little doubt that Hayek´s methodological views evolved, arguably even considerably, during the remainder of his long career. It is commonly believed that Hayek´s methodological views evolved in a direction that made them more akin to those of his friend K. R. Popper. These issues are not considered here. Only Hayek´s early methodological views are considered.

[2] In fact falsificationist methodology gives rise to several problems, such as (a) the characterization of the notion of truthlikeness and the conditions under which we may speak of increasing verisimilitude of our theories; (b) the specification of a possible measure of the "degree of corroboration"; (c) the issue of how to detect possible "immunizing stratagems" introduced "ad hoc" in order to save the concerned theory from falsification; and, last but not least, (d) the possibility of finding a convincing answer to the objection contained in the Duhem-Quine thesis. The latter issue is here considered in more detail.

[3] Trying to answer some of these objections, Gillies (1990) points out that it is unlikely that the standard methods of statistical testing will be given up. For a standard introduction to some conventional tests, see e.g. chapter 1 in Hayashi (2000).

[4] This set of ideas is also known as the Duhem-Quine Thesis, because of a similar – and slightly stronger – thesis to be found in Quine´s *Two Dogmas of Empiricism*, reprinted in Quine ([1953] 1980, 20-46).

Here we find Quine´s famous statement that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body (…)" (ibid. 41) but also his stronger and more questionable claim that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system (…)". (ibid. 43)

[5] For a sensible defense of data mining by an author who states that "[t]o have created by data mining a regression with certain properties is not in itself to have discovered anything of significance about the world" (ibid. 251), see Hoover (1995). An interesting discussion of the methodology of econometrics, containing an overview of the main econometric approaches, is contained in Hoover (2005).

[6] That any economic prediction is subject to a *ceteris paribus* assumption has also been recognized from a praxeological perspective. As Ludwig von Mises reminds us: "The assumption *ceteris paribus* is the self-evident appendage of every scientific doctrine and there is no economic law that can dispense with it." (Mises 1981, 152)

[7] An interesting analysis of the meaning of the Duhem-Quine Thesis in a macroeconomic context is contained in Cross (1982).

[8] This author has argued from within a Bayesian framework that economic laws will be *de facto* nonfalsifiable. (1992a, 208; 1992b, 64) Praxeological methodology offers a more principled argument against the falsifiability of economic laws.

[9] See section 4 hereafter. It is interesting to note that Mises wrote with respect to the quantity theory: "Neither can any sort of refutation or limitation of the quantity theory be deduced from the fact that a number of writers claim validity for it only on the assumption *ceteris paribus*; not even though they state further that

this supposition never is fulfilled and never could be fulfilled." (Mises 1981, 151–2)

[10] Popper apparently envisages the following situation. When we are deciding between H1 and H2, we are comparing (A&B...&D) & H1 against (A&B...&D) & H2. If the former entails X, while the latter does not, and a test gives not–X, then H1 is taken as falsified. Now one might contend that anyone is free to suspect that H1 is nevertheless true; Popper´s suggestion seems to be that in that case the work will shift to attempts to falsify (at least) one of "A, B,… D". In other words, A is taken as a new H1 and the process is repeated, with our knowledge growing at each step. However, this is not the logic of falsification according to which it is possible to conclusively reject a particular hypothesis by purely deductive means alone. Nothing guarantees that each one of the separate hypotheses can be taken apart. Moreover, even if one would be somehow justified in taking H1 as falsified, it does not follow from this that H2 is thereby somehow corroborated. There may exist countless other hypotheses H3,…,Hn all of which are not contradicted by the evidence either but which are pairwise incompatible with H2.

[11] Certain differences between the views of the different protagonists of Austrian methodology and apriorism can be discerned, see e.g. the views expressed in Hoppe (1995), Huerta de Soto (1998), Smith (1996), among others. An elucidation of these differences may be instructive but falls outside the scope of this article.

[12] At best it could be said that facts can test historical hypotheses, that is, hypotheses about the ways in which particular theoretical propositions are *relevant* in particular historical constellations of facts. In fact this is often what researchers are referring to when they talk about "facts testing theory". But such hypotheses are not "theory" as the Austrian sees it. Theory always

has a conditional, that is, an *if-then* structure. For instance, researchers could possibly disagree about whether there was policy-induced malinvestment going on during the 1920s which rendered the boom ultimately unsustainable. This is a dispute concerning whether the Austrian theory of boom and bust *applies* to – and is *relevant* with respect to – a particular historical episode. The outcome of this discussion can never be a test of the theory itself, which only maintains that *if* there is policy-induced malinvestment etc., *then*, ceteris paribus, the resulting boom is unsustainable. A lot of research of a statistical and econometric nature is really about such questions of *applicability* and *relevance* and not about testing the fundamental propositions of economics. In other words, such research is, correctly interpreted, a contribution to historiography and not to economic theory. As such it is perfectly legitimate.

[13] Keuzenkamp (2000, 159 ff. and *passim*). In particular, this author rightly expresses his scepticism regarding the claim that econometric modelling may serve as a substitute for the experimental method. On the other hand this author rejects apriorism. (ibid. 6-7)

[14] That credit expansion and malinvestment indeed explain the unsustainability of the boom of the 1920s has typically been held by Austrian economists. In a recent paper, Eichengreen and Mitchener argue that the perspective provided by the credit-boom view is indeed a useful supplement to more conventional interpretations. (Eichengreen and Mitchener 2003) The suggestion, however, that the severity of the recession that followed the crisis was caused by the magnitude of the preceding credit (asset price) boom remains contested. It is widely believed that it was monetary policy failures that explain why the 1920s experience was followed by the greatest depression of all time; in other words, according to this

interpretation, it was the policy response after 1930 and not the credit boom that accounts for the severity of the bust.
The exact role of the interwar gold standard remains equally subject to controversy. Econometric studies can help sort out these matters; however, the conclusion that the significant contraction of the money supply that accompanied the depression would have been impossible under a gold standard with a 100 percent reserve requirement – as has been consistently pointed out by the advocates of such an arrangement – follows from straightforward conceptual considerations. See also the comments by Michael D. Bordo and Charles Goodhart on the Eichengreen-Mitchener paper. (ibid. 82 ff.)

[15] When Hayek uses the expressions "verification" and "corroboration" in this context, he really means that certain statistically established facts *illustrate* or *exemplify* the theory. Thus Hayek writes that "empirical studies (…) can, at best, afford merely a verification of existing theories; they cannot, in themselves, provide new insight into the causes or the necessity of the Trade Cycle." (p. 27) "The reason for this is clear. The means of perception employed in statistics are not the same as those employed in economic theory; and it is therefore impossible to fit regularities established by the former into the structure of economic laws prescribed by the latter." (p. 28) "Just as no statistical investigation can prove that a given change in demand must necessarily be followed by a certain change in price, so no statistical method can explain why all economic phenomena present that regular wave-like appearance which we observe in cyclical fluctuations." (p. 30) "The statistical approach, unlike deductive inference, leaves the conditions under which established economic relations hold good fundamentally undetermined; and similarly, the objects to which they relate cannot be determined as unequivocally as by theory. Empirically established relations between various economic

phenomena continue to present a problem to theory until the necessity for their interconnections can be demonstrated independently of any statistical evidence. The concepts on which such an explanation is based will be quite different from those by which statistical interconnections are demonstrated; they can be reached independently. Moreover, the corroboration of statistical evidence provides, in itself, no proof of correctness. (pp. 30–1)

"In thus emphasizing the fact that Trade Cycle theory, while it may serve as a basis for statistical research, can never itself be established by the latter, it is by no means desired to deprecate the value of the empirical method. On the contrary, there can be no doubt that Trade Cycle theory can only gain full practical importance through exact measurement of the actual course of the phenomena which it describes. But before we can examine the question of the true importance of statistics to theory, it must be clearly recognized that the use of statistics can never consist in a deepening of our theoretical insight." (pp. 31–2) "Thus it is not by enriching or by checking theoretical analysis that economic statistics gain their real importance. This lies elsewhere."(p. 35)

[16] It has been argued that the Duhem problem can be solved by Bayesian means. This conclusion can be related to the fact that evidence of the kind that refutes a test system may have a sharply asymmetric effect on the probabilities of the different components of this test system. For the Bayesian approach toward a solution of the Duhem problem, reference can be made to Dorling (1979); see also the discussion in Howson and Urbach (2006) 103 ff. and in Jeffrey (2004) 35 ff.

References

Blaug, M. (1992), *The Methodology of Economics: Or How Economists Explain*, 2nd edition, Cambridge: Cambridge University Press.

Boettke, P. J. 'Von Mises, Ludwig', in: *The Handbook of Economic Methodology*, Davis J. B., Wade Hands D. and Uskali Mäki (eds.), Cheltenham: Edward Elgar, 534-539.

Boland, L. A. (1998) 'Understanding the Popperian Legacy in Economics', downloaded version.

Cox, R. T. (1961), *The Algebra of Probable Inference*, Baltimore: The John Hopkins Press.

Cross, R. (1982), 'The Duhem-Quine Thesis, Lakatos and the Appraisal of Theories in Macroeconomics', *The Economic Journal*, **92** (June), 320-40.

Dorling, J. (1979), 'Bayesian Personalism, the Methodology of Research Programmes, and Duhem´s Problem', *Studies in History and Philosophy of Science*, vol. 10, 177-187;

Duhem, P. ([1914] 1991), *The Aim and Structure of Physical Theory*, Princeton: Princeton University Press.

Eichengreen, B. and Mitchener, K. (2003), 'The Great Depression as a credit boom gone wrong', *BIS Working Papers No. 137*, 81 pp.

Garrison, R.W. (2001), *Time and Money – The Macroeconomics of Capital Structure*, London: Routledge.

Garrett, A. J. M. (1989), 'Probability, Philosophy and Science: a briefing for Bayesians', in Skilling, J. (ed.) *Maximum Entropy and Bayesian Methods*, Kluwer Academic Publishers, 107-116.

Gillies, D. (1990), 'Bayesianism versus Falsificationism. Review of Howson and Urbach 1989', *Ratio* (New Series) III (1), 82-98.

Hammond, J.D. (1992), 'An Interview with Milton Friedman on Methodology', *Research in the History of Economic Thought and Methodology*, **10**, pp. 91-118.

Hammond, J. D. (1996) *Theory and measurement – Causality issues in Milton Friedman´s monetary economics*, Cambridge University Press.

Hausman, D. M. (1992a), *The inexact and separate science of economics*, New York: Cambridge University Press.

Hausman, D. M. (1992b), *Essays on philosophy and economic methodology*, Cambridge University Press.

Hayashi, F. (2000), *Econometrics*, Princeton University Press.

Hayek, F.A. ([1933] 1966), *Monetary Theory and the Trade Cycle*, New York: Augustus M. Kelley.

Hayek, F.A. ([1935] 1967), *Prices and Production*, 2nd edn (revised and enlarged), New York: Augustus M. Kelly.

Hennecke, H.J. (2000), *Friedrich August von Hayek-Die Tradition der Freiheit*, Düsseldorf: Verlagsgruppe Handelsblatt GmbH.

Hicks, J. (1979), *Causality in economics*, New York: Basic Books.

Hoover, K. D. (1995), 'In Defense of Data Mining: Some Preliminary Thoughts', in: *Monetarism and the Methodology* *of Economics*, Hoover K. and Sheffrin S. M. (eds.), Aldershot: Edward Elgar, 242–257.

Hoover, K. D. (2005), 'The Methodology of Econometrics', prepared for the Palgrave *Handbooks of Econometrics, volume 1: Theoretical Econometrics,* downloaded version.

Hoppe, H.-H. (1995), *Economic Science and the Austrian Method*, Auburn: Ludwig von Mises Institute.

Hoppe, H.-H. (2006), 'Is Research Based on Causal Scientific Principles Possible in the Social Sciences', in: *The Economics and Ethics of Private Property*, Auburn: Ludwig von Mises Institute.

Howson, C. and P. Urbach (2006), *Scientific Reasoning – The Bayesian Approach*, La Salle: Open Court.

Huerta de Soto J. (1998), 'The Ongoing Methodenstreit of the Austrian School', *Journal des Economistes et des Etudes Humaines*, Vol. 8, No. 1, 75–113.

Jaynes, E.T. (2003), *Probability Theory – The Logic of Science*, Cambridge: Cambridge University Press.

Jeffrey, R. (2004), *Subjective Probability – The Real Thing*, Cambridge University Press.

Keuzenkamp, H. A. and J. R. Magnus (1995), 'On Tests and Significance in Economics,' *Journal of Econometrics* 67 (1), 5–24.

Keuzenkamp, H. A. (2000), *Probability, Econometrics and Truth–The methodology of econometrics*, Cambridge: Calbridge University Press.

Klant, J. J. (1984), *The rules of the game – The logical structure of economic theories*, Cambridge: Cambridge University Press.

Lakatos, I. (1978), *The methodology of scientific research programmes*, Cambridge: Cambridge University Press.

Mises, L. von (1998), *Human Action*, Auburn: The Ludwig von Mises Institute.

Mises, L. von (1981), *The Theory of Money and Credit*, Indianapolis: Liberty*Classics*.

Popper, K. R. ([1957] 1994), *The Poverty of Historicism*, London: Routledge.

Popper, K. R. ([1959] 1980), *The Logic of Scientific Discovery*, London: Hutchinson.

Popper, K. R. ([1963] 2002), *Conjectures and Refutations*, London: Routledge.

Popper, K. R. (1983), *Realism and the Aim of Science*, London: Hutchinson.

Quine, W. V. O. ([1953] 1980), *From a Logical Point of View*, London: Harvard University Press.

Rothbard, M. N. ([1963] 1975), *America´s Great Depression*, Kansas City: Sheed and Ward, Inc.

Rothbard, M. N. (1979), *Individualism and the Philosophy of the Social Sciences*, Cato Paper No. 4, San Francisco: Cato Institute.

Rothbard, M. N. (1997), *The Logic of Action I: Method, Money, and the Austrian School*, Cheltenham: Edward Elgar.

Smith, B. (1996), 'In Defense of Extreme (Fallibilistic) Apriorism', *Journal of Libertarian Studies*, 12:1, 179-192.

Summers, L. H. (1991), 'The Scientific Illusion in Empirical Macroeconomics,' *Scandinavian Journal of Economics* 93(2), 129-48.

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