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Hugo A. Keuzenkamp
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1 The philosophy of induction

> [S]ome other scientists are liable to say that a hypothesis is definitely proved by observation, which is certainly a logical fallacy; most statisticians appear to regard observations as a basis for possibly rejecting hypotheses, but in no case for supporting them. The latter attitude, if adopted consistently, would reduce all inductive inference to guesswork.
>
> Harold Jeffreys ([1939] 1961, p. ix)

1 Introduction

Occasionally, the aspirations of econometrics are frustrated by technical difficulties which lead to increasing technical sophistication. More often, however, deeper problems hamper econometrics. These are the problems of scientific inference – the logical, cognitive and empirical limitations to induction. There is an escapist tendency in econometrics, which is to seek salvation in higher technical sophistication and to avoid deeper philosophical problems. This is reflected by the erosion of an early foothold of empirical econometrics, *Econometrica*. The share of empirical papers has declined from a third in the first (1933) volume to a fifth in recent volumes. This is not because most empirical values for economic variables or parameters have been settled. Despite the 'econometric revolution', there is no well established numerical value for the price elasticity of bananas. If *Econometrica* were to publish an issue with well established econometric facts, it might be very thin indeed. The factual knowledge of the economy remains far from perfect, as are the ability to predict its performance, and the understanding of its underlying processes. Basic economic phenomena, such as the consumption and saving patterns of agents, remain enigmatic. After many years of econometric investigation, there is no agreement on whether money causes output or not. Rival theories flourish. Hence, one may wonder what the added-value of econometrics is. Can we learn from experience in economics, and, if so, does econometrics itself serve this purpose? Or, were the aspirations too high after all, and does the sceptical attitude of Keynes half a century ago remain justified today?

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2 Humean scepticism

An important issue in the philosophy of science is how (empirical) knowledge can be obtained.[1] This issue has a long history, dating back (at least) to the days of the Greek Academy, in particular to the philosopher Pyrrho of Elis (*c.* 365–275 BC), the first and most radical sceptic. Academic scepticism, represented for example by Cicero (106–43 BC), is more moderate than Pyrrho's. The ideas of Pyrrho (who did not write books, 'wisely' as Russell, 1946, p. 256, remarks) are known via his pupil Timon of Phlius (*c.* 320–230 BC) and his follower Sextus Empiricus (second century AD), whose work was translated into Latin in 1569. A few earlier translations are known but they have probably only been read by their translators. The 1569 translation was widely studied in the sixteenth and seventeenth centuries. All major philosophers of this period referred to scepticism. René Descartes, for example, claimed to be the first philosopher to refute scepticism.

One of the themes of the early sceptics is that only deductive inference is valid (by which they mean: logically acceptable) for a demonstrative proof, while induction is invalid as a means for obtaining knowledge. Perception does not lead to general knowledge. According to Russell (1946, p. 257),

> Scepticism naturally made an appeal to many unphilosophic minds. People observed the diversity of schools and the acerbity of their disputes, and decided that all alike were pretending to knowledge which was in fact unattainable. Scepticism was a lazy man's consolation, since it showed the ignorant to be as wise as the reputed men of learning.

Still, there was much interest in scepticism since the publication of the translation of Sextus Empiricus' work, not only by 'unphilosophic minds'. Scepticism has been hard to refute. Hume contributed to the sceptical doctrine (although he did not end up as a Pyrrhonian, *i.e.* radical sceptic). The result, 'Humean scepticism', is so powerful, that many philosophers still consider it to be a death blow to induction, the 'scandal of philosophy'.[2]

Hume ([1739] 1962) argues that the empirical sciences cannot deliver causal knowledge. There are no rational grounds for understanding the causes of events. One may observe a sequence of events and call them cause and effect, but the connection between the two remains hidden. Generalizations deserve scepticism. Hume (Book I, Part III, section 12, p. 189) summarizes this in two principles:

> *that there is nothing in any object, considered in itself, which can afford us a reason for drawing a conclusion beyond it;* and, *that even after the observation of the*

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frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience.

The 'scandal of philosophy' is fundamental to empirical scientific inference (not just econometrics). It has wider implications (as Hume indicates) than denying causal inference. For example, does past experience justify the expectation of a sunrise tomorrow? The question was raised in discussing the merits of Pierre Simon de Laplace's 'rule of succession', a statistical device for induction (see chapter 2).[3] Another example, popular in philosophy, deals with extrapolation to a population instead of the future: if only white swans have been observed, may we infer that all swans are white? (This is the classic example of an affirmative universal statement.)

The sceptical answer to these questions is negative. The rules of deductive logic prohibit drawing a general conclusion if this conclusion is not entailed by its propositions. There is no *logical* reason why the next swan should be white. Of course, swans can be defined to be white (like statisticians who define a fair die to be unbiased), making black swans a contradiction in terms. An alternative strategy is to conclude that all *known* swans are white. The conclusion is conditional on the observed sample. Hence, the choice is between formulating definitions or making conditional enumerations. But most empirical scientists want to make generalizations. This is impossible if the induction problem proves insurmountable. Therefore, an understanding of induction is essential.

The logical form of the induction problem is that all *observed* X are Φ does not entail that all X are Φ. The next three chapters, dealing with probabilistic inference, consider a more delicate, probabilistic form of the induction problem: given that *most* observed X are Φ, what can be said about X in general? The source of Humean scepticism follows from the conjunction of three propositions (Watkins, 1984, p. 3):

(i) there are no synthetic *a priori* truths about the external world;
(ii) any genuine knowledge we have of the external world must ultimately be derived from perceptual experience;
(iii) only deductive derivations are valid.

The conjunction of (i), (ii) and (iii) does not allow for inferring knowledge beyond the initial premises. In this sense, inductive inference is impossible.

John Watkins (p. 12) argues that a philosophical, or 'rational' answer to scepticism is needed, because otherwise it is likely to encourage irrationality. Watkins holds that Hume himself regarded philosophical scepticism as an academic joke. Indeed, Hume uses the expression *jeux d'esprit* (in *A letter From a Gentleman to his Friend in Edinburgh*, included

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as an appendix to Hume [1748] 1977, p. 116). Describing the person who is afflicted by Pyrrhonism, Hume (p. 111) concludes:

> When he awakes from his dream, he will be the first to join in the laugh against himself, and to confess, that all his objections are mere amusement.

Amusement, Watkins (1984, p. 12) argues, does not qualify as a rational answer to scepticism. In fact, Hume's response is more elaborate than the quotation suggests. It relies on conventionalism (see below). I agree with Watkins that, formally, conventionalism is not very appealing (although conventions have much practical merit). Fortunately, there are alternatives. Once the source of Hume's problem (the threefold conjunction just mentioned) is clarified, the merits of those alternative responses to scepticism can be appraised.

Watkins (pp. 4–5) discusses a number of strategies as responses to Hume's problem. The most interesting ones are:

- the naturalist (ignoring the conjunction of propositions (i)–(iii));
- the apriorist (denying proposition (i));
- the conjecturalist (amending proposition (ii)); and
- the probabilist strategy (which takes odds with proposition (iii)).

A more detailed discussion of the probabilist strategy will be given in the next three chapters, while the remainder of this book considers how well this strategy may work in econometrics.

3 Naturalism and pragmatism

Descartes argued that one should distrust sensations. Insight in causal relations results from mere reasoning. Hume, criticizing Cartesian 'dogmatic rationalism', argues that such plain reasoning does not suffice to obtain unique answers to scientific questions. Cartesian doubt, 'were it ever possible to be attained by any human creature (as it plainly is not) would be entirely incurable' (Hume [1748] 1977, p. 103). It would not yield true knowledge either: 'reasoning *a priori*, any thing might appear able to produce anything' (*Letter From a Gentleman*, p. 119). Cartesian doubt is unacceptable to Hume ([1739] 1962, p. 318). It gave him a headache:

> The *intense* view of these manifold contradictions and imperfections in human reason has so wrought upon me, and heated my brain, that I am ready to reject all belief and reasoning, and can look upon no opinion even as more probable or likely than another.

But this does not make Hume a Pyrrhonian or radical sceptic. He is rescued from this philosophical 'melancholy and delirium' by nature.

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His naturalist strategy is to concede that there is no epistemological answer to scepticism, but to deny its importance. It is human nature to make generalizing inferences, the fact that inference is not warranted from a logical point of view has no practical implications. Hume (*An Abstract of a Book Lately Published, Entitled, A Treatise of Human Nature, Etc.*, in Hume [1739] 1962, p. 348) concludes,

> that we assent to our faculties, and employ our reason only because we cannot help it. Philosophy would render us entirely *Pyrrhonian*, were not nature too strong for it.

The great subverter of Pyrrhonism, Hume ([1748] 1977, p. 109) writes, is 'action, and employment, and the occupations of common life'. Not reasoning, but custom and habit, based on the awareness of constant conjunctions of objects, make human beings draw inferences (p. 28). This response is known as conventionalism. According to Hume (p. 29), custom is the 'great guide of human life', and without custom or habit, those who are guided only by Pyrrhonian doubt will 'remain in a total lethargy, till the necessities of nature, unsatisfied, put an end to their miserable existence' (p. 110). Reason is the slave of our passions.

A pinch of Pyrrhonian doubt remains useful, because it makes investigators aware of their fallibility (p. 112). The fact that one cannot obtain absolute certainty by human reasoning does not imply universal doubt, but only suggests that researchers should be modest (*Letter From a Gentleman*, p. 116). But many scientists will feel embarrassed by the conclusion that custom is the ultimate foundation of scientific inference. Watkins, for example, rejects it. However, conventionalism may be rationally justified. This has been attempted by some adherents of the probabilistic approach. Other strategies related to Hume's conventionalism are instrumentalism (developed by John Dewey) and pragmatism, or pragmaticism, as Charles Peirce christened it. These hold that hypotheses may be accepted and rejected on rational grounds, on the basis of utility or effectiveness. The pragmatic approach can be combined with the probabilistic strategy. But it is not free of problems. Most importantly, it is an invitation to scientific obscurantism (should a theory be useful to the learned – who qualifies? – or to the mighty?). A problem with conventionalism is to give an answer to the question 'where do these conventions come from?' and to provide a rational justification for the conventions (evolutionary game theory has been directed to this question). Lawrence Boland (1982; also 1989, p. 33) argues that neoclassical economists deal with the induction problem by adopting a conventionalist strategy. Econometricians base much of their work on another convention concerning the size of a test: the well known 5% significance level. This

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convention has its roots in a quarrel between Karl Pearson and R. A. Fisher, two founders of modern statistics (see chapter 3, section 3.2).

4 Apriorism

The *apriorist strategy* to the problem of scepticism denies proposition (i), concerning the absence of synthetic *a priori* truth. Immanuel Kant invented this notion of *a priori* synthetic truth, true knowledge that is both empirical and based on reasoning. It is neither analytic nor synthetic.[4] The canonical example of an *a priori* synthetic truth is Kant's Principle of Universal Causation, which is his response to Humean scepticism. Kant argued that everything must have a cause: 'Everything that happens presupposes something upon which it follows in accordance with a rule' (translated from *Kritik der reinen Vernunft*, Kant's most important work, published in 1781; in Krüger, 1987, p. 72). This doctrine is also known as causal determinism, or simply as causalism (Bunge [1959] 1979, p. 4).

Unlike Hume, John Stuart Mill endorsed Kant's principle: for Mill, induction is the search for causes. Mill distinguishes four 'canons of induction', given in his *Logic*, III (viii); Mill [1843] 1952):

- the method of agreement;
- the method of difference;
- the method of residues;
- the method of concomitant variations.

These methods are based on the 'principle of uniformity of nature', which holds that the future will resemble the past: the same events will happen again if the conditions are sufficiently similar. The method of difference starts from the premise that all events have a cause. The next step is to give an exhaustive list of possible causes, and select the one(s) which always occurs in common with the event, and does not occur if the event does not occur. A problem is to select this exhaustive list of possible causes.

Keynes ([1921] CW VIII, p. 252) refers to the principle of uniformity of nature in his discussion of reasoning by analogy, and suggests that differences in position in time and space should be irrelevant for the validity of inductions. If this principle forms the basis for induction, it cannot itself be founded upon inductive arguments. Furthermore, it is doubtful that experience validates such a strong principle. Nature seems much more erratic and surprising than the principle of uniformity of nature suggests. Still, the late philosopher Karl Popper ([1935] 1968, p. 252) explicitly argues that 'scientific method presupposes *the immutability of natural processes*, or the "principle of the uniformity of nature" '.

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Likewise, Bernt Stigum (1990, p. 542) argues that this principle is a necessary postulate of epistemology. Some probability theorists advocate a statistical version of this synthetic *a priori* truth: the stability of mass phenomena (see in particular the discussion of von Mises in chapter 3, section 2).

In the social sciences, it is not the uniformity of nature which is of interest, but the relative stability of human behaviour. A more apt terminology for the principle would then be the 'principle of stable behaviour'. Consider the axioms of consumer behaviour. If one assumes that preferences are stable (Hahn, 1985, argues this is all the axioms really say), then accepting these axioms as *a priori* truths warrants inductive generalizations. This principle solves, or rather, sidesteps, the Humean problem. If it is accepted, generalizations from human experience are admissible. But again this postulate is doubtful. Too frequently, humans behave erratically, and on a deeper level, reflexivity (self-fulfilling prophecies) may undermine uniform regularities in the social sciences. It suffers from the same problems as the principle of uniformity of nature: either it is false, or its justification involves infinite regress. But a weaker principle of stable behaviour may be accepted, by giving a probabilistic interpretation to the generalization. There should be an appreciable (non-zero) probability that stable behaviour may be expected. This is the basis for rational behaviour. A fair amount of stability is also necessary (not sufficient) for scientific inference: otherwise, it is impossible to 'discover' laws, or regularities.

It is hard to imagine interesting *a priori* synthetic truths specific to economics. The axioms of consumer behaviour are not generally accepted as true. An investigation of their validity cannot start by casting them beyond doubt (chapter 8 provides a case history of 'testing' consumer demand theory). Bruce Caldwell (1982, p. 121) discusses praxeological axioms of Austrian economists as an example of Kant's *a priori* synthetical propositions. The Austrian Friedrich von Wieser argued that a cumbersome sequence of induction is not needed to establish laws in economics. He claimed (cited in Hutchison, 1981, p. 206) that we can 'hear the law pronounced by an unmistakable inner voice'. Ludwig von Mises made apriorism the cornerstone of his methodology. The problem of this line of thought is that inner voices may conflict. If so, how are we to decide which voice to listen to?

5 Conjecturalism

The *conjecturalist strategy* denies Watkins' proposition (ii) and instead holds that scientific knowledge is only negatively controlled by experi-

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ence: through falsification. Popper provided the basic insights of the conjecturalist philosophy (also known as methodological falsificationism) in his *Logik der Forschung* in 1934 (translated as Popper, [1935] 1968). This nearly coincides with one of the first efforts to test economic theory with econometric means (Tinbergen, 1939b). Followers of Popper are, among others, Imre Lakatos and Watkins. I will first discuss Popper's views on inference, then Lakatos' modified conjecturalism.

5.1 Popper's conjecturalism

Popper's impact on economic methodology has been strong. Two pronounced Popperians in economics are Mark Blaug (1980) and Terence Hutchison (1981). Moreover, statisticians and econometricians frequently make favourable references to Popper (Box, 1980, p. 383, n.; Hendry, 1980; Spanos, 1986) or believe that Popper's is 'the widely accepted methodological philosophy as to the nature of scientific progress' (Bowden, 1989, p. 3). Critics claim that the real impact of Popperian thought on economic inference is more limited (see also De Marchi, 1988; Caldwell, 1991).

5.1.1 Falsification and verification

Scientific statements are those which can be refuted by empirical observation. Scientists should make bold conjectures and try to falsify them. This is the conjecturalist view in a nutshell. More precisely, theories are thought of as mere guesses, conjectures, which have to be falsifiable in order to earn the predicate scientific. The *modus tollens* (if p, then q. But not-q. Therefore, not-p) applies to scientific inference – if a prediction which can be deduced from a generalization (theory) is falsified, then that generalization itself is false. The rules of deductive logic provide a basis for scientific rationality and, therefore, make it possible to overcome the problems of Humean scepticism. Falsifiability distinguishes science from non-science (the demarcation criterion). The growth of knowledge follows from an enduring sequence of conjectures and refutations. Theories are replaced by better, but still fallible, theories. Scientists should remain critical of their work.

So far, there seems not much controversial about the conjecturalist approach. The tentative nature of science is a commonplace. Popper went beyond the commonplace by constructing a philosophy of science on it, methodological falsificationism. A source of controversy is Popper's critique of logical positivism, the philosophy associated with the *Wiener Kreis*.[5] A related source is his obnoxious rejection of induction.

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Logical positivism holds that the possibility of empirical *verification*, rather than falsification, makes an empirical statement 'meaningful' (the meaning lies in its method of verification). There are many problems with this view, but Popper aimed his fire at an elementary one: affirmative universal statements, like 'all swans are white', are not verifiable. In response to Popper's critique, Carnap dropped the verifiability criterion and started to work on a theory of confirmation (see also chapter 4, section 3.1). Again, this theory was criticized by Popper.

The logical difference between verification and falsification is straightforward. The observation of a white swan does not imply the truth of the claim 'all swans are white'. On the other hand, observing a black swan makes a judgement about the truth of the claim possible. In other words, there is a logical asymmetry between verification and falsification. This asymmetry is central to Popper's ideas: 'It is of great importance to current discussion to notice that falsifiability in the sense of my demarcation principle is *a purely logical affair*' (Popper, 1983, p. xx; emphasis added). This logical affair is not helpful in guiding the work of applied scientists, like econometricians. It should have real-world implications. For this purpose, Popper suggests the crucial test, a test that leads to the unequivocal rejection of a theory. Such a test is hard to find in economics.

According to Popper, it is much easier to find confirmations than falsifications. In the example of swans this may be true, but for economic theories things seem to be rather different. It is not easy to construct an interesting economic theory which cannot be rejected out of hand. But if verification does not make science, Popper needs another argument for understanding the growth of knowledge. Popper ([1935] 1968, p. 39) bases this argument on severe testing:

> there is a great number – presumably an infinite number – of 'logically possible worlds'. Yet the system called 'empirical science' is intended to represent only *one* world: the 'real world' or 'world of our experience' . . . But how is the system that represents our world of experience to be distinguished? The answer is: by the fact that it has been submitted to tests, and has stood up to tests.

Experience is the sieve for the abundance of logically possible worlds. The difference with induction results from a linkage of experience with falsifications: experience performs a *negative* function in inference, not the positive one of induction.

Popper's idea that the truth of a theory cannot be proven on the basis of (affirming) observations, is not revolutionary – indeed, it basically rephrases Hume's argument. Obviously, it was known to the logical positivist. And it had already been a common-sense notion in the statistical literature for ages (in fact, Francis Bacon had already made the

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argument, as shown by Turner, 1986, p. 10). One can find this, explicitly, in the writings of Karl Pearson, Ronald Aylmer Fisher, Harold Jeffreys (see the epigraph to this chapter), Jerzy Neyman and Egon Pearson,[6] Frederick Mills, Jan Tinbergen,[7] Tjalling Koopmans (1937) and probably many others. They did not need philosophical consultation to gain this insight, neither did they render it a philosophical dogma according to which falsification becomes the highest virtue of a scientist.[8] Econometricians are, in this respect, just like other scientists: they rarely aim at falsifying, but try to construct satisfactory empirical models (see Keuzenkamp and Barten, 1995). Of course, 'satisfactory' needs to be defined, and this is difficult.

Jeffreys' epigraph to this chapter can be supplemented by a remark made by the theoretical physicist Richard Feynman (1965, p. 160): 'guessing is a dumb man's job'. A machine fabricating random guesses may be constructed, consequences can be computed and compared with observations. Real science is very different: guesses are informed, sometimes resulting from theoretical paradoxes, sometimes from experience and experiment. Jeffreys argues that one may agree with Popper's insight that confirmation is not the same as proof, without having to conclude that confirmation (or verification) is useless for theory appraisal, and induction impossible.

5.1.2 The crucial test

An important example to illustrate Popper's ([1935] 1968) methodological falsificationism is Einstein's general theory of relativity, which predicts a red shift in the spectra of stars.[9] This is the typical example of a prediction of a novel fact which can be tested. Indeed, a test was performed with a favourable result. But Paul Feyerabend (1975, p. 57, n. 9) shows that Einstein would not have changed his mind if the test had been negative. In fact, many of Popper's examples of crucial tests in physics turn out to be far more complicated when studied in detail (see Feyerabend, 1975; Lakatos, 1970; Hacking, 1983, chapter 15, agrees with Lakatos' critique on crucial tests, but criticizes Lakatos for not giving proper credit to empirical work).

For several reasons, few tests are crucial. First, there is the famous 'Duhem–Quine problem'. Second, in many cases rejection by a 'crucial test' leaves the researcher empty handed. It is, therefore, unclear what the implication of such a test should be – if any. Third, most empirical tests are probabilistic. This makes it hard to obtain decisive inferences (this will be discussed below).

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