

Missing the Target. The Unhappy Story of the Criticisms of Falsificationism

David Miller
Department of Philosophy
University of Warwick
COVENTRY CV4 7AL UK
<http://www.warwick.ac.uk/go/dwmiller>

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Abstract

A few days after the twentieth anniversary of the death of Karl Popper (September 17, 1994), and a few days before the fiftieth anniversary of my first meeting with him (at the beginning of the Autumn term at the LSE in October 1964), my thoughts turn again to his most glorious successes in the epistemology and methodology of science, namely his subtle resolutions of the problems of demarcation and induction. In the eighty years that have elapsed since the presentation of these ideas in the original German text (*Logik der Forschung*) of *The Logic of Scientific Discovery* countless criticisms, large and small, have been adduced. Several of these criticisms undoubtedly expose defects that need correction, but it seems to me that almost all the black marks awarded are mere minor stains that are easily washed away. This applies especially to all those objections that depend essentially on the thesis that our knowledge, in order to be valid, demands some justification, be it partial and inconclusive. Although I have no hope that I shall be able to open eyes already closed, I plan to examine in this lecture some criticisms, recent and less recent, that have not to date been adequately dealt with. After all, an occasion designed to honour the memory of Karl Popper, and to celebrate his intellectual achievement, provides an opportunity, even an obligation, to expound his beautiful ideas as simply as possible, and to explain why so many of the prevailing criticisms miss their target. Nothing shows that Popper's ideas are one and all correct, since they are not all correct, but I shall perhaps be able to make it evident that most of them are not mistaken in the respects in which they are commonly supposed to be mistaken.

A somewhat free English translation of the text (Miller 2015) of a lecture prepared for the colloquium *El Legado de Karl Popper: a Veinte Años de su Muerte* [*The Legacy of Karl Popper: Twenty Years after his Death*], which took place at the UNIVERSITY OF VALPARAÍSO on September 22 and September 23, 2014. Several errors have been corrected in the translation, and several additions made (in particular the exquisite quotation from Minto 1893 in the final section). Both the translation, and its original, include some extended passages from two texts that, at the time of the colloquium, had not been published. One (now Miller 2014) was the outcome of a lecture at the congress *Rethinking Popper*, which took place in Prague from September 10 to September 14, 2007 at the ACADEMY OF SCIENCES OF THE CZECH REPUBLIC; the other was the script of a lecture at the *IV Congreso Colombiano de Filosofía*, which took place in Manizales from September 24 to September 28, 2012 at the UNIVERSITY OF CALDAS. For valuable critical comments, both philosophical and linguistic I am grateful to Diego Rosende, Alexander Gómez, Nélica Gentile & Rodolfo Gaeta, Carlos Verdugo, and Rodrigo López. None of them is to be implicated in the outcome. I acknowledge also the use that I have made of material in the Karl Popper Archive at the HOOVER INSTITUTION ON WAR, REVOLUTION, AND PEACE at STANFORD UNIVERSITY and in the KARL POPPER LIBRARY at the ALPEN-ADRIA UNIVERSITY in Klagenfurt, and I thank the KARL POPPER COPYRIGHT OFFICE for permission to include a quotation from an unpublished letter (here cited as Popper 1964).

0 Introduction

I was enormously honoured to receive an invitation from the University of Valparaíso to participate in the commemoration of the death twenty years ago of Karl Popper, my teacher and my friend. For me it is not only a sad event whose anniversary we have the opportunity to mark today, but also something more joyful, my first exciting meeting with Karl, almost exactly fifty years ago at the beginning of the autumn term in October 1964 at the LSE. If I am able to stimulate my audience today one fiftieth as much as I was stimulated by that face-to-face meeting, and innumerable many others that followed, I shall have achieved something worthwhile.

The organizers of this colloquium have suggested that I should devote my contribution to an evaluation of the provocative and, in the eyes of many, disconcerting, innovations with which Popper has enriched the philosophy of science, and more generally the theory of human knowledge. Since these involve issues that are both wide and deep, and moreover the members of my audience hail from equally varied backgrounds, I am obliged to be quite selective. On this account I shall say almost nothing about what is called the philosophy of the special sciences, that is, the natural sciences, mathematics, and all those activities that are assembled optimistically under the banner of the social sciences. I can do no more here than register some scattered examples of the considerable impact that Popper's ideas have had in the fields of quantum theory (Shields 2012, Qureshi 2012), thermodynamics (Esfeld 2006), cosmology (Kragh 2012), evolutionary theory (Niemann 2014, Noble 2014, Vecchi & Baravalle 2015), economics (de Marchi 1988, Boland 2006, Boyland & O'Gorman 2007), sociology (Adorno & al. 1976), education (Bailey 2000, Swann 2012, Chitpin 2016), and other disciplines. I regret especially that there is neither space nor time to present some of the original ideas with which Popper illuminated the theory of probability and its applications. (These ideas are discussed in some detail in Miller 2016.) You may be sure that there is much more to be said than I could say in twenty, or even fifty, commemorative lectures.

1 Demarcation and Induction

All those who have studied even a fragment of the philosophy of science of the previous century will without hesitation link the name of Karl Popper with two striking and unorthodox ideas:

- the proposal that what makes a theory or a hypothesis suitable for scientific investigation is above all its susceptibility to be shown to be false, in short its *falsifiability*,

and

- the insistence that science does not depend on inductive inference, and has no use for it, but performs all its reasoning using only *deductive logic*.

The *criterion of falsifiability* is the solution that Popper proposed to the *problem of demarcation*, which he described as ‘the central problem of the theory of knowledge’ (1930–1933, §1) and nicknamed *Kant's problem*. As for the *problem of [the justification of] induction*, called by Kant *Hume's problem*, which has been widely considered to be the fundamental problem of modern philosophy of science, Popper offered a solution that is both simple and compelling: since there is no need to use induction in science, its justification is of no urgency. Something, however, must be added to this brusque dismissal, for until Hume's demolition of its pretension to correctness, induction had been understood by everyone to be the custodian of scientific rationality. Popper's great achievement was to elaborate a theory of rationality, known as *critical rationalism*, that is purely deductive, both in its spirit and in its execution.

Popper consistently placed at the centre of his philosophy of science his solutions to these two problems, especially Kant's problem. He wrote concerning Lakatos's thesis that ‘*exactly the*

most admired scientific theories simply fail to forbid any observable state of affairs', which is equivalent to a denial of falsifiability in some of the most important cases: 'Were the thesis true, then my philosophy of science would not only be completely mistaken, but would turn out to be completely uninteresting' (1974, p. 1005). He was not less loyal to his deductivist transformation of Hume's problem, which includes a painstaking explanation of the complementary roles of reason and imagination in the development of science and of human knowledge. Up to his death he continued to write about the impossibility and the uselessness of induction (Popper 1984b, Popper 1994b; Popper & Miller 1983, 1987, 1994) and to take delight in a science freed of the fetters of inductivism.

In Popper's hands the two problems, the problem of demarcation and the problem of induction, were intimately linked. With some justice he maintained that the former problem had traditionally been dealt with by the assertion that science — and more generally, all our knowledge of the empirical world — consists of the results of observations together with whatever conclusions we manage to infer from them correctly by means of induction. That response becomes wholly unacceptable in the event that Hume's condemnation of induction is valid. If both Hume's sceptical conclusion and the traditional picture of science are correct, then science is deprived of all rational support, and consists only of our observations and of the expectations imprudently and precariously built upon them. The ambitious theories constructed by Copernicus, Kepler, Galilei, and Newton are shown up as not as intellectual fortresses but as imaginary castles in the air.

Inspired by the example of Einstein's special theory of relativity, Popper accepted this conclusion unflinchingly. He accepted too the correctness of Hume's destructive argument; yes, the hypotheses and theories of science and of everyday life possess no rational foundation in experience. On the contrary, they are sheer conjectures, the fruits of the untrimmed imagination. Nonetheless Popper rejected the associated conclusion that learning, the cultivation of new knowledge, is an activity in which reason plays no part. Reason has nothing at all to do with the germination of a hypothesis, but comes into action as soon as it enters the process of evaluation and cross-examination (which may happen long before it has been properly formulated). Nobody believes that because the conjectures arrive first, they have an automatic right of residence independently of whether or not they describe the world correctly. It follows that if our objective is the discovery of what is objectively true and what is objectively false, we are obliged to examine carefully every conjecture that is put forward. This job is done most directly by means of an analysis of the logical consequences of the conjectures, since a conjecture that implies something false is itself false. To the extent that a conjecture tries to say something about the empirical world, that is, the world of our shared experience, it has to have consequences that could, in some conceivable circumstances, reveal themselves to us as false. This is the core of the criterion of falsifiability. A worthwhile empirical investigation of a hypothesis must entertain in its range of judgements not only pats on the back but also kicks in the teeth.

2 Conjectures and Refutations

This is a concise summary of the philosophy of *conjectures and refutations*, which Popper has discussed with a wealth of detail in several places, especially (1957), (1972), Chapters 1 and 2, (1974), §§ 1–19, and (1983), Part I, Chapter 1. The doctrine has an epistemological and descriptive component, which may be called *fallibilism*, according to which our knowledge rests on no basis, neither sensation nor intuition; its integrity is maintained by its success in independent tests, and not by any dependence on a trustworthy foundation. There is in addition a methodological and prescriptive component, which may be called *falsificationism*, according to which we must not relax for a single moment either the elaboration of bold conjectures nor

the task of exposing and eliminating the errors lurking within them. We must not be frightened of errors, but we must do everything possible to unmask them. The following suggestive words are taken from the Author's note on p. xlvii of the English edition of Jung's *Psychology of the Unconscious* (1912):

I am not in sympathy with the attitude which favours the repression of certain possible working hypotheses because they are perhaps erroneous, and so may possess no lasting value. Certainly I endeavoured as far as possible to guard myself from error, which might indeed become especially dangerous upon these dizzy heights, for I am entirely aware of the risks of these investigations. However, I do not consider scientific work as a dogmatic contest, but rather as a work done for the increase and deepening of knowledge.

Every investigation consists in effect of a painstaking give-and-take of acute conjectures and blunt refutations. It is, however, rarely difficult to distinguish the inventive phases, the *conjectures*, from the rational phases, the *refutations*, with which they are closely intertwined.

This may be a good moment to comment on the common complaint that the English title of Popper's book, *The Logic of Scientific Discovery* (1959), 'is a mistranslation' of the original German title, *Logik der Forschung* (1934), and is 'incompatible with Popper's thesis that there *is no* logic of scientific discovery' (Haack 2013, Chapter 12, note 1; for the one accusation see also Aliseda (2014), § 1.3, and for the other, Bogen 2013, § 1). The first observation seems to me to be uninteresting. Popper cannot have supposed that the English title is a literal translation — that is to say, a translation that could have been offered by a translator who had not even opened the book — of the German title (whose literal translation into English is *The Logic of Research*). Besides, there exists no precept that the title of a translation must be a translation of the title. For a stark counterexample we need look no further than *La responsabilidad de vivir* (1998), the Spanish translation of the book by Popper that goes under the title *Alles Leben ist Problemlösen* (1994d) in German and *All Life Is Problem Solving* (1999) in English. Observe that the Spanish title does not even belong to the same grammatical category as the other titles (which would be translated into Spanish as *Toda vida es solución de problemas*). Sometimes a title may mutate in the journey across the Atlantic: P. G. Wodehouse's book *Ukridge* (which is a proper name), published by Herbert Jenkins in London in 1924, changed into *He Rather Enjoyed It* when George H. Doran published it in New York in 1925. Other examples in the Wodehouse canon include *Three Men and a Maid*, published in 1922 by Doran, whose British title is *The Girl on the Boat*. My anthology *A Pocket Popper* (1983) became *Popper Selections* (which is now its title throughout the English-speaking world) when it was issued by Princeton in 1985 for the north-American market. Nothing more needs to be said.

In the second paragraph of his (1959) Popper had decided to translate *Forschungslogik* as *logic of scientific discovery*. It is plain that he considered that this label captures better than does *logic of research* the meaning of the expression *logic of knowledge* (*Erkenntnislogik*), which he also used. He suggested that it is 'the task of the logic of scientific discovery, or the logic of knowledge, ... to analyse the method of the empirical sciences', which he had described a few lines earlier in the words: 'A scientist ... proposes statements — or systems of statements — and tests them step by step' (1934, proem to Chapter 1). Note that Popper includes in the process of discovery both the proposing of statements and the testing of statements. We must therefore distinguish firmly the complex process of *discovery* from the process of *invention*, which Popper characterized as 'the initial stage, the act of conceiving or inventing a theory, [which] seems to me neither to call for logical analysis nor to be susceptible of it' (*ibidem*, § 2). Musgrave (2011), pp. 218f., citing McLaughlin (1982), p. 71, and Aliseda *op.cit.*, § 1.1, sanction this distinction, in terms of which we may say that Popper denied the possibility of a logic of invention, but endorsed the logic of discovery as something purely critical and purely deductive. In § 8 I shall

say a little about Aliseda’s attempt to develop an ampliative logic of discovery. In any case, the judgement of Haack (and others) quoted above is a jejune oversimplification. Although some of the phases of the process of discovery are not regulated logically, this by no means implies that ‘there *is no* logic of scientific discovery’. Many aspects of the process of discovery (or investigation) are ethically neutral (in the words of Ramsey 1926, p.177), but there exists an ethics of investigation.

It ought perhaps to be made clear that in this context the expression *logic of discovery* always refers to an inferential process, or to the rules appropriate to such a process. It should not be confused with the expression *situational logic*, which is much broader and applies to almost all our conscious activities. Indeed, even invention can be analysed in terms of situational logic (which is better called *situational analysis*). According to Popper we cannot explain logically the creative act of an investigator, but his creative activity can be explained in principle in terms of the ‘objective problem situation’, as he dubs it in Chapter 4, § 7 of *Objective Knowledge* (1972). The method of *trial and error* (or conjectures and refutations) can be identified with situational logic in the state of ignorance (Miller 2006b, § 2). We must resist any temptation to assimilate it to any logic of inference.

Unlike the majority of present-day philosophers I think that Popper’s solutions to the problems of demarcation and induction are fundamentally correct, despite a few blemishes that need attention and modification. In Chapter 2 of my *Critical Rationalism* (1994), the solution to Hume’s problem was defended against an assortment of chronic objections to the effect that induction is being sneakily appealed to. As for the so-called *pragmatic problem of induction*, which, in the opinion of many philosophers, is the point at which the philosophy of conjectures and refutations is at its most vulnerable, I agree with his many critics that Popper’s proposed solution in Chapter 1, § 9, of (1972) does not work well, and in Chapter 5 of *Out of Error* (2006a) I have tried to repair it in line with the discussion in his (1974), § 14 (reprinted in § x of selection 7 of Miller 1983). More can be said about this problem, which to my mind does not deserve to be classified as one of the main realizations of the problem of induction; for this point I must refer you to § 2.3 of my (2014) and to a still unpublished work entitled ‘Deductivist Decision Making’ (2011). In Chapter 4 of *Out of Error*, and in an abbreviated but improved version (2007a) that appeared in *Philosophia Scientiæ*, I tried to explain how natural is the requirement, embodied in the criterion of falsifiability, that only those hypotheses that are empirically falsifiable deserve to be investigated empirically. Only someone who believes in the powers of the inductive method — powers that are utterly mysterious to me — could suppose otherwise. I do not want to repeat here the content of these articles. There are, however, some strident criticisms that have not yet been dispatched with sufficient firmness. A little more must accordingly be said in defence of the beautiful philosophy that Popper bequeathed to us.

3 The Real Problem of Demarcation

My thesis that the problem of demarcation is to be solved more or less as Popper solved it may seem surprising to those who are acquainted with such titles as ‘The Demise of the Demarcation Problem’ (Laudan 1983) and ‘The Degeneration of Popper’s Theory of Demarcation’ (Grünbaum 1989), or the writings of Kuhn (1962) and Lakatos (1973, 1974). But like many others, the authors of these criticisms have thoroughly mistaken the crucial philosophical task that Popper intended a criterion of demarcation to perform. Its task is not to ‘distinguish scientific and non-scientific matters in a way which exhibits a surer epistemic warrant or evidential ground for science than for non-science’, laid down by Laudan (p.118) as a minimal condition for ‘a *philosophically* significant demarcation’, nor is it ‘to explicate the paradigmatic usages of “scientific” ’ (*ibidem*, p.122). Questions of sureness, warrant, and grounds, are of interest

principally to justificationists who live in mighty dread that they may not be ‘entitled to *believe* any scientific theories’ (Papineau 2006, p. 63); questions of usage, classification, and status, are of interest principally to essentialists, to philosophers who prefer to pursue philosophy unphilosophically, and to educational administrators; and inevitably, of course, to lawyers. Contrary to what Grünbaum resolutely supposed, the problem of demarcation is only incidentally concerned to ratify the unscientific status of psychoanalytic theory (whatever psychoanalytic theory is taken to be), and contrary to what Lakatos likewise supposed, it is only incidentally concerned to ratify the scientific status of Newton’s theory (whatever Newton’s theory is taken to be). These classifications were incontrovertibly among Popper’s aims, and it is important to know whether they have been accomplished. But the main problem of the theory of knowledge, at least for an empiricist, is quite different in kind. This problem, described by Popper also as ‘the main problem of philosophy’, is ‘the critical analysis of the appeal to the authority of experience’ (Popper 1934, § 10). A few pages earlier (*op.cit.*, § 4) he had written: ‘my business . . . is not to bring about the overthrow of metaphysics . . . [but] to formulate a suitable characterization of empirical science . . . in such a way that we shall be able to say of a given system of statements whether or not its closer study is the concern of empirical science.’ However this problem is understood, it is an abstract problem that is not directly concerned with any of the celebrated scientific and pseudoscientific theories of previous centuries.

I therefore have some doubt that the substance of Popper’s protest quoted above, that if Lakatos’s thesis that ‘*exactly the most admired scientific theories simply fail to forbid any observable state of affairs*’ were true, then ‘my philosophy of science would not only be completely mistaken, but would turn out to be completely uninteresting’ should be taken literally, despite its being true (because it is a conditional whose antecedent, what Lakatos asserted, is a monstrously false exaggeration). I shall return to this point below.

These popular misreadings of the problem of demarcation are to no small extent excusable, I am sorry to say, since Popper himself often introduced the problem by means of a comparison between Einstein’s general theory of relativity on the one side, and those of Freud, Adler, and Marx, on the other (1957, § II); and in a rather prominent place he presented it as ‘an urgent personal problem . . . [that] I did not first think of as a philosophical problem’ (1974b, p. 976). More often than not he extolled falsifiability as a criterion of what is scientific rather than of what is empirical, a misplacement of emphasis of which many of us have sometimes been guilty, but in truth an insignificant distinction before the publication of Kuhn’s book *The Structure of Scientific Revolutions* (1962) — think of *Tractatus* ¶ 4.11: ‘The totality of true proposition is the whole of natural science . . .’ (Wittgenstein 1921) —, but a source of confusion ever since. At a more scholarly level, however, emphatically justificationist, essentialist, and naturalistic misreadings are inexcusable. Popper’s philosophy is potently and expressly opposed to all these fashionable tendencies, and to all visions of science as ‘a body of knowledge’ (1952, Chapter 11, note 6) exciting awe and deference and enjoying magisterial authority (1983, Part I, § 33), and he should have been given the credit for understanding the problem of demarcation in a manner that did not so sympathetically subscribe to these philosophical solecisms. A recent book, *Philosophy of Pseudoscience. Reconsidering the Demarcation Problem* (Pigliucci & Boudry 2013), continues to endorse the naturalistic approach, and contains many articles (for example, Hansson 2013) that perpetuate the untruth that scientific theories are reliable and justified, but the book deserves credit for repudiating the essentialist approach followed by Laudan.

Not wisely overlooked is the following passage, which occurs at the end of Popper’s analysis (1983, Part I, § 18) of parts of Freud’s *The Interpretation of Dreams* (it differs only stylistically from a passage in the draft of the *Postscript* from the 1950s that is preserved in the Hoover Institution Archives [235, 15]). It indicates that my own reading of the problem of demarcation

is not a petulant product of my imagination:

In the present context, it hardly matters whether or not I am right concerning the irrefutability of any of these three theories [those of Freud, Adler, and Marx]: here they serve merely as examples, as illustrations. For my purpose is to show that my ‘problem of demarcation’ was from the beginning the practical problem of assessing theories, and of judging their claims. It certainly was not a problem of classifying or distinguishing some subject matters called ‘science’ and ‘metaphysics’. It was, rather, an urgent practical problem: under what conditions is a *critical appeal to experience* possible — one that could bear some fruit?

Here is a clear philosophical, even logical, problem: under what circumstances is an empirical investigation worth undertaking? The solution is also clear: since the formulation of a hypothesis, its acceptance as a candidate for the truth, must precede its consideration, the task of an empirical investigation cannot be to promote hypotheses, but only to demote them. Empiricism demands that a hypothesis be retained unless it clashes in an appropriate way with experience. An accepted hypothesis therefore remains accepted until it is rejected. No further action is called for (Miller 2006a, Chapter 4, § 1; 2007, § 1).

I hope that a logician may be forgiven for according this logical problem, and its solution, prominence over partly factual inquiries (whose interest I do not care to contest) concerning how, and to what extent, specific theories (such as classical mechanics, the theory of evolution, psychoanalysis, parapsychology, and the doctrine of intelligent design) can be investigated empirically. Despite the unambiguousness of the above passage from the *Postscript*, Grünbaum insinuates that ‘it insouciantly repudiates ... [the] major, central tenet of his [Popper’s] whole philosophy’ (*op.cit.*, § III; but see also § IV, p. 157, where he affects to recognize that ‘the actual falsifiability of psychoanalysis does not entail that Popper’s ... demarcation ... [is] in trouble’), and goes on to demand, as if it mattered much, ‘what *other* theories for which scientificity has been wrongly claimed can be adduced to furnish such a vindication *vis-à-vis* the much older criterion of evidential support, which he wants to replace as unduly permissive?’ (p. 156). I suggest that one need only browse awhile in the annals of pseudoscience. What is patently absent from Grünbaum’s advocacy of a ‘criterion of evidential support’, and all its inductivist and justificationist congeners, is any explanation of the objective advantage that is imagined to accrue to a hypothesis when it is empirically supported. That you learn anything when you are told something that you already know is a fairy tale that Popper’s solution to the problem of demarcation ruthlessly discards.

4 Duhem’s Thesis

The criterion of demarcation, in short, is not a thesis about the history of science, nor a thesis about the history of pseudoscience or the history of metaphysics. It is not an empirical or factual thesis. It is not refuted by examples of unfalsifiable hypotheses that have captured the interest of scientists. It would be puzzling, to be sure, if the popular form of Duhem’s thesis, which Quine generalized and Lakatos appropriated, the thesis that, at least in physics, falsification is an exception rather than the rule, were a faithful description of the principal theories of science and of the problems that they encountered. Like others (for example, Božić 2010) I see this as an exaggeration. I do not dispute that many of the statements that belong to a rich theory like celestial mechanics are unfalsifiable if considered individually. Despite what Popper maintained in his exposition of ‘the fourth stage’ of the problem of induction (1983, Part I, § 5), I incline towards including such statements within science, but with a modified form of membership. Elsewhere (1994, pp. 10f., 89f.; see also Sceski 2007, p. 71) I have likened the status of an unfalsifiable consequence of a falsifiable theory to a person, for example a member of the family of an accredited diplomat’s family, to whom we offer by courtesy some privileges

and immunities that are, strictly speaking, due only to some other person. In the event that an ambassador, because of bad behaviour, loses his right to remain in the country to which he was posted, the members of his family are required to depart along with him, despite their having done nothing amiss. In the same way, Newton's three laws of motion, for example, and the law of gravitation, are to be classified as scientific when they are constituents of a more exuberant theory that is open to empirical refutation, and are rejected whenever that theory is rejected. Nothing prevents the retention, or re-emergence, in a new falsifiable theory, of a hypothesis that is rejected in this way. Indeed, some relatively uninformative hypotheses, such as *Matter exists*, and especially logical truths, may survive any number of rebuffs, and their elimination may effectively be impossible. Nonetheless, a hypothesis (for instance, a piece of idle metaphysics) that comes to all the parties but never participates in any of the games, will eventually stop receiving invitations.

It is one thing to treat these unfalsifiable consequences as scientific by courtesy, but something else to give them any credit in the event that the host theory passes a test — that is, when it is corroborated. Popper's suggestion that 'if a theory has been well tested, then we can say that its high degree of corroboration applies also to the statements that it implies' (1972, Chapter I, § 8(4)) was seized on avidly by Grünbaum (*ibidem*) in order to back up his thesis that the criterion of falsifiability is unable to deal adequately with statements (such as *All men are mortal* and *Every metal has a melting point*) that contain mixed quantification, a challenge thrown out lightly by Carnap (1936–1937, § 25), unemotionally by Hempel (1945), § 10(d) and by Maxwell (1974), § I, and more caustically by Carus (2007), pp. 34f. (see also Rosende 2009, pp. 139f.). I agree that this suggestion of Popper's was imprudent. In fact, in a letter (1964) in response to a criticism (1964) from L. J. Cohen, Popper himself stated decisively 'the important principle . . . [that] degree of corroboration is *not* generally transmitted from a premise to its conclusion'; this principle, which explicitly contradicts the *special consequence condition* of Hempel *op.cit.*, had been stated on Popper's behalf by Bartley (1961), p. 8. Grünbaum is surely correct that the 1972 suggestion, if adopted, would constitute 'a *major switch*, rather than an extension, of Popper's demarcation enterprise'. But no such major switch is needed in order to authorize the scientific status of *Every metal has a melting point*, which is a logical consequence of the statement *Every metal has a melting point below 10,000°C*. If the latter is corroborated, then it is not refuted; and the former is neither corroborated nor refuted. We must not suppose that science contains only corroborated statements. Once again, Popper himself said something quite similar: (1963, Chapter 10, § XVII): 'once a theory is refuted, its empirical character is secure and shines without blemish'.

5 Educational Aspects

The most incongruous use of the criterion of demarcation has been its artless arraignment in law courts in the USA in connection with the teaching of Darwinian theory, and of the theory of intelligent design, side by side in science classes in public schools. (For an up-to-date account of the controversy see Pennock & Ruse 2009.) Both sides of this dispute appear to adopt a vision of science as 'a body of knowledge' that is to be transmitted in the educational process. This vision, which is almost universal among those with a certain level of education, is in sharp disagreement with the vision of the open and exploratory mind advocated by critical rationalism. In note 6 to Chapter 11 (on Aristotle) in *The Open Society and Its Enemies* (1945) Popper wrote:

. . . in our day no man should be considered educated if he does not take an interest in science. The usual defence that an interest in electricity or stratigraphy need not be more enlightening than an interest in human affairs only betrays a complete lack of understanding of human affairs. For science is not merely a collection of facts about electricity etc.; it is one of the

most important spiritual movements of our day. Anybody who does not attempt to acquire an understanding of this movement cuts himself off from the most remarkable development in the history of human affairs. Our so-called Arts Faculties, based upon the theory that by means of a literary and historical education they introduce the student into the spiritual life of man have therefore become obsolete in their present form. There can be no history of man which excludes a history of his intellectual studies and achievements; and there can be no history of ideas which excludes the history of scientific ideas.

Without doubt Popper had in mind here some of the literary and historical courses taught at Canterbury University College, where he was teaching at that time. Having quoted the opinion of T. H. Huxley that a liberal education, far from inculcating ‘the power of seeing things as they are without regard to authority’, does its best to suppress it, Popper went on

I admit that, unfortunately, this is true also of many courses in science, which by some teachers is still treated as if it was ‘a body of knowledge’, as the ancient phrase goes. But this idea will one day, I hope, disappear; for science can be taught as a fascinating part of human history — as a quickly developing growth of bold hypotheses, controlled by experiment, and by *criticism*.

Writing in the 1950s what would be published only in 1983, in *Realism and the Aim of Science*, Volume I of his *Postscript*, Popper asserted (Part I, § 33):

Science is not only, like art and literature, an adventure of the human spirit, but it is among the creative arts perhaps the most human: full of human failings and shortsightedness, it shows those flashes of insight that open our eyes to the wonders of the world . . .

Kant challenges us to use our intelligence instead of relying upon a leader, upon an authority. This should be taken as a challenge to reject even the scientific expert as a leader, or even science itself. Science has no authority.

From this perspective it seems to me to be evident that the teaching of science ought to involve much more than the theories and facts of science, and should include a consideration of the controversies that provoked those theories and spurred an interest in those facts. Popper’s criterion of demarcation has beyond doubt a part to play in this campaign, but I do not see how it could be our sole guide. As early as his *Logik der Forschung* (1934), § 4, and later in the three volumes of his *Postscript*, especially Volume III, *Quantum Theory and the Schism in Physics*, Chapter IV, Popper stressed the importance of metaphysical ideas in the development of science. It is no less important in the teaching of science. I may add that some methodological ideas — falsificationist ideas of course — also deserve to be included in a good programme of scientific studies. The following fragment of dialogue from P. D. James’s 1977 detective novel *Death of An Expert Witness* (Chapter 8) is instructive.

‘He explained to me what science is about.’

‘And what is science about?’

‘He explained that scientists formulate theories about how the physical world works, and then test them out by experiments. As long as the experiments succeed, then the theories hold. If they fail, the scientists have to find another theory to explain the facts. He says that, with science, there’s this exciting paradox, that disillusionment needn’t be defeat. It’s a step forward.’

‘Didn’t you do science at school? I thought you’d taken physics and chemistry at O-level.’

‘No-one ever explained it like that before.’

‘No. I suppose they bored you with experiments about magnetism and the properties of carbon dioxide. . . .’

6 The Curse of False Security

In my book *Critical Rationalism* (1994), Chapter 2, §3, and again in a short article (2007b), I compared justification in epistemology with an addictive drug. Unless it is not used at all, no quantity is sufficient, simply because every justification demands further justification. It is of course possible to stop the infinite regress; it is possible to change the meaning of the word ‘justify’, in such a way that a hypothesis can be justified even though what justifies it itself needs no justification. This is the strategy of *reliabilism*, for example, if I understand well its contention that a hypothesis is justified if it is obtained by means of a reliable method or process. Reliabilism would be usefully clarified if its proponents were to distinguish between what Carnap (1950) might well have called *reliability*₁, which is an epistemological matter, a measure of confidence, and *reliability*₂, which is factual (and typically empirical), a measure of the success of the process in question. (On this distinction, without the neo-Carnapian terminology, see Miller 2006a, Chapter 5, §2, and Chapter 6, §1. A recognition that there is more than one sense of reliability is to be found in Chapter 5 of Foster & Huber 1997.) I am unable to discern any advantage gained by a hypothesis that is reliable in the reliabilist sense, that is, a hypothesis, which may not be true, that has achieved a high degree of *reliability*₁, in comparison with a true hypothesis. Nothing is therefore lost if all dependence on justification is abandoned, which is what critical rationalism counsels. In the great majority of cases we do not know for certain whether or not a hypothesis is true, but hypotheses that are justified in a reliabilist sense are no more comfortably situated. Since our aim is truth, a journey there via any process of justification is a pointless detour.

The comparison between justification and an illegal narcotic takes on a heightened appropriateness in the light of Bird’s imputation that ‘[it] is a feature of Popper’s philosophy . . . [that] when the going gets tough, induction is quietly called upon to help out’ (1998, p. 180). This is a topsy-turvy inversion of the truth, that when the inductivist authorities find their allegations falling apart, their typical reaction is quietly to plant on deductivism a recourse to induction (or some similar device of supposed justification), and then dramatically to expose it. This is nothing short of entrapment. Bird himself offers an example of this shabby behaviour when he considers an empirical investigation that reveals that the proportion in a sample of plants displaying a dominant trait is 74.881%. It is agreed that the hypothesis that the proportion in the population lies outside the interval [70%, 80%] has been falsified; accordingly, says Bird, ‘we have confirmed a relative of Mendel’s hypothesis, the hypothesis . . . that the proportion is 75 ± 5 percent [and] the confirmation clearly is inductive’ (*loc.cit.*). On the contrary, an honest report says only that the hypothesis has survived the test. The declaration that it is confirmed may add an appearance of induction, but no inference is involved, inductive or deductive. Before the investigation, that is, before the test, the hypothesis in question, and its negation, were candidates for the truth, and the result of the test was that one of them was rejected and the other was retained. Similar considerations apply to all empirical tests (Miller 1980, p. 154).

We should say the same in almost all those cases in which it is claimed that induction is needed, for example in the case of what is called *inference to the best explanation*. I do not deny that the falsificationist or critical rationalist also wants to identify the best explanation of the phenomena, but this is done (and only most tentatively) by eliminating its competitors, not by any slippery and unnecessary act of inductive inference. The candidates are already in the investigator’s pocket, and there is no need for them to be pocketed again. Refuted hypotheses have to be eliminated, because they are false, but there is no need to eliminate other possibilities precipitously.

It would not be unfair to draw my attention at this point to an apparent discrepancy between what I have just said and the theory of the empirical basis expounded by Popper in Chapter V of

The Logic of Scientific Discovery. I concede that there is a discrepancy, but I dare to suggest that this is one of the few points in that meticulously written book where it is possible to introduce a modest improvement. In §29, Popper describes a procedure in which a scientist, having conducted an experiment, or made an observation, decides to accept ‘some basic statement’. Neither the decision, nor the accepted basic statement, is justified in this process: ‘experiences can *motivate a decision*, and hence an acceptance or a rejection of a statement, but a basic statement cannot be *justified* by them, no more than by thumping the table’ (*loc.cit.*). I suggest that it would be more in the spirit of falsificationism to recognize that the scientist, in order to plan a test (experimental or observational), and especially the measurement of a physical quantity, needs to have in mind from the outset a set of basic statements that encompass a range of possible observational results. The effect of the test is to eliminate some, and perhaps all, of those statements. No basic statement is accepted in the light of the test, but many are rejected (unless the test was very carelessly designed).

In the absence of induction, the traditional problem of induction evaporates. Nonetheless, we need not stop our housekeeping here, since the problem can be disposed of as easily by renouncing the aim of justification. Despite the endless and fruitless philosophical attempts to dismiss his arguments, the sceptic is right to say that the demand for justification, or even for good reasons, or for ‘reasonable beliefs’, is in any field an illogical dream. I think that rather than struggle more against the possibility of justification, it is better to attack its alleged usefulness. It seems to me to be obvious that everything that we can achieve with it — except justification, of course — we can achieve without it (Miller 1994, Chapter 3, §4). I reject with all my heart the call of those, such as Mayo (2006), for critical rationalism to show how empirical tests lead to reasonable beliefs, that is, justified beliefs. Until someone explains the intellectual, as opposed to emotional, function, of giving a justification for an opinion (or a hypothesis, or a theory), we do well to have nothing to do with it.

The endurance of justificationist and positivist ideals despite the strength of the sceptical opposition demonstrates the value of the criterion of falsifiability, which shows with absolute clarity, though not with absolute precision (Popper 1974, §7), the only way in which empirical investigations can be expected to bear fruit: never positively, nor through confirmation, nor even through corroboration, but negatively, through refutation. There are many philosophers, nevertheless, though fewer scientists, who plainly have still to see the point. Haack, for example, in an unthinking lament steeped in antiquated scientism, complains that ‘in fact it never became entirely clear what, exactly, Popper’s criterion was, nor what, exactly, it was intended to rule out, nor . . . , what exactly, — besides the honorific use of “science” — the motivation was for wanting a criterion of demarcation in the first place’ (2013, p.111). The blindness of many justificationists to their own distortions is extraordinary.

7 Negativism

In the introduction to his discussion of verisimilitude in *Conjectures and Refutations*, Popper wrote: ‘I am at times inclined to classify philosophers as belonging to two main groups — those with whom I disagree, and those who agree with me. . . . The members of the first group — the verificationists or justificationists — hold, roughly speaking, that whatever cannot be supported by positive reasons is unworthy of being believed, or even of being taken into serious consideration. On the other hand, the members of the second group — the falsificationists or fallibilists — say, roughly speaking, that what cannot (at present) in principle be overthrown by criticism is (at present) unworthy of being seriously considered. (I have here compressed the first three paragraphs of §IX of Chapter 10 of Popper 1963 into one continuous passage.) He went on:

Considering their views about the positive or negative function of argument on science, the first group — the justificationists may also be nicknamed the ‘positivists’ and the second — the group to which I belong — the critics or the ‘negativists’. These are, of course, mere nicknames. Yet they may perhaps suggest some of the reasons why some people believe that only the positivists or verificationists are seriously interested in truth and in the search for truth, while we, the critics or negativists, are flippant about the search for truth, and addicted to barren and destructive criticism

This mistaken picture of our views seems to result largely from the adoption of a justificationist programme, and of the mistaken subjectivist approach to truth

Nonetheless, one of the most stubborn objections to the methodology of falsificationism is that it yields only negative conclusions, or that our knowledge consists exclusively of judgements that some theory or other is not true. In § 2 of Chapter 13 of my (2006a) I quoted this judgement of Burke (1983, p. 52): “Science is never in a position to go beyond what is said to have been one of H. A. Prichard’s favourite comments on his students’ essays — “Whatever may be the truth, that can’t be.”’. Admittedly, deductive inferences from “‘empirical reasons”, . . . that is, test statements or observation statements’ (in the words of Popper 1972, Chapter 1, § 5) do not yield anything positive, but inductive inference is not the solution. The principal purveyors of illumination in science are not the negative refutations but the positive conjectures. Here is another example of this misunderstanding.

. . . Popper’s philosophy may be dubbed *logical negativism*. . . This is why his philosophy, though readable and interesting, is rather shallow and fragmentary (unsystematic). It is . . . [according to this philosophy] more helpful to spot errors or wrongs than to search for truth or fairness.

Popper was undoubtedly right . . . in emphasizing the role of rational criticism in the management of social conflicts as well as in the pursuit of knowledge. But surely statements and proposals must be made before they can be subjected to critical examination: creation precedes criticism, just as trees preexist logs and sawdust. Besides, falsifying a proposition is the same as confirming its negation.

Thus spake Mario Bunge (2001), Chapter 7, § 5. The objection in the second paragraph that ‘creation precedes criticism’ may seem to be utterly platitudinous, something that neither Popper nor anyone else would deny. Indeed, it is said unequivocally in § 1 of *The Logic of Scientific Discovery* (but not in the original *Logik der Forschung*) — that ‘the theory to be developed in the following pages . . . might be described . . . as the view that a hypothesis can only be empirically tested — and only *after* it has been advanced (see also Miller 2006a, Chapter 4, § 1; p. 89). The following section, which was in the original German, was equally explicit that ‘statements and proposals must be made before they can be subjected to critical examination’ (in Bunge’s words): ‘In order that a statement may be logically examined . . . , it must already have been presented to us. Someone must have formulated it, and submitted it to logical examination.’ Twenty years later, in *Realism and the Aim of Science*, Part I, § 7, Popper wrote:

. . . the empiricist philosophers, from Bacon to Hume, Mill, and Russell, . . . never saw clearly that it is not the origin of ideas which should interest epistemologists, but the truth of theories; and that the problem of the truth or falsity of a theory can, obviously, only arise *after* the theory has been put before us — that is to say, *after* it has originated with somebody — and that the ‘history of its origin has hardly any bearing on the question of its truth.

Bunge’s second point, that ‘falsifying a proposition is the same as confirming its negation’, obviously pleases him greatly, since, as he recounts in an autobiographical summary (2010, p. 536),

... I was also the first to discuss Popper in class, and to write a long review of his *Logic of Scientific Discovery* for *Ciencia e Investigación* in 1959, ... I praised the book but said that refutationism is no alternative to confirmationism, falsifying *A* amounts to confirming not-*A*. Moreover, I held that it is simply not true that scientists are eager to have their pet conjectures refuted.

From a methodological point of view (that is to say, the most important point of view) what Bunge asserts here (and again on p. 146 of his 2012) is mistaken. The business of falsification, or of criticism, is logically immaculate, while for many reasons — including the fact that observation is always theory-laden — the business of verification, or of confirmation, is logically defective (Miller 1994, Chapter 3, § 5). The empirical report that ‘scientists are anxious to have their views confirmed rather than falsified’, another persistent theme in Bunge’s writings (for example, on p. 111b of his 2003), is of little moment for methodology. It could as well be said that because few people are eager to learn that they are suffering from a mortal illness, voluntary submission to potentially disturbing medical examinations is foolhardy.

An effective battery needs (in addition to an electrolyte) both a cathode, which is negatively charged, and an anode, which is positively charged. Without these two distinct components, there is no electric current. It is not much different with regard to the energetic growth of knowledge. This process need both refutations, which incontestably are negative, and conjectures, which are positive (or better put, *affirmative*, so as to discourage the suggestions that they need any justification). What Bunge, astonishingly, seems not to appreciate, is that falsificationism is a theory of both conjectures and refutations. There is, in other words, no symmetry between logical negativism, as Bunge labels falsificationism, and logical positivism (or verificationism, or justificationism), which functions ineffectively with two anodes. I am much indebted to Michael Duggan (2012), who has suggested that a more revealing name than *logical negativism* would be *positive negativism*.

The same lack of attention to the part played by unjustified conjectures has led Rowbottom (2010), Chapter 7, to conclude that, if the rational (and empirical) side of science is negative, and directed exclusively towards the falsification, the aim of science according to falsificationism cannot be the discovery of true hypotheses and theories, but only the elimination of those hypotheses and theories that are false. The critical rationalist, who genuinely wants to uncover the truth — but not justified truth — may naturally wonder what would be a method appropriate to this aim. He may, furthermore wonder why we bother ourselves in propounding any hypotheses and theories at all if we have no intention of hanging on to those that offer us a glimpse of the truth. Science is rather more than a complicated and expensive game of skittles.

In a recent lecture (Haack 2013, Chapter 12) a one-time colleague of mine in the Department of Philosophy at the University of Warwick advised the public ‘Just Say “No” to Logical Negativism’, a title that sums up rather well the argumentative content of the lecture. (‘Saying “No” to’ is a technical term for a critical argument with a determinate direction but zero magnitude.) In this lecture Haack described Popper’s falsificationism as ‘covertly sceptical and utterly indefensible’, ‘radically flawed’, and ‘almost literally incredible’ (*ibidem*, pp. 13, 29, 183), but it is impossible to find in what she says any substantial objection to falsificationism beyond the correct imputation that it excludes all justification: ‘his is a startlingly irrationalist picture of “objective scientific knowledge” as a mesh of *unjustified and unjustifiable conjectures anchored in nothing more than unwarranted decisions on the part of the scientific community*’ (*loc.cit.*; almost the same words are to be found on p. 29). The writer of this appraisal evidently has not considered seriously the possibility that rationality has nothing to do with justification. In reality it is the doctrine that knowledge stands in need of justification that leads to the pessimistic and negative scepticism of Hume, while the critical approach leads to the optimistic scepticism

of Xenophanes that is, the third position discussed in § 1 of the Introduction (1978) to Popper 1930–1933). In contrast to Haack, a former colleague in the Warwick Business School (WBS) wrote many years ago : ‘Popper, who among other things took the conjectural character of all knowledge as a central idea in his enquiry into the logic of increasing scientific understanding, . . . showed that such an apparently negative starting point leads to a more fruitful way of looking at the open-ended always-likely-to-be-challenged status of scientific theories than the older science-as-the-discovery-of-[justified]-truth picture which it overturned’ (Boothroyd 1978, p. 22). I ought to mention also three articles by another former colleague in WBS, that is to say, Ormerod (2009), (2013), and (2014), that contain most interesting discussions of the pertinence of Popper’s ideas to operational research. There are many assertions in these articles with which I am in substantial disagreement, but they show an understanding of the principles, the subtleties, and the importance of critical rationalism that is more perceptive and more enlightened than anything evinced by many professional epistemologists.

8 Abduction

Permit me to finish this defence of falsificationism in the face of contemporary criticism by saying a few words about the supposed relevance of ampliative logic to the methodology of empirical science. In recent years, in various places, such as Paris, Moscow, Barranquilla, and even Coventry, I have been told that my emphasis on Popper’s solution of the problem of the justification of induction is a sorry anachronism, since no one outside the classroom any longer has any interest in this unsolvable problem. Enlightened thinkers these days concede that all our knowledge is speculative and conjectural, perhaps for ever, and have replaced Hume’s problem by the problem of *how to construct conjectures more effectively*. If I understand well this project, which is popular not only among some philosophers and logicians, but also among workers in the fields of artificial intelligence, computational chemistry and biology, and the exploration of minerals, what is sought is a mechanical or quasi-mechanical method for the generation of solutions to explanatory and practical problems that is more successful than random guessing. No one expects a method that invariably yields conjectures that are true or nearly true, but the goal is to realize the old dream of mechanizing the elaboration of promising new ideas. This problem is the modern transfiguration of the problem of induction.

Before responding to the meat of this criticism, I wish to make it absolutely plain that I welcome any programme that effectively reduces the wastage involved in the formulation of hypotheses and conjectures. Where I am in disagreement is in the supposition that what would be beneficial is any kind of *ampliative logic* or *abductive logic*.

At every moment we are using what we know in order to explore the unknown, and to gain more knowledge. An unsophisticated example is when, to find out what language we should use to greet an unknown person whose surname is known, we take advantage of what we do know about the correlation between people’s mother tongues and their surnames: to someone called *Gómez* (or *Gomes*) we may conjecture that the most appropriate language is Spanish (or Portuguese), and to someone called *Ivanov* we may conjecture that the most appropriate language is Russian. Of course, the conjecture that Gómez understands Spanish is just a conjecture, which needs to be checked. But we cannot doubt that the diligent use of general knowledge will usually speed up an investigation of this kind. Although some reasoning is used here, the leap from surname to language is not a matter of logic properly speaking.

It may be that there are many investigations in the computational sciences that are not very different from this simple example. On the other hand, those scientists and logicians who are searching for explanations as well as descriptions and classifications usually have in mind a grander and more abstract mode of inference, what Peirce (1903), Lecture VI.2, called

abduction, ‘the process of forming an explanatory hypothesis’ (*ibidem*, Lecture VI.4). Peirce distinguished succinctly three types of reasoning (*loc.cit.*): ‘Deduction proves that something *must* be; Induction shows that something *actually is* operative; Abduction merely suggests that something *may be*. . . . No reason whatsoever can be given for it, as far as I can discover; and it needs no reason, since it merely offers suggestions.’ (See also Miller 2012, § 3.) I find it difficult to distinguish abduction, so characterized, from guesswork. It does not matter too much whether it is counted as a kind of reasoning, although my own preference should be clear. In any case, it seems to me to be unnecessary to squeeze it into a logical formalism, as Aliseda (2014), Chapter 5, recommends.

In Part II, § 13, of *Realism and the Aim of Science* (1983), Popper himself proposed an inductive machine with the ability to discover universal laws from experience in appropriate circumstances. He stressed that there exists no machine that is successful for the most part in all imaginable circumstances. There exists no true principle, universal or statistical, of the uniformity of nature. The need to specify the conditions under which the machine works well applies just as much to systems of inference. Of course, the statement that some system of ampliative logic, or some learning machine, performs better than guesswork, incorporates a factual hypothesis, which should be testable. I do not deny, or affirm, the possibility of developing a system of ampliative or abductive logic, or a learning machine, that would be successful and, up to a certain point, neutral with respect to the topic under discussion; that functions well, for example, in psychology, chemistry, criminology, palaeontology, horse racing, and other fields. If there really were to exist such a system, or machine, then its designers must have discovered something remarkable about the structure of the world. The rest of us deserve to be told about it (Miller *op.cit.*, § 6).

The complementary roles of *conjectures* and *refutations* in the growth of knowledge are well described in three passages written many years ago. In Cicero’s *De Oratore*, Antonius says about logic (¶ XXXVIII): ‘In this art [the art of reasoning], if it is an art, there are no directions concerning how truth may be discovered, but only how it may be judged.’ In greater detail, Bertrand Russell wrote (1914a, p. 788; 1914b, Lecture I, p. 21; 1917, § 1): ‘Instinct, intuition, or insight is what first leads to the beliefs which subsequent reason confirms or confutes; but the confirmation, where it is possible, consists, in the last analysis, of agreement with other beliefs no less instinctive. Reason is a harmonising, controlling force rather than a creative one. Even in the most purely logical realm, it is insight that first arrives at what is new.’ This point of view, which was independently endorsed years later by Popper (1957, § VII; (1962), the fifteenth thesis), has received its most complete statement, however, in § II of the remarkable Introduction to Minto (1893):

Why describe logic as a system of defence against error? Why say that its main end and aim is the organization of reason against confusion and falsehood? Why not rather say, as is now usual, that its end is the attainment of truth? Does this not come to the same thing?

Substantially, the meaning is the same, but the latter expression is more misleading. To speak of logic as a body of rules for the investigation of truth has misled people into supposing that logic claims to be an art of Discovery, that it claims to lay down rules by simply observing which investigators may infallibly arrive at new truths. Now, this does not hold even of the Logic of Induction, still less of the older Logic, the precise relation of which to truth will become apparent as we proceed. It is only by keeping men from going astray and by disabusing them when they think they have reached their destination that logic helps men on the road to truth. Truth often lies hid in the centre of a maze, and logical rules only help the searcher onwards by giving him warning when he is on the wrong track and must try another. It is the searcher’s own impulse that carries him forward: Logic does not so much beckon him on to the right path as beckon him back from the wrong. In laying down the conditions of correct interpretation, of valid argument, of trustworthy evidence, of

satisfactory explanation, Logic shows the inquirer how to test and purge his conclusions, not how to reach them.

I cite these passages not as arguments in favour of the conclusion that no logic of abduction is possible, which they are not, and could not be, but as statements of an approach to evidence and argument that, when criticized, is invariably criticized from the justificationist standpoint that it seeks to supersede. Cicero, Russell, and most of all Minto, as well as Popper, the modern champion of the critical approach, deserve not to be ignored.

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