

Sokal & Bricmont: Back to the Frying Pan*

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If you can distinguish truth from knowledge, you should be able to distinguish the radical disavowal of truth [which usually takes the form of relativism, or sometimes nihilism] from the radical disavowal of knowledge [scepticism]. The former doctrine is false, but uninterestingly so. The latter doctrine is true. By persistently presenting them as a single doctrine, would-be spokesmen for rationalism are driven to defend the indefensible, and unwittingly to offer the best possible incitement to irrationalism.

0 Introduction

Without necessarily agreeing with all that they say, rationalists can be expected to look with favour on the denunciation by Sokal [1996] and Sokal & Bricmont [1998] of the intellectually insulting infantilism afflicting parts of current continental philosophy (or European philosophy, as it now describes itself). But it would be rash to hope for a prompt return, amongst those adversely influenced by Lacan, Kristeva, Irigaray, Baudrillard, Deleuze & Guattari, and the rest of them, to the much maligned Enlightenment ideals of truth, reason, and intellectual responsibility. Lofty profundity is such a useful material from which to build reputations, careers, and graduate schools. A more fundamental obstacle to a general return to rationalism is that rationalism itself is

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usually presented, as it is by Sokal & Bricmont, in a way that so unedifyingly infringes its own standards of honesty that the only conversions that it can hope to procure are conversions to irrationalism. Unless the champions of rationalism can make their candidate decidedly more presentable, postmodernist fads will simply give way in due course to other fads that are equally scornful of reason. (Postmodernism may or may not be *dernier cri*, but it is surely not the last word in meretricious desipience.) The purpose of this article is to indicate how such a rehabilitation of rationalism should be undertaken. The outcome is a smarter, leaner, tougher, more wholesome, and less dissolute rationalism than most rationalists are accustomed to. If I may say so, it is unexpectedly attractive.

It should be stressed that, although I quote plentifully from Chapter 4 of Sokal & Bricmont [1998], and little from other works, Sokal & Bricmont are not my principal target. I am gunning here (and in [1999b]) for all those traditional rationalists who manage unintentionally, or indeed intentionally, to portray science as an irrational enterprise. The work of Sokal & Bricmont is just the most recent failure to defend the rationality of science properly, and taken in isolation it is by no means the most objectionable one. But its salience makes it one of the most significant, a failure that calls for the firmest and most concentrated correction.

1 Radical Scepticism

The primary purpose of *Intellectual Impostures* (Sokal & Bricmont [1998]) is to censure the comically inept misappropriation, by a loosely connected group of French writers, of a variety of intoxicating ideas drawn from twentieth century science, especially from relativity theory, quantum mechanics, non-linear dynamics, fluid mechanics, topology, and mathematical logic. Although Sokal & Bricmont add a few words of commentary to the extended passages that they quote, 'here, to expound is to expose' (as Medawar [1961], p. 247, remarked in his review of *The Phenomenon of Man*). I have nothing to add to their onslaught. But *Intellectual Impostures* contains also a chapter, entitled 'Intermezzo: Epistemic Relativism in the Philosophy of Science', that grapples with a more serious concern: the doctrine, now so common, that there is no absolute truth, that truth is never more than a matter of opinion, or of consensus, or of (perhaps unconscious) social construction. Sokal &

Bricmont rightly object to this pernicious doctrine of radical relativism, according to which western science is just one item on a menu of competing but not really comparable ideologies or superstitions. Unfortunately they mistakenly identify radical relativism with a doctrine that is decisively different from it: the doctrine of radical scepticism, according to which nothing that we call knowledge, science included, is known with any authority, that we cannot know anything in the traditional sense of the word (very crudely: knowledge = justified true belief). Radical scepticism says that however convincing and intuitive some of what we call knowledge may seem, it is never more than a wild conjecture at the truth. What I want to suggest here is that, however unconvincing and unintuitive it may seem, radical scepticism is true.

Pyrrho of Elis, the Greek philosopher who is usually regarded as the founder of the radical sceptical tradition, was said by Timon of Phlius, his spin doctor, to have been a 'negative dogmatist', who held that 'knowledge is impossible, not because our cognitive apparatus is flawed, but because the world is fundamentally indeterminate: things themselves are "no more" cold than hot, or good than bad' (*The Cambridge Dictionary of Philosophy*, p. 665a). If this is so, then Pyrrho was close to being a radical relativist, as well as being a radical sceptic. Indeed, it might be better to call him a [cognitive] nihilist rather than a relativist; for it is one thing to say that truth is inescapably relative, but plainly another thing to say that truth does not exist at all or that, even if it does, it is not worth bothering about. (The positions are importantly similar because they both deny the existence of absolute truth.) It cannot be doubted that sex appeal is a relative matter — someone who is attractive to one may not be attractive to another — yet it exists, and is of some consequence.

Sokal & Bricmont confuse radical scepticism with radical relativism. They begin their epistemological discussion (pp. 51-54) with a brief consideration of solipsism, the mad doctrine that there is literally nothing outside oneself and one's mental states; the external world is one's invention. They observe correctly that this doctrine is irrefutable; and that there is a much better explanation available of what is experienced (namely that there exists an external world). They also voice the doubt that anyone is sincerely a solipsist. They might have added that, even if solipsism were true, it would be a dead end of no interest. So they discard solipsism, and pass on to radical scepticism, the doctrine that 'there exists an external world, but it is impossible for me to obtain any reliable knowledge of that world' (p. 52; for a warning about the duplicity of the

word 'reliable', which here means more than merely 'true' or 'accurate', see the last paragraph of this section). This doctrine was stated pungently by Hume, the father of modern scepticism about knowledge, and has been fully endorsed by Popper (see p. 60). Sokal & Bricmont observe, again correctly, that radical scepticism is completely general: it applies as much to the commonsense knowledge of our everyday surroundings as it does to the abstract hypotheses of theoretical physics. So far there is no criticism. They go on (p. 53):

The universality of Humean scepticism is also its weakness. Of course, it is irrefutable. But since no one is systematically sceptical (when he or she is sincere) with respect to ordinary knowledge, one ought to ask *why* scepticism is rejected in that domain and *why* it would nevertheless be valid when applied elsewhere, for instance, to scientific knowledge. Now, the reason why we reject systematic scepticism in everyday life is more or less obvious and is similar to the reason we reject solipsism. The best way to account for the coherence of our experience is to suppose that the outside world corresponds, at least approximately, to the image of it provided by our senses.

This is a remarkable passage. Let us work through it in reverse. Provided that the word 'best' in it does not mean 'best confirmed', or 'most likely', or 'most reliable', or anything of that sort, the final sentence is in order (and so is the accompanying footnote, which appeals to the theory of evolution for a deeper explanation of the excellent decoding powers of our sensory apparatus). But to say that our hypotheses about the external world correspond to the world at least approximately (that is, that they are approximately true) implies nothing about whether those hypotheses are empirically supported, or grounded, or well founded, or reliable, or proved, or warranted, or justified, or (to use more modish jargon) legitimated. It may be that they are true but unsupported and unjustified. Realism about the external world is no doubt a good explanation of the coherence of experience, and we do well to reject radical relativism. But the claim that the bulk of our scientific theories, in addition to being approximately true, are also justified, explains nothing that needs explanation. Sokal & Bricmont have wrongly understood an argument against relativism ('we cannot obtain true knowledge of the world') to be an argument against scepticism ('we cannot obtain reliable [justified]

knowledge of the world'). And although the fact that someone holds a particular opinion is hardly pertinent to its truth or falsity, I wish to go on record as one person who is systematically sceptical (when I am sincere) with respect to ordinary knowledge. I am not a relativist, and like everyone else I think that some theories are true and that others are false. I have no grounds for my opinions, but that does not disqualify them from being opinions about the external world. Nor, I hasten to insist, does it mean that they are typically held without regard to rational considerations; some of them — not all — are considered opinions. (I shall return to this point, which is of cardinal importance, in section 3.) Finally, returning to the beginning of the passage quoted, we may agree that radical scepticism is irrefutable (this is hardly surprising, given that Hume refuted its negation), yet resist the odd idea (repeated on pp. 60 and 66) that a problem that is universal stops being a problem. Those who fear death are rarely comforted by the thought that it comes to us all.

The realist argument against solipsism and radical relativism fails completely as an argument against radical scepticism. This should be plain, since scepticism ('there exists an external world, but it is impossible for me to obtain any reliable knowledge of that world', in the words of Sokal & Bricmont) does not contradict realism. Many professional philosophers succumb to it at one time or another, this confusion of radical relativism (= absolute truth does not exist) with radical scepticism (= reliable [justified] knowledge does not exist); some use it to argue, as do Sokal & Bricmont, from the untenability of relativism to the untenability of scepticism, while others (such as Feyerabend, if I am not wrong) argue contrapositively, terminating in relativism. The confusion of relativism with scepticism is, to be sure, not as dumb an error as confusing velocity with acceleration (p. 160), but it is nonetheless a somewhat primitive confusion; it is all the more surprising on the part of Sokal & Bricmont since for most of their book they show a lively appreciation of the distinction between truth and knowledge, and several times emphasize the difference (see note 87 on p. 69; note 130 on p. 94; and note 259 on pp. 184f., which is not, as it suggests, about the distinction between knowledge and belief). Section 2 below explains why the confusion of radical scepticism with radical relativism is such an insidious one. Section 3 indicates how radical scepticism and rationalism may be brought into harmony.

Before proceeding, let us check that we have the terminology more or less straight, since some of the crucial words here are used in different ways by different philosophers, myself included. Cognitive nihilism

[Pyrrho] is the doctrine that truth, absolute or relative, does not exist. Radical relativism is the doctrine that there may be statements that are true relative to an individual [subjectivism], class [vulgar Marxism], culture [cultural relativism], form of life [Wittgenstein], scientific paradigm [Kuhn], or something similar, but there are no truths that transcend these milieux. Radical scepticism [Hume, Popper] is the doctrine that, at least for factual statements, nothing can be known with any positive degree of justification. Although radical relativism blunts the point of radical scepticism, it may be that these positions can be combined; be that as it may, they are plainly independent — Hume was not a relativist, and Popper ceaselessly stressed his adherence to Tarski's absolutist theory of truth ([1963], Chapter 10; [1972], Chapters 2, 8, and 9). These general philosophical doctrines must be distinguished from the unexceptionable attitude of doubt or scepticism that anyone may adopt towards specific scientific theories (p. 179); and from general methodological scepticism, the critical approach that one hopes that all scientists adopt in principle (at least towards the theories of others). Note also that the word 'knowledge' and its cognates will normally be used here in a manner that allows scientific knowledge to count as knowledge; in other words, it is not assumed that knowledge requires any positive degree of justification, and radical scepticism is not laxly identified with the doctrine that knowledge is impossible (only with the doctrine that justified knowledge is impossible). Finally, let me identify a weasel word whose ambiguity generously contributes to the confusion of radical scepticism with radical relativism: the word 'reliable'. Used of an instrument, it may mean simply that the instrument does not break down; but it may mean more — that it is trustworthy, that the user is justified in expecting it not to break down. In the same way, talk of reliable knowledge is ambiguous between knowledge that works (perhaps because it is true) and knowledge that is justified. Radical sceptics deny only the second kind of reliable knowledge. It is interesting that although the *New Shorter Oxford English Dictionary* records only the second (epistemologically loaded) sense of 'reliable' and 'reliability' (except when used as technical terms in statistics), all the illustrations are equally, and perhaps even better, illustrations of the first sense. He uses other words, but the different ideas are splendidly contrasted in Freedman's striking remark ([1981], p. 399): 'Nuclear deterrence may be a viable strategy, even if it is not credible.'

2 Traditional Rationalism

As I have explained, Sokal & Bricmont wrongly understand a central argument against radical relativism to be an argument against radical scepticism. They also rightly reject radical relativism. They are accordingly led to a re-affirmation of the traditional rationalist view, combated by radical scepticism, that science is epistemically superior to mere guesswork. Science, it is held, has an authority that is missing from other systems of thought — such as most religious creeds as well as the host of scatty fantasies that are customarily lumped together as pseudo-science (for a discriminating survey see Friedlander [1995]). Yet the conclusion of Hume's elementary but subtle argument is that science cannot have this rational authority that it arrogates to itself. Rationalists are rationally forced by Hume into radical scepticism *bon gré mal gré*. Any rationalist who tries to resist Hume's conclusion traps himself in a most uncomfortable position.

The usual way of coping with the discomfort, I am afraid, is bluff, and Sokal & Bricmont provide no exception. They write (p. 56):

the radical sceptic or relativist will ask what distinguishes science from other types of discourse about reality — religions or myths, for example, or pseudo-sciences such as astrology — and, above all, what *criteria* are used to make such a distinction. Our answer is nuanced. ... there are some general (but basically negative) epistemological principles, which go back at least to the seventeenth century the experience accumulated during three centuries of scientific practice has given us a series of more-or-less general methodological principles ... that can be justified by rational arguments. However, we do not claim that these principles can be codified in a definitive way, nor that the list is exhaustive. In other words, there does not exist (at least at present) a complete codification of scientific rationality, and we seriously doubt that one could ever exist. ... Nevertheless — and this is the main difference between us and the radical sceptics — we think that well-developed scientific theories are in general supported by good arguments, but the rationality of those arguments must be analysed case-by-case.

Shortly afterwards (pp. 57f.) they are even bolder in their claims:

there is no *general* justification of the principle of induction (another problem going back to Hume). Quite simply, some inductions are justified and others are not; or, to be more precise, some inductions are more reasonable and others are less so. ...

In a sense, we always return to Hume's problem: No statement about the real world can ever literally be *proven*; but ... it can sometimes be proven beyond any *reasonable* doubt.

It is not mentioned here that the impossibility of a *general* justification of the principle of induction (whatever that might be) is a mere corollary to Hume's conclusion that no *particular* induction can be justified. This conclusion is simply denied — or perhaps I should say that it is defied. Nor is it mentioned that Hume's arguments confound not only purported *proofs* of statements about the real world — attempts to render them certain — but all attempts to supply good empirical arguments in their favour. Briefly put, Hume's result was that there can be no empirical arguments in support of scientific theories or predictions; for such arguments, to be any good, need premises in addition to the empirical evidence, and these premises themselves require empirical support; and so on. An infinite regress of justification is unavoidable. No empirical statement, in other words, can be proved to even the slightest degree, let alone 'beyond any *reasonable* doubt'. It may very well be questioned what use empirical support would be even if we could obtain it (Miller [1994], Chapter 3; [1996]); but in any case, we cannot obtain it.

Sokal & Bricmont make no remotely adequate response to Hume's objection. Anyone reading their work attentively, but not too critically, will be forced to conclude that the rationality of science is an audacious confidence trick: scientists are like other people in holding unwarranted opinions but, unlike more scrupulous citizens, some of them try to hide the precariousness of their opinions behind magisterial declarations that they can be 'supported by good arguments' or even 'proven beyond any *reasonable* doubt'. In the same vein, Polkinghorne's 'answer to the Humean criticism ... is simply to assert that we shall rely upon inductive method exercised with an appropriate degree of skill' ([1986], pp. 18f.); that is, we shall continue to make reckless guesses, but we shall continue to advertise them as creations of reason. This pseudo-rationalistic squalor has turned out to be an exuberant breeding habitat for irrationalism and nihilism.

It may be said that I am judging needlessly harshly the valiant attempts of some who are not professional philosophers to cope with a philosophical problem that, it must be admitted, has flummoxed most professionals; and that I have read too much into their use of traditional philosophical vocabulary. It may be said that when Sokal & Bricmont talk of justification, or empirical support, or good arguments, they are not really thinking of a process that itself needs a foundation, or further justification, thus falling foul of Hume's criticism; when they say that the rationality of the arguments mustered in support of well-established scientific theories 'must be analysed case-by-case', they do not really think that these arguments are so singular that they cannot be assessed by general canons of logical validity. Rather they wish to draw attention to the uncontested fact that scientific theories, unlike some of their pseudo-scientific cousins, are not simply propounded and then defended tenaciously against any attack, but are examined, probed, tested by observation and experiment, corrected, perhaps re-formulated, perhaps rejected, over and over again, in an uncompromisingly objective manner, and that the way in which a theory is steered through this regimen is indeed idiosyncratic and unsystematic. With this account there really need be no serious quarrel. The difference between us, however, is not methodological but epistemological. The question is not how a scientific theory should engage with experience, on which there is passable agreement (p. 65), but what positive conclusion about the theory's value may be legitimately drawn if it is not bettered in its engagement with experience. Radical sceptics say that no conclusion should be drawn, though we may continue to conjecture that the theory is true; we should merely record that the theory has yet to be refuted. If words mean anything, then Sokal & Bricmont and other defenders of science genuinely go further when they say that a theory 'can sometimes be proven beyond any *reasonable* doubt'. What they mean when they say that '[o]ne need not be a strict Popperian to realize that any theory must be supported, at least indirectly, by empirical evidence in order to be taken seriously' (p. 180) I do not know.

There are philosophers who have recognised the cogency of Hume's sceptical argument, but have devoted much honest work to the cause of deflecting it, usually through the deployment of simple results in the theory of probability. The vanity of all such endeavours may be elucidated by a brief consideration of the so-called miracle argument, to which Sokal & Bricmont also appeal (pp. 55f.). If our best theories were not

approximately true, it is said, it would be a miracle that they should yield as many exquisitely accurate predictions as they do; given the accumulated empirical evidence, that is, it is highly probable that quantum electrodynamics [QED] — Sokal & Bricmont's example — is approximately true. Let us grant this calculation, though there are several respects in which it deserves to be challenged (Miller [1994], p. 49; for the intractability of the idea of approximate truth, see *op.cit.*, Chapters 10 and 11). Now comes the difficulty. Why is the high probability of the approximate truth of QED a good reason to think that QED is approximately true? Not all highly probable theories are true, or even approximately true. Perhaps, it may be responded, this is a matter of definition: high probability (given sufficient evidence) is what we mean by a good [empirical] reason. But then the question becomes: given that there is a good reason to think that QED is approximately true, is QED approximately true? The rationalist cannot now say that this follows from what is meant by the term 'good reason'. The chasm between 'There is a good reason to think that QED is approximately true' and the approximate truth of QED is the chasm between what has been observed and what has not been observed that those who seek the truth desire to bridge; more accurately, QED is a bridge, itself inaccessible to observation, that makes the unobserved accessible from the observed. The theory of probability provides no bridge. As Hume made plain, the problem is not that the observed is not fully informative about the unobserved; it is that the observed is not informative about the unobserved at all.

3 Critical Rationalism

Fighting Hume's sceptical argument is an honourable if unprofitable trade. Pretending that it never happened is disingenuous, and inevitably provokes a slide into irrationalism. For Hume and some of his successors, radical scepticism too became a harbinger of irrationalism (though not of relativism). Only in Popper's critical rationalism ([1945/1966], Chapter 24), the direct descendant of the falsificationist philosophy of science of his [1934/1959], did a workable rationalistic response to Hume finally fetch breath. Here Popper replaced the traditional rationalist theory of positive justification by a revolutionary rationalist theory of negative criticism. He agreed with Hume that no theory is justified by argument or by evidence, even to the smallest extent, but released the theory of

rationality from the deadly unspoken doctrine that a theory is held rationally only if it is justified, or supported by evidence, or backed by good argument. (On this liberation see especially Bartley [1962/1984], Chapter V/5, and Miller [1994], Chapter 4.) Provided that a theory is open to criticism it may be held rationally, Popper maintained, for as long as it has not been damagingly criticized. For unlike a theory that has been falsified, one that survives all attempts at falsification and criticism remains a candidate for the truth. It is clear that if this exclusive concentration on criticism is not to be an empty sham, then every effort must be made to provide criticism in the fullest measure. Reality must be ransacked for refutations.

As Sokal & Bricmont remind us, Popper was not the first to call for scientific theories to be tested to destruction, though both history and logic are distorted in the remark that '[t]he necessity of empirical tests ... is simply the lesson of empiricism: the rejection of *a priori* or revealed truths' (p. 65). Still less is Popper the only philosopher who enjoins criticism. Where he did make an unprecedented advance was in proposing that critical testing is, rationally speaking, sufficient as well as necessary. Only negative evidence counts. Notice how deftly, as a bonus, this critical rationalist indifference to positive evidence takes the sting out of the problem of the theory-ladenness of observation, a problem constantly harped on by those keen to undermine the objectivity of science. For if there is a conflict between a theory and a 'fact' interpreted in terms of the theory, then there is incontestably something amiss with the theory — independently of whether the 'fact' is really a fact (which does not much matter, provided that the report of the test is a true report). But if the theory-impregnated 'fact' and the fact-impregnating theory cosily agree with each other, the only possible comment is: 'How very nice for both of them.'

What Popper seems to have been also the first to grasp is that rationality is a matter only of methodology, and has no epistemological significance. Theories can be handled rationally — because the method of conjectures and refutations submits constantly to the control of reason and argument; but there is nothing rational about the theory that, for the time being, we adopt, beyond its being a theory that has been (or is about to be) the target of rational investigation. This is not as odd as it may sound; think what is meant by calling a picnic 'well-organized', meaning that as a compliment.

Critical rationalism rarely receives a good press. It is time and time again arraigned on trumped-up charges of being ‘ambiguous’ and ‘inadequate’ (Sokal & Bricmont, p. 59), ‘chilling’ and ‘gloomy’ (Polkinghorne op.cit., p. 18), and similar indefinite lapses. It is vilified as counterintuitive, a feature that only those still in fee to the matter-of-fact philosophy of the later Wittgenstein could judge to be inauspicious. It is injuriously associated with relativism; and because it does not countenance the justification of scientific theories, it is supposed to countenance their violation. (A good example is the illustration on p. 44 of O’Hear [1985], which depicts a reader of Popper [1959] at a table that is in the process of losing its solidity; the message appears to be that, according to Popper, the laws of physics cannot be expected to hold.) It is frequently misreported. Although Popper often said (for example in the 1958 Preface to [1959]) that there is not much more to scientific method than the method of trial and error, he is reproached by Sokal & Bricmont for having tried ‘to formalize the scientific method’ (p. 58). He is repeatedly accused of not noticing possible objections that indeed he had noticed, and responded to, in 1934. For example, Sokal & Bricmont raise as an objection to critical rationalism the fact that ‘falsification is much less certain ... [and] more complicated than it seems’ (pp. 60f.), even though the difficulty of obtaining clear-cut counterexamples (and, less importantly, their uncertainty) had been fully acknowledged in §9 and §85 of [1934/1959]. It is not easy to imagine how Sokal & Bricmont (note 62 on p. 59) could think that Putnam [1974] (‘Popper has, *contre lui* a theory of induction’, p. 223), Newton-Smith [1981] (Chapter 3: ‘Popper: the Irrational Rationalist’), and Stove [1982] (subtitle: *Four Modern Irrationalists*) are up-to-date, let alone decent, guides to critical rationalism. I mean, how could they?

Sokal & Bricmont have two main criticisms of critical rationalism. One, already noted, is that ‘falsification is much more complicated than it seems’. This objection has little force except for those who assume that ‘the logic of scientific discovery’ is some kind of empiricist liturgy to which scientists’ thought processes are required to conform. But logic is not used as a manual of correct procedure even in the formal disciplines of mathematics and logic. The uncoordinated ideas come first; logic marshals them later for inspection. Sokal & Bricmont’s second main criticism is that if radical scepticism were correct, ‘if rationality were to consist only of deductive logic — it would imply also that there is no good reason to believe that the sun will rise tomorrow, and yet no one *really* expects the

sun not to rise' (p. 60). Presumably what is intended by this last clause is that 'there is no one who does not *really* expect the sun to rise'; for after all, no one is obliged, if he does not expect the sun to rise, to expect it not to rise. Be mindful too that the radical sceptic is not prevented, by lack of good reason, from expecting the sun to rise (though he will not expect it with confidence). But even if we disregard this last crucial point, what Sokal & Bricmont allege is breathtakingly irrelevant; indeed, it is no more an objection to radical scepticism than it is an objection to Copernicanism.

Sokal & Bricmont are rightly dismissive of consensus theories of truth (pp. 80f.), yet seem frightfully impressed by the fact (if it is a fact) that almost no one thinks that radical scepticism is true. In this they echo many previous critics, for example O'Hear, who wrote (*loc.cit.*):

Few would, I think, agree with Popper's sceptical refutation of any talk of justification in our system of knowledge. ... I also imagine that few of those scientists who derive undoubted inspiration from Popper's work would accept his claim that survival of severe tests by a theory adds nothing to its probability. If it did not, why should we be so sure that the next time an atomic explosion occurs, life in the vicinity of the explosion will be endangered?

This passage exemplifies a characteristic feature of most writing against critical rationalism, the resort to insufferably complacent but routinely answerable rhetorical questions. The straight-thinking answer to O'Hear's question is : 'We are not sure. Is the prediction mistaken?' If the word 'know' is being used in its traditional sense (knowledge = justified true belief), the answer to the recurrent question 'How do you know?' is 'I do *not* know: my assertion was merely a guess. ... help me by criticizing it as severely as you can' (Popper [1963], p. 27). What opponents of critical rationalism have evidently not registered is that, if truth is our prime concern, it does not matter much where a theory came from, or who proposed it, or whether there is any evidence for it (for no source has authority over truth); what matters is what it says, and whether we are in a position to investigate it minutely. Sokal & Bricmont make this point admirably when they defend their own right to write about philosophy (pp. 10f.), but seem not to understand that, even after it has been exposed to searching scrutiny, it is the content of a theory that counts, not its credentials.

The usual rejoinder is that ‘one of the roles of science is to make predictions on which other people (engineers, doctors, ...) can reliably base their activities, and all such predictions rely on some form of induction’ (p. 61). This is utterly wrong. Except in some highly stereotyped situations, applied scientists do not in any interesting way ‘base their activities’ on predictions made by pure scientists (Miller [1994], pp. 38-45; [1998a], §3), or on scientific theories (though theories do perform an indispensable critical service). The production of effective vaccines, to take a simple example, requires far more than a solid understanding of immunology. For what scientific theories do is predict which effects follow from which causes, whereas the task of technology is the converse: given an idea of the effect that we want, to realize something that causes it (the delusion that causes can in some manner be inferred from effects is, of course, most congenial to those wedded to a belief in induction). The interventions of engineers and doctors, moreover, are reliable, if at all, only in the weaker sense expounded at the end of section 1 above; that is, they work. One of the enduring misconceptions of 20th-century philosophy of science is that in the absence of scientific theories that are well-confirmed, inductively supported, trustworthy, or reliable in the stronger sense, the application of science would be so hazardous as to be impossible. Hazardous it is, I agree, but far from impossible. The truth is that at the theoretical level truth is enough; but what the progress of technology essentially depends on is a stream of bright ideas. Nothing said here implies that there may not be some activities, for example walking tightropes, in which confidence in the correctness of a certain prediction is more valuable to the agent than any amount of rational deliberation on the vagaries of the external circumstances. Ignorance is sometimes bliss. But such examples are scarcely to the point. After all, the confidence that keeps the tightrope walker a tightrope walker need not be rational, even in the most jejune sense of the term.

I have no intention of enumerating here the standard critical rationalist responses to all the other standard objections to critical rationalism. (See my [1994], especially Chapters 2 and 3.) One only I shall mention, the complaint that without some justification somewhere the business of science is doomed to futility. O’Hear *op.cit.*, p. 45a-b, asks:

Popper often compares science to evolution, and in nature the fittest survive only until they are proved unfit. There is no absolute

[presumably he means ‘guaranteed’] fit in nature, or in science. Even falsification is an uncertain business. Scientists, not nature, say ‘no’ to discarded theories, as we have seen. The scientific game is played, and what for practical purposes does Popper’s realism amount to? What is this *quest* for truth, when the rules of the game give us no assurance that we are on the road to truth?

It might be said that, by citing the theory of evolution, O’Hear supplies the makings of his own answer. For we may figuratively talk of a ‘quest’ for biological fitness without supposing that the mechanics of variation and selection give any ‘assurance’ of even limited success. Nonetheless, several species do seem to be rather well adapted, innocent though they are of their good fortune. It was precisely because it pointed out the possibility of talking about truth in the absence of any general criterion of truth that Popper was so much cheered by Tarski’s theory of truth when it was explained to him in the *Volksgarten* in Vienna in 1935 (Popper [1974], §20; Miller [1999a], pp. 63-65).

The point has been made above, but it deserves to be made again. Unless it is identified with radical relativism, radical scepticism is not only ‘logically impeccable’ but logically compelling. It was an error on Russell’s part to suggest that it is ‘psychologically impossible’, and an unworthy slur to insinuate that ‘there is an element of frivolous insincerity in any philosophy which pretends to accept it’ ([1948], p. 9). On the contrary, it can be only ‘[c]arelessness and inattention’, as Hume ironically put it ([1739], Part IV, the end of §ii), that allow men so easily to banish sceptical doubts, and to be ‘so sure’ of themselves and of their knowledge of the world. But scepticism need not keep company with relativism, and (as critical rationalism shows) it need not keep company with irrationalism either.

4 Conclusion

In conclusion, I should like to stress again that this article is not an outburst from an outraged philosopher, infuriated that scientists should have blundered into his territory. On the contrary, I welcome Sokal & Bricmont’s friendly incursion; and, if I may say it without sounding patronizing, I am impressed by how well they report what they have experienced on their expedition. To be sure, they commit some subtle

errors, such as the conflation (pp. 66f.) of the Duhem/Quine thesis (according to which a refuted prediction indicates the falsity only of the conjunction of a large ensemble of hypotheses, and not of any particular one of them) and the thesis of the underdetermination of theories by evidence (which is just Hume's problem of induction re-issued with a fancy name). But with the exception of the catastrophic mistake pinpointed above, Sokal & Bricmont's account of some of the staple topics of methodology is generally sound, and certainly more appreciative and well balanced than those of some other scientists (such as, for example, Wolpert [1992] and Friedlander [1995]; for reflections on the latter book see my article-review Miller [1998b]), not to mention quite a few philosophers. Nonetheless, the mistake in question is one of incalculable significance if we are interested in the workings of science, its standing, and its appeal to rational attention.

An invading power that suppresses a lawless insurrection may often, understandably but regrettably, taint its success by re-instating the same despotic regime that the rebels had been trying to overturn; whereas a wiser policy, one might think, would be to encourage the formation of a new government not implicated in the evils of the old. It seems to me that much the same mistake has been made here. Sokal & Bricmont have driven the postmodernist ruffians back into the jungle of the *quartier latin*; but, neglecting the claims of Popper's critical rationalism to offer just the right mixture of absolutism and scepticism, they have taken the advice of the old oligarchs, and have re-endorsed all those discredited authoritarian principles whose hollowness irrationalists have so often and so readily discerned. It is really rather shocking of Sokal & Bricmont to suggest as they do that 'ambiguities or inadequacies in Popper's *The Logic of Scientific Discovery*' (p. 59) bear some responsibility for the rising tide of irrationalism in the philosophy of science. The prime culprit in any objective sense is the evasive chicanery practised by traditional justificationism.

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