

Reply to Turnbull, Krips, Dusek and Fuller
For *Metascience*

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Biographical Note

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1 Introduction

In the preface to the second edition of *Intellectual Impostures*¹, we wrote that the criticisms of our book

can be divided roughly into four types. A (very) few reviewers discuss what we wrote and try to refute it. Other commentators raise objections (often perfectly valid ones) to ideas that are not in fact ours — and that we may have expressly *rejected* in the book — while attributing them to us implicitly or explicitly. Yet a third group of critics pretend to discuss our book, while actually doing something completely different: for example, attacking our personalities, our alleged motivations for writing the book, or the failings of scientists in general. And finally, some reviewers agree with us but think that we do not go far enough. (II, p. xv)

The comments by Turnbull and Dusek fall squarely into the second and third categories (apart from occasional brief excursions into category #1), while Krips and Fuller offer a mixture of the first and second categories. It would be a hopeless task to address *all* the issues raised in these essays, since in most cases it would simply amount to explaining, over and over again, that we do not hold — and most certainly have never written — the views attributed to us. Instead, we shall simply give, for each reviewer, a few examples of his misrepresentations or misunderstandings of our ideas, and then do our best to address the intellectually interesting issues that he raises.

Before proceeding further, however, let us remind the reader that our book comprises “two distinct — but related — works under one cover” (II, p. x). The largest part of the book is devoted to demonstrating that

famous intellectuals such as Lacan, Kristeva, Irigaray, Baudrillard and Deleuze have repeatedly abused scientific concepts and terminology: either using scientific ideas totally out of context, without giving the slightest justification — note that we are not against extrapolating concepts from one field to another, but only against extrapolations made without argument — or throwing around scientific jargon in front of their non-scientist readers without any regard for its relevance or even its meaning. We make no claim that this invalidates the rest of their work, on which we suspend judgment. (II, pp. ix–x)

In two vastly more subtle chapters (Chapters 4 and 7), we address widespread misconceptions about “postmodern science” and

dissect a number of confusions that are rather frequent in postmodernist and cultural-studies circles: for example, misappropriating ideas from the philosophy of science, such as the underdetermination of theory by evidence or the

¹Profile Books, London, 1999, hereafter denoted II. All citations of page numbers refer to this edition, which is identical to the first British edition except for the addition of a new preface (which does not alter the subsequent pagination). The American edition, entitled *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science* (Picador USA, New York, 1998), is identical to the first British edition except for spelling and occasional small differences of diction, but has different pagination.

theory-ladenness of observation, in order to support radical relativism. (II, p. x)

These two parts of our book must be evaluated separately; each reader has the perfect right to agree with our arguments on one topic but not the other, on both, or on neither.

2 David Turnbull

The most striking aspect of Turnbull's essay is its profusion of derogatory characterizations of our book ("nasty", "sneering", "overstate[s] the case", "foolishly overinflated rhetoric") and of our alleged personalities ("totalitarian inquisitor[s]", "lust for annihilation"), unsupported by even one concrete example. We challenge Turnbull to supply evidence to back up his purported descriptions of our book; and we submit that he will be unable to do so, because our book is in fact a measured and carefully reasoned critique of some texts that are, to say the least, rather extraordinary.²

Furthermore, Turnbull attributes to us views that are not ours and that are in many cases the exact *opposite* of what we have written unambiguously in the book:

1) "Why attribute failings of individual's [*sic*] arguments to all of some supposedly homogeneous group, be they constructivists, sociologists of science, postmodernists or whatever?" In fact we write:

Let us emphasize that these authors differ enormously in their attitude toward science and the importance they give it. They should not be lumped together in a single category, and we want to warn the reader against the temptation to do so. (II, p. 7)

And again:

[T]he intellectual abuses criticized in this book are not homogeneous; they can be classified, very roughly, into two distinct categories, corresponding roughly to two distinct phases in French intellectual life. The first phase is that of extreme structuralism ... The second phase is that of poststructuralism ... [O]ur arguments must be judged, for each author, independently of his or her link — be it conceptually justified or merely sociological — with the broader 'postmodernist' current. (II, pp. 11–12)

2) "Problematizing progress is one of the currents of postmodernism that Sokal and Bricmont find so objectionable." In fact we stress that

²A more accurate description of our book was given by the American philosopher Thomas Nagel in his review for *The New Republic*:

Nearly half the book consists of extensive quotations of scientific gibberish from name-brand French intellectuals, together with eerily patient explanations of why it is gibberish. This is amusing at first, but becomes gradually sickening. ... We are offered reams of this stuff, from Jacques Lacan, Julia Kristeva, Bruno Latour, Jean-François Lyotard, Jean Baudrillard, Gilles Deleuze, Régis Debray, and others, together with comments so patient as to be involuntarily comic. (Nagel 1998, p. 33)

many ‘postmodern’ ideas, expressed in a moderate form, provide a needed correction to naive modernism (belief in indefinite and continuous progress, scientism, cultural Eurocentrism, etc.). What we are criticizing is the radical version of postmodernism, as well as a number of mental confusions that are found in the more moderate versions of postmodernism and that are in some sense inherited from the radical one. (II, p. 174)

3) “They [Sokal and Bricmont] are seeking to dismiss the possibility of the critical examination of science.” In fact, we explicitly *encourage* such examination; we object only to sloppy ways of doing it, of which we provide myriad examples. We begin by noting that

it is crucial to distinguish at least four different senses of the word ‘science’: an intellectual endeavour aimed at a rational understanding of the world; a collection of accepted theoretical and experimental ideas; a social community with particular mores, institutions and links to the larger society; and, finally, applied science and technology (with which science is often confused). All too frequently, valid critiques of ‘science’, understood in one of these senses, are taken to be arguments against science in a different sense. (II, p. 190)

We then go on to state:

Thus, it is undeniable that science, as a social institution, is linked to political, economic and military power, and that the social role played by scientists is often pernicious. It is also true that technology has mixed results — sometimes disastrous ones — and that it rarely yields the miracle solutions that its most fervent advocates regularly promise. Finally, science, considered as a body of knowledge, is always fallible, and scientists’ errors are sometimes due to all sorts of social, political, philosophical or religious prejudices. We are in favour of reasonable criticisms of science understood in all these senses. (II, pp. 190–191)

Sokal’s *House Built on Sand* essay (cited in II, p. 175, footnote 244) provides further details:

The following propositions are, I hope, noncontroversial:

1) Science is a human endeavor, and like any other human endeavor it merits being subjected to rigorous social analysis. Which research problems count as important; how research funds are distributed; who gets prestige and power; what role scientific expertise plays in public-policy debates; in what form scientific knowledge becomes embodied in technology, and for whose benefit — all these issues are strongly affected by political, economic and to some extent ideological considerations, as well as by the internal logic of scientific inquiry. They are thus fruitful subjects for empirical study by historians, sociologists, political scientists and economists.

2) At a more subtle level, even the content of scientific debate — what types of theories can be conceived and entertained, what criteria are to be used for deciding between competing theories — is constrained in part by the prevailing attitudes of mind, which in turn arise in part from deep-seated historical factors.

It is the task of historians and sociologists of science to sort out, in each specific instance, the roles played by “external” and “internal” factors in determining the course of scientific development. ...

3) There is nothing wrong with research informed by a political commitment, as long as that commitment does not blind the researcher to inconvenient facts. Thus, there is a long and honorable tradition of socio-political critique of science, including antiracist critiques of anthropological pseudo-science and eugenics and feminist critiques of psychology and parts of medicine and biology. ... [E]mpirical studies of this kind, if conducted with due intellectual rigor, could shed useful light on the social conditions under which good science (defined normatively as the search for truths or at least approximate truths about the world) is fostered or hindered.³

Note, finally, that Sokal stresses (p. 17) his *agreement* with Philip Kitcher on nearly every issue under debate; Turnbull’s failure to mention this, at the same time as he praises Kitcher by way of contrast with us, borders on dishonesty.

4) “Sokal and Bricmont’s other general claim [is] that those authors they name have committed some scientific error or used science’s name in vain and must therefore stand condemned as heretics.” The heavy-handed allusion to the Inquisition is Turnbull’s own embellishment (dare we call it “foolishly overinflated rhetoric?”), unsupported by any honest reading of our book. Regarding “errors”, we explain in the Introduction that

it could be argued that we are splitting hairs, criticizing authors who admittedly have no scientific training and who have perhaps made a mistake in venturing onto unfamiliar terrain, but whose contribution to philosophy and/or the social sciences is nevertheless important and is in no way invalidated by the ‘small errors’ we have uncovered. We would respond, first of all, that these texts contain much more than mere ‘errors’: they display a profound indifference, if not a disdain, for facts and logic. Our goal is not, therefore, to poke fun at literary critics who make mistakes when citing relativity or Gödel’s theorem, but to defend the canons of rationality and intellectual honesty that are (or should be) common to all scholarly disciplines. (II, p. 6)

Furthermore,

it should be remembered that our criticism does *not* deal primarily with errors, but with the manifest *irrelevance* of the scientific terminology to the subject supposedly under investigation. In all the reviews, debates and private correspondence that have followed the publication of our book in France, no one has given even the slightest argument explaining how that relevance could be established. (II, p. 11, italics in the original)

Let us now discuss the three serious intellectual issues that Turnbull raises: whether any “writer in science studies” thinks that science is an illusion or a waste of time; the meaning of Latour’s Third Rule of Method; and the discovery of Neptune.

³Sokal (1998, p. 10).

For the first point, consider the following assertions by prominent figures in science studies:

[T]he validity of theoretical propositions in the sciences is in no way affected by factual evidence.⁴

The natural world has a small or non-existent role in the construction of scientific knowledge.⁵

For the relativist [such as ourselves] there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such.⁶

Science legitimates itself by linking its discoveries with power, a connection which *determines* (not merely influences) what counts as reliable knowledge ...⁷

Let us explain why we think that these statements effectively assert that science is an illusion. The goal of all scientists is to find out (some aspects of) how the world really is: if, for example, a biologist says that the (proximate) cause of a certain disease is a virus, she means to assert that as an objective fact about the world. She may of course be wrong⁸, but that does not change the fact that her *goal* is to accurately describe the world. If you tell her that her connection to power “determines (not merely influences)” what she regards as reliable knowledge, or that her beliefs can never be “really rational” but only “locally accepted as such”, then you are telling her in effect that her goal is impossible to reach or even to approach, i.e. that science — as she conceives it — is an illusion.

Statements as clear-cut as those just cited are, however, rare in the science-studies literature. More often one finds assertions that are ambiguous but can be interpreted (and quite often *are* interpreted) as meaning that science — viewed as an attempt

⁴Gergen (1988, p. 37).

⁵Collins (1981, p. 3). Two qualifications need to be made: First, this statement is offered as part of Collins’ introduction to a set of studies (edited by him) employing the relativist approach, and constitutes his summary of that approach; he does not *explicitly* endorse this view, though an endorsement seems implied by the context. Second, while Collins appears to intend this assertion as an empirical claim about the history of science, it is possible that he intends it neither as an empirical claim nor as a normative principle of epistemology, but rather as a methodological injunction to sociologists of science: namely, to act *as if* “the natural world ha[d] a small or non-existent role in the construction of scientific knowledge”, or in other words to *ignore* (“bracket”) whatever role the natural world may in fact play in the construction of scientific knowledge. In our contribution to a forthcoming volume of essays co-edited by Collins and Jay Labinger, we argue that this approach is seriously deficient *as methodology* for sociologists of science.

⁶Barnes and Bloor (1981, p. 27), clarification added by us.

⁷Aronowitz (1988, p. 204), emphasis in the original.

⁸Moreover, since standard philosophical arguments show that a consistent radical sceptic cannot be refuted, one can never be *absolutely sure* that she is right, no matter how much evidence she may offer to support her theory. But this observation, although correct, is basically irrelevant because of its generality: it applies to all our statements, including those made in everyday life and — it hardly need be added — those made by sociologists and historians.

to obtain an objective (albeit approximate and incomplete) understanding of (some aspects of) the world — is an illusion.

Indeed, Turnbull himself provides examples of such assertions in his essay. He admits that “science [is] the best problem solving process yet built”, but fails to make clear for solving *what* problem. For discovering objectively valid approximate truths about the world, perchance? Turnbull scrupulously avoids saying anything of the sort, and his slipperiness on this crucial issue — what is the goal of science? — is typical of many social constructivists. Later he asserts that “what counts as ‘fact’, ‘experiment’, ‘proof’, ‘evidence’ and ‘testing’ are historical products and hence similarly conventional and contingent.” Of course, standards of experimentation and analysis of evidence have evolved over time, so they are in some sense “historical products”; but that does not mean that they are “conventional” in anything like the ordinary sense of the word “convention”⁹, unless one thinks that science makes no progress — for otherwise, our “historical products” could simply be viewed as being better and better approximations to an ideal state of knowledge that would be far from being a convention. If you tell a scientist offering you evidence that the (proximate) cause of a certain disease is a virus that her evidence is “conventional and contingent”, and that this will be true no matter how much evidence she may gather, then you are indeed telling her that what she does is a waste of time and an illusion.

Concerning Latour’s Third Rule of Method [“Since the settlement of a controversy is the *cause* of Nature’s representation, not the consequence, we can never use the outcome — Nature — to explain how and why a controversy has been settled”¹⁰], Turnbull writes:

If we add the required clarification: i.e. Since the settlement of a controversy is the cause of Nature’s representation, not the consequence, we can never use the outcome — what Nature is now taken to be — to explain how and why a controversy has been settled. Latour’s point is clear but the stylistic impact is lost.

But that is exactly the first of the five interpretations that we give of Latour’s ambiguous formulation (II, p. 85). It is, as we note, true but trivial. Why turn such a banal statement into a Rule of Method? Even a Whiggish historian of science would not dispute this rule, so interpreted.¹¹

Concerning the discovery of Neptune, our main point was not the historically valid observation that this discovery reinforced (psychologically) people’s belief in

⁹E.g. that Australians drive on the left side of the road and Americans eat with the fork in the right hand.

¹⁰Latour (1987, pp. 99, 258), italics in the original.

¹¹Actually, Turnbull’s reference to “Whiggish history” suggests that perhaps he has another interpretation in mind, namely that we can never use Nature to explain how and why a controversy has been settled. This interpretation is also discussed in our book (II, pp. 85–86), and is either again banal (if it means that Nature *alone* cannot explain those settlements) or obviously false (if it means that Nature plays *no role* in explaining them). Note, by the way, that we explicitly *criticize* Whiggish history of science (II, pp. 69–70, footnote 88).

the validity of Newtonian mechanics, but that “it is hard to believe that such a simple theory could predict so precisely *entirely new* phenomena if it were not at least approximately true” (II, p. 61). The validity of this latter observation is independent of whether Adams and Le Verrier correctly computed the Newtonian prediction for the position of Neptune or found it partly by accident. The key fact is that if one does make the correct calculations based on Newton’s theory, then one indeed finds the actually observed position of Neptune (the same remark applies, of course, to thousands of other scientific predictions). This point is conceded by Turnbull when he writes: “Sokal and Bricmont are right, it does show Newton’s mechanics are approximately true”.

3 Henry Krips

Krips should be commended because he belongs to that small minority of our critics who actually try to refute our arguments in the “impostures” part of the book (as opposed to attacking our alleged motivations or our personalities). He attempts to show that we are unfair to Lacan and that Lacan’s “metaphoric extensions” of mathematical topology do play an intellectually useful role, contrary to what we claim.

However, as we shall show, Krips’ attempt fails miserably and actually provides more support for our thesis. First of all, his exposition of basic mathematical concepts such as closed, bounded and compact sets contains quite a few glaring errors. Here are some examples:

1) Contrary to Krips’ assertion, the family of open sets is *not* “closed under the operations of union and intersection . . . [e]ven when the union or intersection of subspaces is ‘extended to infinity’”. While it is true that arbitrary unions of open sets are open, in general only intersections of *finitely many* open sets give rise to open sets.

2) *Pace* Krips, a closed set is *not* one that is “not open”, but rather one whose *complement* is open. In most topological spaces, there are many sets that are neither closed nor open.¹²

3) Krips gives at first a correct statement of the Heine-Borel theorem; but while attempting to “transfer it metaphorically” to psychoanalysis, he makes an utter hash of it, saying it “means that even though the space as a whole is closed, there exists a finite open cover”. Later he even speaks of the “property of having a countable cover”.¹³ But in any topological space, *any* set has an open cover consisting of *just one* element, namely the space itself (which is always open, by definition of “open”).

¹²Indeed, in most topological spaces (and in particular in Euclidean spaces), there exist sets of *all four* logically possible types: open but not closed; closed but not open; neither closed nor open; both closed and open. The construction of such sets is a standard exercise for students in the first week of an undergraduate topology course.

¹³Krips and Lacan notwithstanding, “countable” in mathematics is *not* a synonym for “finite”; rather, it means a set that is *either* finite or else is infinite and can be put into one-to-one correspondence with the set $\{1, 2, \dots\}$ of the positive integers. See II, p. 39, footnote 38 for a slightly more detailed explanation.

The point of the Heine-Borel theorem is not to establish the (trivial) existence of finite open covers, but rather to establish the existence of finite open *subcoverings* of any given open cover.¹⁴

Now, Krips could very well reply to these criticisms by saying something like: “Here they go again, splitting hairs about irrelevant details. I am interested in psychoanalysis, not in pure mathematics.” Sure, those details are irrelevant to psychoanalysis, but that is exactly our point. If Krips or Lacan were trying to make useful structural analogies between psychoanalytical concepts and mathematical ones, then details *would* matter. After all, the main characteristic (and value) of mathematical statements is that they can be precisely formulated and, when they are theorems, rigorously proven. This is why mathematics offers such a powerful tool for making precise deductive reasoning in the natural sciences (and to some extent also in the social sciences). But when mathematical concepts are imprecisely or inaccurately stated, it is not clear what they can be good for.

In fact, Krips wants to have his cake and eat it too: he wants to claim that he limits himself to a metaphorical use of mathematics, and at the same time to use mathematics for deductive purposes, e.g. when he writes that if we “transfer this [Borel’s] theorem metaphorically to the space of sexual relations, then we are licensed to infer the following conclusion.” No! Vague metaphors with ill-stated mathematical theorems do not “license” any inference whatsoever.

Note also that Krips, like Lacan, speaks repeatedly of “the space of sexual relations — also referred to as the space of *jouissance*”; but he fails to come to grips with our central objection to Lacan’s use of topology, namely that

Even if the concept of ‘*jouissance*’ had a clear and precise meaning, Lacan provides no reason whatsoever to think that *jouissance* can be considered a ‘space’ in the technical sense of this word in topology. (II, p. 19)

How, precisely, is the “space of sexual relations” defined? What are its points, and what are its open sets? Unless Krips can answer these questions, any “transfer” of the Heine-Borel theorem is beside the point.

Suppose, finally, that instead of using mathematics for deductive purposes, we use it solely in order to suggest fruitful analogies.¹⁵ Krips argues that such a use of topology in psychoanalysis would be analogous to physicists’ speaking of (classical) waves and particles in order to help them understand quantum objects; but exactly the opposite is the case. Physicists speak of waves and particles in order to relate a new and hard-to-understand quantum object to simpler and more familiar classical

¹⁴Krips also asserts that we “failed to spot Lacan’s references to Borel’s theorem”. Quite the contrary: we were perfectly aware that the Lacan passages quoted in II, pp. 21–23 contain garbled allusions to the mathematical theory of compactness (including the Heine-Borel theorem); that is why we supplied the reader with a brief but un-garbled explanation of this theory (II, p. 21, footnote 22). The upshot is that “although Lacan uses quite a few key words from the mathematical theory of compactness, he mixes them up arbitrarily and without the slightest regard for their meaning. His ‘definition’ of compactness is not just false: it is gibberish.” (II, p. 21)

¹⁵Please note that any such analogies would have heuristic value but no demonstrative value. Any assertions suggested by such analogies would have to be supported by evidence *in the target field*.

objects. But connecting sexual relations to the Heine-Borel theorem relates more-or-less intuitive human notions to abstract mathematical concepts that Krips himself — like Lacan — manifestly does not understand.¹⁶

4 Val Dusek

Dusek’s “review” of *Intellectual Impostures* — the most erudite of the bunch — contains some interesting ideas, but few of them have much to do with anything we wrote. His essay is, rather, a scattershot attempt to cast aspersions on our book without actually addressing its arguments. In some cases, Dusek riffs at length on themes that are at best peripherally related to the topics we address, while nevertheless implying that his ideas somehow constitute a refutation of ours; in other cases, Dusek explicitly misrepresents our positions. Here are a few examples:

1) Dusek defends at length Bergson’s philosophical ideas, but we do not discuss those ideas at all in our book, much less “dismiss Bergson as a fool”; we limit ourselves to analyzing Bergson’s misunderstandings of relativity (see below). It goes without saying that we never accuse Bergson’s philosophy of being “proto-Nazi”.

2) Dusek asserts that we “take errors of particular applications of philosophical ideas to science as refutations of the whole general framework utilized”, but in fact we explicitly warn the reader *against* any such inference (II, pp. ix–x, 6).

3) Dusek claims that we “reassure non-scientists that chaos theory and quantum mechanics have not radically changed the nature of the universe presented by sci-

¹⁶Along the way, Krips also concocts a novel (to our knowledge) misunderstanding of our ideas when he asserts that “S&B require . . . [that] metaphoric extension[s] of concepts . . . be ‘tested empirically’” (whatever that might mean). In fact, he is running together two quite distinct discussions in our book. On the one hand, we criticize Lacan and others for

importing concepts from the natural sciences into the humanities or social sciences without giving the slightest conceptual or empirical justification. If a biologist wanted to apply, in her research, elementary notions of mathematical topology, set theory or differential geometry, she would be asked to give some explanation. A vague analogy would not be taken very seriously by her colleagues. Here, by contrast, we learn from Lacan that the structure of the neurotic subject is exactly the torus (it is no less than reality itself, cf. p. 19), from Kristeva that poetic language can be theorized in terms of the cardinality of the continuum (cf. p. 38), and from Baudrillard that modern war takes place in a non-Euclidean space (cf. p. 137) — all without explanation. (II, p. 4)

On the other hand, we observe several pages later that

Many authors, including some of those discussed here, try to argue by analogy. We are by no means opposed to the effort to establish analogies between diverse domains of human thought; indeed, the observation of a valid analogy between two existing theories can often be very useful for the subsequent development of both. Here, however, we think that the analogies are between well-established theories (in the natural sciences) and theories too vague to be tested empirically (for example, Lacanian psychoanalysis). One cannot help but suspect that the function of these analogies is to hide the weaknesses of the vaguer theory. (II, p. 9)

ence” and that we “debunk claims that twentieth century science has undermined determinism or the independence of the observer from the observed”; but in fact we nowhere discuss quantum mechanics or the status of determinism and observation in twentieth-century physics (much less do we “debunk” claims about them!).¹⁷ Finally, our discussion of chaos theory has the modest goal of giving non-scientists a succinct introduction to the basic ideas of that theory and of warning them against certain misuses and “hasty philosophical conclusions” (II, pp. 128–136). We never assert that chaos theory has *no* philosophical implications.¹⁸

4) Dusek discusses Feynman’s well-known antipathy to philosophy; but what does that have to do with our book? As should be clear from our long philosophical chapter (II, pp. 49–95), our own attitude is quite the opposite of Feynman’s (and rather closer to Einstein’s). And what on earth does our book have to do with Allan Bloom and the Oxford ordinary-language philosophers (who are nowhere mentioned or even alluded to)?

5) Dusek, engaging in a bit of extrasensory perception, asserts that “evidently Anglo-American analytic philosophers convinced Sokal and Bricmont to ignore Bergson in the English edition”. In fact, we decided on our own to omit the Bergson chapter, simply because Bergson is much less well known in the English-speaking world than in France. (And anyway, we were weary of translating and glad for any excuse to reduce our workload.)

6) Dusek terms Bourdieu our “ally”, on the alleged grounds that we thank him in the preface. For what it’s worth, we thank a lot of people who made remarks or suggestions concerning our book, including some people who were violently hostile to it (e.g. Vincent Fleury).

7) Finally, Dusek insinuates that our criticism of postmodernist writers is motivated by “jealousy” and, in Bricmont’s case, by “resentment”. This type of facile psychologizing is a perfect example of attacking our alleged motivations rather than our reasoning.¹⁹ Even if our motivations were as ascribed — and they most certainly are not — how would that affect the validity or invalidity of our arguments?

Let us now reply briefly to Dusek’s remarks concerning Bergson and Deleuze:

Bergson. As noted above, we do not “dismiss Bergson as a fool”, nor do we suggest that his philosophy of time is “nonsense”.²⁰ We explicitly say that we “leave

¹⁷For what it’s worth, quantum mechanics *does* radically change the fundamental conceptual structures of physics, but the exact nature of that change (e.g. whether it involves indeterminism, nonlocality, etc.) is far from clear today and is in fact hotly debated by physicists and serious philosophers of physics. For an exposition of one point of view, see Bricmont (1995b, 1999).

¹⁸Indeed, in footnote 176 (p. 130) we say that “Kellert (1993) gives a clear introduction to chaos theory and a sober examination of its philosophical implications, although we do not agree with all of his conclusions.” For a more detailed discussion of some philosophical issues related to chaos theory, see Bricmont (1995a).

¹⁹See II (second edition), pp. xviii–xix for further examples of name-calling and attacks on our alleged motivations.

²⁰Should someone asserting that Einstein’s views on quantum mechanics are wrong be accused of

open” the question of whether “Bergson’s conception of time can be reconciled with relativity” (1997 French edition of II, p. 167). We do not criticize later French philosophers for “praising Bergson”, but rather for repeating his errors concerning relativity, almost literally, long after they were pedagogically corrected by physicists (pp. 167–168, 178–182). And, finally, our main point is that Bergson actually defends a physical theory that makes *different empirical predictions* from Einstein’s, but without recognizing that fact; he and his followers persist in claiming that the debate is only about the “interpretation” of relativity theory (pp. 166, 176).

Concerning the twin effect²¹, we are well aware that the solution can be discussed in several different ways (using only inertial frames or using accelerating frames). Indeed, we mention this in a footnote (no. 229, p. 177), where we stress that “even some physics textbooks” make a “subtle mistake”. So we are not surprised that Marder could write a book discussing alternative analyses of the twin effect offered by physicists, some of which are “mutually incompatible” and some of which are, as Marder observes, simply wrong.²² But if Dusek finds our own solution objectionable, why doesn’t he say so and explain why?²³ Dusek seems to assert, like Bergson, that because “velocity is relative”, the two twins must be in a symmetrical situation, so that one could not be younger than the other; however, as we explain in detail in our book (pp. 169–170, 176–177), velocity is relative but acceleration is not, and only one of the twins undergoes accelerations (with respect to an inertial frame). Finally, the fact that some mathematicians, physicists or philosophers find Bergson’s intuitions about time interesting does not in any way constitute an objection to our analysis of Bergson’s errors.

Concerning the “third observer”, introduced by Latour, but which Dusek reinterprets as being, from Bergson’s point of view, the “self-conscious theorist” thinking about himself and other observers, we explicitly discuss this interpretation of Bergson’s writings (p. 178; see also pp. 173, 179, 182). We note, however, that Bergson makes definite predictions concerning the behaviour of moving *clocks* — and not only conscious beings — that contradict those of relativity theory (pp. 175–176).

Deleuze. Dusek admits that Deleuze’s books co-authored with his “buddy” Guattari are “wild and unbuttoned” (to put it mildly), but he wants to defend the value of

dismissing him as a fool?

²¹Sometimes misleadingly called the “twin paradox”. As we emphasize (pp. 180–181),

The assertions of relativity are indeed shocking at first sight. But they are “paradoxical” only in the sense that they contradict our *prejudices*, not in the sense that they contain any logical contradiction. Moreover, these “paradoxical” predictions have been verified experimentally (at least for clocks); our prejudices are quite simply *false* (though they are good approximations when speeds are small compared to the speed of light).

²²The book in question is Marder (1971).

²³Our solution is by no means original: it is the standard one taught in all special-relativity courses, and is identical to the one explained personally to Bergson by Becquerel and Metz.

Deleuze's earlier works of academic philosophy. That's a perfectly respectable goal, and we take no position on its ultimate merits. How many times need we repeat (II, pp. ix–x, 6) that the purpose of our book is not to analyze all of Deleuze's works, or even all of his references to mathematics? Our aim is much more limited: to question the usefulness, philosophical or otherwise, of the “avalanche of ill-digested scientific (and pseudo-scientific) jargon” (II, p. 146) that permeates several of Deleuze's books both with and without Guattari.²⁴

Dusek correctly summarizes our belief that “scientific metaphors ... would not be illuminating to an audience ignorant of science.” He retorts that mathematical structures can be used as “models for metaphysical speculation”, but without giving a single example where such models are put to fruitful use by Deleuze or Guattari, much less where they are explained in a way that could be “illuminating to an audience ignorant of science”.

Dusek objects that philosophical and mathematical problems related to infinitesimals could still be raised after the work of Cauchy. We agree entirely: for example, serious philosophers of mathematics have been discussing, in recent years, the conceptual issues raised by non-standard analysis. But that is light years from what Deleuze does, which is to launch long meditations on classical (mostly pre-Cauchy) topics, characterized by a bizarre mixture of pedantry and confusion. Neither Dusek nor anyone else has yet offered even a glimpse of which genuine intellectual role is fulfilled by this display of mathematical pseudo-erudition in Deleuze's work. Nor is Dusek's throwing around the names of mathematicians and philosophers (Wronski, Maimon, Lautman, Vuillemin, Herbrand, Cavailles and Dieudonné all in the space of a single paragraph!) a substitute for concrete analysis of ideas and arguments.

Dusek observes correctly that

Ironically, two of the passages in Deleuze that they [S+B] ridicule assert that relativity theory, measurement in quantum theory, and information in statistical mechanics should not be interpreted subjectively (pp. 149–150). This agrees with Sokal and Bricmont's own position, but they do not note this. It would spoil the fun.

We were certainly aware of the irony; but the passages in question are so garbled they are useless as arguments in favour of *any* position.

Finally, Dusek asks:

Marx's side-kick Friedrich Engels wrote far worse stuff concerning elementary algebraic operations and the dialectic. Would leftist Sokal move from a similar discussion of Marx and Engels on mathematics to discrediting Marx's insights about capitalism as *Intellectual Impostures* moves from Lacan's, Irigaray's or Kristeva's mathematical errors to question their honesty?

First of all, as we make abundantly clear (II, pp. ix–x, 6), we do *not* draw general conclusions about the value of Lacan's, Irigaray's or Kristeva's work, and we explicitly *refrain* from guessing whether their abuses result from dishonesty or from gross

²⁴Note also that we give many references to texts that, for lack of space, we refrain from quoting, in order to illustrate “the ubiquity of pseudo-scientific language in Deleuze and Guattari's work” (as well as in some of Deleuze's sole-authored work): see II, p. 151 (footnote 207) and p. 158.

incompetence (II, pp. 5–6). Our goal is, rather, to expose discourses that have a reputation of being “deep but obscure” and about which we can show, at least when mathematics and physics are invoked, that the obscurity is unnecessary. Engels’ writings on mathematics, by contrast, are deeply confused but not terribly obscure.

In conclusion, Dusek’s comments remind us of the observation made by Jacques Bouveresse, professor of philosophy in the Collège de France, that whereas our background as scientists should allow us to understand the technical concepts invoked by Deleuze *et al.*, were they to make any sense, we face people who, without providing any details, “nevertheless claim that what they do not understand may actually very well be understood.”²⁵

5 Steve Fuller

Like Krips, Fuller should be commended because he tries to address some of the issues raised in our book, focusing on its epistemological part (II, Chapter 4). His criticisms — that we are naive in linking science to everyday rationality and that our views on the history of science are naive as well — reflect misunderstandings that are sufficiently common to merit some discussion.

Let us begin, however, by pointing out some rather gross deformations of our arguments. Fuller claims that we “want to trace lapses from professionalism to a relativist philosophical sensibility, which in turn is held responsible for the dissipation of the US academic left”, and that we “attempt to trace all postmodern crimes against science to an omnibus bogey, relativism”. This conflates at least three distinct topics treated in our book, which we take pains to distinguish already in the preface (II, pp. x–xii); it attributes to us alleged links that we did not assert and do not, in fact, believe. Indeed, we repeatedly stress that the “impostures” part of our book has only a tenuous link with relativism, and we are careful to separate both of these issues from political considerations.

Let us now discuss Fuller’s “six curiosities”:

1. Fuller radically misunderstands the connection we draw between the scientific attitude and everyday rationality, by disregarding our clearly-stated distinction between the *methodology* of science (which, in our view, extends and perfects ordinary rationality) and the *substantive content* of scientific knowledge (which frequently *contradicts* “common sense”):

For us, the scientific method is not radically different from the rational attitude in everyday life or in other domains of human knowledge. Historians, detectives and plumbers — indeed, all human beings — use the same basic methods of induction, deduction and assessment of evidence as do physicists or biochemists. Modern science tries to carry out these operations in a more careful and systematic way, by using controls and statistical tests, insisting on replication, and so forth. Moreover, scientific measurements are often much more precise than everyday observations; they allow us to discover hitherto unknown phenomena;

²⁵Bouveresse (1999, p. 8).

and they often conflict with ‘common sense’. But the conflict is at the level of conclusions, not the basic approach. (II, p. 54)

We are thus perfectly aware of the difference between the manifest and scientific images of the world, to which Fuller refers; indeed, we stress it.²⁶ And in a footnote, we add:

Throughout this chapter, we stress the methodological continuity between scientific knowledge and everyday knowledge. This is, in our view, the proper way to respond to various sceptical challenges and to dispel the confusions generated by radical interpretations of correct philosophical ideas such as the underdetermination of theories by data. But it would be naive to push this connection too far. Science — particularly fundamental physics — introduces concepts that are hard to grasp intuitively or to connect directly to common-sense notions. (For example: forces acting instantaneously throughout the universe in Newtonian mechanics, electromagnetic fields ‘vibrating’ in vacuum in Maxwell’s theory, curved space-time in Einstein’s general relativity.) And it is in discussions about the meaning of these theoretical concepts that various brands of realists and anti-realists (e.g., instrumentalists, pragmatists) tend to part company. Relativists sometimes tend to fall back on instrumentalist positions when challenged, but there is a profound difference between the two attitudes. Instrumentalists may want to claim either that we have no way of knowing whether ‘unobservable’ theoretical entities really exist, or that their meaning is defined solely through measurable quantities; but this does not imply that they regard such entities as ‘subjective’ in the sense that their meaning would be significantly influenced by extra-scientific factors (such as the personality of the individual scientist or the social characteristics of the group to which she belongs). Indeed, instrumentalists may regard our scientific theories as, quite simply, the most satisfactory way that the human mind, with its inherent biological limitations, is capable of understanding the world. (II, p. 55, footnote 56)

While the subtle debates between moderate forms of realism and instrumentalism lie beyond the scope of our epistemological chapter — whose goal is to dissect far grosser confusions about science and its methods — we are quite aware of them and have discussed them elsewhere.²⁷

2 and 3. Fuller’s claim notwithstanding, we do *not* “assum[e] that there is some pre-scientific (natural?) need to explain the coherence of our experience”, nor do we make any assertions whatsoever concerning the historical, social and biological factors leading to the development or non-development of scientific thought; these questions are not addressed at all in our book.²⁸ It goes without saying that we

²⁶For example, we note that “water appears to us as a continuous fluid, but chemical and physical experiments teach us that it is made of atoms.” (II, p. 55, footnote 55)

²⁷See e.g. Bricmont (2000).

²⁸We do, of course, make the trivial observations that social factors play *at least* the role of enabling or disabling conditions on science, and that our biological constitution may impose limits on what we are able to know.

are not “committed to the idea that the search for knowledge [except in post-17th-century Europe] has been usually retarded or otherwise perverted”, nor do we hold a “crypto-teleological vision of epistemic growth”.

4. Fuller writes:

The intractability of the problem of induction is presented as a major philosophical reason for science studies going down the path of relativism-scepticism. While there is some truth to this observation, it is cast in the wrong light. The source of concern is not that there is no foolproof means of determining whether the sun will rise tomorrow; rather it is that there is no foolproof means of determining whether, if the sun rises tomorrow, it will be for the same reason as it did yesterday. Clearly, if the sun fails to rise, then the background assumptions that made us think it would are thrown into doubt. But we still have reason to be sceptical, even if the sun does rise.

Of course! But several issues need to be disentangled:

First of all, if we begin our discussion of epistemology by briefly addressing the problems of solipsism and radical scepticism (II, pp. 51–54), it is precisely in order to get red herrings out of the way by conceding that these doctrines are irrefutable (which is not to say that anyone really believes them, or that there is any reason to believe they are true). But as we stress,

the key observation is that such scepticism applies to *all* our knowledge: not only to the existence of atoms, electrons or genes, but also to fact that blood circulates in our veins, that the Earth is (approximately) round, and that at birth we emerged from our mother’s womb. Indeed, even the most commonplace knowledge in our everyday lives — there is a glass of water in front of me on the table — depends entirely on the supposition that our perceptions do not *systematically* mislead us and that they are indeed produced by external objects that, in some way, resemble those perceptions.

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. (II, p. 53)

Secondly, Fuller (following Goodman) is correct in pointing out that the problem of induction involves, among other things, that of determining which perceived commonalities in the world correspond to categories to which induction could validly be applied — in other words, the problem of determining which of our theories (and metatheories) are at least approximately true. Science addresses this problem not by finding some *a priori* solution to it — which in our opinion is impossible — but rather by developing theories with wide-ranging explanatory and predictive power and using the empirical successes of those theories as *a posteriori* justifications of their probable approximate truth, in full knowledge that there are no absolutely certain *a priori* grounds on which to base *any* empirical knowledge (even in everyday life).

But neither of these is our main point, for, *pace* Fuller, we do *not* regard sceptical arguments about induction “as a major philosophical reason for science studies going

down the path of relativism-scepticism". Rather, as we demonstrate through numerous examples in our book, science-studies practitioners sometimes invoke radical-sceptical arguments, *but only in a highly selective manner*: to cast doubt on the validity of theories in the natural sciences. But if, as Fuller rightly observes, there is no foolproof way to determine "whether, if the sun rises tomorrow, it will be for the same reason as it did yesterday", then surely there is no foolproof way to determine whether some theory about the relations between Boyle, Hobbes, the Royal Society and the social classes of seventeenth-century England is true or not. Physicists, historians and sociologists are in the same boat: that of empirical research, from which "absolute certainties" (such as those of speculative philosophy or revealed religions) are excluded. So why rehearse constantly that fact, and state it as if it applied only to the natural sciences?²⁹

5. In our book we argue, as Fuller accurately summarizes, that although the empirical evidence in favour of the atomic hypothesis was less than overwhelming when Dalton first proposed it, we nevertheless "have *today* so much evidence in favour of atomism (much of which is independent of chemistry) that it has become irrational to doubt it" (II, p. 69). Fuller objects that this is "a superstitious appeal to history" and that (unspecified) alternative theories not been given a "fair trial". But does Fuller *really* believe this claim, in the face not only of 150 years of evidence from chemistry and statistical mechanics, but above all in the face of the stunning successes of quantum mechanics in *explaining* and *predicting* a vast range of chemical and

²⁹Indeed, if the accurate prediction of unexpected phenomena, and the subsuming of diverse phenomena under a simple explanatory theory, are indications (albeit not foolproof ones) that a theory is on the right track, then surely there is no contest between physics and history or sociology. As we pointed out in the book,

the evidence of the Earth's rotation [to take just one example] is vastly stronger than anything Kuhn could put forward in support of his historical theories. This does not mean, of course, that physicists are more clever than historians or that they use better methods, but simply that they deal with less complex problems, involving a smaller number of variables which, moreover, are easier to measure and to control. It is impossible to avoid introducing such a hierarchy in our beliefs [based on the quantity and quality of the evidence supporting them], and this hierarchy implies that there is no conceivable argument based on the Kuhnian view of history that could give succor to those sociologists or philosophers who wish to challenge, in a blanket way, the reliability of scientific results. (II, pp. 72–73)

physical phenomena, sometimes to 11 decimal places accuracy?³⁰ And if Fuller really does believe this, would he then be willing to renounce using drugs that were designed by computer simulations based on the atomic theory?³¹ Atomic theory, grounded nowadays in quantum mechanics and electrodynamics, is not simply “better than its competitors in explaining and predicting what interests us today”, as Fuller asserts; it also succeeds in explaining and predicting what interested chemists and physicists 200 years ago, in ways that they would have considered utterly miraculous.

6. Regarding our objection to a strong interpretation of Kuhn’s incommensurability thesis, here is what we said (quoting the philosopher of science Tim Maudlin):

If presented with a moon rock, Aristotle would experience it as a rock, and as an object with a tendency to fall. He could not fail to conclude that the material of which the moon is made is not fundamentally different from terrestrial material with respect to its natural motion. Similarly, ever better telescopes revealed more clearly the phases of Venus, irrespective of one’s preferred cosmology, and even Ptolemy would have remarked the apparent rotation of a Foucault pendulum. The sense in which one’s paradigm may influence one’s experience of the world cannot be so strong as to guarantee that one’s experience will always accord with one’s theories, else the need to revise theories would never arise. (Maudlin 1996, p. 442, cited in II, pp. 71–72)

Fuller objects that “such a thought experiment begs the question against the incommensurability thesis”, because it undermines the “crucial supposition” that “the mutual incomprehensibility of paradigms is supposed to reflect a breakdown in communication between two largely self-contained research communities.”

³⁰One stunning example is cited in our book:

[Q]uantum electrodynamics predicts that the magnetic moment of the electron has the value

$$1.001\,159\,652\,201 \pm 0.000\,000\,000\,030 ,$$

where the “±” denotes the uncertainties in the theoretical computation (which involves several approximations). A recent experiment gives the result

$$1.001\,159\,652\,188 \pm 0.000\,000\,000\,004 ,$$

where the “±” denotes the experimental uncertainties. This agreement between theory and experiment, when combined with thousands of other similar though less spectacular ones, would be a miracle if science said nothing true — or at least *approximately* true — about the world. (II, pp, 55–56)

In particular, the predictive success of quantum mechanics would be a miracle if electrons and atoms did not really exist in some sense or other. (We say “in some sense or other” in order to emphasize that electrons, quarks, etc. may not belong to the fundamental ontology of the universe, but may only be — as we now know that Dalton’s “atoms” are — merely approximations objectively valid at certain scales of size and energy.) For further discussion of the history of the electron-magnetic-moment problem, see Lautrup and Zinkernagel (1999).

³¹Questions like this are often considered demagogic, but we fail to see why. The fact that new molecules can be designed by computer simulation and *then* synthesized in the laboratory is far from the cleanest evidence in favour of atomic theory, but it *is* powerful evidence nonetheless.

But this reply misses our point, which was to refute the radical idea — which people often attribute to Kuhn, whether or not he intended it — that “changes of paradigm are due principally to non-empirical factors and, once accepted, they condition our perception of the world to such an extent that they can *only* be confirmed by our subsequent experiences” (II, p. 70). Indeed, Fuller himself adopts this radical interpretation when he says that “the benchmark here is Kuhn’s incommensurability thesis, according to which scientists cannot recognize evidence that contradicts the fundamental tenets of their paradigm.” But if that were true, then, as Maudlin observes, the need to revise fundamental theories (such as Newtonian mechanics) would never arise.

Sure, Aristotle might or might not agree with Galileo — either at first or upon reflection — in interpreting the fall of a moon rock. He might even fail to understand the theory Galileo is trying to present. But all that is irrelevant to our (and Maudlin’s) point, which concerns not the theoretical interpretations but simply the perceptions. Could Aristotle really fail to perceive that a moon rock, when dropped, falls to the ground? Could Ptolemy fail to notice the precession of a Foucault pendulum? No doubt all perception is in part theory-laden; but, as Maudlin observes, the sense in which one’s paradigm may influence one’s experience of the world cannot be *so* strong as to guarantee that one’s experience will always accord with one’s theories.

Let us conclude by pointing to a “fundamental curiosity” in Fuller’s text, namely his statement that

constructivist narratives of ‘science in action’ typically show that there is no fact of the matter which statements are true or false until closure (often misleadingly called ‘consensus’, so as to mask the power relations involved) is reached over what the relevant agents are thought to have accomplished.

Does Fuller *really* mean to assert that there was no fact of the matter about the existence and orbit of (the celestial object we now call) Neptune before the astronomers (the “relevant agents”) reached “closure”? And if not, what on earth *does* he mean to assert?

Fuller continues:

In that respect, the distinction between ontology and epistemology collapses, as the existence of an entity becomes dependent on our mode of access to it, which once established may change and perhaps even be reversed over time, thereby rendering intelligible the idea that entities can go in and out of existence. To be sure, this is a controversial position that presupposes an open-ended, process-oriented metaphysics. However, as such, it does not require the commission of any philosophical errors; rather it implies a philosophically respectable position, known most broadly as ‘antirealism’.

Suppose that, after a nuclear war, the human species becomes extinct. Will Neptune then kindly go “out of existence” — since our “mode of access to it” will have disappeared along with our race — in order to vindicate Fuller’s “open-ended, process-oriented metaphysics”?

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