Commentary on Professor Barnes' paper "On Social Constructivist Accounts of the Natural Sciences"

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There is much in Professor Barnes' paper with which I can agree. The only trouble is: the parts with which I can agree are so uncontroversial that virtually *anyone* can agree with them.

> [S]ocial constructivism does not entail a denial of the existence of an external world ... and it entails neither a refusal to regard scientists as competent specialists who investigate aspects of that world nor the implication that what scientists themselves believe about that world is false. All that it does entail is a commitment to the study of how knowledge of that world is created and sustained ...

If that is indeed *all* that social constructivism entails, how could even the most rabidly realist scientist or philosopher object?

> [T]he provenance and credibility of all human knowledge should be understood causally and naturalistically, through the study of contingent social processes ...

Here some philosophers who believe in *a priori* knowledge might object, but I won't; I'm all in favor of studying the world "causally and naturalistically" wherever that's feasible. Furthermore, Barnes, unlike many constructivist sociologists, takes care to emphasize the causal impact of the external world on human beings: he is aware (despite the phrasing of the sentence just quoted) that the contingent processes involved in generating human belief are not purely "social".[1] So once again I applaud. The only thing I fail to understand is why Barnes calls this view "relativism".[2]

Sometimes what Barnes says is thoroughly unobjectionable, and the trouble comes only in what he *omits* to say. For example, Barnes observes that all human communities exhibit "trust in existing custom and practice, and a willingness to take account

[1] Thus, my belief that the earth is approximately spherical is due in part to the fact that it *is* approximately spherical: for if the earth were (for example) flat or tetrahedral, present-day technology would allow me to know that. On the other hand, the earth's approximate sphericity is not by itself *sufficient* to cause my belief: for if I had grown up in an isolated village without access to modern communications or transportation, I would probably believe (with good reason, in fact) that the earth is approximately flat. All these observations are, of course, utter trivialities.

[2] Perhaps Barnes is alluding here to the *methodological relativism* that he (along with many other sociologists) thinks is "consistent with and even required by a scientific approach" to the study of human beliefs. But, as Bricmont and I have argued in detail elsewhere, methodological relativism — such as that embodied in the Strong Programme's "symmetry principle" — is not only unnecessary to the scientific study of human belief; it is in fact antithetical to it. See Bricmont and Sokal (2001, especially pp. 39–43, 179–182 and 244–253).

of the authority of existing users" — which is absolutely true (though hardly a revelation). But what Barnes fails to add is that trust may in different communities be more or less sweeping, more or less critical, more or less rationally justified. To lump together scientists' (limited) trust in the probable accuracy of their colleagues' raw data with fundamentalists' trust in the literal truth of the Bible is to note a trivial commonality ("trust") while ignoring the crucial epistemological and sociological difference.

Likewise, Barnes urges that

> for all bodies of knowledge without exception, not only their initial evaluation and acceptance, but their use and application in every particular case, are contingent human actions, and need to be understood as such.

Absolutely correct: but it is crucial to observe that "contingent" does not mean "arbitrary" or "not subject to rational epistemological evaluation". Barnes goes on to note that "theories do not come with users' handbooks", and he observes that the magnetic moment of the electron was predicted not by "Quantum Electrodynamics" as such, but rather by humans exercising judgment in how to apply the theory called Quantum Electrodynamics. All this is true and important: scientific theories are far more complex objects than the logical systems of pure mathematics; they are expressed largely in natural language (with all its potential ambiguities), invoke many auxiliary hypotheses (which are frequently implicit), and require judgment in application (for example, in deciding which effects need to be taken into account and which are so negligible that they can safely be neglected). But this does not mean that such judgments are arbitrary or are immune from rational evaluation. And the fact that these judgments are exercised by human beings working in a community does not mean that the most important factors causing these judgments are necessarily "social".[3,4]

In some cases, however, I must part company with Barnes.

> [Social constructivist] studies identify no fundamental differences between the various contexts, within the sciences and without, wherein knowledge is sustained and applied.

Does this mean that constructivist sociologists, after careful empirical study of the communities of astronomers and astrologers, have been unable to "identify" any fundamental differences between the methodologies and epistemologies that these two

[3]For a careful history of the theory and experiments concerning the magnetic moment of the electron, in which the judgments made by both theorists and experimenters are analyzed, see Lautrup and Zinkernagel (1999).

[4]In the same passage, Barnes objects to the phrase "the goal of science" used by Bricmont and myself: he complains that we are "reifying science into a thing with its own readily identifiable properties". But we are doing nothing of the kind: "the goal of science" is simply a convenient shorthand for "the goal of the scientific endeavor", and is analogous to the phrase "the goal of chess is to capture your opponent's king."

groups employ? (If so, I can only conclude that the sociologists in question are incompetent.) More likely, however, it means that constructivist sociologists start from the *presupposition* that there are no fundamental differences between astronomers and astrologers.[5]

Referring to the relativism he defends, Barnes says:

> I myself like to turn to cartography for an appropriate metaphor. There is a certain very obvious sense in which different maps of a given terrain should be understood relativistically, as *all on a par with each other, with none standing in any closer correspondence with the mapped terrain than any other.* [emphasis added]

This is, in fact, an excellent example, which shows clearly what is *wrong* with radical relativism. Different maps — for instance, a Mercator projection and a polar projection, or a road map and a contour map — can indeed depict different aspects of reality and can be more useful or less useful for specified purposes.[6] But to say that *no* map stands in any closer correspondence with the mapped terrain than any other is absurd. Would Barnes really want to spend his holiday in Paris navigating with a street map of New York?

Barnes juxtaposes his "relativist" view to something he calls "strong realism", which according to him

> holds there to be just one conceptual scheme that describes the basic structure of reality and stands in correspondence with it.

But I know of no philosopher who actually holds such a view. (Rather, realist philosophers hold that there is just one *world*, and that theories are accurate or inaccurate to the extent that they do or do not stand in correspondence with it.) Barnes goes on to claim that

> [unnamed] proponents of this view often single out the concepts and theories of our current physical science, and account them this one scheme ...

— which would be even more absurd, since we know for a fact that our current theories *cannot* be the last word about the universe.[7] Fortunately, Barnes immediately adds

[5] Of course, whether this presupposition is true or false depends on what one means by "fundamental". To a biologist, there are indeed no fundamental differences between astronomers and astrologers: all are *homo sapiens*. But for a philosopher or sociologist to assert that there are no fundamental differences between astronomers and astrologers seems to me proof of either willful blindness or gross incompetence (most likely the former). For a concrete example of three prominent sociologists' extraordinarily sympathetic attitude toward astrology — in which they assert that Gauquelin's data allegedly demonstrating a "Mars effect" affecting the destiny of sports champions "could conceivably come to be accommodated as a triumph of the scientific method" — see Barnes, Bloor and Henry (1996, p. 141); and for a critique, see Mermin (1998a, pp. 621–622 and 1998b, p. 642) and Bricmont and Sokal (2001, pp. 45 and 250–253).

[6] For example, wandering around the streets of San Francisco, I soon learned that the street map I was carrying was almost useless. I would have much preferred a contour map showing altitudes.

[7] See e.g. Section 3.2 of my paper with Bricmont in this volume.

the words “or else as the closest approximation to it we have so far achieved”, which brings us closer to what realists actually think, provided that “it” is reinterpreted as meaning “the way the world is”.

One final conceptual point: In view of the central role played by the word “knowledge” in Barnes’ paper, it is curious that he nowhere clarifies what he means by this word. Is he using the word “knowledge” to mean “justified true belief” (as most philosophers do), or does he mean merely “any collectively accepted system of belief” (as in his previous writings)?[8] The distinction is crucial, and depending on it, many of Barnes’ assertions can be interpreted either as true but banal or as radical but false.[9]

Let me now turn to the other major theme of Professor Barnes’ paper, namely the example of the caloric theory of heat. Barnes claims that there does not exist in Nature

> anything remotely like caloric, anything of which accounts of caloric as a material substance might have been reckoned an ‘approximate description’, or ‘approximately true’

and he asserts that

> scarcely anyone is going to dispute that a strong realist account [of caloric theory] is inferior to a constructivist one.

Well, I am going to dispute it! I propose to show that the modest realism advocated by Bricmont and myself is not only compatible with the history of caloric theory but in fact answers questions that the constructivist account dodges.

Physicists nowadays no longer believe that heat is a substance (“caloric”), but we continue to teach Fourier’s theory of heat conduction. Why? Because it turns out

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[8] See e.g. Barnes and Bloor (1981, p. 22n).

[9] Please note that I am *not* insisting on using the word “knowledge” in any particular way; Barnes, like everyone else, is free to use any word as he pleases, *provided that he makes the intended meaning clear to his readers*. I object only to the logically invalid arguments that arise from inadvertent (or advertent) slippage between two distinct meanings of the same word. For example, Barnes’ co-author Bloor begins his opus *Knowledge and Social Imagery* by offering a radical redefinition of the word “knowledge”:

> Instead of defining it as true belief — or perhaps, justified true belief — knowledge for the sociologist is whatever people take to be knowledge. It consists of those beliefs which people confidently hold to and live by. ... Of course knowledge must be distinguished from mere belief. This can be done by reserving the word ‘knowledge’ for what is collectively endorsed, leaving the individual and idiosyncratic to count as mere belief. (Bloor 1991, p. 5; see also Barnes and Bloor 1981, p. 22n)

However, only nine pages after enunciating this non-standard definition of “knowledge”, Bloor reverts without comment to the standard definition of “knowledge”, which he contrasts with “error”: “[I]t would be wrong to assume that the natural working of our animal resources always produces knowledge. They produce a mixture of knowledge and error with equal naturalness ...” (Bloor 1991, p. 14).

that the equations of heat conduction are the same irrespective of whether heat is a substance or simply an aspect of molecular motion. Consequently, Fourier's theory of heat conduction is not just empirically adequate; it is also "approximately true" in the sense discussed by Bricmont and myself:

> not only may the older theory [in this case the caloric theory] be approximate rather than exact in a quantitative sense; it may also get the fundamental ontology all wrong. But this does not mean that its ontology is *simply wrong*; rather, it means that what appears in the older theory to be a fundamental entity [e.g. caloric] is, in reality, a non-fundamental entity derivable as a "coarse-grained" version of something deeper [i.e. molecular motion].

Moreover, using the deeper theory (i.e. the kinetic theory of heat) one can *deduce* the approximate validity of the older theory in specified situations (namely, where heat energy is not converted to or from mechanical or other forms of energy).

The caloric theory also made *surprising* predictions that were experimentally confirmed: for example, the Laplace–Poisson relation between the speed of sound in a gas and the ratio $\gamma = C_p/C_v$ of its specific heats.[10] We can now see, using our deeper theory, that this prediction succeeded not by mere coincidence, but because its derivation really did capture some aspects of reality — that is, because the theory was "approximately true" in the sense just explained. In this respect, caloric theory is quite unlike some other now-rejected theories, such as Ptolemaic astronomy, which (as discussed in some detail in the paper by Bricmont and myself) is fundamentally wrong and was empirically successful only in *unsurprising* ways that do not give evidence for its approximate truth.

A constructivist (or instrumentalist) account of science, by contrast, is incapable of explaining *why* the caloric theory made successful surprising predictions; it simply accepts this as a brute fact, and goes on to say that "the theory was accepted and used by engineers and technologists, so that in France it informed practices within a remarkably effective economic and military apparatus." But the French military would not have used the caloric theory unless it *worked*, i.e. was empirically adequate at least for a specified range of purposes; and this empirical adequacy requires *explanation*. Now, a theory's approximate truth is not the only possible explanation of its empirical adequacy — the empirical success of Ptolemaic astronomy, for instance, requires a different explanation[11] — but *some* explanation is clearly needed. Constructivists and instrumentalists simply dodge this question.

Finally, let me mention — though this is logically unnecessary for the defense of our philosophical position — that leading scientists of the early nineteenth century appear to have taken a cautious view toward the reality of "caloric" that accords almost perfectly with the modest realism advocated by Bricmont and myself. Historian of science Stephen Brush, in his voluminous work *The Kind of Motion We Call Heat*, says:

[10]See Psillos (1999, pp. 119–121) for a brief account.

[11]For such an explanation, see e.g. Section 2.3 of my paper with Bricmont in this volume.

> In reading the literature of the early 19th century, I have been impressed by the great caution and open-mindedness with which many scientists presented their views on heat, particularly some of the writers who are usually labeled as supporters of the caloric theory. It was very common to say that most of the phenomena can be explained equally well by considering heat as a substance or as a quality (or "mode of motion"); even if the former view was to be adopted for the sake of convenience, it was not to be regarded as firmly established beyond any doubt ...[12]

Philosopher Stathis Psillos has also presented a detailed analysis of the history of caloric theory, in which he focuses explicitly on the implications for realist philosophy of science.[13] Psillos correctly notes the importance of *localizing* relations of evidential support, i.e. of determining "which parts of a theory are supported by the evidence at hand, or at any rate, which parts are better supported than others"[14], and he analyzes in detail the ways in which empirical evidence can support theoretical claims strongly, weakly, or not at all. He concludes that

> scientific realists need not accept a theory in its entirety. Instead, realism requires and suggests a *differentiated attitude to*, and *differentiated degrees of belief in*, the several constituents of a successful and mature scientific theory. The degree of belief one has in a theory is, in general, a function of the extent of its support by the available evidence. Since different parts of a theory can be supported to different degrees, realists should place their bets on the truth of a theory accordingly.[15]

Barnes notes Psillos' work in passing, and criticizes it as an attempt

> to rationalise a pre-existing entrenched commitment to strong realism in the face of certain kinds of difficulty. Indeed, it comes close to offering the faithful a means of justifying practically any theory at all in realist terms.

But Barnes gives no details at all to support this criticism; and the counterexample of Ptolemaic astronomy shows that a realist need not, and indeed does not, label every empirically successful theory as "approximately true". We realists simply feel obliged to give *some* plausible explanation of a theory's empirical success — an obligation that Barnes seems not to share.

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[12] Brush (1976, p. 312, note 2).

[13] Psillos (1999, Chapter 6).

[14] Psillos (1999, p. 125).

[15] Psillos (1999, pp. 126–127), emphasis in the original.

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