

Science Made Up: Constructivist Sociology of Scientific Knowledge.

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❖❖ It must be acknowledged, that rational assent may be founded upon proofs, that reach not to rigid demonstrations, it being sufficient that they are strong enough to deserve a wise man's acquiescence in them.

—Robert Boyle

The idea of the social construction of knowledge belongs to a tradition in sociology that includes such seminal figures as Marx, Mannheim, and Durkheim, as well as George Herbert Mead, whose social constructionism also had a very broad range.<sup>1</sup> Until recently social-constructivist ideas had not been central to investigations in the sociology of science, which was dominated instead by the institutional approach of the Merton school.<sup>2</sup> This situation has changed over the past decade or so with the rise of a movement, referred to by its practitioners as the "sociology of scientific knowledge,"<sup>3</sup> that incorporates the idea of social construction in an especially striking way. Witness such titles as *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, *Laboratory Life: The Social Construction of Scientific Facts*, and *Constructing Quarks: A Sociological History of Particle Physics*.<sup>4</sup> For brevity, I will refer to this movement simply as constructivism.

Turned now to scientific knowledge, constructivism has begun to capture the attention—although not always the admiration—of historians, philosophers, legal scholars, literary theorists, and "postmodern" culture critics.<sup>5</sup> Major university presses (e.g., Chicago, Harvard, Princeton) have begun to publish work in this genre. In philosophy proper, its impact so far has been largely among those versed in the Continental literature, where it seems

especially intriguing to sympathetic readers of Foucault, and to some others who, in Steven Shapin's exasperated words, "wish to ape the modern French manner of academic posing."<sup>6</sup> In the philosophy of science the scope of the constructivist program fits in with the "big methodologists," like Popper and Lakatos and Kuhn, each of whom, not surprisingly, rejects it—but perhaps rather too strenuously. Larry Laudan's exchange with David Bloor is typical of the generally unsympathetic reaction of analytic philosophers to constructivist ideas,<sup>7</sup> where Laudan castigates the "strong" version of the program as the "pseudo-science of science."

Two doctrines conspicuously embraced by constructivism seem to generate much of the philosophical interest and reaction: its antirealism and its relativism. Although these doctrines are certainly much discussed in the constructivist case studies and methodological writings, their treatment there is philosophically careless and naive. To put it bluntly, constructivists write a great deal of nonsense on these topics. This expository sin is compounded by a dialectical one, for when it comes to defending their doctrines, constructivists tend to rely more on polemics than on careful argument. Their rhetorical style, moreover, is at once romantic and apocalyptic. They portray themselves as in the vanguard of a new dawn in understanding science, a profound awakening that sweeps away oppressive philosophical categories—truth, reality, rationality, universality. But although broad in understanding, their party is also depicted as small in numbers, an embattled minority pitted against the huge Scientific Establishment and their academic apologists (philosophers, historians, other sociologists, and so on). Of course this antiestablishment pose is increasingly exposed by growing support for constructivism from the academy, in terms of positions, publications, and grants—not to mention admiring followers. So, increasingly, their polemics seem disingenuous. For philosophers, moreover, with their special regard for clarity of exposition and force of argument, the expository and dialectical sins of constructivism pose special obstacles to treating its doctrines with much sympathy. Still, that is what I should like to attempt here with regard to the antirealism issue, since I believe that one can use constructivist ideas to sketch an interesting and defensible program that presents a serious challenge to realism.<sup>8</sup>

### A Constructivist Platform

I should like to begin with a description of the constructivist ideology and its background. However, despite its short tenure there are already several different constructivist schools, each with its more and less radical wings. It is, therefore, no easier to characterize constructivism than it is to characterize realism or antirealism. Still, as in these other -isms, I think there is a cluster of important doctrines that distinguish the constructivist party. Because their own rhetoric tends to the political, and they are clearly conducting a campaign for party members, I will refer to the cluster as a constructivist platform and to the doctrines that fall under it as the planks.<sup>9</sup>

*Plank 1.* Beliefs on a topic can (and do) vary. Prevailing beliefs are relative to particular prevailing social circumstances.

*Plank 2.* For any belief (whether true or false, rational or irrational) the question of why it is held (or not) is appropriate, and the answer is to be an explanation framed in terms of locally operating causes, and not in terms of the character of the belief (e.g., whether true or false) or in terms of rationality conditions (e.g., "It would be irrational not to hold").

*Plank 3.* Contingent sociological factors are (must be) relevant to explaining beliefs and judgments. In particular, beliefs are produced and judged to further local, collectively sustained goals and interests. The scientists' role in belief formation is active. They are agents doing things: making choices, forming alliances, pursuing local goals, advancing interests, and so on. All these are done in a rich field of social, cultural, institutional, and political forces; that is, they are all done together with other agents behaving similarly.

I have framed these planks in the language of "beliefs" and "judgments." Typically, however, constructivists do not distinguish between belief formation and the "making" ("construction," "manufacture," "production") of facts, using one idiom more or less interchangeably with the other, even when the result is literal nonsense. If we were to follow their lead in formulating the platform, and speak of facts rather than beliefs, then the first plank would issue in a rather striking relativism, and the third plank would amount to a strong sort of constructivism. Surely the con-

flation of the languages of beliefs and facts by the constructivists is not just ignorant usage, but a particularly forceful (and, to some, annoying) way of expressing a central doctrine—namely, that what makes a belief true (if I may use oldspeak) is not its “correspondence with an element of reality” (i.e., a “fact,” realist-style) but its adoption and authentication by the relevant community of inquirers. This amounts to a loose consensus theory of truth and constitutes a special sort of semantic antirealism. If we adopt the customary semantic convention according to which facts are identified with what makes beliefs true, and if we also subscribe to such a consensus theory of truth, then facts turn out (literally) to be constituted by processes of belief formation. Moreover, whatever drives these processes then (literally) makes the facts. Of course, according to Plank 3, what drives belief formation is the activity of the scientist-actors. Hence, facts are made by scientists (literally!). Thus an antirealist, consensus theory of truth binds the three planks together to form a specifically constructivist platform (and one that is also relativist). Although the details of the consensus theory are not too important for constructivism, it is necessary for them that beliefs do get fixed, so that facts do get made. Hence the consensus is not, as with Peirce, in the sweet by-and-by. Constructivists require truth to be made by actual consensus, and not by some long-run idealization.<sup>10</sup>

The sociological turn, the emphasis on the social determinants of belief, is evident in these planks and in the underlying consensus theory of truth as well. In both instances social behavior is afforded not just a dominant but actually an exclusive role. Between the second and third planks, anything other than social behavior is actually excluded from playing an explanatory role in the fixation of belief. In a consensus theory, the social fact of the fixation of belief is promoted from being one of the marks of truth to being the whole of it. This is behaviorism with a vengeance, and, as we shall see, it is one of the places where constructivism comes undone. But apart from behaviorism, which is not in philosophical fashion, there are other, more fashionable doctrines to which the platform also owes some debt.

Central ideas in postwar philosophy of science, in particular the doctrines of the theory-ladenness of observation and of the underdetermination of theory by data, lend support to the planks. The

former suggests that the observational data of science depend on the theories in the field. This makes for the ever-present possibility of variation emphasized in the first plank, and, when combined with underdetermination, it opens the door to the move to sociological explanation of both theory and data made in the third plank. A similar role is played by the Poincaré-Duhem thesis, according to which falsification in science is necessarily inconclusive. For this raises the question of how, then, is the target and conclusion of a falsifying experiment actually fixed? Again sociological explanation, as in the third plank, presents itself to fill the gap. In addition to these recent fashions in the philosophy of science, ones whose cogency not everyone acknowledges (and among whose dangers some might well include support of irrationalist platforms like constructivism!), there are also certain central disciplinary programs whose shortcomings make additional room for the constructivist moves.

I have in mind, in the first instance, various aspects of the confirmation industry. To begin with, the well-known paradoxes of confirmation, along with the “grue” problem, certainly warrant some skepticism about the viability of any general notion of “evidential support.” That skepticism is reinforced by the wide range of inadequacies of all general programs in confirmation theory: hypothetico-deductive, Bayesian, and others.<sup>11</sup> Indeed it seems time to acknowledge that the idea of a general, explanatory theory of confirmation has turned out to be a philosophical dead end. The variety of evidential practices seems to have a “situatedness” that the philosophical search for a general theory has obscured. Therefore it is certainly not far-fetched to look at how “evidence” works *in situ*, in an open-minded way, without demanding that its operation in any one place must have a set of explanatorily relevant features in common with its operation absolutely everywhere else. It is also not so far-fetched to think that local social groupings and social factors may indeed determine in context how evidence does work, and how it “compels.” Thus the failure of philosophical theories in this area brings us round to the constructivist platform. Of course, not inevitably: that is, it does not follow from the fact that “confirmation” fails of a general analysis and theory, nor even from the fact that the practices are context-bound and not generalizable (if that is a fact), that the contextual features that make for

confirmation are exclusively (or even primarily) social. That latter is a guess, a hunch, a programmatic suggestion—but not an irrational one, given the presently moribund circumstances of the confirmation game, nor one especially far-fetched or implausible, either. It is, in fact, a rather interesting and intriguing suggestion for research. I believe that the constructivist platform as a whole can be looked at similarly.

What constructivism needs to take from philosophy of science are not specific, established doctrines. It needs only to take the programs of investigation that sought after general accounts of the structure and dynamics of theories, of observation, of confirmation, of hypothesis testing, of explanation, of discovery, and so on, and to note their widely acknowledged inadequacy.<sup>12</sup> The failure in every case is of the general theory to give an illuminating account of the known range of scientific practices. All the posited structures, rules, and maxims still leave unanswered why particular things occurred in particular contexts. Constructivism then adopts a sort of principle of sufficient reason (in the second plank) according to which there are indeed causes for what happens. Finally, it turns to the social to suggest that the causes are to be found in social interactions. I emphasize that this is not an argument for the constructivist platform but only an attempt to set it intelligibly in relation to philosophical practice, and thereby I hope to make it at least interesting to consider.

So set, constructivism shows up as social particularism. It does not see the operation of general algorithms in science, but only particular sets of local practices. These are associations of people, doing a large variety of different things no particular one of which, at any time, is forced upon them. Thus whether one is gathering data, deciding when to end an experiment (or how to interpret it), judging the relevance of a new item of information, revising a theoretical model, setting up standards, plotting a curve, responding to or making a criticism, reading a meter, and on and on without end, options are available and choices are made. In this sense, at every level and in every kind of endeavor (observational, experimental, classificatory, calculational, theoretical, methodological, and normative) science is open, and its judgments unforced. To do science is to make judgments and decisions that always outstrip any set rules of the game. The constructivist pro-

gram, most simply put, is to try to explain what goes on in this open arena by reference to social factors: networks, interests, or whatever. So understood, constructivism offers an interesting contrast to other programs for treating science, in particular to realism and instrumentalism.

### Realism, Instrumentalism, and Constructivism

To compare these programs it is useful to adapt a scheme that I have used elsewhere to contrast realism with instrumentalism.<sup>13</sup> The adaptation here involves comparison along five dimensions: (1) general valence with respect to science (pro or con), (2) reductionist attitude (or not) to scientific concepts, (3) treatment of truth in science, (4) hermeneutical orientation (or lack thereof), and (5) teleological stance with respect to science.

*General valence.* Realism is pro-science, advertising itself as progressive in this regard, by contrast with antirealist programs, which it labels as anti-science. But despite the realist polemic, instrumentalism (certainly in this century after being so baptized by John Dewey) is also pro-science, and it counts itself as no less progressive than realism.<sup>14</sup> As for constructivism, I'd better let a constructivist speak for himself: "There is no obligation upon anyone framing a view of the world to take account of what twentieth-century science has to say. . . . World views are cultural products; there is no need to be intimidated by them."<sup>15</sup> This reference to "intimidation," and the general debunking attitude expressed by Pickering, is typical of the romantic antiestablishment rhetoric of constructivist texts. Despite occasional disclaimers, the tenor of their preaching is against science.

*Reductionism.* Realism has no special interest in reductionist programs. Unlike some of its antirealist companions (e.g., idealism, phenomenalism, empiricism), neither does instrumentalism. They are both inclined to take scientific concepts as they come. In part the particularism of the constructivists supports the same inclination. In their relativist and ethnomethodological moods they are inclined to accept the conceptual framework of the natives at face value. Their social behaviorism, however, provokes different moods. "For the argument is not just that social networks mediate between the object and observational work done by participants.

Rather the social network constitutes the object (or lack of it). . . . There is no object beyond discourse, . . . the organization of discourse is the object. Facts and objects in the world are inescapably textual constructions."<sup>16</sup> By an "object" here they mean both concrete particulars, like individual hormones (or proteins), as well as general things, like mental illness. Thus not only is behaviorism allowed to drive the consensus theory of truth, with its implications for facticity; it also drives an ontological reductionism as sweeping in scope as old-fashioned idealism ever was. That scope includes science itself. "It is not that science has its 'social aspects,' thus implying that a residual (hard core) kernel of science proceeds untainted by extraneous non-scientific (i.e., 'social') factors, but that science is itself constitutively social."<sup>17</sup>

*Truth.* Realism is associated with a correspondence theory of truth, where descriptive terms in the scientific vocabulary are supposed to correspond to mind-independent objects in the world (at least for the nonhuman sciences). Instrumentalism is often represented as withholding predication of truth from the theoretical (= nonobservational) components of science. This is a poor way of expressing instrumentalism, however, since it makes instrumentalism vulnerable to questioning the divide between the theoretical and the observational, an issue over which it need not take a stand. It also makes instrumentalism seem arbitrary in focusing on just this divide. Better to think of instrumentalism as Dewey did, which is to subscribe it to the pragmatic account of truth as general reliability (or utility), right across the board. This amounts to taking "'P' is true" and "'P' is generally reliable" as synonymous, or near enough. Thus high-level laws in physics, for example those involving commitment to quarks, could be counted as true both by realists and by instrumentalists, although they would each understand something different thereby. Constructivists follow an earlier pragmatic route, which is to identify truth with fixed belief. In this enterprise they take William James's road, rather than that of C. S. Peirce, insofar as they opt for actual community acceptance rather than the acceptance of ideal agents (or acceptance at the end of inquiry).<sup>18</sup> Consistently with the general behaviorist reductionism discussed in the preceding paragraph, they take community acceptance not merely as a criterion of truth but as constitutive of it.

*Hermeneutics.* Realism and instrumentalism share a common attitude toward science. They see it as an enterprise in need of

further understanding and interpretation. Not only do they look at specific forms of scientific practice—say, inference to the best explanation or the use of correlational data to support causal models—and seek a good way to interpret, to explain, and to assess the validity of what is going on. They also shape that good way to accord with, perhaps even lend support to, their (realist or instrumentalist) programs. For those programs themselves embody a general interpretation of scientific discourse and practice, the general interpretation that accords with the semantics embedded in their different conceptions of truth.<sup>19</sup> Constructivism joins realism and instrumentalism in treating science as fair hermeneutical game. This is very striking in the investigation of Latour and Woolgar at the Salk Institute, and their adoption of the anthropologist's pose of "strangeness" in order to make room for the interpretations they propose of laboratory practice. More fundamentally, however, it is built into the sociological part of constructivism, which seeks to expose the social character of what is ordinarily taken for granted in scientific activity. In this regard constructivism is closest to realism. They have in common an unmasking impulse, a basic inclination to peel away the conventional surface in order to see what is "really" going on. Instrumentalism shares with them exactly the opposite impulse: what you get is what you see. These impulses are the two sides of a common hermeneutic orientation, one that sets a perfectly general interpretive agenda for which all instances of practice are candidates (recall that the social explanations of Plank 3 apply to any belief) and into which they must all be made to fit. I would just note that this hermeneuticism is at cross-purposes with particularism, which ought to leave open whether episodes are candidates for interpretation at all, and if so what kind of understanding applies. Thus although hermeneuticism is built into constructivism, it is not a comfortable fit.

*Teleology.* Realism and instrumentalism each propose goals for the scientific enterprise as a whole. For realism, the fundamental imperative for science is to seek the truth: that is, the realist, correspondence-to-the-external-world type of truth. Instrumentalism is said to go for less: in particular, to be content with positing utility (read "empirical adequacy") as the aim of science. But this utility is nothing other than the general reliability that occurs in the instrumentalist's pragmatic account of truth. (Recall *Truth*, above.) Hence, understood in its own terms, instrumentalism too sets up

truth as the goal of the scientific enterprise. By contrast, for constructivism, truth is just fixed belief, which is not a terrific goal for me as an interested scientist unless it is my belief that gets fixed. So constructivism parts with realism and instrumentalism, demoting truth as of secondary importance. Indeed the particularist strand in constructivism recognizes the variety of goals and aims of the many different scientific activities and groupings, and how they change over time, and so resists the impulse to collect them all up in one global telos. But the social-interest strand, nevertheless, does require one overriding aim. Like all social institutions, according to the constructivists, science seeks to perpetuate itself. When all is said and done, that is the name of the game. For constructivism that goal functions as a framework into which are set the more specific analyses of interests, influences, reward structure, expectations, training protocols, and so on, that make up the social net. Despite the animadversions about scientific rationality, for constructivism, no less than for realism or instrumentalism, positing an overriding goal for science provides a vehicle for seeing the functional rationality of scientific practice: that is, for seeing that practice as an appropriate means for achieving its end.

These comparisons of constructivism with realism and instrumentalism should help identify areas where constructivism, like these other programs, is vulnerable to criticism—a task to which I now turn.

### Deconstructing Constructivism

Elsewhere in criticizing realism and instrumentalism I have focused on their treatment of truth, and on their hermeneutic and teleological stands.<sup>20</sup> These are the areas where I see problems for constructivism as well. To bring out the problem with truth let me attend to the central thesis in the second, explanatory plank of constructivism and contrast two versions of constructivism, respectively, methodological and metaphysical, in terms of their different rationales for the thesis.

**THESIS (PLANK 2):** *In explaining the fixation of belief, one should not bring in the truth (or falsity) of the belief itself.*

*Methodological rationale.* The scientist, considered as an agent in the process of investigation, has no access to the truth of the posits

he is investigating independently of the process itself that fixes belief. So, given that the belief does get fixed, there is some way that it happens. The thesis of Plank 2 can be thought of as a reasonable methodological rule that directs us to search for that way, which we presume will involve a causal story, but not the inaccessible-at-the-time truth value.

*Metaphysical rationale.* There is no truth or falsity of the matter until beliefs about it get fixed. Hence (logically) one cannot bring truth values into the explanation of belief.

The methodological rationale involves a certain ambiguity, for one might agree that the truth value of a belief is not known until it is known, without agreeing that the truth (let us say) of the matter does not influence what comes to be believed. I might, after all, not know what is in a package until I unwrap it. But the contents do, nevertheless, influence what I come to believe as I proceed with the unwrapping. We have to be a little careful, however, because the contents are not truth values. And although the contents may affect what I come to believe, the truth of the proposition that the package has such-and-so contents is (indeed) not accessible to me along the way, and so (indeed) it does not influence the final belief. That is an elementary point about truth values. But still, one might object, it is because there was (let us say) an apple in the box that I came to believe that there was an apple in the box. If we follow the methodological prescription and bracket the truth of that, then how am I expected to account for the belief?

It is important to take up this challenge because the point at issue, although distinct from underdetermination and theory-ladenness, is liable to be confused with them. Those doctrines would suggest (underdetermination) that the fact of the matter does not determine our beliefs (or “theories” about it), and (theory-ladenness) that the particular representation of the situation (e.g., as an apple that is in the package) depends on a learned, prior set of possible representations, ones that might have been other than they are. To be sure, accepting these suggestions would open the way to supplementing the fact of the matter (i.e., that there is an apple in the package) by a sociological account to explain why this fact contributed to the particularly represented belief that it did. But the methodological rationale above is more radical than this. That line asks us to bracket the fact of the matter entirely, not merely to supplement it with some sociology.

To see whether this radical proposal is feasible, suppose we try the opposite. How might we bring the fact of the matter into an explanation of belief? Presumably the way would be something like this. We explain our belief that the package contains an apple by saying that at the end of the unwrapping activity (with all the socialization that certainly involves) I saw an apple. That is why I came to believe there was an apple in the package. This explanation sketch relies on a basic (perhaps even primitive) relation between seeing (in context) and believing (in that context). The issue raised by methodological constructivism is whether that relation is, so to speak, apple-specific. After all, we know that we might have formed the same belief about the apple without its actually being an apple that we saw. So even though in the case at hand we agree that there was an apple in the package, the question is whether in this very case it is the apple (*qua* apple) that makes for the connection between seeing and believing. Perhaps we do not need to refer to the apple specifically in order to explain the belief? Indeed we do not, for we can simply say this: it is because of what I saw when I unwrapped the package that I came to believe that there was an apple in the package. The phrase "what I saw" is noncommittal as to the character and qualities of its referent. This formulation, nevertheless, maintains the basic relation between seeing and believing (in context) that explains the formation of belief. I think this minimal account is what the methodological constructivist is after. He need not be committed to more—that is, to responding to the further question as to what "really" is the referent. (After all, his project is to explain how we answer this question.) In particular he need not (and in my judgment should not) give the phenomenalist answer, that it is a sense datum, or the like. The minimal formulation is sufficient. It succeeds in bracketing the truth of the belief, and thereby it opens the way to showing (provided it can be shown) "why particular accounts were produced and why particular evaluations were rendered . . . by displaying the historically contingent connections between knowledge and the concerns of various social groups in their intellectual and social setting."<sup>21</sup>

I have taken up the methodological rationale in the case of individually held perceptual beliefs, which is, I should think, the hardest case. For theory-and-evidence-based beliefs, especially those of a community, there would seem to be even more slack between the

truth of the matter and acceptance of beliefs about it. Hence for more theoretical beliefs the methodological rationale for bracketing seems quite plausible, and sensible. The same cannot be said for the metaphysical rationale, whatever the character of the belief.

In maintaining that there is no truth of the matter until the belief gets fixed, the metaphysical rationale simply applies the consensus theory of truth. As Woolgar puts it, "Truth or falsity is perceived (and achieved) rather than inherent."<sup>22</sup> This account of truth, with its implicit appeal to specific communities of inquirers and contexts of inquiry, is relativist, and subject to the familiar array of philosophical objections to relativism. Similar objections apply to the theory of truth itself. Among these are the peculiarities forced on the grammar of "truth," if we adopt a consensus theory. For community opinion shifts, and, as the constructivists are fond of pointing out, this occurs even in the hardest of sciences and over the most central principles. Are we then to say that such principles were once neither true nor false, then became true, then false, and may become true again? The language of opinion, or shared belief, is structurally different from the language of truth. Most of us take this as a sign that there are jobs to do in tracking consensus and dissensus different from the jobs to do in tracking truth and falsity. Most of us, that is, understand that truth is different from consensus. This difference shows up strikingly if we consider the redundancy property of truth: that such-and-so is true holds in the case and only in the case that such-and-so (at least for nice such-and-sos). But the language of opinion is not redundant; it is neither necessary nor sufficient for a community holding the opinion that such-and-so for it to be the case that such-and-so. Precisely the point to having a language of opinions, it would seem, is to be able to deal with the acceptance of beliefs when the beliefs are not true. The failure of redundancy distorts that function, and has even worse consequences.

If the consensus theory has any virtue at all, one would think, it is at least definite about what is or is not true, even if the truth does vary with opinion. For when the community has settled on a view (perhaps firmly enough to last for a while) then it is true (according to the theory), and that is definite. But is it? That is, is it definite for us? For us to be able to judge that something is true is for us to be able to judge that the relevant community has settled on that



opinion. But how do we judge the truth (or falsity) of whether the relevant community has settled on an opinion, on the consensus theory? Well, we have to judge whether the relevant community has settled on the opinion that they have settled on an opinion about the original item.<sup>23</sup> But to be able to judge whether that is true, given the consensus theory, we have to add yet another level of community judgment, and to be able to judge that. There is no end to this process, which is to say that there is no way to collapse the infinite tower of judgments required by the consensus theory so as to enable us to make any definite determination of truth in any given case whatsoever. Appearances to the contrary, as the unmasking constructivist might say, on the consensus theory, truth turns out to be just as transcendent and just as inaccessible as on the realist correspondence theory, or if picked out by Peirce's retrospective judgments at the end of inquiry. Moreover, on standards of intelligibility that the constructivists insist on elsewhere—that is, according to the idea that what makes sense has to make sense for us as social creatures—the failure of the consensus theory with regard to the determinability of truth judgments for us shows that this attempt to frame an intelligible notion of truth fails. The attempt is idle; it frames no notion at all.

The preceding considerations are arguments against the consensus theory of truth that underwrites the metaphysical constructivist project, arguments built on principles many of which the constructivist accepts. But the constructivist is a virtuoso at the Poincaré-Duhem defense: expert, that is, at dodging refuting arguments. So I expect that he could wriggle around these too, even if that means renouncing some of his other principles. Where that price is too high, he can always opt for accepting the counterintuitive consequences and announcing those as part of the new dawn in understanding science. "We used to think that truth was stable, and not fickle. We were wrong." "From Aristotle to Tarski we were persuaded that truth had the redundancy feature. No doubt that suited interests then, but it does not suit ours now. We were wrong." "As for accessibility, why trust these regress arguments? They make one dizzy. If on first glance it seems accessible, that is good enough for us." No doubt I exaggerate. The point is correct, however. Not even the best-looking arguments need persuade the committed believer. What is required, as Duhem understood, is

good judgment (*bon sens*), and at best that can only be shaped by argument but not compelled. This is a feature of the openness of scientific discourse that constructivism itself (correctly) highlights.

That very openness, however, ought to make one sensitive to a different sort of consideration. The consensus theory of truth, like other attempts to frame a substantive account, hopes to find an informative formula to fill out the right-hand side of

'P' is true if and only if——.

The difficulty with all such efforts is this: if they were successful, they would yield a finished notion of truth; truths would be those things, and only those things, that satisfied the slogan on the right. The supposition behind such attempts, therefore, is that the concept of truth is closed: that is, that truth is a concept with determinate boundaries, and hence one amenable to such necessary and sufficient conditions. In particular the theory favored by metaphysical constructivism holds that all truths, in all historical eras, in all cultures, in all contexts, in all sciences, in all communities, with regard to every subject whatsoever, always in the past and always in the future, always have had, do now, and always will have something in common with each and every other truth, namely, consent by the relevant community. Never will anything be true that does not have the brand of consent. In the move to the consensus theory we must ask: What happened to particularism, and to the openness of science? Those key ideas of constructivism depend on the perception that science is a social activity, and so every scientific endeavor is as malleable and subject to change and revision as it is possible for human affairs to be. To protect that perception it is necessary that constructivism not close off the central concepts that underwrite scientific life, not by means of necessary and sufficient conditions, or anything else (since it is not logical gimmicks that are at issue here). Those central concepts certainly include the concept of truth, which is basic to the textual and representational aspects of science with which constructivism has been especially concerned. To say that the practices of truth-saying and truth-judging are open, however, is to say that one cannot project future practice from past practice. That means one cannot hope to fill out the right-hand side of the truth schema above with the description of some set of practices, for the "set" is not a well-

defined entity. Thus the consensus theory of truth espoused by the metaphysical constructivists is at cross-purposes with the entire ethos of constructivism. The consensus theory is not just logically flawed and otiose; it runs counter to the insights over the openness of science that motivate the constructivist program. To be true to those insights constructivism must let truth be whatever it is and will be, and not consensus (or anything else) necessarily.

These considerations are perfectly general. That is, they apply not only to the constructivist attempt to fasten onto a reductive theory of truth; they apply as well to all the areas where constructivism is reductionist, closing things off rather than leaving them open to growth and change (see the preceding section, especially the paragraph *Reductionism*). Behind this reductionism is a leaning to behaviorism that manifests itself in the tendency to take the social in science as absolutely all of science. There is a sense in which this may well be true, just as there is a sense in which it is true that people are nothing but material objects. That sense might tempt one to think that the mental and the social and the political, to mention just a few handy realms, are reducible to the motions and interactions of material objects. That would be a mistake. It is no less a mistake to move from the sense in which science is nothing but the activities of human beings to think that all the realms of science can be reduced to the social. The mistake is to move from the fact that scientific concepts and activities are embodied in human practices, to conclude that this constitutes their essence and exhausts their content. That conclusion depends on (among other things) the assumptions that there is an essence (i.e., something like defining conditions, as in the consensus theory of truth), and that there is some definite thing that counts as content (that could be exhausted). Science is not like that, however, as constructivists well know. Scientific concepts and activities have no essence guiding their development (like Adam Smith's hidden hand). That is a central constructivist point. Because scientific activity is social activity, its practice is always liable to change. Past activity does not determine the shape of future development. Science is open. The constructivists see very well that for this reason the demarcation games and the old projects for methodology in philosophy of science are defunct. What they ought to see equally well is the futility of all reductionist games, including their own.

The social character of science makes it open. The openness blocks any "deep" characterizations of the constitution of scientific concepts, activities, and products. That includes realist characterizations as well as the characterization of them as essentially constituted by the social. So, ironically, the behaviorism to which constructivism is inclined is actually incompatible with the social turn that it has taken.

So far we have seen that metaphysical constructivism, with its consensus theory of truth, is an idle doctrine. The metaphysical part is at cross-purposes with the openness of the social-constructivist part. More fundamentally, however, that openness constitutes a wedge separating the social from the constructivist—that is, keeping the social from degenerating into a behaviorist reductionism. The hermeneutic and teleological dimensions of constructivism show a similar tendency for constructivism to be divided against itself. For if scientific activity is open, with no one's hand forced at any point, then how does it happen that one and only one mode of explanation and interpretation applies uniformly, right across the board? Since scientific activity is not generated by the uniform application of a general algorithm, why must all its varied activities be understood and explicable in terms of any single theoretical scheme, like a causal, interest-dominated model of social practice? As with any such scheme we can, of course, ask whether it is adequate, and, in this case, we can reasonably suspect that it will not turn out to be so. To go to basics, however, we can ask more fundamentally why the constructivists feel the need to approach science with any global, ready-made framework at all. That idea is central to the hypothetico-deductive model of theory testing that constructivism rejects. It is no part of the sociological tradition of field studies from which the constructivist program is drawn. To the contrary, the methodology of field studies resists the conception that what they are about is testing preset hypotheses in the field. That tradition is after a different kind of understanding, the kind that comes from subject-specific ideas arising in the course of close association, and examined in context to see whether they stand the test of time. The knowledge so acquired is framed in terms of concepts shaped piecemeal to suit the particulars of local circumstances, and not (or, at any rate, not necessarily) involving universal social mechanisms. The way in which the sociology of

scientific knowledge breaks faith with its sociological roots suggests that constructivism has special concerns that call for a more universalistic approach. Those concerns, I believe, show up in the romanticism of constructivist rhetoric, and in its debunking attitude toward science. (Recall the reference to myth and intimidation in the quote from Pickering in the preceding section, and catch the sneer in Woolgar's *Science: The Very Idea!*) That has the stamp of antiestablishment psychology, suspicious of authority and needing to cut culture heroes down to size. So if one can see SCIENCE, overall, as just another bunch of people doing their own things (especially, pursuing their own interests, ultimately to insure the perpetuation of their kind), then we pull the sting of science's authority. The central metaphors of "making" and "producing" lead the same way. They suggest the image of scientists as laborers: no-hat construction workers, or laboratory-coated "proles" on the science-factory line.<sup>24</sup>

If this diagnosis is correct, then imposing a universal scheme for the interpretation and explanation of scientific activity serves an ideological function. Constructivists need it to cut the establishment down to size. Addressing that need, however, creates a rift in their program. It pulls them away from the piecemeal understanding characteristic of their own sociological tradition, and likewise, since no universal scenario can be preset for a truly open activity, it prevents them from conceiving of the scientific enterprise as truly open. Thus the unmasking hermeneutics that, as we have seen, characterizes both realism and constructivism derives from a related source. Just as the constructivists show realism as needed to foster regard for the scientific enterprise, so, similarly, the hermeneutics of constructivism is needed to tear science down. The same is true for teleology. The incongruity of inventing a single goal for an enterprise conceived of as emphatically multiform, plastic, and variable is striking. But that makes perfect sense if the purpose is to reduce our estimation of the institution of science by having us see it, like a political institution, as merely engaged in an effort to aggrandize and perpetuate itself. This is the constructivist downside of the realists' uplifting slogan about seeking the truth. It is the other side of the coin that connects realism with constructivism as opposing global programs. That currency is badly in need of deflation.

### Methodological Constructivism and NOA

Realist and positivist presentations of science project an objectification that is false to scientific practice. That picture colors the public perception of science and makes it difficult for public policy to develop in an informed way. That false picture is also the image frequently picked up and elaborated by philosophers. (Not to embarrass my friends, let me just point to Husserl and the objectifying attitude that he imputes to Galileo and his more modern successors.)<sup>25</sup> It is the official line of many scientists, even if privately they know and speak better. Emphasizing the humanness of science is surely a needed and important antidote to these realist and positivist distortions. Thus it seems to me we can be sympathetic to the fact that the ideological and metaphysical needs of constructivism promote an important corrective to the standard objectification of science. We should recognize, however, that this corrective function is only contingently tied to those needs. Moreover, the needs themselves are subject to change and variation, and under competing pressures they can be even separated from the programs as such. This is especially true for constructivism, because those needs lead to the reductive, hermeneutic, and teleological excesses demonstrated above. These excesses fracture the program. I believe that if we hang on to the piece-by-piece approach to science as an open, social activity we can counter the misleading philosophical images of science, and salvage the best parts of constructivism as well. The result is a program for methodological constructivism that runs as follows:

1. Bracket truth as an explanatory concept.
2. Recognize the openness of science at every level, especially the pervasive activities of choice and judgment.
3. Concentrate on local practices without any presupposition as to how they fit together globally, or even as to whether they do fit together.
4. Remember that science is a human activity, so that its understanding involves frameworks and modalities for social action.
5. Finally, on the basis of all the above, try to understand the phenomena of opinion formation and dissolution in science in all its particularity.

This methodological program retains most of the original con-

constructivist platform. Its most controversial feature is probably the bracketing of truth in (1), for which I would offer the methodological rationale already much discussed in the preceding section. The program dispenses with any theory of truth, and so, unless we tack one on, it will not lend itself to the reductionism of metaphysical constructivism (nor to its relativism, either). Similarly there is no global hermeneutic orientation to this program, although in (4) it places a clear emphasis on local, social factors. It does not enter into the game of teleology, inventing overriding goals or ends for science as a whole, either, a game antithetical to the openness theme of (2) and the particularism of (3). By stripping constructivism of its metaphysical attachments, this program does not prejudge the constitution of the scientific world—that is, whether the scientific facts and objects are essentially social, or essentially mind-independent, or whatever. Its attitude is to let the chips fall where they may. Thus blanket social constructivism, which derives from the metaphysics, is let go in order to retain the openness, particularity, and social orientation of the program consistently. I believe that a great deal of existing constructivist work, especially the detailed case studies, can easily be stripped of constructivist metaphysics (and rhetoric) with no loss in the contribution it makes to understanding science, and read instead as exemplifying this methodological program. Of course without the constructivist covering the theoreticians of constructivism may well feel exposed and insecure.

That feeling of exposure is a normal part of growth, and I think constructivism is already growing beyond science-bashing and postmodern bad faith (i.e., making one's way into the establishment while pretending otherwise). As a sign of further growth (and good faith) constructivists will have to let go of some of their romantic slogans and labels, and stop playing "Let's apply constructivist makeup to the face of science." As for a better general label, the "sociology of scientific knowledge" is a rather ponderous and establishment-sounding fallback that they seem already to have prepared for the occasion. In "SSK," however, it has a quite nondescript nickname. They might, instead, want to adopt NOA (pronounced "Noah"), which has a comfortable feel and is a philosophical attitude already made to accompany the methodological (or epistemological) program laid out above. For NOA (the "natural ontological attitude") is an open, particularist, and nonessential-

ist attitude to science.<sup>26</sup> It promotes a no-theory attitude toward truth, and thus avoids the metaphysics of realism or metaphysical constructivism. It places science squarely among other human activities, and so invites the social orientation of methodological constructivism to fill the ubiquitous gaps in understanding scientific practice left by overly rationalist methodological programs.

### The Challenge to Realism

Despite the attitude of letting the ontological chips fall where they may, an attitude that NOA shares with methodological constructivism, these positions still offer a serious challenge to realism. For realism, when it pretends to be a system of beliefs supported by science, rather than a metaphysics simply imposed on science, relies on two forms of argument to give it support. The first is explanationist: roughly, that realism is to be believed because it is the best explanation for why science is successful. Since the issue between realism and instrumentalism, for example, is whether even the best explanatory hypotheses are to be believed (literally) at all, rather than just to be pragmatically used, it ought to be clear that the explanationist argument is question begging, and cannot offer reasonable grounds for the truth of realism.<sup>27</sup> One can also challenge the claim that realism actually does well in explaining the success of science, and even the presupposition that science is successful, when that is filled out and hedged in the requisite way.<sup>28</sup> Constructivism contributes a different dimension to the discussion. It offers the prospect for "explaining" much of the success of science, insofar as that obtains and is reasonable to inquire about, by showing the extent to which what counts as scientific success is tailor-made by the various scientific communities to fit just what those communities have the skills to do. This is, for example, Pickering's theme in his study of high-energy physics in the 1970's, especially with respect to the acceptance of the neutral current in 1973–74, and its connection with the flowering of gauge theory during that period.<sup>29</sup> We will not know what "success" this leaves for realism to address until the constructivists have had time enough to show what they can do.<sup>30</sup>

The second line of realist argument is an attempt to read realism off the details of scientific activity. Given its case-study orientation,

and attention to detail, this is the sort of enterprise where constructivist sociology ought to have something to contribute. The realist idea is to see realism as integral to particular features of experimental practice. The implementation of this idea can take different forms. One is Ian Hacking's "experimental argument" for realism, with its injunction that when we can build a successful scientific instrument using an entity (an instrument like an electron gun), then the entity is real (i.e., really exists).<sup>31</sup> The challenge from constructivism is the prospect it holds out of giving a detailed accounting of the formation of belief while bracketing the truth of the belief. It holds out the prospect, for example, of accounting for the belief that we have built a reliable electron gun, without commitment to the truth of the description—that is, without presupposing the truth of the claim that what the "gun" does is shoot little electron bullets. If constructivism can do this, then the existential conclusion enjoined by Hacking would be seen as an ontological gloss not required in order to make sense of experimental practice.

Another way of trying to read realism off scientific practice is the interesting contextual approach of Richard Miller.<sup>32</sup> Miller restructures the debate over realism by deflating realism in two important respects. He eschews any global enterprise, so that not all of science nor even all of any one scientific theory (or whatever) need be realist. (Sensibly, he insists that we get down to particulars.) He also drops out the specifically metaphysical component of realism associated with the correspondence theory, which amounts to making no general assumption about the nature of the things referred to by science (e.g., that they are mind-independent, or even real). For Miller the only issue for realism is an epistemological one over the existence of unobservables: In science are we often in a position to claim that descriptions of unobservables are approximately true?<sup>33</sup> The realist says yes; the antirealist, no. Without assessing his complex account of approximate truth (which involves explanatory goals, historical consequences, and even extra-scientific interests), and of what grounds such a claim to approximate truth, we cannot really judge whether this epistemological reconstruction defines any sort of realism at all. It is not yet clear to me, for instance, whether historically well-defined individuals even get put in the right camp. Do serious instrumentalists, for example, come out as antirealists? Nevertheless the argument for

the position that Miller calls "realism" is interesting. It consists of the many individual scientific arguments for the existence of the various unobservables in the scientific zoo. Miller is committed to the view that most of these (the "often" in the realism slogan) are good enough to support belief. We see here, I think, the vestige of the generalizing passion. For the universal "all" has got deflated to "most of" (or "often"), but the individual cases (of which Miller recognizes that there are indefinitely many) are still presupposed generally to stack up on the realist side. (How do we quantify the stack of an indefinite number?) I say "presupposed" because of course Miller himself can examine only a few cases (actually he treats just two: microbes and molecules), and he must then fall back on general considerations to try to persuade the reader either that other cases will be relevantly like these, or that in general the opposite supposition is hard to credit. The opposite supposition is that for the most part things turn out the antirealist way. It seems to me, however, that a particularist can make neither of Miller's suppositions. We cannot declare any general ontological faith, no matter how watered-down; for we are already committed to take things just as they come, judging individual cases on individual merit. That is how the scientific community does it, anyhow, and it is NOA's attitude as well.

Methodological constructivism suggests something a little stronger. To the extent to which that explanatory program works, it would subvert all the little arguments for realism that Miller anticipates. The constructivist program hopes to show how to account for the formation of the relevant beliefs without relying on the scientist's own account of what was compelling in the evidence and argument with respect to the truth of those beliefs, since were the scientist's account credited we would not be able to bracket the truth of the beliefs. Where this is successful, it undercuts Miller's idea that we can just follow the scientist's rationale for how they came to believe in one unobservable or another, supposing that rationale to give us good grounds for the approximate truth of the belief. If constructivism is correct about the openness of scientific rationality, close examination of scientists' accounts will show gaps. Those beliefs are grounded, rather, in contingent social factors. But that grounding in a historical social network does not lead to the truth of the beliefs. It could not generally lead to their

approximate truth either, even on Miller's liberal understanding of that concept, unless Miller has built a consensus theory (or the like) of approximate truth into his account, which I do not think he has.

Unlike metaphysical constructivism, the methodological version is not an *-ism* that competes with realism or instrumentalism as a general philosophy of science. In this respect, too, it is like NOA. Thus methodological constructivism does not challenge the truth of realism. It challenges whether support for realism can be found in the practices of science. Most simply, if the explanatory tasks set for methodological constructivism can successfully be carried out, the case for realism will have lost even its apparent grounding in science. This prospect helps to show these other positions for what they are: namely, appendages to science that neither are supported by it nor contribute to its understanding. This profile of idleness is the one NOA has cast them in all along.

The realism/antirealism debate largely sidesteps science. The debate over a constructive reshaping of constructivism may be more important. For the hope is to liberate constructivism from its own global ideology, with its overblown rhetoric and poor philosophical understanding. The aim is to urge constructivism in the direction of an open, social particularism. That seems to me the heart of the program, the right corrective to philosophical (especially realist) distortions of science, and the place where lots of good work can be done too. Among the work to be done is to achieve some understanding of what is actually involved in rational acceptance and proof in science, of what, in Boyle's words, deserves "a wise man's acquiescence." (Recall the epigraph.) This job involves exploring the diverse range of contexts, historical and contemporary, in which inquiry is carried out. In that endeavor, and others, NOA joins hands with its constructivist allies, and wishes them well!

## END NOTES

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<sup>2</sup> Later developments in this line include ethnomethodology. See Garfinkel (1967) and Sharrock and Anderson (1986).

<sup>3</sup> For a sample of some especially sympathetic admirers see the articles in Lawson and Appignanesi (1988).

<sup>4</sup> Of course it is not only analytic philosophers who take a combative stance towards constructivism. Some Critical Theorists do too. See, for example, McCarthy (1988).

<sup>5</sup> The platform below is an amalgam that constitutes an "ideal type" description of constructivism. I believe it captures important features of the school (at least in many of its stages). Nevertheless I have no doubt that each member of the school will find some things in my mix with which to quarrel. In any case, I have drawn the amalgam from the social interest Edinburgh school; especially Barnes (1977), Bloor (1976), Knorr-Cetina (1981) and Pickering (1984); the historical sociology of knowledge promoted by Shapin (1982) and the actor-network analysis of texts and transcription devices pioneered by Latour and Woolgar (1979) and developed further in Latour (1987).

<sup>6</sup> My claim here (namely, that a loose consensus theory of truth binds the planks together into a constructivist platform) might be questioned -- or even denied -- by some constructivists. Thus Bloor writes, "The question may be pressed: does acceptance of a theory by a social group make it true? The only answer that can be given is that it does not." (1991, p.43). Yet in the Afterward Bloor tells us that truths form not a natural but a social kind in the sense that their membership in the class "is a result of how they are treated by other people" (p.174) and he goes on to hedge as to whether this conception of truth amounts to idealism or, more accurately, he qualifies the *kind* of idealism it does amount to (p.175). (A decade earlier one also finds a similar superposition of realist and idealist conceptions in the seminal paper of Barnes and Bloor (1982) where their well-known relativist tribesman "might claim that not even the fact that his own tribe believes something is, *in itself*, sufficient to make it true. But he would then have to mend the damage [sic] of this admission by adding that it just was a fact that what his tribe believed *was* true." My case here is that constructivists *need* a consensus theory to make their planks hold together as constructivism and that, in fact, they use consensus for this very purpose. Below, however, I argue that this conception of truth undermines their core insight into the openness of science.

<sup>7</sup> Richard Miller (1987) advances this theme, and identifies the "worship of generality" as the philosophical legacy of neopositivism. That may be too strong an historical claim, for there are after all other sources of that worship, starting with Socrates and carrying on, for example, with Descartes and especially with Kant who was himself an important influence on neopositivism. Still, Miller's critique of specific generalizing programs is insightful, and his context sensitive realist alternative very interesting. See section (6) below for some discussion, and also Fine (1991).

<sup>8</sup> The essays in Traynham (1987) support the instrumentalist claim, especially the article by Rocke on styles of nineteenth century organic chemistry.

<sup>9</sup> This idealizing option has been revived in Putnam's internal realism (Putnam 1981),

which he now prefers to call pragmatic realism (1987, p.17). James' way was part of Rorty's (1979) epistemological behaviorism, a part to which he no longer subscribes. See especially the introduction to his essays on pragmatism (1982). I suggest a general line of criticism of these pragmatic theories, under the label of "truthmongers", in my (1988).

<sup>10</sup>

See (Fine 1986) for more on this theme.

<sup>11</sup>

This bypasses the issue of "Which community?" by supposing in effect that there is only one relevant community. Thus, as promised, I ignore the problem of relativism. For the constructivists, relativism stand or falls with the consensus theory of truth and, as we shall see, it falls.

<sup>12</sup>

The vocabulary of production also seems tinged with a Marxism not only consonant with the sociological idea of knowledge as a superstructure, but also with an anti-establishment (i.e., anti-capitalist) politics. I am not sure whether this political ideology applies to all constructivists.

<sup>13</sup>

See (Fine 1988), especially chapter 7, and (Fine 1986), especially Metatheorem 2, for the details.

<sup>14</sup>

See the works referenced in the preceding Note. Miller (1987, pp.448-461) attacks the explanationist argument over the "success" theme, showing just how carefully this has to be qualified.

<sup>15</sup>

Galison (1987), for example, who is also sensitive to social factors in science, takes issue with Pickering's (1984) account of the neutral current episode. The verdict on this is not yet in. For a negative assessment of what constructivism can do in explaining scientific success see Sargent (1988). I am more optimistic.



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