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JOHN LIE

Multiethnic Japan

John Lie

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What is social construction?

Flaws and contradictions in the claim that scientific beliefs are "merely locally accepted"

PAUL A. BOGHOSSIAN

Social construction talk is all the rage. But what does it mean and what is its point? The core idea seems clear enough. To say of something that it is socially constructed is to emphasize its dependence on contingent aspects of our social selves. It is to say: This thing could not have existed, had we not built it; and we need not have built it at all, at least not in its present form. Had we been a different kind of society, had we had different needs, values, or interests, we might well have built a different kind of thing, or built this one differently. The inevitable contrast is with a naturally existing object, something that exists independently of us and which we did not have a hand in shaping.

There are certainly many things, and facts about them, that are socially constructed in the sense specified by this core idea: money, citizenship and newspapers, for example. None of these things could have existed without society; and each of them could have been constructed differently had we so chosen.

As Ian Hacking rightly observes, however, in his insightful monograph, *The Social Construction of What?* (reviewed in the *TLS*, February 18, 2000), social construction talk is often applied not only to worldly items — things, kinds and facts — but to our beliefs about them. Consider *The Social Construction of Women Refugees* by Helene Moussa (1992). Clearly, the intent is not to insist on the obvious fact that certain women come to be refugees as a consequence of social events. Rather, the idea is to expose the way in which a particular belief has been shaped by social forces: the belief that there is a particular kind of person — the woman refugee — deserving of being singled out for special attention.

Talk of the social construction of belief, however, requires some elaboration of the core idea. For it is uninterestingly true of almost any belief that we have that it is not necessary that we should have had it and that we might not have had it, had we been different from the way we actually are. Consider our belief that dinosaurs once roamed the earth. It is obviously not inevitable that we should have come to this belief. We might never have considered the question. Having considered it, we might have arrived at a different conclusion, for a variety of causes: we might not have been interested in the truth; we might not have been as intelligent at figuring it out; we might never have stumbled across the relevant evidence (the fossil record).

These observations supply various boring senses in which any belief might be considered dependent on contingent facts about us. The important question concerns the role of the social, once all of these factors have been taken into account: that is, keeping our skills and intelligence fixed, and given our interest in the question and our desire to learn the truth about it, and given our exposure to the relevant evidence, do we still need to invoke contingent social values to explain why we believe that there were dinosaurs? If the answer is "Yes" — if it is true that another society, differing from us only in their social values, would have arrived at a different and incompatible belief — then we could say that our belief in dinosaurs is socially constructed.

It is crucial, therefore, to distinguish between a constructionist claim that is directed at things and facts, on the one hand, and one that is directed at beliefs on the other, for they are distinct sorts of claim and require distinct forms of vindication. The first amounts to the metaphysical claim that something is real but of our own creation; the second to the epistemic claim that

the correct explanation for why we have some particular belief has to do with the role that that belief plays in our social lives, and not exclusively with the evidence adduced in its favour. Each type of claim is interesting in its own way.

If a thing were shown to be socially constructed in the first sense, it would follow that it would contravene no law of nature to try to get rid of it (which is not the same as saying that it would be easy to do so — consider Manhattan). If a belief of ours were shown to be socially constructed in the second sense, it would follow that we could abandon it without fear of irrationality: if we have the belief, not because there is adequate evidence in its favour but because having it serves some contingent social purpose, then if we happen not to share the social purpose it subserves, we ought to be free to reject it.

Much important work has been done under each of these headings, most significantly, it seems to me, for the topics of gender and race. Simone de Beauvoir (*The Second Sex*, 1953), and other feminist scholars since have illuminated the extent to which gender roles are not inevitable but are rather the product of social forces. Anthony Appiah (*Color Conscious: The political morality of race*, 1996, with Amy Gutman) has been particularly forceful in demonstrating that nothing physical or biological corresponds to the racial categories that play a pervasive role in our social lives, that these categories owe their existence more to their social function than they do to the scientific evidence.

Other claims are more controversial. Mary Boyle has argued that our belief in schizophrenia is socially constructed (*Schizophrenia: A scientific delusion?*, 1990). Her claim is that there is no adequate reason to believe that the symptoms commonly lumped under this label are manifestations of a single underlying disease, and, hence, that the search for its aetiology by neurochemistry is doomed. Perhaps she is right; our understanding of mental illness is certainly in its infancy. On the other hand, there appears to be increasing evidence that the symptoms associated with schizophrenia are predictable significantly before their onset and that the condition is highly heritable. These facts point in the opposite direction.

In a flourishing research programme, we find the expected mixture of important and debatable work. However, while some particular social-construction claims may be empirically controversial, the templates of which they are instances are in no way philosophically controversial. Both the abstract thought that some things are created by societies, and the thought that some beliefs owe more to social values than they do to the evidence in their favour, are as old as reason itself. Whence, then, the widespread impression that social constructionists are anti-rationalist, anti-realist and anti-objectivist?

The answer is that it stems not from the forms of the claims themselves, and not from their application to this or that empirically debatable subject matter. It stems, rather, from the desire of some prominent theorists in this tradition to extend social-construction talk to absolutely everything and, in particular, to every fact studied by, and every knowledge claim emanating from, the natural sciences. If we are to find our way through the muddy battleground on which these now famous science wars are being waged, it will help to observe certain distinctions.

To begin with the worldly things, money, citizenship and newspapers are transparent social constructions, because they obviously could not have existed without societies. Just as obviously, it would seem, anything that could have — or that did — exist independently of societies could not have been socially constructed: dinosaurs, for example, or giraffes, or the elementary particles that are supposed to be the building blocks of all matter and that physicists call "quarks". How could they have been socially constructed, if they existed *before* societies did?

Yet when we turn to some of the most prominent texts in the social-construction literature, we find an avalanche of claims to the effect that it is precisely such seemingly mind- and society-independent items that are socially constructed. Take Andrew Pickering's book *Constructing Quarks* (1984). As his title suggests, Pickering's view seems to be that quarks were socially constructed by scientists in the 1970s, when the so-called "Standard Model" was first developed. And the language of the text itself does not disappoint: "... the reality of quarks was the upshot of particle physicists' practice..." But how can this be? If quarks exist — and we are assuming for present purposes that they do — they would have had to have existed before there were any societies. So how could they have been constructed by societies?

Perhaps Pickering does not mean what he says; perhaps he intends only to be making a claim about our belief in quarks rather than about the quarks themselves. Whether or not Pickering intended the worldly claim, however, claims like that seem to be all around us. Take, as another example, Bruno Latour and Steve Woolgar on the subject of the facts studied by natural science (*Laboratory Life: The social construction of scientific facts*, 1979): "We do not wish to say that facts do not exist nor that there is no such thing as reality... Our point is that "out there ness" is a consequence of scientific work rather than its cause."

But it is not easy to make sense of the thought that facts about elementary particles or dinosaurs are a *consequence* of scientific theorizing. How could scientific theorizing have caused it to be true that there were dinosaurs or that there are quarks? Of course, science made it true that we *came to believe* that dinosaurs and quarks exist. Since we believe it, we *act as though* dinosaurs and quarks exist. If we allow ourselves some slightly florid language, we could say that *in our world* dinosaurs and quarks exist, in much the way we could say that in the world of Shakespeare's *Hamlet*, Ophelia drowns. So, still speaking in this vein, we could say that science made it true that in our world there are dinosaurs and quarks. But all we could coherently mean by this is that science made it true that we *came to believe* that dinosaurs and quarks exist. And this no one disputes. Despite all the evidence in their favour, these beliefs may still be false, and the only thing that will make them true is whether, out there, there really were dinosaurs and there really are quarks. Surely, science cannot construct those things; at best, it can discover them.

The views apparently on offer here hark back to the discredited "transcendental idealism" of Immanuel Kant. On Kant's picture (or at least on one influential way of reading it), there is a world that exists independently of human minds, so we do not have to go so far as to say that we created the world. But, in and of itself, this world is structureless; it is not broken up into things, kinds of things, or facts. We impose

structure on the world by thinking of it in a certain way, by having one set of beliefs about it rather than another.

There are two different ways to understand the Kantian claim that we impose structure on the world. In the first, we literally make it the case that there are certain kinds of things in the world – mountains – by thinking of the world in terms of the concept “mountain”, by believing there to be mountains. In the second, the structure remains entirely on our side of the divide: the claim that there are mountains is just a way of talking about what is true according to our conceptual scheme or language game. It is not even to try to make a claim about how things are in some mind-independent reality.

The first alternative, the one that Pickering’s and Latour’s language most closely suggests, is hopelessly bizarre. How could the mind carve the world out there into kinds? How could it create things and give them properties? And what happens when the world is carved up in two incompatible ways by two different societies? Some of us believe in immaterial souls, and others of us do not. Does the world out there then both contain and not contain immaterial souls?

However, Richard Rorty has suggested that talk of the social construction of facts and kinds is perfectly cogent, provided it is understood along the lines of the second alternative:

One reason the question of mind-independent reality is so vexed and confusing is an ambiguity in the notion of “independence”. [My critics] sometimes [write] as if philosophers who, like myself, do not believe in “mind-independent reality” must deny that there were mountains before people had the idea of “mountain” in their minds or the word “mountain” in their language. But nobody denies that. Nobody thinks there is a chain of causes that makes mountains an effect of thoughts or words Given that it pays to talk about mountains, as it certainly does, one of the obvious truths about mountains is that they were here before we talked about them. If you do not believe that, you probably do not know how to play the language games that employ the word “mountain”. But the utility of those language games has nothing to do with the question of whether Reality as It Is In Itself, apart from the way in which it is handy for human beings to describe it, has mountains in it.

No way of talking could be said to be more faithful to the way things are in and of themselves than any other, because there is no way things are in and of themselves. There is just how we talk about how things are and the fact that some of those ways are better for our purposes than others. It is, therefore, correct to say that we do not make the mountains; that is a claim that is licensed by a way of talking that it pays for us to adopt. However, that does not mean that it is just plain true that there are mountains independently of humans; it never makes sense to say that anything is just plain true. All we can intelligibly talk about is what is true according to this or that way of talking, some of which it pays for us to adopt.

This, however, is an impossible view, as many critics have pointed out (see especially *The Last Word* by Thomas Nagel, 1997, and Bernard Williams’s review of it in the *New York Review of Books*, 1998). First, even Rorty doesn’t succeed in distancing himself from any commitment to the idea that some claims are just plain true, and not just true relative to this or that way of talking; he simply commits himself to the implausible view that the only kinds of claim that are just plain true are claims about which ways of talking it pays for us to adopt, rather than claims directly about mountains. Otherwise, he could not simply assert, as he does, that it pays for us to talk about mountains, but only that it pays for us to talk about its pay-

ing for us to talk about mountains, and so on without end.

Second, if we accept his view that there is no higher authority concerning what’s true than how it pays for us to talk, and if, as Rorty admits, it pays for us to say that science discovers a ready-made world, replete with mountains and giraffes, then there is simply no perspective from which he can also say, as he must if he is to express his distinctive view, that there isn’t a ready-made world for science to discover, replete with mountains and giraffes. He can’t have it both ways; but having it both ways is what his view requires.

If the preceding considerations are correct, social-construction talk does not cogently apply to the *facts* studied by the natural sciences; does it fare any better when applied to the *beliefs* about those facts produced by those sciences? The issue is not whether science is a social enterprise. Of course, it is. Science is conducted collectively by human beings who come equipped with values, needs, interests and prejudices. And these may influence their behaviour in a variety of potentially profound ways: they may determine what questions they show an interest in, what research strategy they place their bets on, what they are willing to fund, and so forth.

The usual view, however, is that none of this matters to the believability of a particular claim produced by science, *if that claim is adequately supported by the factual evidence*. Kepler may have become interested in planetary motion as a result of his religious and occult preoccupations, and, for all I know, he may have been anxious to obtain a certain outcome. But so long as his eventual claim that the planets move in elliptical orbits could be justified by the evidence he presented for it, it does not matter how he came to be interested in the question, nor what prior aim he may have had. The view is now there, with a claim on our attention, and the only way to reject it is to refute the evidence adduced in its favour.

To put this point another way, we commonly distinguish between what philosophers of science call the “context of discovery” and what they call the “context of justification”. And while it’s plausible that social values play a role in the context of discovery, it’s not plausible that they play a role in the context of justification. Social constructionists about knowledge deny this; for them, it is naive to suppose that while social values may enter into the one context, they need not enter into the other.

Well, how could social values enter into the context of justification? There are *four* distinct ways of articulating the thought a constructionist may have in mind here.

To begin with, a constructionist may hold that it is not the factual evidence that does the justifying, but precisely the background social values. It may seem incredible that anyone could have seriously thought anything like this, but there are certainly assertions out there that seem to demand just such a reading. Here is one (Kenneth Gergen, “Feminist critiques of science and the challenge of social epistemology”, in *Feminist Thought and the Structure of Knowledge*, edited by Mary Gergen, 1989): “The validity of theoretical propositions in the sciences is in no way affected by factual evidence.”

However, anyone who really thought that, say, Maxwell’s equations could be *justified* by appeal to Maxwell’s, or anyone else’s, social or political beliefs would betray a complete incomprehension of the notion of justification. An item of information justifies a given belief by raising the likelihood that it is true.

If one were absolutely determined to pursue something along these lines, a *slightly* better

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avenue, and the second of our four options, would be to argue that, although social values do not justify our beliefs, we are not actually moved to belief by things that justify; we are only moved by our social interests.

This view, which is practically orthodoxy among practitioners of what has come to be known as "science studies", has the advantage of not saying something absurd about justification; but it is scarcely any more plausible. On the most charitable reading, it stems from an innocent confusion about what is required by the enterprise of treating scientific knowledge sociologically.

The view in question derives from one of the founding texts of science studies, David Bloor's *Knowledge and Social Imagery* (1977). Bloor's reasoning went something like this: If we wish to explain why certain beliefs come to be accepted as knowledge at a given time, we must not bring to bear *our* views about which of those beliefs are true and which false. If we are trying to explain why *they* came to hold that some belief is true, it cannot be relevant that we know it not to be true. This is one of the so-called "Symmetry Principles" of the sociology of knowledge: treat true and false propositions symmetrically in explaining why they came to be believed.

It is possible to debate the merits of this principle, but on the whole it seems to me sound. As Hacking rightly emphasizes, however, it is one thing to say that true and false beliefs should be treated symmetrically and quite another to say that justified and unjustified ones should be so treated. While it may be plausible to ignore the truth or falsity of what I believe in explaining why I came to believe it, it is not plausible to ignore whether I had any evidence for believing it. For some reason that is never explained, however, Bloor and his colleagues seem to think that the two principles are on a par and are both equally required by the enterprise of treating scientific belief sociologically. Bloor builds both into the very foundation of the subject:

[The sociology of knowledge] would be impartial with respect to truth and falsity, rationality or irrationality, success or failure.

However, in the absence of an argument for being sceptical about the very idea of a good reason for a belief – and how could there be such an argument that did not immediately undermine itself? – one of the possible causes for my believing what I do is that I have good evidence for it. Any explanatory framework that insisted on treating not only true and false beliefs symmetrically, but justified and unjustified ones as well, would owe us an explanation for why evidence for belief is being excluded as one of its potential causes. And it would have to do so without undermining its own standing as a view that is being put forward because justified.

This is not, of course, to say that scientific belief must *always* be explained in terms of the compelling evidence assembled for it; the history of science is replete with examples of views – phrenology, for example – for which there never was any good evidence. It is simply to insist that scientific belief is *sometimes* to be explained in terms of compelling evidence, and that the history and sociology of science, properly conceived, need have no stake in denying that.

This brings us to a third, milder conception of how social values might be indispensable for the justification of scientific belief. On this view, although evidence can enter into the explanation for why a particular view is believed, it can never be enough to explain it. Any evidence we might possess always *underdetermines* the specific belief that we arrive at on its basis. Something else must close the gap between what we have evidence for and what we actually believe, and that something else is

provided by the thinker's background values and interests.

This idea, that the evidence in science always underdetermines the theories that we believe on its basis, has exerted considerable influence in the philosophy of science, even in non-constructionist circles. In its modern form, it originated in the thought of the French physicist and philosopher, Pierre Duhem (1861–1916). Suppose that an experimental observation is inconsistent with a theory that you believe: the theory predicts that the needle will read "10" and the needle does not budge from zero. What Duhem pointed out is that this does not necessarily refute the theory. For the observational prediction is generated not merely on the basis of the theory, but, in addition, through the use of auxiliary hypotheses about the functioning of the experimental apparatus. In light of the recalcitrant observational result, *something* has to be revised, but so far we do not yet know exactly what: perhaps it is the theory, perhaps it is the auxiliary hypotheses. Perhaps, indeed, it is the very claim that we recorded a genuinely recalcitrant result, as opposed to merely suffering some visual illusion.

Duhem argued that reason alone could never decide which revisions are called for, and, hence, that belief revision in science could not be a purely rational matter; something else had to be at work as well. What the social constructionist adds is that this extra element is something social. This is a clever argument that does not long conceal its difficulties. Consider Duhem's example of an astronomer peering through his telescope at the heavens and being surprised at what he finds there, perhaps a hitherto undetected star in a galaxy he has been charting. Upon this discovery, according to Duhem, the astronomer may revise his theory of the heavens, or he may revise his theory of how the telescope works. And rational principles of belief fixation do not tell him which to do.

The idea, however, that in peering at the heavens through a telescope, we are testing our theory of the telescope *just as much* as we are testing our astronomical views is absurd. The theory of the telescope has been established by numerous terrestrial experiments and fits in with an enormous number of other things that we know about lenses, light and mirrors. The point is not that we might *never* have occasion to revise our theory of telescopes: one can certainly imagine circumstances under which that is precisely what would be called for. The point is that not *every* circumstance in which something about telescopes is presupposed is a circumstance in which our theory of telescopes is being tested, and so the conclusion that rational considerations alone cannot decide how to respond to recalcitrant experience is blocked.

Perhaps, however – to come to the fourth and final way in which belief and social values might be intertwined – the correct thought is not that the social must be brought in to fill a gap left by the rational, but simply that the rational itself is constitutively social. A good reason for believing something, according to this line of thought, only has that status relative to variable social factors; a sharp separation between the rational and the social is illusory.

This is currently one of the most influential constructions of the relation between the rational and the social in constructionist circles. What it amounts to is a relativization of good reasons to variable social circumstance, so that the same item of information may correctly be said to justify a given belief under some social circumstances, in some cultures, but not in others. It is nicely expressed in the following passage (Barry Barnes and David Bloor, "Relativism, rationalism and the sociology of knowledge", 1981):

... there is no sense to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such.

But this is an impossible construal of reasons for belief, as Plato understood some time ago (see his *Theaetetus*). We cannot coherently think of ourselves as believing and asserting *anything*, if all reasons for belief and assertion are held to be inexorably tied to variable background perspective in the manner being proposed. There are many ways to show this, but perhaps the most telling is this: not even the relativist would be able to adopt such an attitude towards his own view. For, surely, the relativist does not think that a relativism about reasons is justified only relative to his own perspective? If he did, why is he recommending it to us who do not share his perspective?

When we believe something, we believe it because we think there are reasons to think it is true, reasons that we think are general enough to get a grip even on people who do not share our perspective. That is why we feel entitled to recommend it to them.

Neither a generalized constructionism about the objects and facts investigated by the natural sciences, nor one about the reasons for belief provided by those sciences carries much plausibility. To what does this matter? Here are two contrasting views. Rorty ("Phony Science Wars", *Atlantic Monthly*, 1999):

The science wars are in part a product of deep and long-lasting clashes of intuition, but mostly they are just media hype – journalists inciting intellectuals to diabolize one another. Diabolization may be helpful in keeping intellectuals aroused and active, but it need not be taken very seriously.

By way of contrast, we have Dorothy Nelkin: Current theories about science do seem to call into question the image of selfless scientific objectivity and to undermine scientific authority, at a time when scientists want to claim their lost innocence, to be perceived as pure unsullied seekers after truth. That is what the science wars are about.

I think that Nelkin is closer to being right. As social constructionists realize only too well, we would not attach the same importance to science if we came to be convinced by constructionist conceptions of it.

In what does the cultural importance of science consist? This is, of course, a vast subject, but there are, it seems to me, two central elements. First, and most importantly, in matters of belief we defer to science. It would be hard to overestimate the significance of this practice, reflected as it is in what we are prepared to teach our children at school, to accept as evidence in courts of law and to base our social policies on. Second, we spend vast sums of money on basic scientific research, research that does not look as though it will have any immediate practical pay-off.

Rorty's laid-back attitude depends on the thought that neither of these practices has any interesting philosophical presuppositions, and so cannot be vulnerable to constructionist critique. But this seems wrong. For deference to make sense, it has to be plausible that science delivers the sort of knowledge that *everyone* has reason to believe, regardless of their political or more broadly ideological commitments. But this would be directly challenged by a constructionist thesis about reasons for belief, on any of its available versions.

If we look at the practice of spending vast sums on basic science, it is arguable that an even greater amount of philosophy is presupposed, that we have to hold not only that science delivers knowledge that everyone has reason to believe, but that it delivers true or approximately true knowledge of the structure

of an independently existing reality. For if we ask why, given the many pressing social problems we face, we should spend tens of billions of dollars to build a super-collider that will smash ever smaller particles into each other in the hope of releasing ones that we have never seen but which our theories predict, what could possibly be a compelling answer if not that doing so will help us to understand the fundamental, hidden constitution of the universe, and that this is worth doing? If it doesn't make sense to think that there is such a hidden constitution to probe, or even if there is, if it doesn't make sense to think that science is capable of probing it, what rationale could there be for spending such vast sums, when that money could equally be spent on AIDS or on poverty? (To be clear: I am not saying that a search for the fundamental truths automatically trumps all other considerations, only that its coherence as a goal is required to make sense of the importance we attach to basic science.)

At its best – as in the work of de Beauvoir and Appiah – social-constructionist thought exposes the contingency of those of our social practices that we had wrongly come to regard as inevitable. It does so by relying on the standard canons of good scientific reasoning. It goes astray when it aspires to become either a general metaphysics or a general theory of knowledge. As the former, it quickly degenerates into an impossible form of idealism. As the latter, it assumes its place in a long history of problematic attempts to relativize the notion of rationality. It has nothing new to add to these historically discredited views; if anything, social-constructionist versions tend to be murkier and more confused than their traditional counterparts. The difficulty lies in understanding why such generalized applications of social construction have come to tempt so many.

One source of their appeal is, no doubt, their efficiency. If we can be said to know upfront that any item of knowledge only has that status because it gets a nod from contingent social values, then any claim to knowledge can be dispatched if we happen not to share the values on which it allegedly depends. There is no need to get into the details, which are often complex.

But that only postpones the real question. Why this fear of knowledge? Whence the need to protect against its deliverances? Hacking writes of certain feminists who

... see objectivity and abstract truth as tools that have been used against them. They remind us of the old refrain: women are subjective, men are objective. They argue that those very values, and the word objectivity, are a gigantic confidence trick. If any kind of objectivity is to be preserved, some argue, it must be one that strives for a multitude of standpoints.

Ian Hacking professes not to know whether to side with this thought. But he should know. Whatever legitimate worry may be at work here, it cannot be expressed by saying that objectivity and abstract truth are tools of oppression. At most, what these observations entitle us to say is that there have been occasions when those concepts have been used as tools of oppression; and no one will want to dispute that. But the fact that a concept can be, and has been, abused can hardly be a basis for indicting the concept itself. Are we to be suspicious of the value of freedom because the Nazis inscribed "Arbeit Macht Frei" on the gate at Auschwitz?

The intuitive view is that there is a way things are that is independent of human opinion, and that we are capable of arriving at belief about how things are that is objectively reasonable, binding on anyone capable of appreciating the relevant evidence, regardless of their ideological perspective. Difficult as these notions may be, it is a mistake to think that recent philosophy has disclosed any good reasons for rejecting them.