WHO'S AFRAID OF SOCIAL CONSTRUCTION? REFLECTIONS ON IAN HACKING'S *THE SOCIAL CONSTRUCTION OF WHAT?*

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Introduction

If we were to judge by the sheer number of recent books and articles that have titles of the form 'The Social Construction of X' or 'Constructing X,' we would have to conclude that the idea that things are socially constructed is among the most influential ideas ever to have captivated the humanities and social sciences. Why all the excitement?

The idea that some things are constructed is hardly new in philosophy. It is just the thought that certain things are not simply 'out there' but owe their existence to mental activity, that they are *mind-dependent*. Among the things that have been thought to be constructed in this sense we could mention colors, tastes, and smells – the so-called secondary qualities – numbers, and other mathematical objects, and moral facts.

At first glance, it may seem that a social constructionist is simply a constructionist with a social bent. Whereas the constructionist about X insists that X is mind-dependent, a social constructionist about X insists that it is *society-dependent*, dependent on the existence of a certain kind of community of minds.

On second glance, however, it becomes quite clear that this cannot be what is meant, at least not in the first instance. Consider to begin with Andrew Pickering's book, *Constructing Quarks*. His title suggests that, on Pickering's view, quarks – the particles themselves – are socially constructed. And the language of the text itself does not disappoint:

...the reality of quarks was the upshot of particle physicists' practice....

But, surely, Pickering cannot seriously mean that quarks themselves were constructed by physicists in the 1970's. Since quarks are supposed to be the building blocks of all matter, and scientists themselves, we may assume, are made of matter, that would amount to a refutation of the theory! What then does he mean?

Consider next the book by Louis Miron, *The Social Construction of Urban Schooling*. Here the problem is the reverse. Urban schooling is trivially and obviously society- dependent; what, then, could conceivably be the point of insisting that it is?

The answer, Hacking suggests, lies in seeing that, despite the misleadingness of the language used by social constructionists, what is intended in the first instance is not that a thing or kind of thing is socially constructed, but rather that the *concept* or *idea* of that thing is. It is the idea of a quark, or of urban schooling, or of woman refugee that is socially constructed.

However, to shift the focus from kinds to concepts or ideas is not entirely to give up on the thought that some worldly items are society dependent. For it is possible that certain things exist only because we have a concept of them. If the concept in question were socially constructed, then, in that special case, we could say that the kind itself is socially constructed, given its dependence on a socially constructed concept. Michel Foucault famously held such a view about homosexual culture.

Socially Constructed Concepts

But what does it mean to say that an idea is socially constructed? Let us start with a banal example:

Although children have watched television since the advent of the box, there is (it is claimed) no definite class of children who are "child viewers of television" until "the child viewer of television" becomes thought of as a social problem... Putting it crudely, what is socially constructed is an *idea*, the idea of the child viewer. Children who watch television need never have been conceptualized as a distinct kind of human being. What seems like a sensible classification to use when thinking about the activities of children, has, it may be argued, been foisted upon us, in part because of certain moralizing interests. (27)

To say, then, of an idea that it is socially constructed is to say that the question, Why this particular way of looking at the world, why this particular classification?, cannot be answered simply with: because the evidence indicates that that is the way things are. A full answer would have to make mention of certain optional social and moralizing interests, interests that the availability of the classification subserves. *Child viewer of television* may seem like a merely sensible classification to use in thinking about the activities of children, something that one would naturally light on in trying to develop a plausible theory of child behavior; according to the social constructionist, however, it is not.

There are two other thoughts at work in this passage, thoughts that are characteristic of social constructionists but which need to be distinguished from the main thesis. The first is a thought not so much about concepts but about human users of concepts: human thinkers are self-aware and so are affected by the self-descriptions that are available. Thus, once the label *child viewer* is available it gets applied. Some parents come to think of their children as child viewers and so do some of the children themselves. And all this may change their behavior, perhaps even in predictable ways. There may now come to be a set of generalizations about child viewers, ones that are the result of having a theory of child viewers to adapt to, react against or reject. Hacking refers to this as the claim that child viewers form an "interactive" kind, to be contrasted with quarks which, by not being self-aware, form an "indifferent" kind. The claim is not about concepts as such because it is presumably possible for the availability of *any* concept to alter a person's behavior.

The second thought is that, as a result of the availability of a concept, a new kind may come to be constituted, one that could not have existed independently of the concept's being available. Thus, prior to the introduction of the concept, we are told, there was no such definite class as the child viewer of television. However, once the concept is in play, and has successfully infiltrated the consciousness of some of its intended referents, there may come to be such a definite class. Whenever a kind is constituted in this way I shall say that the kind in question is "concept-generated."

In the example, these three ideas interact: the fact that children form an interactive kind is used to make it seem plausible that the kind child viewer is concept-generated; and this latter fact (if such it be) is in turn used to motivate the claim that the concept *child viewer* could not have been introduced on exclusively evidential grounds, that it is socially

constructed. But the three ideas are in principle separable and need to be distinguished for theoretical purposes. Just because a concept figures in altering the behavior of some person does not mean that a previously non-existent kind has been constituted – not every alteration in behavior amounts to the creation of a kind. Just because a kind antedated the introduction of its concept does not mean that our reasons for introducing it were exclusively evidential. And just because our reasons for introducing a concept were not exclusively evidential does not mean that there is no concept-independent kind there.

What is supposed to be the upshot of talk about social construction? As Hacking rightly emphasizes, constructionist theses are typically motivated by the conviction that the availability of a particular classification -- race, for example, or gender -- has caused a great deal of harm and that we would be better off if it were abandoned. Viewing construction theses as directed at ideas or concepts in the first instance helps make sense of this motivation in a way that thinking of them ontologically – as directed at the kinds themselves – cannot. For the claim that a certain kind of thing is society-dependent has no epistemological consequences. Without societies there could be no such thing as money; but that does not mean that, as things stand, there are not perfectly objective truths about money or that the concept of money is epistemically arbitrary.

By contrast, the constructionist thesis formulated by Hacking would have immediate epistemological ramifications. If one could show that our reasons for accepting a particular classification depend not merely on the evidence but on optional social and moralizing interests, then that would provide one with a very strong basis for arguing that the classification in question ought to be abandoned.

But we have to be careful in exactly how this is understood, for there are two trivial senses in which *any* concept could be said to be the result of extra-evidential factors. First, any concept is the result of a process of inquiry, an inquiry that is likely to be instigated not merely by a desire for the truth but by other interests as well. We might not have cared about our children as much as we do and so not engaged in any theorizing about the effects of television watching on them. And it is plausible that social or moralizing interests play a role in explaining why we came to care about these issues in the first place. Second, any inquiry will be conducted by human beings who may – and perhaps are likely to – have all manner of prior investment concerning the outcome.

Neither of these senses will do for constructionist purposes. 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 strongly invested in getting 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 investment 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 favor. It is irrelevant that Kepler would not have engaged in his research had it not been for preoccupations that we do not share or that he may have had extra-evidential motives for hoping for a certain outcome.

All this may look a little silly as applied to the relatively innocuous example of the child viewer of television, but it is easy to appreciate its significance in the case of a potent concept such as that of race. It would be enormously significant to show, as Anthony Appiah has contributed much to doing, that the racial categories that play a pervasive role in our lives were socially constructed, that they are not mere reflections of facts about people that everyone has reason to recognize. [Ref]

Controversy

It is important to appreciate that nothing that has been said up to this point is in any way *philosophically* controversial. None of the three theses that are characteristic of constructionist talk -- that certain concepts are socially constructed, that certain phenomena are concept-generated, that human behavior is affected by the available self-descriptions – would be resisted by even the most positivistic of analytic philosophers. In particular, the principal thesis, that certain concepts cannot be justified in evidential terms but merely reflect social and moral prejudice, is as old as reason itself. So why exactly is social constructionism so controversial?

Some people seem to think that its provocativeness stems from the application of social construction talk to concepts that denote real, concept-independent phenomena. But there need be no contradiction in that combination of claims. Even if a phenomenon antedated the availability of a concept, one can still ask why it is being singled out for special attention – there are many ways the world is, why focus on this one? – and the answer to that question may be more social than evidential.

Matters get a little trickier when social construction talk is applied to kinds that we have reason to believe are both real *and* ought to be singled out for special attention because they ground important generalizations.

Consider schizophrenia. There is increasing evidence both that the symptoms characteristic of this condition can be predicted before their onset and that it is highly heritable. Both items of evidence strongly suggest that there is a natural phenomenon here with an underlying physical basis. And yet we have Mary Boyle's claim, in *Schizophrenia: A Scientific Delusion?*, that schizophrenia is a socially constructed concept. Are these two claims compatible?

Discussing a similar problem in connection with the phenomenon of childhood autism, Hacking says:

Someone writes a paper titled "The Social Construction of Childhood Autism." The author could perfectly well maintain (a) that there is probably a definite unknown neuropathology *P* that is the cause of prototypical and most other examples of what we call childhood autism; (b) the idea of childhood autism is a social construct that interacts not only with therapists and psychiatrists in their treatments, but also interacts with autistic children themselves, who find the current mode of being autistic a way for themselves to be. (121)

But this attempted reconciliation seems to confuse social construction with the interactive character of humans. There is no worry about the compatibility of the latter with the underlying physical nature of schizophrenia: any concept, including that of a natural kind, might interact with its users in such a way as to affect their behavior. But the minimal content of saying that schizophrenia is socially constructed, as opposed to saying that it forms an interactive kind, is in direct conflict with the claim that there is reason to believe that it forms a natural kind: for the latter claim entails that it ought, rationally, to be singled out for special attention, whereas the former denies precisely that. So how exactly are they to be reconciled?

To switch examples, if there really were reason to believe that racial categories correspond to biological categories, categories that could explain the higher-level characteristics of their members, how could one simultaneously claim that race is socially constructed? There would look to be only one way: if biological categories themselves are socially constructed.

Natural Science

With this thought we finally arrive at the point at which much of the controversy about social construction is concentrated, the now-famous "wars" about the nature and status of knowledge in the natural sciences. Hacking quotes feminist scholar Donna Haraway:

Recent social studies of science and technology have made available a very strong social constructionist argument for all forms of knowledge claims, most certainly and especially scientific ones.(H, p41)

And although he expresses skepticism about various elements of this generalized constructionism, he is far too kind to it, as we shall see.

Perhaps the first point to make about a constructionism about natural science is that it conflicts with the interest of the sort of local constructionism about the human sciences that we have just been exploring. For constructionist theses about the human sciences tend to rest on a *contrast* with natural science. The import of saying that race is a social construction is supposed to be that we have no reason to believe it to be a biological natural kind and so may safely abandon it if we so chose. If, however, biological kinds themselves are said to be social constructions, then what was the point of that argument? There is no need for the sort of selective constructionism about race that Appiah has developed, if a generalized constructionism about biology is also available .

Well, perhaps there is no need for it. Hacking has a useful, detailed account of what the most prominent proponents of a constructionism about natural science tend to maintain. Here he does not rest content with applying his formula: evidential considerations alone do not explain why we believe what we believe. He fills the formula in with special attention to the work of Andrew Pickering and Bruno Latour, identifying three "sticking points" that divide constructionists from their opponents. According to Hacking, the constructionist affirms, while his opponent denies: (1) The acceptance and stability of scientific belief is to be explained, at least in part, by factors that are external to the content of the science, for example, by such factors as a scientist's career interests, or the networks of alliances that he has formed. (2) Given any major successful scientific theory, science did not have to take a route that involved that theory. It could have opted for a different theory while remaining just as good at explaining and predicting observations. For example, physics did not have to take a route that involved Maxwell's Equations, the Second Law of Thermodynamics or quarks. Finally, (3)

Reality does not have an intrinsic structure, a structure in and of itself; the only things that have structure are our representations of reality. It is important to look closely at each of these claims in turn. Hacking professes himself neutral on the first of these disputes, anti-constructionist about the second and pro-constructionist on the third. In each case, however, he regards them as updated versions of classical disputes within the theory of knowledge and, hence, as worthy of extended discussion. In this crucial respect, I believe, he goes seriously astray.

External Explanations of Scientific Belief

There are three distinct ways to understand the thought that what explains acceptance and stability is not evidence but something social; a failure to distinguish between them has been the source of much confusion. To begin with, someone may hold that although there is an intelligible distinction between something's being *evidence* for a belief and its being a merely non-justifying *motive* for the belief, the only sorts of causes for belief at work in science are non-justifying motives rather than reasons. A second, milder view has it that, although there are reasons for belief and although they do operate to cause belief, the reasons by themselves never suffice to explain why we believe what we do; something else must be at work and that something else is social. Finally, there is the most radical view of all, that there can be no robust distinction between something's being evidence for a belief and its being just a social cause of it, because something can only be evidence for a belief relative to certain sorts of social factor. As I understand it, these views correspond to the views of the Edinburgh School, Andrew Pickering and Bruno Latour, respectively.

On reflection, it is incredible that anyone could have seriously proposed the first of these views, but in fact it is quite common among historians and sociologists of science. Its principal source appears to be a confusion about what would be required by a sociology of knowledge.

Some twenty years ago, when the idea of a sociology of science was first beginning to take hold, David Bloor and Barry Barnes argued as follows. 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 beliefs are true and 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 the so-called "Symmetry Principle" of the sociology of knowledge: treat true and false beliefs symmetrically in explaining why they came to be believed.

One can debate the merits of this principle, but on the whole it seems to me sound. But notice that what the principle says is that in explaining what people believe, we must not bring to bear facts about the truth of those beliefs. It does not say: in explaining what people believe, we must not bring to bear facts about whether they had any *evidence* for those beliefs! Absent an argument for being skeptical about the very idea of a 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.

Over the years, the original symmetry principle concerning truth has been allowed to mutate into a symmetry principle about evidence or justification. But there is no rationale for this, nor, so far as I can see, can there be one. This is not, of course, to deny the existence of numerous examples of pseudo-science masquerading as confirmed knowledge. Nor is to deny that social and moral interests may have played a role in generating them. But the existence of such isolated examples of bad scientific reasoning cannot be the basis for a *global* thesis of scientific irrationality, and in fact militates against it: recognizing cases of pseudo-science presupposes an ability to recognize the genuine article.

This brings us to the second of the views described, according to which although reasons do operate in science, they never suffice by themselves to explain why scientists believe what they believe; something else, of a social nature, must also be invoked.

The idea that the evidence in science always *underdetermines* the theories that we believe on its basis has yielded considerable influence in the philosophy of science. In its modern form, it originated in the thought of the turn of the century French physicist and philosopher, Pierre Duhem. 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, say. 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; one must assume, in addition, auxiliary hypotheses about how the experimental apparatus works. 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 evidence alone could never decide which revisions are called for and, hence, that belief fixation 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. To work, it must assume that which it should prove, namely, that at any given point in time, some claim cannot be more confirmed than another. 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. It is simply not plausible that, in coming across an unexpected observation of the heavens, a rational response might be to revise what we know about telescopes! 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 third of our three options – 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 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. As Latour is prone to say, "we have never been modern."

This is currently perhaps the single most influential construal of the relation between the rational and the social in constructionist circles. It is a general view about reasons and knowledge, not a view that is specific to science as such. What it amounts to is a relativization of reasons to variable social circumstance, so that the same item of information may correctly be said justify a given belief under some social circumstances, in some cultures, but not in others.

But relativizing reasons for belief in this way looks to be very problematic. It implies that every claim to knowledge depends for its cogency on a background ideological commitment, and so may be rejected provided only that the background commitment is found wanting.

But this is an impossible conception of the relation between reasons and values, as Plato understood quite some time ago. We cannot coherently conceive 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 her own view. For, surely, the relativist does not think that relativism is justified only relative to her own perspective? If she did, why is she recommending it to us, who do not share her perspective?

One often hears it said that there is something question-begging about these self-refutation arguments, that they depend in some unspecified way on the very system — classical epistemology — that relativism is supposed to place in question. But what do these arguments presuppose? Nothing more than the observation that when we believe something we believe it because we think there is reason 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 so that we feel entitled to recommend it to them. In other words, they presuppose nothing more than the very attitude that the relativist must adopt towards her own claim. Can we really imagine a way of thinking about belief and assertion that will leave no room for that attitude?

One of Hacking's most startling claims is that the dispute between external and internal explanation of acceptance and stability is

analogous to some versions of the opposition between empiricists and rationalists. In the traditional sense, however, both rationalists and empiricists are firmly within the internalist camp; their disagreement centers only on the role played by the senses in the content-internal evidence at work in science. Empiricism as traditionally understood has nothing to do with the claim that it is a scientist's network of alliances that explains his acceptance of a given theory, nor is it committed to the underdetermination of belief by evidence or to the relativization of reasons to optional social interests.

Contingency

So much for the claim that acceptance and stability in science must be explained, at least in part, by factors external to the content of the science. We should consider now the distinct, though closely related, thesis that Hacking calls contingency.

Consider some successful scientific theory, for example, the theory that all matter is built up out of particles with particular physical properties and that are called "quarks." This theory, formulated in the 1970's in response to a variety of experimental and theoretical considerations, has held up very well in subsequent testing. It has been repeatedly confirmed. No one imagines that it is in its final state, but there is much reason to believe that it is on the right track.

According to Andrew Pickering, however, physics did not have to take a quarky route. Without any loss in its ability to predict and control observations, it could have opted for a very different sort of theory of matter, one that is neither equivalent to quark theory, nor a proper part of it.

Hacking evinces little sympathy for contingency so understood, and minimizes its significance. On his view, no important philosophical thesis about science is engaged by contingency, only an "inevitabilist" conception of scientific progress championed by physicists. He is right, I believe, about contingency's plausibility, but not about its importance. If contingency were true, it would significantly impact upon the viability of a *realist* understanding of science.

According to scientific realism, science aims to discover the truth and is succeeding in doing so with ever increasing accuracy and scope. To sustain the second conjunct of this view, it is vital that current scientific theories be seen as refinements and extensions of previous

theories, rather than as outright refutations of them. For otherwise, as the philosopher Hilary Putnam pointed out, the following meta-induction will come to seem unavoidable: just as prior theories have been refuted by current theories, so current theories will be refuted by future theories; hence there is no reason to believe that our best contemporary theories are even approximately true.

Interestingly enough, it does seem as though the picture of science, as proceeding by refinement and extension rather than by outright refutation, is correct. At velocities that are small relative to the speed of light, Einstein's theory of relativity, for example, appears to generate a version of Newton's laws of motion. So Einstein does not straightforwardly refute Newton, but rather shows his theory to be approximately correct, correct in the limited domain of low velocities.

But this neat little vindication of a realist conception of the development of science would be immediately threatened by Pickering's contingency thesis. If it is in fact arbitrary that physics adopted a theory of motion that has Newton's theory as a special case, if it is true that it could equally well have adopted a theory that did not have this feature, then the bruited vindication of scientific realism fails.

How exactly should the contingency claim be understood? Is it the claim that physics, had it adopted the alternative theory, would actually have been as successful as it currently is; or is it just the milder claim that at the time it chose to go one way rather than another, it had no reason to believe that one route would be more successful than the other?

If it is the former, then even a non-physicist (like me) is bound to find it breathtakingly overconfident. How could Pickering, or anyone else, possibly know how well any alternative research program to quark theory would have worked out had it been pursued with the same expenditure of brains, money and dedication? Even in this particular isolated case such a claim seems impossible to substantiate, let alone that we are in a position to assert that there is a true such counterfactual in connection with *every* successful theory in the history of science.

If, on the other hand, the view is just the milder claim, then it is simply the Duhem-style underdetermination thesis all over again, and we have already seen that there is no general reason to believe that. I conclude that we have been given no reason to take contingency seriously.

Facts and Truth

So far we have been considering views of the following form: Regardless of how the world is in and of itself, independently of our beliefs about it, our beliefs about how it is are not constrained exclusively by the evidence. We now need to consider constructionism at its most radical: there is no way the world is independently of our beliefs about it; indeed, it is precisely our choice of theory that makes it the case that some facts obtain and not others. Here, for example, are Latour and Woolgar:

We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist. Our point is that "out there ness" is a consequence of scientific work rather than its cause. (180-182)

As I have already indicated in my discussion of the symmetry principle, I am not concerned about the second clause of the final sentence, the denial that the truth of a belief can sometimes explain why we believe it. What concerns me is the far more controversial suggestion made in the first clause, that something's *being* a fact is a consequence of scientific theorizing. How could science have made it true – made it a fact – that there was a big bang, or that Jupiter has sixteen moons, or that dinosaurs roamed the earth? Was not the solar system waiting for Kepler, were there not dinosaurs before there were scientists?

Hacking tries to rescue the constructionist from total absurdity by suggesting that what they really mean is not the view that all truths are *causally* dependent on human minds, but that they are *representationally* dependent on them. He equates the first view with idealism and the second with nominalism; he even signals his sympathy for nominalism so understood. I have qualms about Hacking's terminology here, but will not pause to elaborate them.[1]

Naively, we might suppose that, prior to and independently of the existence of human beings and their thoughts, there was a world and it contained certain things with certain properties. Neutrinos, perhaps, dinosaurs, mountains. Human thought attempts to match this independently existing reality, by picking concepts that refer to existing kinds of thing and by attributing properties to them that those referents actually possess.

According to nominalism (in Hacking's sense), this picture is hopelessly wrong-headed. In and of itself, the world does not have a structure: it is nonsense to suppose that, prior to humans inventing a

[1] Historically, nominalism is ...

vocabulary in which the world is to be described, there was a particular distribution of properties across objects. Once a vocabulary is selected that carves things up in a certain way – neutrinos, dinosaurs, mountains – then that imposes a certain structure on the world. But prior to the selection of a vocabulary, we cannot say that the world contained certain things and not others.

But this still sounds absurd does it not? The selection of a vocabulary makes it come true that there are planets and mountains when there were none previously? Is this not just the absurd view that humans made the planets and the mountains by coming up with a word?

Richard Rorty, the chief contemporary proponent of Hacking's brand of nominalism, attempts to escape from this difficulty. Referring to the criticisms of the philosopher John Searle, he writes in a now-famous passage:

One reason the question of mind-independent reality is so vexed and confusing is an ambiguity in the notion of "independence." Searle sometimes writes 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.

There is no mind-independent structure, so there is nothing for our concepts to be faithful to. Nevertheless, according to this line of thought, it pays for us to use some concepts over others. Among the concepts that it pays for us to use is the concept of a mountain, that is, a concept of a formation that is causally independent of us. As a result, it would be a mistake to say that mountains are created by us, for that would be to misunderstand how the concept "mountain" is used. But this does nothing to indicate that there really are mountains independently of human representation. The casual independence of mountains is itself merely a reflection of the concepts we use, and the fact that we use some concepts and not others is answerable to nothing other than what is expedient for us in the way of belief.

There is, however, a well known objection to this sort of view that has never been answered properly and it is, in my opinion, devastating. The nominalist wants to say that there are no facts independent of human representation. Once a vocabulary is selected, then some things come out true and others come out false. If we ask why we should pick one vocabulary over another, the answer is that some vocabularies are better than others at predicting the future course of experience and those are the ones that it pays for us to adopt.

What, however, about the fact that some vocabularies or theories better conform to the course of experience than others? Are those sorts of fact also generated by our choice of theory or are they simply facts? If the nominalist says that they too are generated by our choice of theory, we are off on an unstable regress. We have chosen a particular theory because it best conforms to the course of experience. But the fact that it best conforms to the course of experience is itself an artifact of our choice of some theory with which to describe theories and experiences. That second theory itself is to be chosen because it best conforms to the course of experience. And so on. The end result is that there can be no choosing one theory over another on this view.

Suppose the nominalist takes the other horn of the dilemma we have posed, and says that facts about which theories best conform to experience are not themselves to be thought of as generated by a choice of vocabulary or theory but are simply facts. But this now seems an odd place to draw the line. If there are facts that are independent of theory why should they simply concern facts about which theories best conform to the course of experience. If there are such complex and high-level facts, is it not much more plausible that there should be theory-independent facts about mountains, electrons or dinosaurs?

If there is something wrong with the naïve picture of a world that is a certain way independently of our representational systems, we have not been shown what it is, and the alternatives to it seem worse than merely false.

The Cultural Authority of Science

None of the three theses characteristic of a constructionism about science withstands much scrutiny. To what does this matter? Here are two contrasting views. Rorty:

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.[2]

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. Philosophical conceptions of science can matter a great deal to the cultural authority that science should be accorded, and it is one of the enduring contributions of social constructionism that it has helped us to appreciate this. As social constructionists realize only too well, much of the importance we attach to science depends on a constructionist conception of it being wrong.

What importance do we attach to science, in what does its cultural authority 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 principle, reflected as it is in what we are prepared to teach our children at school, in what we accept as evidence in courts of law, and in what we are prepared to build our social policies around. If a cherished belief of yours – for example that smoking is not harmful to you – comes into conflict with the deliverances of the best science of the day, you would be considered grossly irrational by most people if your cherished belief did not give way. Second, we spend vast sums of money on basic scientific research – money that could easily be spent on pressing social problems instead, on research that does not look as though it will have any immediate practical payoff.

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

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[2] "Phony Science Wars," *Atlantic Monthly*, November 1999.

be directly challenged by the thesis of external explanations of scientific 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. I am not, of course, referring to those areas of science, like molecular biology, biochemistry, condensed matter physics, and so forth, that have a very close relation to actual or potential applications in real life. I am talking about those arcane areas of fundamental physics and applied mathematics – quantum cosmology, string theory and the like – that are far removed from any immediate or foreseeable practical benefit. 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 that seems worth doing. If, however, it makes no sense to think that there is such a hidden constitution to probe, or even if there is, if we do not think that science is capable of probing it, what rationale could there be for spending such vast sums, when that money could be spent on AIDS or on poverty? [3]

These debates about the nature of science, knowledge and truth do matter, and not only because they serve to arouse academics from their boring routines. We owe to social constructionism a renewed awareness of the extent to which the principles by which society is organized depend on conceptions of truth and the means of acquiring it that were bequeathed to us by the Enlightenment. Social constructionism's mistake

[3] Even here matters are not so clear cut, if only because the history of science is replete with examples of theories that looked as though they could not possibly have any practical consequences but which were later crucial to the development of some applied benefit. But this inductive rationale for basic science can look weak when weighed against urgent social needs.

lies not in this but in thinking that we have discovered compelling reasons for abandoning those conceptions.