Chapter Three

WHAT ABOUT THE NATURAL SCIENCES?

Is there any point in talking about social constructs in connection with the natural sciences? Yes, there is a point in doing so, but that may not be the best way to examine the issues. We should separate out some fundamental disagreements about natural science that are made contemporary by using the phrase "social construct." Call them *sticking points.* They begin with philosophy and go almost as far as politics. Many would prefer to proceed the other way round. Dorothy Nelkin (1996) wrote a one-page essay asking "What are the science wars really about?" Her answer is that "current theories about science do seem to call in 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."[1] Or, more dramatically, the science wars are fueled by the rage against reason-masquerading-as-innocence. We should never forget that, but neither rage nor an image promises a clear view of constructionism about science. We first must grasp some basic philosophical issues that separate the two sides.

The issues may be irresoluble, for they are contemporary versions of problems that have vexed Western thinkers for millennia. I shall deliberately avoid traditional formulations, because old words tend to become ancient hulks encrusted with barnacles. But scrape off the parasites for yourself, and you might glimpse the gleaming hull of an Aristotle or a Plato shining through. My observation is not that we ought to be doing the same old things that they began, but that the same old things are still being done.

Only towards the end of this chapter will I get around to two less highbrow and more politically engaged confrontations. One is in the spirit of Nelkin's diagnosis, and comes from parties to constructionism who challenge a comfortable image of science. The other, in a spirit of symmetry, comes from the scientific side, and expresses betrayal.

What Are the Natural Sciences?

"Natural science" and "social construct" are the keywords. There is no need to define the natural sciences because the old favorites, chemistry and physics, and the new favorite, molecular biology, will do. They are the sites where the battle must be joined. We are not surprised to hear that the results of primatology bear strong traces of their discoverers. We can well imagine what Donna Haraway (1989) and others have taught us in detail: accounts of the behavior of primates reflect the societies of the scientists who study them. We all know the bad jokes about British apes with stiff upper lips, ruthlessly enterprising American apes, hierarchical and communitarian Japanese apes, promiscuous French apes. Primates, perhaps, have been a field for working out ourselves as much as describing animal communities. But many readers blanch when they come across the idea that the results of physics, chemistry, and molecular biology are social constructs.

Who Are the Social Constructionists about Science?

Trevor Pinch and Wiebe Bijker (1987, 18–19) call all recent work in Science and Technology Studies "social constructivist." I shall be more narrow and literal. My two exemplars of social construct thought have already been mentioned quite often.[2] Both have "construct-" in the title or subtitle: Pickering's (1984) *Constructing Quarks,* and Latour and Woolgar's (1979) *Laboratory Life: The Social Construction of a Scientific Fact.* Old books, for sure, but ones whose authors are vigorously at work, and who are almost universally regarded as constructionists. Scientists reported in each book got Nobel prizes, so this is first-class science; no shoddy goods on view here. It is a further convenience that the two books target the natural sciences just mentioned. One is about high-energy physics, the other about organic chemistry.

My examples share a feature that may prompt suspicion. Some knowledgeable scientists quite like the books. Have not the authors sold out? For example, the longest critical notice of *Constructing Quarks* says that no one has any excuse for not understanding the basics of the high-energy physics of the 1970s. The reviewers state that despite the fearful constructionist ideology to be found in a couple of chapters, Pickering's book is a first-rate history and explanation of the subject, accurate and readable at the same time (Gingras and Schweber 1986). Latour and Woolgar worked in the Salk laboratory founded by Jonas Salk of the polio vaccine. He wrote a preface for their book, bemused but admiring. He had no problem with Latour and Woolgar's description of activities in the laboratory he founded.

I like that; it is important that accounts of laboratory science, no matter how subversive their intent, should on the surface sound realistic to people who know the field in question. But does not the very fact that a physicist says Pickering's book is quite good history of physics, or the fact that the patron of the laboratory liked Latour's version of events, show that the authors are not critical enough? I think not. Both Latour and Pickering have been reviled by men on the other side in the science wars. For some thinkers, they are public enemies numbers one and two.

My choice of examples may be criticized on other grounds. There is an entire group of fields named Sociology of Scientific Knowledge, Science and Technology Studies, and Social Studies of Science. Practitioners are widely lumped together as "constructionists," despite the fact that construction, per se, does not loom large on their agendas. Should I not use them as my examples?

There is the Edinburgh school, including Barry Barnes (1977, 1995) and David Bloor (1976). It became famous early for its "Strong Programme in the Sociology of Knowledge."[3] Lewis Wolpert (1993), the distinguished British embryologist and public spokesman about science, connects the Strong Programme with social constructionism. "Those who hold to the Strong Programme believe that all knowledge is essentially a social construct, and so all science [good or bad] merits the same attention" (p. 110). I have not found this argument (the *A,* therefore *B*) in the writings of Barnes or Bloor. I shall mention their symmetry thesis later, but constructionism does not seem to be so intimately involved in the Strong Programme as is commonly made out. We come to the Strong Programme chiefly at sticking point #3, where we reflect on the stability of some scientific knowledge. The Edinburgh school wants to explain it by considerations which most scientists consider to be external to what is known, that is, to the content of the science.

Then there is the Bath school, including Harry Collins (1985, 1990, 1998), Trevor Pinch (1986), (Collins and Pinch 1982, 1993). I have heard Collins described as the "gate keeper" of Sociology of Scientific Knowledge. Many other individuals also practice science studies with a slightly iconoclastic bent. David Gooding (1990), Karin Knorr-Cetina (1981), Michael Lynch (1985, 1993), Simon Schaffer and Steven Shapin (1985) (Shapin 1994, 1996). Latour and his colleague Michel Callon are held to be engaged in a slightly different project, named "actor-network theory." Latour's original co-author, Steve Woolgar, has gone off in other directions (1988), and has concerned himself with questions about how the social study of science, being a science, has theses that refer to itself—"reflexivity."

The fairly recent state of play among these workers can be found in Pickering's (1992) collection of specially commissioned papers. Should I not give all these alleged "constructionists" equal time? By using Pickering and Latour as exemplars, will I not skew things? Doubtless, but I prefer to skew things towards two workers, Latour and Pickering, who (a) were there in the beginning with *Construct*-titled books about specific branches of science, (b) whose work proceeds apace, at this very moment, in innovative ways, and (c) whose descriptions of laboratory science were held to be faithful, if idiosyncratic, by some scientists who knew the fields well—even when the philosophical conclusions of the books looked bizarre to the very same scientists. Finally, (d) they are held by some scientists to be public enemies.

Distinctions

"*I take it for granted* that science is a historically situated and social activity and that it is to be understood in relation to the *contexts* in which it occurs." So writes Steven Shapin (1996) in the introduction to his book on the scientific revolution. The excessive emphases suggest he is worried. I am not worried. So I can say it without emphasis: I take it for granted that science is a social activity, to be understood in its contexts. But only after a distinction!

What distinction? According to the physicist Sheldon Glashow (1992, 28), "the assemblage of these [universal] truths is what we call physical science." Well, an assemblage of truths, or even of falsifiable hypotheses, is not a social activity. So in Glashow's perfectly legitimate sense of the word, science is not an activity of any sort whatsoever. On the other hand, if by science we mean scientific activity, then it is (trivially) social.

Even those scientists who work mostly on their own have to communicate their work.

This distinction, between an activity and an assemblage of truths, does not beg any questions about social construction. But it does point to what should be at issue. Recall the distinction between process and product. For sociologists the processes of science, the scientific activity, should be the main object of study. But for scientists the most controversial philosophical issues are about science, the product, the assemblage of truths.

We must mind our distinctions. Most people dislike distinctions.[4] You may find that my discussion smells of the study. Why not just roll with the punches and talk straight? No. It is bad to cave in to careless talk and enthusiastic bravura. In a book review in *Nature,* Harry Collins (1995) recalls Richard Dawkins's statement that no one is a social constructionist at 30,000 feet. Dawkins, continues Collins, has money in his pocket up there in the friendly skies. And money is socially constructed! So how can Dawkins reject social constructionism? This tomfoolery allows us to state two home truths.

First, against Collins, nobody doubts that things whose very existence requires social institutions and contracts are social products. Nobody doubts that many things dear to us, including money, are the product of our society and our history, and require social practices to stay in place. Collins has ample ground to feel that he and his colleagues are misunderstood, but he seems to direct his spleen at the wrong target.

Second, against Dawkins. Many social constructionists about the natural sciences appear to dislike the sciences. Nevertheless, constructionists do not maintain that the propositions received in the natural sciences are in general false. They no not believe that artifacts, such as airplanes, engineered in the light of scientific knowledge, usually fail to work. Constructionists are creatures of Humian habit. They expect airplanes to get you there, and know that science, technology, and enterprise are essential for air travel. Dawkins has plenty to get mad about, but he too seems to direct his spleen at the wrong target.

What is true is that many science-haters and know-nothings latch on to constructionism as vindicating their impotent hostility to the sciences. Constructionism provides a voice for that rage against reason. And many constructionists do appear to dislike the practice and content of the sciences. When Collins (1993, 262) insists that "Most of us love science, include Einstein among our top five all time heroes . . ." (and on and on in a sentence with 65 more words), one cringes and mutters something about protesting too much. But Pickering and Latour manifestly like the science they study and do not have to say so. They may query some self-serving images of science that are in circulation, and exalted pictures of what scientists do, why they do it, and how they do it. That is very different from doubting the truth or applicability of any propositions widely received in the natural sciences.[5] If they are social constructionists, they are so at 30,000 feet.

Sometimes making a distinction can put an end to controversy: the opponents were speaking of different things, and there is no real conflict. On other occasions distinctions can foster dissent. In Chapter 1, and below, in Chapter 5, I try to make sense of the claim that something can be both real and a social construction. That is a conciliatory gesture. In this chapter I pursue the opposite strategy, of finding irresoluble differences between realists and constructionists. This is because the science wars are founded upon, among other things of a more political or social nature, profound and ancient philosophical disputes. Thus my strategy here is the exact opposite of Sergio Sismondo. He is a peacemaker. One "reason for the lack of realist/constructivist debate lies in the fact that each side usually views the other position as obviously untenable" (Sismondo 1996, 10). By lopping off extremism on the edges of both doctrines, he hopes to find common ground. In contrast, my sticking points emphasize philosophical barriers, real issues on which clear and honorable thinkers may eternally disagree.

STICKING POINT #1: CONTINGENCY

The boldest title in the natural science arena is *Constructing Quarks.* Pickering plainly meant social construction. But according to the Standard Model, quarks are the building blocks of the universe! How then could they be constructed, let alone socially constructed?

When someone speaks of the social construction of *X,* you have to ask, *X* = what? A first move is to distinguish between objects, ideas, and the items named by elevator words such as "fact," "truth," and "reality." Quarks, in that crude terminology, are objects. But Pickering does not claim that quarks, the objects, are constructed. So the *idea* of quarks, rather than quarks, might be constructed.

That is a bit of a let-down. Everyone knows that ideas about quarks emerged in the course of a historical process. To say that Pickering was writing about the idea of quarks, rather than the objects quarks, deprives his startling title of its novelty. That will not do. Pickering intended more than a history of events in high-energy physics during the 1970s, more than a history of ideas. What is this more?

One radical notion, which prompts talk of construction, is that Pickering does not believe that the emergence of the quark idea was inevitable. You have to be careful here. Obviously the march of high-energy physics was not inevitable—the debacle of the Super-Conducting Super-Collider reminds us of that. Funding might have ended in 1946. Gell-Mann, the quark-namer and author of *The Quark and the Jaguar* (1994), might have become the world expert on jaguars. When Pickering says that the actual development of high-energy physics was highly contingent, he intends us to think of something like high-energy physics as a rich and triumphant international science that evolved after World War II and is regarded as a tremendous success—but this imagined fundamental and equally successful physics does not proceed in anything like a quarky way.

Pickering does state some options that he believes were open to high-energy physics in the early 1970s (they are ably summarized for the lay reader by Nelson 1994, 538–540). He distinguishes what he calls the new physics from the old prequark physics that transformed high-energy work during the 1970s. The changes were not only in theory but also in instrumentation. The bubble chamber, which had long been the tool of preference for producing tracks of particle decay, was partially superseded by new kinds of detectors. Pickering thinks that the "old physics" could well have carried on, and that it was not predetermined that its vision of the world, and its methods of interfering with and interpreting the world, would cease to bear fruit. He argues that the old physics was in an important sense incommensurable with the new physics—a sense that is more perhaps precise than in Thomas Kuhn's writing.

Let us, however, attend not to the details but take the general claim: alternative "successful" science is in general always possible. What does successful mean? The standards of success in a science are partly determined by the science itself. If the standards of successful science are to some extent internal to a science, what can be meant by an equally successful but nonquarky fundamental physics? Successful by what criterion? One content-neutral criterion is Imre Lakatos's (1970) idea of progressive and degenerating research programs. A research program (in Lakatos's sense: he is not talking about research programs in the ordinary sense used in talking, say, about grant proposals) is a series of theories. For Lakatos a program is empirically progressive if the successive theories make new predictions not covered by predecessors, while retaining most earlier corroborated predictions. It is conceptually progressive if its theories regularly produce new concepts with rich and simplifying structures. We could add "technologically progressive" to the list of virtues. A program is degenerating if it lacks these virtues and if, when confronted by difficulties, it produces new theories that merely skirt the problems, saying, "none of our business."

I am not offering Lakatos's methodology of scientific research programs as a correct philosophy of science. It is one standing proposal that says, in a way that at present seems to be fairly neutral, what a successful branch of science is. It enables us to explain the notion of "an equally successful physics that did not proceed in a quarky way." We mean a research program that does not incorporate anything equivalent to the standard model, but which is as progressive as contemporary high-energy physics. It might even carry cosmology and the origin of the universe along with it, but with a different world view emerging, and nothing like a quark in sight. Most scientists think this is absurd. So here we have one substantive sticking point.

Pickering never denies that there are quarks. He maintains only that physics did not have to take a quarky route. His type of claim is quite general. Physics did not need to take a route that involved Maxwell's Equations, the Second Law of Thermodynamics, or the present values of the velocity of light. Applied mathematics did not need to pass through quaternions (a mathematical example of Pickering 1995a), and geology could have shunned dolomite (my final example in Chapter 7). Most scientists find such assertions ridiculous.

This sticking point is not about the truth, or reality, or whatever, of dolomite or Maxwell's Equations. But does not Pickering have to get down to questions about truth sooner or later? It is a merit of his approach that he leads to a basis for serious disagreement in which we need not (yet) become ensnared by philosophy-laden words like "truth." The two words with the biggest role in Pickering's recent work are *resistance* and *accommodation.*

Resistance and Accommodation

When Pickering wrote about high-energy physics, he was well aware of its materiel, the spacious accelerators, the intricate detectors, the problems of getting the beam running right. His more recent book, *The Mangle of Practice* (1995a), is perhaps the most materialist contribution to social studies of science to date. He examines a complex dialectic of theory, experiment, and above all the machinery, instrumentation, computing equipment, and so forth, the substance of the science. The old motto used to be, "Science proposes, nature disposes." People put up conjectures, test them in experimental situations, and nature gets rid of the ones that are false. Pickering's view adds some much needed structure to that maxim. Research scientists have theoretical models, speculative conjectures couched in terms of those models; they also have views of a much more down-to-earth sort, about how apparatus works and what you can do with it; how it can be designed, modified, adapted. Finally, there is that apparatus itself, equipment and instrumentation, some bought off the shelf, some carefully crafted and some jerry-built as inquiry demands it. Typically, the apparatus does not behave as expected. The world *resists.* Scientists who do not simply quit have to *accommodate* themselves to that resistance. They can do it in numerous ways. Correct the major theory under investigation. Revise beliefs about how the apparatus works. Modify the apparatus itself. The end product is a robust fit between all these elements.

Robust Fit

Pickering's picture can be compared to a thesis advanced at the start of the twentieth century by the French physicist, philosopher, and historian of science, Pierre Duhem (1906/1954). Suppose that an experimental observation is inconsistent with a speculative conjecture expressed within the context of a theoretical model. That does not automatically refute the conjecture. For the observation is inconsistent only with the conjecture as it is used in the model, when taken together with auxiliary hypotheses about how the apparatus works. In the light of a negative experimental result, one is forced to revise, yes, but one can revise either the major theory under investigation or the auxiliary hypotheses about the apparatus. In Duhem's illustrative fable of an astronomer probing the heavens and not finding what is expected, the stargazer could revise the theory of the celestial vault *or* revise the theory of how the telescope works. Pickering adds: or rebuild the telescope.

When it comes to apparatus there is "the concrete instrument that [the scientist] manipulates," and a "schematic model of the same instrument, constructed with symbols by the aid of theories" (Duhem p. 155). In physics there is also what physicists call the phenomenology, the interpretation and analysis of experimental results; phenomenologists are responsible for the mesh between overarching physical theory and data. Duhem emphasized that we could change the schematic model. In modern physics, we can also revise the phenomenology. Pickering adds that it is also open to us to modify the concrete instrument—the telescope, or whatever.

The dialectic of resistance and accommodation sometimes comes to a temporary halt. Does this halt become a sort of permanent benchmark? Can it be used to manufacture reliable reproducible technology if wanted? If so, let us say that the fit between theory, phenomenology, schematic model, and apparatus is *robust.*

In ordinary English, this word means strong, or sturdy.[6] The idea is familiar. The fit between theory, phenomenology, schematic model, and apparatus is robust when attempts to replicate an experiment go pretty smoothly—and when other groups of workers, with new apparatus, new tacit knowledge, and a different experimental culture do not encounter important new resistance. I do not want to overemphasize the replication of experiments: more commonly, people try to improve on an experiment, not to repeat it (Radder 1995). I do not want to exaggerate the ease with which tacit knowledge is transferred (Collins 1985). I say only that there is an intelligible sense in which a fit between theory, phenomenology, schematic model, and apparatus becomes robust.

Contingency Means No Predetermination

To sum up Pickering's doctrine: there could have been a research program as successful ("progressive") as that of high-energy physics in the 1970s, but with different theories, phenomenology, schematic descriptions of apparatus, and apparatus, and a different, and progressive, series of robust fits between these ingredients. Moreover—and this is something badly in need of clarification—the "different" physics would not have been equivalent to present physics. Not logically incompatible with, just different.

The constructionist about (the idea of) quarks thus claims that the upshot of the process of accommodation and resistance is not fully predetermined. Laboratory work requires that we get a robust fit between apparatus, beliefs about the apparatus, interpretations and analyses of data, and theories. Before a robust fit has been achieved, it is not determined what that fit will be. Not determined by how the world is, not determined by technology now in existence, not determined by the social practices of scientists, not determined by interests or networks, not determined by genius, not determined by anything.

Contingency Does Not Mean Underdetermination

This vision must be sharply distinguished from Quine's famous notion of the underdetermination of theory by experience.[7] Quine observed that many incompatible theories are logically consistent with any given body of experience. Even if all possible data were in, there would still "in principle" be infinitely many theories that were formally consistent with such data. That is a logical point.

Pickering's point is not a logical one. He claims that, at any stage in research, it is not predetermined what will happen next. Even if it is predetermined that an experiment will not work as hoped, how it will not work, and more importantly how people will adapt to resistance, are not predetermined. What is to be done is not a matter of "choosing" a theory, but of meddling with theory, apparatus, and accounts of what the apparatus is doing. Pickering is talking about what will count next as data, what the research personnel will do, how the world will resist, what won't work, how the researchers will interpret that. None of that, in his view, is predetermined. Hence he is opposed even to the modest doctrine of Peter Galison that theoretical and instrumental traditions place constraints on the results of research (Galison, Pickering 1995c). The spat between Pickering and Galison has nothing to do with Quine's merely logical and hypothetical ideas. In his early work Pickering himself may have attempted some alliance with Quine in early work (1986, 5f, 404), but that was a mistake. His current analysis has nothing to do with Quinean underdetermination.

The constructionist believes that many robust fits were possible, although in the end only one seems conceivable. The actual fit that is arrived at is contingent. Physics did not have to develop in a quarky way. This is not because physicists, by some collective act of decision, could have wittingly chosen one account of the world over another. No such fanciful libertarianism is to be found in Pickering's work. The claim is that there are different ways of adapting to resistance, involving not only thinking, but also making different types of apparatus, and many ways of working in and adapting to the resultant material world.

The words "accommodate" and "adapt" immediately make one think of biological adaptation and evolution. One distinguished reviewer of Pickering's *Mangle of Practice,* John Ziman (1996), picked up the idea, and in a recent talk Pickering (1997) carried that forward. No set of conditions determines future biological evolution. In the same way, no set of conditions—including "how the world is"—predetermines the evolution of a science. In particular, in my terminology, they do not predetermine the shape of any robust fit that evolves.

Who might be troubled by contingency, so understood? Physicists, not metaphysicians.

Alien Science

Many physicists find it inconceivable, in retrospect, that there could have been a successful fundamental physics of the 1970s that did not take something like the quark road. Of course quarks are not the end. Perhaps there are lepto-quarks. Maybe quarks themselves drop out of the cosmotemporal mush to which our apparatus is directing us. But smart and well-supported groups addressing anything like the topics addressed by physicists in the sixties and seventies would inevitably have developed ideas very much like those that actually evolved. They reject Pickering's suggestion that the "old physics" and its detectors need not have been displaced. They agree that there is plenty of trivial contingency. Solemn names rather than joke names such as "quark" and "charm" might have been used, but the fundamental structure of any physics would be much the same. So would the material structure of apparatus, by and large. Some may even argue that the institutional structure would have to have evolved in something like the way it did, but most physicists are not interested in making claims like that.

Any successful science would have to have been equivalent to actual science. What does that mean? Some physicists take the transhuman stance, parodied by Donna Haraway (1991) as the God-trick. Here is Sheldon Glashow (1992, 28), co-winner of a Nobel prize in physics with Abdul Salam and Steven Weinberg: "Any intelligent alien anywhere would have come upon the same logical system as we have to explain the structure of protons and the nature of supernovae.''

Glashow does not doubt, as part of his faith, that the alien would, if sufficiently intelligent, have hit on protons and supernovae as something whose structure needs explaining. Perhaps Pickering would query whether a successful alien physics would need to investigate protons, but I want to attend to a different problem. What is ''the same'' logical system? And what exactly does ''logical'' mean here? (We hope that Glashow is not using the word ''logical'' in a merely rhetorical way, without much more content than ''jolly good.'')

Glashow holds that any system of fundamental physics that emerged would in some important sense be equivalent to what we have arrived at (or will arrive at, after resolving remaining anomalies). But what sense is that? His fellow prize winner Steven Weinberg (1996, 14) offers an apparently operational test of equivalence. ''If we ever discover intelligent creatures on some distant planet and translate their scientific works, we will find that we and they have discovered the same laws.'' Weinberg means, of course, the same laws of fundamental physics; those aliens might not even have the same biological make-up as we do, and hence not have hit on the same fundamental biological laws.

Philosophers have troubles with translation. There is Quine's doctrine of the indeterminacy of translation. A reader of Quine, or of Donald Davidson, might agree with Weinberg, but not to Weinberg's satisfaction. We find aliens speaking Alien. How do we know that Alien is a language at all? Only, says Davidson, if we can translate it, by and large, into our language. That requires (argues Davidson) that we assume that aliens share a good many beliefs with us. So we think we have translated the language of these beings only if we have translated their physics into something like ours. Hence translation begs the question of equivalence.

Or to use a thought that Quine used for basic formal logic, we would say that Alien sentences express statements of physics only if they are translatable into something recognizable as our physics. On that view of matters, Weinberg's claim turns out to be an empty tautology.

I have a lot of problems with this use of Quine or Davidson, but I do not see how to turn Weinberg's criterion into a substantive definition of equivalence. Weinberg (1996b, 56) has been more explicit. He says that Maxwell's Equations for electricity and magnetism must be deducible from any sound physics. Does deducibility do the trick?

There are several difficulties, one small, one large, and one curious.

First the small one.[8] World history could have been fundamentally different. Pascal, Leibniz, and above all Charles Babbage had the basic idea of the modern computer that has transformed the late twentieth century. Suppose (what is impossible) that Babbage had got it right in the early nineteenth century. Suppose we had something like massive high-speed Cray computers by 1850. Then the analytical mathematics in which Maxwell's Equations are cast would have been unnecessary. We could have bypassed Maxwell's Equations! On this fanciful hypothesis, it was not absolutely inevitable that physics took a Maxwellian route. Maxwell's equations would not even have been deducible.

Nevertheless, the physicist interjects, the formal structure of the computations done by the imagined Babbage Supercomputer would have in a certain sense conformed to what we call Maxwell's Equations—because that is how the world is. In my opinion, this notion of "conforming to" is even more obscure than the notion that any theory arising would be "equivalent to" Maxwell's, but let that stand.

The big difficulty with deducibility as a criterion of equivalence is at a different level. Figuring out the deductions does not leave everything the same. Weinberg conveys a picture akin to the schoolchild set an exercise in Euclidean geometry. If the child solves the problem, she writes Q. E. D. at the end of her proof. *Quod erat demonstrandum.* In a developing science the *"quod"* is usually not there before the proof. The great figures of what was once called rational mechanics, men like Laplace and Lagrange working around 1800, were in some sense obtaining consequences of Newton's laws of motion and gravitation. But they had to invent the mathematics that would do it. They had to invent the language in which the conclusions could be expressed. They had to articulate the theory. They were not just joining up the dots to complete a picture. They had to put in the dots. I am here only pointing to enormously difficult questions. Deducibility, translatability, and equivalence are not transparent ideas.

The curious difficulty is best stated by another physicist, Richard Feynman. He was discussing three distinct presentations of what we now call the law of gravitation:

> Mathematically each of the three different formulations, Newton's law, the local field theory and the minimum principle, gives exactly the same consequences. What do we do then? You will read in all the books that we cannot decide scientifically on one or the other. That is true.

> They are equivalent scientifically. It is impossible to make a decision, because there is no experimental way to distinguish between them if all the consequences are the same. But psychologically they are very different in two ways. First, philosophically you like them or do not like them; and training is the only way to beat that disease. Second, psychologically they are very different because they are completely unequivalent when you are trying to guess new laws (Feynman 1967, 53).[9]

An older case is the claimed equivalence of Newton's and Leibniz's formulations of the differential calculus; one may argue that it is not just a matter of arbitrary choice that we ended up with the Leibizian vision rather Newton's doctrine of fluxions. A more familiar and modern case is the equivalence of wave and matrix mechanics. Long ago, Norwood Russell Hanson (1961) drew attention to the ways in which formal proven equivalence may still allow of different uses, goals, and understanding of different "formulations." These matters do not strike most physicists as troubling, but they do deeply perplex the historian. One of the things that happens, in the evolution of a science, is that functionally nonequivalent systems become, are made, equivalent, and all traces of the former nonequivalence are obliterated.

Philosophers have been a little more cautious than some physicists in formulating what Bernard Williams calls "an absolute conception of the world." Williams's project has, however, mainly been to draw a contrast between scientific and moral reasoning. In the course of making that distinction he writes that "In a scientific inquiry there should ideally be convergence on an answer, where the best explanation of the convergence involves the idea that the answer represents how things are" (Williams 1985, 136).[10] We could explain "convergence on an answer" in three distinct ways: small-scale, big-scale, and unique-ultimate. These distinctions are not germane to the point Williams was making, for he wanted to separate ethics and science. In ethics, even if there were convergence on an answer or answers to fundamental moral dilemmas, the best explanation for that convergence would not be that the answer represents how things are. But since someone might invoke Williams's absolute conception for the science wars, we should make more clear what could be meant by convergence.

Small-scale convergence—of course! In the sciences we converge on answers all the time, whenever we obtain robust fits between theory, phenomenology, models of the apparatus, and apparatus. There is nothing ideal about that. It is a regular achievement. What about the best explanation of getting a robust fit? "That represents how things are"? That is not helpful if someone really wanted to know why theory, experiment, and apparatus fitted together, but such an answer, if someone were to give it, does not challenge contingency. Pickering claims that "how things are" does not uniquely predetermine which robust fits are achieved, from day to day. Williams gives no reason to disagree.

Big-scale convergence: Williams shows in context that he has in mind not little real-life answers to scientific questions, but something more in the grand scheme of things. By a big answer does he mean that science should converge on *an* answer, or that there is one and only one answer upon which we could converge? If he meant only *an* answer, then the notion of contingency is altogether consistent with Williams's absolute conception of the world.

Unique-ultimate: perhaps Williams wanted us not to think of *an* answer upon which inquiry should converge. Perhaps he meant that, ideally, there is only one answer upon which we could converge, if we were to converge. Glashow (1992: 28) expresses the idea more poetically. He holds that there are "eternal, objective, ahistorical, socially neutral, external and universal truths, and that the assemblage of these truths is what we call physical science." He did not exactly say that there is just one such assemblage, but we are pretty sure that is what he intended.

Formally speaking, the contingency thesis is entirely consistent with the ultimate one-and-only picture upon which inquiry in the physical sciences will converge. For there could be many roads to the one true ultimate theory, or none at all. If there were many roads, then the physics at each way station on each road would be different from the physics at way stations on every other road.[11] Once again, Williams's absolute conception of the world does not really cross the contingency thesis. This is hardly surprising, for Williams had a different motivation, namely to state a fundamental principle to distinguish science from ethics.[12]

The Sticking Point

The constructionist maintains a *contingency thesis.* In the case of physics, (a) physics (theoretical, experimental, material) could have developed in, for example, a nonquarky way, and, by the detailed standards that would have evolved with this alternative physics, could have been as successful as recent physics has been by *its* detailed standards. Moreover, (b) there is no sense in which this imagined alternative physics would be equivalent to present physics. The physicist denies that. Physicists are inclined to say, put up or shut up. Show us an alternative development. They ignore or reject Pickering's discussion of the continued viability of the old physics.

The sticking point need not be at quarks. But some things definitely are noncontingent, say the physicists, and their appearance in physics was inevitable if the science was to progress at all. When the physicist's sticking point is placed under severe challenge, there are several fall-back examples: Maxwell's Equations, the Second Law of Thermodynamics, the velocity of light. The contingency claim is that neither the law nor the equations nor the velocity (nor anything equivalent) are inevitable parts of any science as successful as present science.

In ordinary philosophical language, necessity is the contrary of contingency. But it would be confusing to call the physicists who oppose the contingency thesis "necessitarians." My physicist protagonists are *inevitabilists.* They do not think that the progress of physics was inevitable (we could have stayed with Zen). They do think that *if* successful physics took place, *then* it would inevitably have happened in something like our way.

Truly metaphysical issues do not yet arise. Strictly speaking, the contingency thesis is formally consistent with any metaphysics. Perhaps that is irrelevant: we do not want to speak strictly and formally in this connection. This is because metaphysics must arise from a certain sense of ourselves in the world. And a sturdy sense of reality—is that not metaphysical?—may find the contingency thesis altogether repugnant. We don't live in the kind of world in which the contingency thesis could be true! That is no empirical exclamation, derived by inference from experience. It is, if not a built-in sensibility, a sensibility that arises in a great many people in Western civilization who are attracted to scientific styles of reasoning. If that is what you mean by metaphysics, then metaphysics is appalled at the very thought of contingency. I shall turn to that kind of metaphysics at sticking point #2. I take it seriously.

When we turn to the metaphysics of the schools, the contingency thesis appears to be consistent with any standard metaphysics. (So much the worse for the standards and the schools, you may say.) For example, contingency is consistent with the scholastic debating point of the 1980s called "scientific realism." Many versions of that doctrine state that physics aims at the truth, and if it succeeds, it tells the truth. If the physics refers to some type of unobservable entity, then, if the physics is true, entities of that type exist. Many social students of science reject any version of scientific realism. So do many philosophers, such as Bas van Fraassen (1980). But the contingency thesis itself is perfectly consistent with such scientific realism, and indeed anti-realists, such as van Fraassen, might dislike the contingency thesis wholeheartedly. Pickering (1995a, 171) has become so mellow that he says he is agnostic about what he calls correspondence realism. He is right. Scientific realism simply does not matter to what he cares about, namely contingency.

STICKING POINT #2: NOMINALISM

High-level semantical words like "fact," "real," "true," and "knowledge" are tricky. Their definitions, being prone to vicious circles, embarrass the makers of dictionaries. These words work at a different level from that of words for ideas or words for objects. For brevity I have called them elevator words. They are used to say something about what we say about the world. Facts, truths, knowledge, and reality are not in the world like protozoa, or being in love. Philosophers keep on fussing with them. Theories of truth and theories of knowledge produce endless books. From the early nineteenth century until the 1930s, epistemology was king. More recently, theories of truth have ruled the roost. It would be feckless to address such mighty topics here, for one is not going to make any quick progress. My plan is to change the vocabulary slightly, so that we do not go on saying exactly the same unfocused things. But first, facts.

Facts

Latour and Woolgar's *Laboratory Life* was originally subtitled, *The Social Construction of Scientific Facts.* It centers on a discovery in endocrinology. Latour had, as an ethnographer, studied one of the two laboratories in which the work was done. Many scientists believe that this book, and Latour's later *Science in Action,* demean their work and treat serious activity as a matter of personal aggrandizement and network building. I shall discuss that reaction briefly towards the end of this chapter. Here let us think about facts.

First a warning. Although I use Latour to introduce a discussion about facts, that is hardly the core of his subsequent work. He has recently been very clear about the center of gravity of his kind of science studies. "Instead of ideas, thoughts, and scientific minds," he writes in the preface to the new French pocket-book edition of *Science in Action,* "one recovers practices, bodies, places, groups, instruments, objects, nodes, networks" (1996, 14). In *Laboratory Life* there was a great deal of emphasis on one type of entity: inscriptions. Indeed we were told that the main products of a laboratory are inscriptions—preprints, graphs, traces, photographs, published papers, and now e-mail. *Science in Action* has, happily, a much more material vision of science.

Latour and Woolgar briefly emphasized etymology. The word "fact" comes from the Latin *factum,* a noun derived from the past participle of *facere,* to do, or to make. Facts, they said, are made. Since made things exist, Latour and Woolgar (1986, 180) did "not wish to say that facts do not exist nor that there is no such thing as reality." Their point was "that 'out-there-ness' is the *consequence* of scientific work rather than its cause." And: " 'reality' cannot be used to explain why a statement becomes a fact."

Philosophical purists like myself feel uncomfortable about statements "becoming" facts. Statements state facts, and scientific facts do not come into being. If they are facts, expressed by tenseless sentences, then they are facts, timelessly, and do not "become." I doubt that ordinary people are so uptight about the timeless character of facts and truths as philosophers. In Chapter 7 I quote Humphry Davy (1812, 3), that master of so many scientific trades, who talks about how, after rigorous testing, a conjecture "becomes scientific truth."

Analytic philosophers do have the strongest inclination to say that facts discovered in the natural sciences are tenseless and timeless ("eternal" as Glashow put it). That's harmless, unless we grant a peculiar explanatory power to these abstractions. Latour and Woolgar were surely right. We should not *explain* why some people believe *p* by saying that *p* is true, or corresponds to a fact, or the facts.[13] For example: someone believes that the universe began with what for brevity we call a big bang. A host of reasons now support this belief. But after you have listed all the reasons, you should not add, as if it were an additional reason for believing in the big bang, "and it is true that the universe began with a big bang." Or, "and it is a fact." This observation has nothing peculiarly to do with social construction. It could equally well have been advanced by an old-fashioned philosopher of language. It is a remark about the grammar of the verb, "to explain."

We need to be careful with words here and not confuse the philosophical idea of "correspondence" with quite ordinary and unexceptionable ways of talking. Someone may come to believe a hypothesis because "it fits the facts." The ordinary word "fits" does not mean the abstruse "corresponds to." We mean that some puzzling facts need explanation, and such and such a hypothesis is palatable, nay plausible, just because it jibes with or even explains those puzzling facts. To continue the example: the big bang theory was widely accepted in 1973, when it was seen to fit the newly discovered facts about uniform background radiation in the universe. Indeed some people came to believe the theory just because it fit the newly discovered facts. That explains why they changed their minds. But we should not explain why some people believe *p,* by saying that they do so because *p* is true, or corresponds to a fact, or the facts. When stated so cautiously, this conclusion about truth and explanation is not challenging. Anyone antagonistic to both the letter and spirit of constructionism could still agree that the truth of a scientific proposition in no way *explains* why people maintain, hold, believe, or assent to that proposition.

Nominalism

So what's the problem? A very old one: a contemporary version of an ancient debate between two metaphysical pictures of the relationship between thought and the world. Sticking point #2 is *nominalism.* There is a big danger in using a philosophical label that has been tossed around ever since Columbus sighted land in the Caribbean. (A philosophical dictionary says that the name "nominalism" came into circulation in 1492). Those who know the word will already understand it in their own way, while those to whom the word is unfamiliar, or for whom it merely reeks of tired old philosophy, will not want even to hear the syllables pronounced. Nevertheless it is part of my argument that the present science wars, especially as they hook up with social construction, have strong resonances with traditional philosophical issues.

Nominalism is a fancy way of saying name-ism. The most extreme name-ist holds that there is nothing peculiar to the items picked out by a common name such as "Douglas fir," except that those items are called Douglas fir. And the same goes for all names whatsoever. (The Douglas fir is a species of tree in the rainforest of the Northwest coast of North America, not a true fir at all, but named in honor of a British governor of British Columbia named Douglas. When the wood is sawed up into planks and unloaded at an English port, the English, who are inveterate nominalists, then call it pine.)

An unpleasant metaphor has been much used, in recent times, in this connection. People quote Socrates out of context and speak of carving nature at the joints. The Douglas fir, they say, is one of the joints of nature, at least in coastal British Columbia. Nominalists deny that nature has joints to be carved. Their opponents contend that good names, good accounts of nature, carve nature herself at her joints.

Rather than rehearse some history of philosophy, I shall try for a contemporary version of old issues of nominalism, tailored for questions about the natural sciences. Allow me two slightly romantic-sounding formulae. I want to convey the spirit of the division.

One party hopes that the world may, of its own nature, be structured in the ways in which we describe it. Even if we have not got things right, it is at least possible that the world is so structured. The whole point of inquiry is to find out about the world. The facts are there, arranged as they are, no matter how we describe them. To think otherwise is not to respect the universe but to suffer from hubris, to exalt that pip-squeak, the human mind.

The other party says it has an even deeper respect for the world. The world is so autonomous, so much to itself, that it does not even have what we call structure in itself. We make our puny representations of this world, but all the structure of which we can conceive lies within our representations. They are subject to severe constraints, of course. We have expectations of our interactions with the material world, and when they are not fulfilled, we do not lie about it, to ourselves or anyone else. In the fairly public domain of science, the cunning of apparatus and the genius of theory serve to keep us fairly honest.

What to call these two sides? I am content to say that the second party is nominalist. What about the first party? "Realism" once named the opposite of nominalism, but the word now means a lot of things, even in technical philosophy. One philosopher, preoccupied by issues raised by Michael Dummett, tells me that nobody nowadays uses "realism" as the opposite of nominalism. So I will take a name that because of its ugliness noone else will use, and speak of *inherent-structurism.* I suppose that most scientists believe that the world comes with an inherent structure, which it is their task to discover.[14]

The Sticking Point

The nominalist hopes only to be true to experience and interaction. The scientific nominalist is the more self-demanding, having to be true to the way in which apparatus does not work, having to accommodate, constantly, to the resistance of the material world. Nominalists are far more radical than the philosophers called anti-realists, who are skeptical about or agnostic about the unobservable entities postulated by theoretical sciences. Nominalists are not concerned with observability. They are as cautious about the needles of a fir tree as they are about electrons, when it comes to the inherent structure of the world.

Every person will describe the roles of these two different metaphysical pictures in different ways. I have tried to give a fair rhetorical shake to both. Various people have said somewhere or other that everyone is born either an Aristotelian or a Platonist.[15] Here, then, is an old and irresoluble ghost lurking behind much of the current folderol about social construction. The schoolmen named it nominalism, but they did not invent that cast of mind.

STICKING POINT #3: EXPLANATIONS OF STABILITY

It is striking how often Maxwell's Equations and the Second Law of Thermodynamics appear in debate, as if they were the last bastions of besieged scientists. They are said to be as real as rocks (I take on rocks in Chapter 7). One reason that they are so effective in argument is that they nicely move up and down in the trio of objects, ideas, and elevator words. They are like *objects:* they are in the world, are they not? If anything is "in the world," says the scientist, it is the Second Law and Maxwell's Equations. But the Law and Equations are also truly profound *ideas:* at the previous turn-of-the-century celebrations, the great American philosopher of science, and founder of pragmatism, Charles Sanders Peirce, said that the Second Law was the crowning intellectual achievement of the nineteenth century. In his famous lecture, *Two Cultures,* C. P. Snow asserted that every humanist should know the Second Law as a minimum literacy requirement. And finally, are not the Law and the Equations *facts?* And of course they are *knowledge.* Finally, they are *real,* "as real as anything else we know" (Weinberg 1996a, 14). The Law and the Equations are wonderfully fitted for rhetoric.

There is a more ordinary, and more important fact about the Second Law or Maxwell's Equations: they are not going to go away. And yet they could, in two ways. One, the universe itself could change (but we would not be here to witness that impossible cataclysm, for the human body is too frail to survive). Or the Law and the Equations would go away if we found out that they are false. That would be some scientific revolution.

The early years of the twentieth century witnessed many profound changes in physics: the theories of relativity; the quantum theories. Philosophers picked up on these novelties. Karl Popper taught that the sciences are in a permanent dialectic of conjecture and refutation. The best theories are the falsifiable ones. Thomas Kuhn took that one step further. He argued that the sciences pass through stages of radical change, followed by some transient stability he called "normal science." He even wrote of the necessity of scientific revolutions.

Future historians of the history and philosophy of the sciences may suggest that Popper and Kuhn worked in unusual times. Events early in the twentieth century made them think that science is essentially unstable. From now on (it is already being said) future large-scale instability seems quite unlikely. We will witness radical developments at present unforeseen. But what we have may persist, modified and built upon. The old idea that the sciences are cumulative may reign once more. Between 1962 (when Kuhn published *Structure*) and the late 1980s, the problem for philosophers of science was to understand revolution. Now the problem is to understand stability.

Stability has to be stated with caution and humility. Scientists have not become infallible, nor do they pretend to be. But there is the sentiment that a lot of science is here to stay. This is elegantly expressed by Steven Weinberg (1996a, 14) writing about Maxwell's Equations. I shall divide his statement into two parts, to be labeled [A] and [B]. [A] is the uncontroversial data. [B] represents what we are supposed to learn from the data. [B] shows that our classification by sticking points is quite useful. [B], which seems to be one point, is in fact several. Two of the points are versions of sticking points #1 and #2, while [B] also directs us to a third sticking point.

> [A]. None of the laws of physics known today (with the possible exception of the general principles of quantum mechanics) are exactly and universally valid. Nevertheless many of them have settled down to a final form, valid in certain known circumstances. The equations of electricity and magnetism that are today known as Maxwell's equations are not the equations originally written down by Maxwell; they are equations that physicists settled on after subsequent work by other physicists, notably the English scientist [and engineer] Oliver Heaviside. They are understood today to be an approximation that is valid for a limited context . . . but in this form and in this limited context they have survived for a century and may be expected to survive indefinitely.

Thus far, no one should take issue with one word of this statement. But we are perilously close to a host of issues.

Culture and Science

Norton Wise (1996, 55), the historian of nineteenth-century physics, did not take exception to what [A] stated but to one of the messages [A] intended to convey, namely, that Maxwell's Equations have nothing to do with human culture. They are just facts that we run up against. Wise argued that culture and science are inseparable. The Equations came "from the work of some of the most deeply religious people who have ever contemplated a battery: Oersted, discoverer of electromagnetism and author of *The Soul in Nature;* Faraday, devout member of the Sandemanian sect, who discovered electromagnetic induction and articulated field theory; William Thomson (Lord Kelvin) and Maxwell . . ." Other scholars would emphasize the role of empire, of the laying of telegraph cables below the seas, and across Persia to India, all of which had high priority in the minds of Kelvin, Heaviside, and Maxwell himself.

Weinberg (1996b, 56) retorted that "Whatever cultural influences went into the discovery of Maxwell's Equations and other laws of nature have been refined away, like slag from ore." The British Empire and Sandemanianism are mere curiosities of bygone days, perhaps still casting their shadows in the worlds of politics and piety, but not in the natural sciences.

The same sort of debate arises for the Second Law of Thermodynamics. Chapter 2 quoted Max Perutz (1996, 69) saying that the law is "an inexorable law of nature based on the atomic constitution of matter," which states that "heat cannot be transferred from a cold to a warm body without performing work." Perutz is one of the handful of people who created molecular biology (Nobel prize shared with John Kendrew for ribonucleic acid, or RNA, 1962). One of his later achievements was the structure of hemoglobin. Hemoglobin with its structure, he would surely say, is not a social construct. It is a fact of life, life itself, our lives. The history of its discovery, the history of Bragg's X-ray crystallography and later events, is a social history of science, including Perutz's leaving Nazi Vienna for England, his post-war collaboration with Kendrew, a British wartime physicist looking for greener pastures, and so forth. But hemoglobin is not a product of that history; it was there even before the emergence of the human race. Put that way, it sounds as if Perutz has to be right!

Some constructionists retort, in connection with the Second Law of Thermodynamics, "we have done the history, you scientists have not." (A less modest man than Perutz might say, I and a few others are the history of the discovery of the structure of hemoglobin, what do you mean, I have not done it?) But that does not really begin the debate, for the scientist says, the history, construction-as-process, does not matter. Yes, thermodynamics takes its name from the thermodynamic engine—the old name for the steam engine. Thermodynamics is vested in that ingenious centerpiece of the industrial revolution and wage capitalism. But the content of the Second Law, what it now means, is independent of its history. The Second Law still uses the concept of "work," which betrays its industrial origins, but that has no consequences for any present use of the Second Law.

Norton Wise made valuable points in criticism of Weinberg, but on this issue the scientists seem to win the day. Maxwell's Equations and the Second Law bear none of their history about them. The only possible case to make against the scientist's firm sense of timelessness is about the form, rather than the content, of electromagnetic theory. The very set of questions that led us to the Second Law or the Equations were formed by certain directions set by religion, empire, and industry. Given the questions, the content of the Law and the Equations was developed and became free of its history. But, it might be protested, the "form" of this kind of knowledge was historically determined, with great consequences for what we have found out. Not that what we have found out is false, but that the entire set of possible questions and answers in terms of which we think was only one option. The very form of what we have found out is not so free of history as scientists imagine. That is an interesting but very obscure idea. I try to explore it in Chapter 6 below. No one has shown how it would apply to the Law or the Equations. Lacking such an argument, we have to regard those icons, in their present form, as independent of their history.

A Big Jump

Steven Weinberg's passage [A] (read strictly and literally) does not bear on sticking point #1. [A] does not entail that physics had to develop along modern Maxwellian lines if it was to be successful (inevitabilism). [A] does not bear on sticking point #2. It does not entail that Maxwell's Equations are part of the inherent structure of the world. [A] says that the Equations have become stable and can be expected to "survive." Physicists will continue to accept them, use them, believe them, take them as favorite paragons of scientific knowledge. So far nothing controversial. Let us see what comes next. The paragraph continues:

> [B]. That is the sort of law of physics that I think corresponds to something as real as anything else we know. On this point scientists like Sokal and myself are apparently in clear disagreement with some of those whom Sokal satirizes. The objective nature of scientific knowledge has been denied by Andrew Ross and Bruno Latour and (as I understand them) the influential philosopher Richard Rorty and the late Thomas Kuhn, but it is taken for granted by most natural scientists.

With [B] we are seamlessly moved up by the elevator, with words such as "real," and "objective," and "knowledge." Those words ("As real as anything else we know") did not occur in [A]. Weinberg (1996b, 56) emphasized this point in a reply to criticisms: "I tried in my article to put my finger on precisely what divides me and many other scientists from cultural and historical relativists by saying that the issue is not the belief in objective reality itself, but the belief in the reality of laws of nature."

"As real as anything else we know": such words also spring naturally from the mouths of mathematicians doing number theory. The theorems are as real as anything we know. That means, first, as irresistible, as "inexorable" (Perutz's word for the Second Law of Thermodynamics) as anything we know. If you are going to think about these things at all, you are going to get *here,* to Maxwell's Equations, and also to the fact there is no greatest prime number and, late in the day, to Fermat's last theorem. That is an inevitability thesis, sticking point #1. Weinberg confirms this reading: "One of the things about laws of nature like Maxwell's equations that convinces me of their objective reality is the absence of a multiplicity of valid laws governing the same phenomena . . ."[16] Less graciously, contingentists who imagine an alternative successful science should put up or shut up.

As real as anything else we know. People who have never *experienced* a mathematical proof (the feeling of, as Wittgenstein put it, "the hardness of the logical must") seldom grasp what Platonistic mathematicians are on about. The sheer inexorability of mathematical proof has persuaded provers that the numbers and their properties are as real as, or even more real than, anything else we know. A physicist may have a similar experience in connection with Maxwell's equations. It is not that we have a lot of evidence that the Equations hold in certain domains. Yes, we have that, but that is not what gives the overpowering conviction that this is how things are, indeed, how things have to be. Weinberg is giving vent to this conviction, one of the deepest that a reflective human being can ever experience. Where we get to from that is to an inherent-structure thesis. When Weinberg states that Maxwell's Equations are as real as anything he knows, he means, among other things, that they are part of the inherent structure of the world. That takes us back to sticking point #2.

Thus [A] is uncontroversial, but it leads Weinberg to [B], which turns out to involve two distinct stances, both of which we have encountered already, namely our first two sticking points, contingency and nominalism. But before examining a third sticking point, let us look at one of the named figures with whom Weinberg disagrees, namely Thomas Kuhn. If taken at his word, he would surely doubt [A]. For he wrote of the necessity of scientific revolutions. He thought that a science could not remain lively unless from time to time it was shaken up by revolution. This is a very different perspective from Weinberg's. Norton Wise drew attention to Weinberg's astounding statement that "as far as culture or philosophy is concerned the difference between Newton's and Einstein's theories of gravitation, or between classical and quantum mechanics is immaterial" (as if Kant were no figure in culture or philosophy). One can think of Kuhn and Weinberg looking down a spyglass in opposite directions. Kuhn magnifies tumultuous events in the sciences. Weinberg makes them minuscule in the grand scheme of things. But this is not the immediate point at which Weinberg takes issue with Kuhn.

In *Structure* Kuhn rejected the idea of scientific progress towards some one final vision of the world. What we see in the history of science is progress away from previous beliefs. Weinberg (1996b, 56) quotes from some of Kuhn's late writings, where Kuhn had said "it's hard to imagine . . . what the phrase 'closer to the truth' can mean." Kuhn (like Nelson Goodman, who calls himself an irrealist) went on to make plain that he did not think there is a reality which science fails to get at. The notion of reality is, on the contrary, idle. Weinberg disagrees. Here we seem to have moved back to sticking point #2. Kuhn was a nominalist, and Weinberg is an inherent-structurist.

I have just made an observation about Weinberg and Kuhn which is intended to respect both. Weinberg said he was trying to put his finger on differences between "cultural and historical relativists" on the one hand, and physicists like himself. He writes as if he is putting his finger on some ephemeral debate that has flourished these thirty years or so. I suggest his finger points at a pair of attitudes that have opposed each other for at least 2300 years. My "sticking-point" analysis is intended to emphasize that this is not the first time that deeply committed and honest persons have, well, stuck. There is also a further point at which they have stuck: the sources of stability.

External Explanations

Historians, philosophers, and sociologists of science have advanced all manner of explanations for the acceptance and persistence of a body of scientific belief and practice. Latour's work (singled out for mention by Weinberg in [B]) has emphasized the network of events and agents that lies behind an item of knowledge. If you doubt the item, you have to challenge endless other items with which it is linked, challenge an expanding host of authorities, undo a net of thousands of directly or indirectly cited experts and results. The Edinburgh school began by emphasizing the interests of scientific workers, which directed their research and molded their conclusions.

Here we move to questions of evidence and reason. Why is the Edinburgh school said to favor social construction? Because instead of reasons for belief, it offers social explanations for belief. If we took the metaphor of "construction" literally, we could hardly call the Edinburgh school constructionist, but they certainly emphasize the social. Latour, while saying more about how construction is done, de-emphasizes the word "social," saying we have never been modern, never in fact separated the social from the natural. To the uncommitted, all such writers emphasize factors in science which strike one as *external* to the content of the sciences they describe.

That is part of what Weinberg, in quotation [B], finds lacking in Latour. Does Latour deny "the objective nature of scientific knowledge"? Yes (for Weinberg), because Latour thinks that external factors are highly relevant to the stabilization of some beliefs as knowledge. Perhaps even the ultimate stabilization, the persistent survival of Maxwell's Equations. And it will do no good for a partisan of Latour to respond that of course he doesn't deny the objective nature of scientific knowledge. Latour has explicitly written even in the first book (Latour and Woolgar 1979) that he and his collaborators do not deny reality, facts, and (adds the partisan) "the reality of laws of nature." All such protests are in vain at the tribunal of the physicist, because Latour thinks that external factors are relevant to the stability of laws of nature, while Weinberg thinks they are irrelevant. That is the nub. That is sticking point #3: external explanations of scientific stability.

Rationalism and Empiricism

This sounds like a recent and ephemeral dust-up. But it is probably analogous to some versions of the opposition between empiricists and rationalists. Indeed, you can even cast historical debates between, for example, Locke and Leibniz in terms of external and internal. Leibniz thinks that the reasons underlying truths are internal to those truths, while Locke holds that (our confidence in) truths about the world is always external, never grounded in more than our experience.

I shall not push the analogy further. Alan Nelson (1994), like myself making heavy use of Latour and Pickering, wrote of what he called constructivists versus rationalists. Rationalists think that most science proceeds as it does in the light of the good reasons produced by research. Some bodies of knowledge become stable because of the wealth of good theoretical and experimental reasons that can be adduced for them. Constructivists think that reasons are not decisive for the course of science. Nelson concludes that this issue will never be decided. Rationalists, at least retrospectively, can always adduce reasons that satisfy *them.* Constructivists, with equal ingenuity, can always find to their own satisfaction an openness where the upshot of research is settled by something other than reason. Something external. That is one way of saying that we have found an irresoluble "sticking point." Nelson is right to use the word "rationalist" to name one side, drawing attention to a lineage. One mark of these two traditionally named attitudes is that the one favors internal understandings of what knowledge is, while the empiricist favors external explanations.

The Sticking Point

The constructionist holds that explanations for the stability of scientific belief involve, at least in part, elements that are external to the professed content of the science. These elements typically include social factors, interests, networks, or however they be described. Opponents hold that whatever be the context of discovery, the explanation of stability is internal to the science itself.

ANTI-AUTHORITY BY UNMASKING

My three sticking points are intellectual, philosophical, in the best senses of those words. But some other problematic points are less philosophical, and they play to the emotions more than the intellect. They are not irresolvable sticking points but, one might say, sticky points that provoke anger more often than debate.

For example, ever since Freud, at least, it has been a common piece of rhetoric to "diagnose" what really troubles your opponent. That makes for bad argument. Even if your opponents are positively ill, they may have reasons worth considering or positions worth acknowledging. Nietzsche went mad, but those who ignore what he wrote before his madness do so at their peril. When it comes to argument, I am loath to diagnose. But I will run through one common diagnosis of the science wars, not much removed from Dorothy Nelkin's assessment quoted at the start of this chapter. It is observed that famous American physicists lead one of the fronts, damning, among other things, the very notion of social construction. (In Great Britain it is not physicists so much as life scientists: Richard Dawkins, Max Perutz, and Lewis Wolpert.) Why would physicists be especially embattled?

It has long been common to distinguish two main branches of physics, high-energy physics and what used to be called solid-state physics, now called by the more abstruse name of condensed-matter physics. Most of the advances that have affected our daily lives are the product of solid-state physics, even at the level of quartz watches, liquid display crystals, and lasers that run my compact disc player and helped fix my eyes after I had gone blind. But ever since World War II, when one read in the newspaper or saw on television some striking story about physics, chances are it was high-energy physics.

For some fifty years high-energy physics was the queen of the sciences, fully funded thanks to supposed military applications that began with the atomic bomb. But with the end of the Cold War, the financing of high-energy physics was abruptly curtailed. For quite independent reasons, the new queen of the sciences became molecular biology. And suddenly solid-state people, ignored by the public for so long, are about to take the driver's seat, partly because of the richness of the applications of their fundamental research. New PhDs in high-energy physics cannot get jobs, and go to work on Wall Street (systems analysis at Goldman Sachs turns out to be not so different from work on very small particles). Even when the new solid-state PhDs cannot get academic research work, they are in demand by industry, especially by start-up companies where the risks are high, but where the profits may be immense.

That is where rhetoric enters. The high-energy physicists (it is argued) are unnerved by their sudden fall from favor. That is why they are kicking up a fuss about social construction and anti-scientism in general![17] In my opinion, even if this were true, it would leave untouched the important question of whether the fuss is well founded. It does, of course, help explain the timing of the fuss.

It is important to consider other factors besides the proportions of national treasuries spent on different kinds of basic research. Money helps, but self-esteem and the respect of others are far more important to living a life. High-energy physicists have to some extent lost their cultural authority. By this I mean not just the ability to command vast resources of money and talent, but also the conviction that their life work is deemed to be profoundly significant not only by their peers but also by their culture, or world culture at large.[18] Great poets mired in poverty may have cultural authority without patrons. Today molecular biology, biological medicine, brain science, and even computer science (despite talk of nerds) have far more cultural authority than physics.

These observations about cultural authority are important to sociologists of debate, but they leave untouched the issue of whether the high-energy physicists and cosmologists are right in their contentions against social constructionists. That is why I spent such a long time distinguishing three rather ancient and philosophical sticking points. But I cannot pretend that we should discuss only issues of high metaphysics.

In Chapter 1 I distinguished some six grades of constructionist commitment. Pickering is not an activist trying to abolish the idea of quarks and give us something better. He does not even want to reform the standard model, or gauge theory, except that, in the spirit of anyone trained in the field, he would like to improve it. Pickering, on my account so far, comes out as an ironist about quarks. Latour and Woolgar look like ironists about their tripeptide, Thyrotropin Releasing Hormone. So why are they taken to be critics of science, when at least Latour has gone out of his way to make a sardonic crack to the effect that, like every good Frenchman, he is filled with admiration for the achievements of science? Because there is a strong element of unmasking in the work of many constructionists.

Their target is not the truth of propositions received in the sciences, but an exalted image of what science is up to, or the authority claimed by scientists for the work that they do. I briefly explained Mannheim's idea of unmasking in Chapter 2. "The unmasking turn of mind" does not try to refute ideas, but to harm them by exhibiting their "extra-theoretical function." Constructionists believe that there is an extra-theoretical function for inevitabilism, inherent-structurism, and the rejection of external explanations of the stability of the sciences. These three serve an ideology of science, in something like the sense intended by Mannheim. They serve the world outlook of a certain social stratum, that of scientists who present themselves as the deepest probers of the universe, discoverers of ultimate truths.[19]

That social stratum does not include the broad mass of scientists, pure and applied, who tend to be a modest lot. Most scientists are fairly humble about their work, which they gladly admit is a string of tentative conjectures, temperamental apparatus, and nervous results. But when they, or the elder statesmen of science, look on the entire activity, a note of authority creeps in. Science has found out, by and large, how things are (we are told), how they must be, in the present state of things. There will be deeper accounts not yet discovered, but present science is, overall, as deep as it gets right now. Constructionists urge that this ideology has an extra-theoretical function: ensuring the cultural authority of science. The received wisdom is that scientists must not be challenged, because they are the deep probers of the inner constitution of things.

Thus what is to be unmasked is both a vision of underlying reality revealed by physics, and the associated claims to profundity of the entire endeavor. Here we have an acrimonious contretemps. Scientists feel deeply hurt, they feel that social constructionists do not take them seriously. It is no use social constructionists trying to cheer everyone up, saying that they love physics, but not for the wrong reasons. The wound has been inflicted.

This contretemps hangs together with the three sticking points. For example, the most knock-down defense of authority has always been metaphysics. The divine right of kings, taken more seriously than we can conceive of today, is a nifty way to ensure the authority of the sovereign. Constructionists want to unmask metaphysics as a bolster for the authority of the sciences. They also want to show that the present state of science was not the only inevitable upshot of dedicated inquiry into the material world that surrounds us. We achieve a robust fit between theories and apparatus, but the fit that we achieve is not the only one we might have arrived at. Contingency also undermines authority, not in the sense of casting doubt upon the propositions received in the sciences, but in the sense of challenging their claim to an unparalleled profundity. And finally, the survival of Maxwell's Equations is not to be explained only by factors internal to electromagnetism, quantum electrodynamics, and cosmology.

LEFT AND RIGHT POLITICS

A heartfelt ethical issue also arises. The traditional right/left spectrum of politics and alliances has run into problems. Although I did not find Sokal's spoof as interesting as most of its readers did, he did raise one genuine issue.[20] He lamented that he, as scientist, identifies himself as someone on the left, in support of the oppressed, while the mantle of the left has been stolen by people who write "theory," among whom he might count the authors of *Constructing Quarks* or *Laboratory Life.*

In terms of the unmasking of established order, constructionists are properly put on the left. Their political attitude is nevertheless very much not in harmony with those scientists who see themselves as allies of the oppressed, but also feel like the special guardians of the most important truths about the world, the true bastions of objectivity. The scientists insist that in the end, objectivity has been the last support of the weak. Here is a disagreement. It is a rather messy matter, a sticky point involving deep-seated but ill-expressed attitudes. Who is on the left?

I take this question very seriously, for I am deeply sympathetic to both sides. Some years ago, after a talk of mine about verisimilitude a freedom fighter of days gone by insisted on the extent to which objective truth is called for, as a virtue, when one is fighting tyranny. The enemy always tries to steal it (*Pravda* and *Trud* were once newspapers named after the noblest ideal, truth). The villains never could get away with that, so long as the last words are: "that simply is not true, liar!" My fighter would have hated those who want to unmask the values of truth, reality, and fact. They want, as he sees it, to remove the last ledge upon which freedom and justice can stand. I saw what he meant, and feel humble towards a man who really worked for the liberation of his people.

Nevertheless a serious issue is joined. Feminists feel most strongly that they well know about oppression. Left/right: what did that mean except an array of men in the French National Assembly! Forget it. They 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.

I have nothing to contribute to this debate, precisely because I am torn. Perhaps it is a generational thing. The values of that freedom fighter are part of my values, and they are values, in his generation, of one standpoint, in the end. But I also grasp the force of some of the critique, and am unable to synthesize my inclinations. I invite others to confess to these difficulties, and to refrain from dogmatism.

KUHN AND FEYERABEND

We cannot leave the sciences without mentioning these two eminences. Most people would guess that the flamboyant anarchist, Paul Feyerabend, was more of a constructionist than that somber revolutionary, Thomas Kuhn. I find the opposite. We now have a check list to see how constructionist each author is. #1 Contingency; #2 Nominalism; #3 Stability. Let us use it.

Kuhn did not mention social construction in his 1962 masterpiece, *The Structure of Scientific Revolutions.* The words were not common parlance until after Berger and Luckman's *The Social Construction of Reality* appeared in 1966. In one chapter he argued that progress in science is "away from" past science, rather than "toward" a right account of an aspect of the world. That is an exceptionally strong contingency thesis. Every revolution is contingent. Nothing determined that one ought to go the way one did. Normal science, in contrast, proceeds in a rather inevitable way. Certain problems are set up, certain ways for solving them are established. What works is determined by the way the world collaborates or resists. A few anomalies are bound to persist, eventually throwing a science into crisis, followed by a new revolution.

Kuhn's normal science follows its ordained route. Given the way the world is and the questions posed by normal science, and the achievement (the paradigm) on which a normal science models itself, the upshot of inquiry is rather inevitable. We are going to get the anomalies that will lead us to a new sense of crisis. (Pickering is far more radical than Kuhn! His account of what I call the contingency of robust fit is all in the realm of normal science.) But the aftermath of revolution, the new paradigm that shines ahead, is entirely contingent.[21] Nothing determines the upshot of crisis. That radical contingency generated the storm that greeted Kuhn's book in 1962.

Kuhn was also a nominalist. This has not excited much interest even among philosophers of the sciences; following Kuhn's publication, they got caught up in drab little case histories and debated questions about when a change in theory is rational. Kuhn himself definitely checks off at sticking point #2 as a nominalist.

Kuhn already entered into the discussion of sticking point #3. One suspects that if he did admit any stability in science, he would explain it at least partly on external grounds. And of course he would be dubious about any permanent stability in any active science.

Kuhn did a great deal to undermine the ideology of science. He did not deliberately write as an unmasker, trying to expose false authority. Kuhn the person was quite well disposed to authority. But Kuhn the book had the effect of unmasking the authority of science in a quite remarkable way. On the one hand we got normal science as puzzle-solving. Most of the time scientists do not probe the deep structure of the universe. They engage in a superior sort of crossword-puzzle activity. What a put-down! The moments of glory, on the other hand, the pinnacles of revolution from which new worlds could be seen, were not predetermined by reason or wisdom, and their triumph was ensured chiefly by the death of old scientists. That is a parody of Kuhn, of course, but not a malicious one. Lakatos's wicked taunt, "mob psychology," captures the way that many people read the book. The authority of science was unmasked as never before.

The great professed anti-authoritarian figure was not Kuhn but Paul Feyerabend. He did not, however, try to disintegrate the ideology of science by unmasking it. He simply opposed it. And he did not do so on anything recognizable as social constructionist grounds. He was far more direct than that. And in the preface to the third edition of *Against Method,* he explicitly deplores the ways in which the sociologists of science want to demystify science.[22] Thus Feyerabend was anti-authoritarian but not by Mannheimian unmasking.

Did Feyerabend subscribe to a contingency thesis? He did think it is far more a matter of choice than we imagine, what kinds of questions we ask, and whether we want to pursue scientific enterprises at all. He mocked the scientific establishment as more dogmatic and exclusive than was the Roman Catholic Church confronting Galileo. But he did not claim that people in pursuit of certain ends could, in their interactions with the world, have gone more than one way. If there is contingency it is at the level of the methodologies that are favored at one time or another. These are not predetermined, but once the methods are in place, then science carries on towards its destinations, or so he may have implied. Feyerabend was a wonderful pluralist. But pluralism does not imply contingency. This is because every route that human beings may choose may develop rather inevitably. He encouraged homeopathy, acupuncture, psychic research, and much else. Those are remarkably stable enterprises, and one could plausibly, if surprisingly, hold that each has evolved rather inevitably—given the places of human beings in the world. I see little reason to attribute a strong contingency thesis to Feyerabend.

What about the stability of science, sticking point #3? He did think that lots of scientists were stuck in dull routines, but he was just being a good Popperian when he said so. The only sticking point at which Feyerabend definitely checks off is his nominalism, apparent, say, in the appendix on archaic Greece in the first edition of *Against Method.*

In conclusion, Kuhn was certainly a nominalist, and Feyerabend was a nominalist by inclination. Feyerabend was anti-authoritarian, but not for reasons of social construction. Kuhn's book did unmask science, while Feyerabend challenged its authority at its own level, the very opposite of unmasking. On the key issue of social construction, namely contingency, it is Kuhn whom, without anachronism, we can call social-constructionist. And although in the early days of social construction talk Feyerabend would have egged it on, he never advocated the contingency thesis. By the time that social construction had become an orthodoxy of that branch of academic studies called "theory" (not theory about something, just theory) he would have jeered at it.

CHECK LIST

The three sticking points form a check list. Where do you stand on social construction? Score yourself from 1 to 5 where 5 means you strongly stick on the constructionist side, and 1 the opposite. For example, on my reading of Kuhn, he scores 5, 5, 5. Here are my own scores, as debilitatingly ambivalent as you may have come to expect. Do your own.

#1 Contingency: 2.
#2 Nominalism: 4
#3 External explanations of stability: 3[23]