PARADIGM WARS: COMPETING MODELS OF UNDERSTANDING

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Contents

Summary

Humans crave understanding. Science tries to deliver. But is it successful? What are the conditions that make it so? This is a hotly debated issue within the philosophy of science. Kuhn and others provide answers, but there is no consensus. Some assert while others deny that only naturalistic answers could be acceptable. Within any theory and within any philosophical camp there are numerous debates over the details of methodology. It is widely acknowledged that methodology evolves, but what drives it along? Do social and political factors play a role, and if so, must they undermine the ideals of objectivity? If so, what is left of our craving for understanding? These are some of the issues discussed in this chapter.

1. Introduction

We crave understanding. It may not be as strong as the craving for food, sex, or social status, but it ranks high in the list of things that motivate us. And when the aim of understanding is truth, then it is usually taken to be among our noblest impulses, along with the craving for beauty and goodness. But what is this thing, understanding, that we desire so much, and when is the craving for it satisfied? These are timeless philosophical questions.

We want to know, for instance, where we came from, how did life begin? One person reads the account in *Genesis* and comes to believe that God created the universe and all living things in six days. He finds this a believable account. He now understands and his craving is satisfied. Another reads Darwin's *Origin of Species* and comes to believe that humans evolved from very basic organisms over a very long period of time. She finds this account believable. She now understands and her craving is satisfied.

Two questions naturally arise: First, what is it about a particular theory or account of how things are that leads to a satisfying sense of understanding? And, second, since no account is satisfying if it is not believable, what makes an account a reasonable thing to believe? Before addressing these questions, a few remarks are in order about the very possibility of what we seek. It may not always be attainable.

2. The Possibility of Understanding

There are situations where understanding may be impossible. They can arise in the most exacting of the sciences and in the most anguished moments of normal life. For instance, a radioactive atom decays at precisely time *t*. Why then? According to quantum mechanics, there is no answer to this question. It is not that there is an answer and we don't know it. Rather, there is no answer. Nature, according to the current consensus, is irreducibly statistical; it is non-deterministic. There is a given probability that the atom would decay at various times, but no particular time of decay is inevitable. Not everyone, however, has joined the consensus. Einstein rebelled, claiming, "God does not play dice." But scientists, for the most part, have learned to live with this and have given up any hope of understanding why. That's just how things are — nothing more can be said on the subject. We may have to live with a craving that is unsatisfiable in principle.

The tragedies of life often leave us with a frustrating lack of understanding. A child dies from cancer. Why? The question is not a request for biological understanding of a disease, but a demand to know the meaning or purpose of the death. The religious might find some sort of satisfactory understanding in terms of God's plans. No-believers must cope with the fact that they will find no answer at all — the universe, very likely, is utterly without purpose.

Important as they are, we will set these cases aside and focus on those for which we do have some understanding or at least some hope of attaining it.

3. Naturalism

One of the most popular current philosophical outlooks is called naturalism. This is the doctrine that all facts are natural facts and that science is the one and only way to know them. It deliberately and explicitly rules out other alleged forms of understanding such as religious or aesthetic.

In detail, there is a spectrum of naturalist views, but there is also a reasonable consensus on a few key points. Natural facts are usually understood to mean facts about material objects inside space and time. It's perhaps easiest to understand this in terms of what it rules out. Moral facts, for instance, are highly problematic for any naturalist. So ethics usually is understood in some other way, for instance, as a set of rules that we have adopted because they are useful to us, not because they are intrinsically right. Or perhaps, morality is a set of beliefs and attitudes that are hard-wired in us, as a result of their survival value in the evolutionary process.

The epistemology that goes along with naturalism's ontology is invariably empiricism. The one and only source of knowledge is sensory experience. All other alleged forms of knowing are denied. Religious experience, authority and tradition, moral or mathematical intuitions, extrasensory experiences, and so on are all dismissed as groundless illusions. Of course, we can go beyond the experiences themselves. For instance, we have justified beliefs in electrons, even though we have not perceived them. What we do perceive is streaks in cloud chambers and we offer explanations for these. The electron hypothesis is the best explanation for them, so the belief in electrons is justified in this indirect way. Even though we do not directly see electrons, our belief in their existence is grounded in experience, nevertheless.

Naturalism, of course, does not say what science is exactly, but there is an implicit understanding that true science is pretty much like current science. If the right science turned out to be Aristotle's (filled with purposes) or Descartes's (which allows *a priori* knowledge), then the spirit of naturalism would surely be violated. This should not be seen as a weakness of naturalism, but a virtue. It means that the philosophical doctrine of naturalism is itself a conjecture that is open to empirical refutation just like any other legitimate science.

Understanding is now straight-forward. To understand X is to explain it by means of a (purportedly) true scientific theory. Why was there an eclipse yesterday? Why did the bridge fall down? How does a disease spread? Why are protons heavier than electron? Why is unemployment rising? We understand the phenomena involved in each of these by seeing how it is explained. We understand yesterday's eclipse when we are told about the motion of the moon, how it was located between us and the sun casting a shadow, and so on. There is nothing more to understanding than that, provided the explanation is true. Suppose someone says, "Yes, yes, I know all about the moon casting a shadow, but I want to know why, what's the purpose of the eclipse, what's its meaning?" Then the naturalist replies that according to current science (which we take as true), there are no purposes or meanings in nature, so there is no answer. It is a meaningless question and should not be asked.

4. Opposition to Naturalism

Naturalism is not new. Philosophers as diverse as the Greek atomists, British empiricists, and Karl Marx would find themselves at home with much of current naturalism. As an account of our scientific knowledge, it seems quite plausible. The greatest opposition comes from outside scientific considerations. What about moral knowledge, mathematical knowledge, and aesthetic knowledge, to name but a few? Mathematical knowledge, for instance, does not seem to be about material objects in space and time. And we do not seem to acquire knowledge of numbers, by sense perception. Of course, we can prove theorems by means of logic, but where do the axioms come from that we need to prove the theorems? Kurt Gödel claimed that we have mathematical intuitions, a non-sensory form of perception that allows us to somehow grasp mathematical objects and mathematical facts that exist outside of space and time. Gödel's view is the antithesis of mathematical naturalism. G.E. Moore claimed that ethics is about non-natural facts and that we can have moral intuitions concerning these facts. Moore's view is the antithesis of ethical naturalism.

It is easy to imagine taking a step back form hard-core naturalism and adopt a fairly liberal version of it. It would amount, more or less, to the Western intellectual tradition and would include most of the great philosophers and scientists of the past two and half thousand years. It would include a liberal empiricism and a distain for (if not a rejection of) abstract entities. It would also include such Enlightenment principles as: Knowledge cannot be based on the authority of any person or any sacred text; nor can it be based on revelations given to a single person or small group of people. The evidence for any knowledge claim must be available, at least in principle, to all.

For the most part, educated people throughout the world accept something along these lines— but not all. In recent polls, half the adult population of the US believes that Darwinian evolution is false and that the *Genesis* account of human origins is correct. To uphold such a belief, one must set aside normal cannons of evidence and take sacred writings to have pride of place in forming beliefs. But even here things are not always straight-forward. Many opponents of evolution by natural selection claim to base their rejection of Darwin on scientific evidence as evidence is normally understood. They propose "Intelligent Design" (known as ID) in its place, claiming, among other things, that there is an irreducible complexity to some biological processes that could not be explained, except by appeal to something like conscious design.

Cases such as this show that it is quite hard, if not impossible, to draw sharp boundaries between science and non-science. However, let the author quickly add that the non-existence of this boundary does not imply the non-existence of a distinction between rational and irrational beliefs. Ptolemaic astronomy and caloric chemistry are both unquestionably science. In fact, they were wonderful theories in their day. But they have now been decisively refuted and anyone who believes them today is not just wrong, but downright irrational.

A great deal of debate in the US focuses on the question, "Is ID a genuine science"? An affirmative answer is taken to be license to teach it alongside Darwinian evolution in the public schools. The proper question should be, "Is ID sufficiently plausible, given available evidence, to justify teaching it in the public schools?" After all, the fact that Ptolemy's earth-centered astronomy is a science is no reason to teach it. Whether ID is genuine science or religion posing as science does not really matter. All available evidence counts heavily against it.

5. Methodological Debates

Very often debates about particular theories turn on differences at the level of methodology. By methodology, it is meant here the set of rules and procedures that are used to create and test theories. Scientists and the public often refer to this confidently as *the* scientific method, as if it were an obvious and well-understood thing. Not so. It is highly controversial. Of course, there are some precepts on which there is a consensus, e.g., "Don't believe a self-contradictory theory." But aside from a few simple rules such as this, there is little agreement.

Karl Popper claims that science proceeds by a method of conjecture and refutation. It is impossible, he says, to ever confirm a theory, since any theory actually implies infinitely many things and there is no hope of us checking each to see if it's true. We could, however, find a single counter-example, and that would refute the theory. This simple logical point is the basis of his claim that *falsification* is the right method for the sciences and that any attempt to confirm a theory is misguided.

Popper's views are not widely shared, even though there is much to be said for the "critical spirit" that they embody. "Is it falsifiable?" are words that are intended to strike fear in the heart of any pseudo-scientist. And yet, the principle may be too strong. Good scientists do not throw out their theories at the first sign of trouble. They modify, they explain away, they put the blame elsewhere. And often this strategy works. It can, as Lakatos so often stressed, lead to a degenerating research program, but it can also bear fruit in the long run. Progressive science is not constantly revolutionary, which it would be if scientists followed Popper's injunctions at every turn.

There are a great cluster of views that stand in opposition to Popper's falsificationism. They champion some form of induction. A method known as *inference to the best explanation* says we should adopt the theory that out of a set of rivals is the best overall explanation of some set of phenomena. Another known as *Bayseanism* bases its form of inductive reasoning on Bayes's theorem, a result in standard probability theory. It offers a method in which the evaluation of the probable truth of a theory is based on the previously unknown probability of some event that turns out to be true. A new theory won't pick up much evidential support from its correct prediction that the sun will rise tomorrow. But if it successfully predicts the return of a comet several years hence (as Halley did using Newton's theory), then our degree of rational belief in the truth of that theory is greatly increased.

6. Kuhn's Paradigms

One of the most important an influential accounts of science is Thomas Kuhn's. His book *The Structure of Scientific Revolutions* has become a classic with a very significant impact almost everywhere in intellectual life. The term "paradigm" is now a common part of the public's vocabulary. All things from the Bush doctrine of "preventive war" to recent brands of toothpaste have been called new paradigms. Some might even use the term to refer to rival accounts of understanding.

Kuhn, himself, sees science as mainly a puzzle-solving activity. He called this *normal science*. It is an activity in which various phenomena are fitted into the reigning theory. But often particular problems can't be solved. They do not, contra Popper, count as refutations — at least not yet. They are merely put on the back burner, to be solved another day. But as their number grows, a crisis can come about. The scientific community comes to the view that the reigning theory is not performing very well, that the problems are serious and must be addressed. At this point new theories are proposed and eventually one of these comes to dominate. It is a new *paradigm*. There was a certain amount of vagueness in Kuhn's first formulation of the idea of a paradigm, but subsequent writings point to a fairly clear notion. A paradigm is a concrete example. Newton's treatment of the motion of Mars, for instance, is such a concrete example. We then abstract from it and try to work out the motion of other plants following the Newton model.

The idea is best seen with language learning. When mastering an inflected language such as Latin, we typically do not master general rules, but rather memorize a particular example. Thus, the first declension Latin noun *porta* (gate) is declined:

	Singular	Plural
Nominative:	*porta*	*portae*
Genitive:	*portae*	*portarum*
Dative:	*portae*	*portis*
Accusative:	*portamportas*	
Ablative:	*porta*	*portis*

When encountering other first declension nouns, *fama, fortuna, philosophia*, etc., we know from the paradigm how to decline them. Explicit rules play little or no role in guiding us. We return to the example, the paradigm, and model new cases on it.

Kuhn is sufficiently important and influential that it is worth describing some of his central views in detail. *Normal science* was already mentioned. But it needs to be stressed. Most science is normal science. It is what is done by scientists who agree on all the basics. This means that they agree on what the world is made of, on how things interact with one another, on symbolic representations (everyone working in the Newtonian paradigm accepts the expression "$F = ma$", for instance), on what the outstanding problems are, and on what is the right way to tackle them. And they all agree on whether a proposed solution really does, or does not work. Basics are taken for granted; the fundamental theoretical framework is beyond criticism.

The most famous of Kuhn's concepts, of course, is *Paradigm*. Normal science is a puzzle-solving activity. It is an attempt to fit things into a pattern. And the pattern is set by a paradigm. This is a concrete example, a specific bit of scientific work that all take to be a great achievement. A paradigm is like a legal precedent or the explicit declension of a particular noun, as in the Latin example above. A paradigm/precedent has no explicit directives associated with it, but familiarity with it suggests how to treat similar matters.

There are a number of things associated with a paradigm and seem to be generated from it. These include: basic ontology (what the world is made of), assumptions as to which are the important problems, heuristic norms (how to go about doing normal science), and evaluative norms (criteria for judging proposed solutions to problems). To hold a paradigm is to adopt a Wittgensteinian "form of life", a *weltanschauung*. During the reign of a paradigm the course of science is cumulative.

The next main idea for Kuhn is *crisis*. Some of the puzzles and anomalies of normal science are very stubborn. When several persist for a long time a period of crisis sets in. The admission of crisis only comes with the greatest reluctance. ("It is a poor worker who blames his own tools.") Crisis induces attacks on the paradigm itself; basic assumptions are questioned. Many begin to consider anomalies as not just tough problems but as outright counter-examples to the theory.

From crisis we move to *extraordinary science*. Normal science, according to Kuhn, is no more; the united community has disintegrated into several diverse schools. All criticize all others. Several new paradigms come into existence. All kinds of devices are used to persuade others to convert — even philosophical arguments, says Kuhn. Eventually one of the rival paradigms comes to prevail. The vast majority of scientists are won over to the new theory. This is a scientific revolution. The conversion that takes place is much like a gestalt shift (e.g., duck-rabbit; see Figure 1).

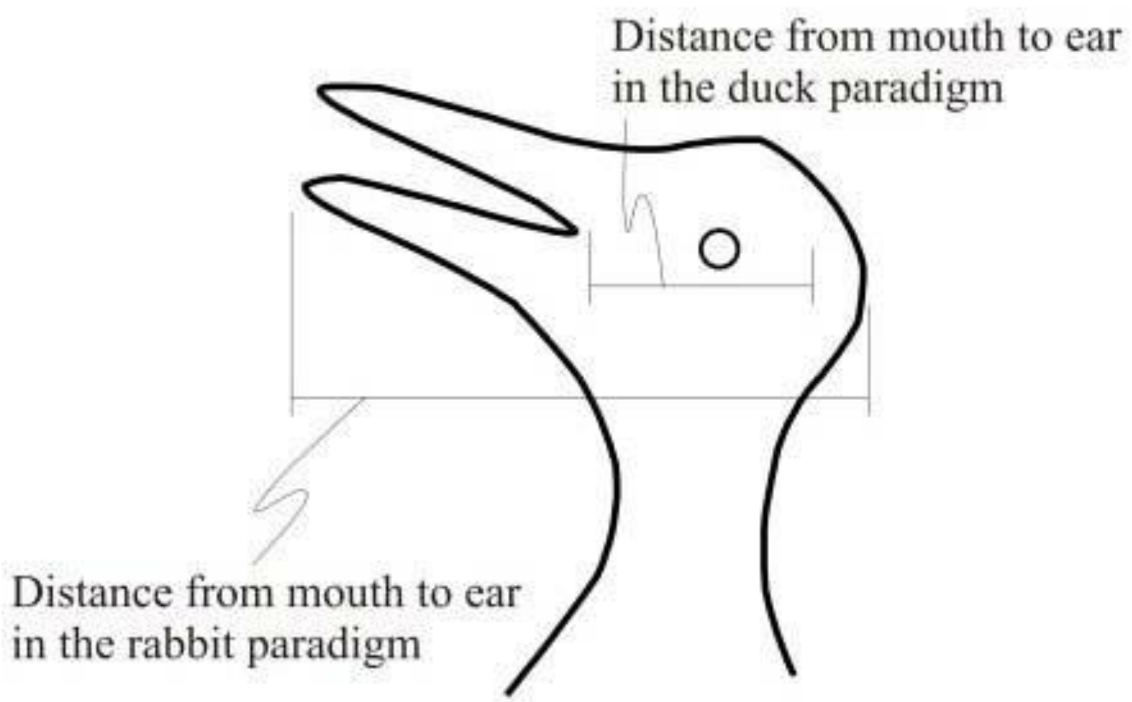

Figure 1: Rabbit paradigm and Duck paradigm.

Next comes the *entrenchment of the revolution*. The way a revolution in science becomes entrenched is not by further empirical evidence or argument, but rather by a kind of indoctrination. Students learn the new theory from textbooks which present the exemplars that *illustrate* the theory as if they were *independent evidence* for it. Textbooks often give a short history of the subject which makes the paradigm look reasonable and old rivals look silly. (Just consider how Galileo's rivals are today often depicted — as dogmatists who refused to look through his telescope.) It is the winners who write the books. Kuhn is not critical of this dogmatic approach; he thinks it helpful for practicing scientists not to be skeptical about basics. Students learn by example in the lab; and much of what they learn is *knowing how* as well as *knowing that*. This non-propositional knowledge is very important and plays a big role in how one does science. (You know how to ride a bike, but try to articulate it!) Kuhn cites Max Planck (the great German scientist who founded quantum theory): "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it." Thus, we return to (a new) normal science.

There are several interesting and contentious points that arise in Kuhn's account of science.

Theory-ladenness of Observation: The thesis, roughly, is that seeing is *seeing as* or *seeing that*; it is never neutral, or merely passive observation. Our beliefs and expectations condition what we see. Perception is paradigm dependent. There is no "given" that is independent of all conceptualization, no "neutral observation" that could be the independent empirical basis for testing rival theories. Under certain conditions, if we expect to see a duck, we will; but if we expect to see a rabbit, we will see that instead.

Meaning and Incommensurability: The meaning of a term is dependent on the role it plays in a theory. Though he is often seen as their chief adversary, it is quite remarkable how much Kuhn took over from the Positivists. He rejected any theory/observation distinction, but he did accept the related Positivist view of language in which key terms are defined contextually; that is, they get their meaning from the role they play in the theory. Consequently a change of theory involves a change in meaning. Even though the term "energy" may appear in several theories, it nevertheless means something quite distinct each time. Scientists with different paradigms may seem to say contradictory things about energy, but actually they are talking about different things. The different theories, says Kuhn, are incommensurable; there is no common or neutral language in which to express them. For example, consider the following pair of statements:

Rabbit paradigm: "The distance from the eye to the ear is X."
Duck paradigm: "The distance from the eye to the ear is Y."

They seem to contradict each other, but what each means by "ear" is different. The meaning of the term "ear" is paradigm dependent. (See Figure 1.)

Is Paradigm Change Rational?: Many think that on Kuhn's account it cannot be. According to Lakatos, it's just "mob psychology." Since methods, observations, assessments, etc., are paradigm dependent, it would seem there can be no neutral ground from which to judge the merits of rival paradigms. In a later essay, "Objectivity, Value Judgement, and Theory Choice" (1977), Kuhn gives five criteria for change: empirical accuracy, consistency, broadness of scope, simplicity, and fruitfulness. These are trans-paradigm criteria. They make Kuhn's views seem more plausible, but are somewhat at odds with the more radical doctrines of *Structure of Scientific Revolutions*, since they are not part of the "inextricable whole" after all (more on this in the following).

Truth and Progress: Scientific realists believe that our best theories are (approximately) true — true in the sense of corresponding to an independent reality. Kuhn, however, flatly denies this. Not only our beliefs, but reality itself is paradigm dependent. With a paradigm, scientists "create" the world they work in. Similarly, progress for Kuhn is not progress *toward the truth* or toward anything else; rather, it is progress *from* — there is no ultimate goal for science; it simply moves away from an earlier anomaly-ridden paradigm to a new one which is more empirically accurate, broader in scope, and so on. Kuhn cites an analogy: species in Darwinian evolution adapt to their environment and in this sense make progress, but there is no ideal form toward which a species is headed. Science is similarly driven from behind; it is not headed toward any goal such as truth. Even when the five trans-paradigm criteria are taken into account, we still do not have progress toward truth, according to Kuhn. Greater accuracy, simplicity, and so on are better only in the sense that having more off-spring is better. They are not a sign of truth any more than having a lot of children makes you a better person.

7. Theory-Methodology Interaction

Kuhn thinks theories and methodologies are parts of a single package, an "inextricable whole," as he puts it. In an important sense he is right: theories and methods have great influence on one another. However, he is certainly wrong in claiming that they are so intimately connected that they stand or fall together. This is clearly not so. Theories can change while methods remain constant and methods can change while theories remain constant. For instance, if Popper or the Bayesians are right, then they are eternally right. Falsificationism would remain fixed above the flux, transcending all theory change.

But not all methodology is at such a high level. Much of it is formulated in response to developments at the level of theory. For instance, it is only during the second half of the 20th century that medical research has been conducted largely by means of randomized clinical trials. Prior to this method of testing, evidence for or against any proposed new drug or medical procedure was based on clinical experience. That is, practitioners would report their experiences with particular patients. By the middle of the 20th century, however, placebos were recognized to have tremendous power. This meant that a practitioner couldn't tell it was if the drug or the placebo that was the true cause of the beneficial outcome. The recognition of a need for randomized clinical trials was the result of this finding.

So, here we have a clear case of the discovery of a fact, namely, that placebos can have a significant effect, leads to the institution of a methodological norm, namely, that testing of drugs and medical procedures should be done by means of randomized clinical trials, not by means of the clinical experience of doctors. The latter is now often dismissed as "merely anecdotal evidence."

8. Social and Political Factors

Methodological debates can turn on narrow issues in logic, on findings in the sciences, and even on social and political considerations. The last of these may seem to entail a perversion of good science, a betrayal of rationality, but this need not be so. Two examples can be cited where a kind of social or political involvement arguably leads to better, more rational science.

8.1 Man-the-hunter

The search for human origins has enormous social ramifications. It informs our picture of ourselves — both anatomical and social — and so plays a role in the determination of social policy and civil life. One prominent hypothesis is the "man-the-hunter" theory. The development of tools, on this account, is a direct result of hunting by males. When tools are used for killing animals and for threatening or even killing other humans, the canine tooth (which had played a major role in aggressive behavior) loses its importance, and so there will be evolutionary pressure favoring more effective molar functioning. Thus human morphology is linked to male behavior. Male aggression (hunting) is linked to intelligence (tool making). On this account women play no role (or only a secondary one) in evolution. They merely tagged along behind the men who carved out evolution's path. We are what we are today because of the activities of our male ancestors.

But this is not the only view of our origin. A theory of more recent vintage is the "woman-the-gatherer" hypothesis. This account sees the development of tool use to be a function of female behavior. As humans moved from the plentiful forests to the less abundant grasslands, the need for gathering food over a wide territory increased. Moreover, women are always under greater stress than men since they need to feed both themselves and their young. Thus, there was greater selective pressure on females to be inventive. And so, innovations with tools were due mainly to females. Why, on this account, should males lose their large canine teeth? The answer is sexual selection. Females preferred males who were more sociable, less prone to bare their fangs and to other displays of aggression. So, on the "woman-the-gatherer" account of our origin, our anatomical and social evolution is based on women's activities. On this account we are what we are today largely because of the endeavors of our female ancestors.

The kinds of evidential consideration thought relevant in deciding this issue, include: fossils, objects identified as tools, the behavior of contemporary primates, and the activities of contemporary gatherer-hunter peoples. Obviously, each of these is problematic. Fossils are few and far between, and are little more than fragments; some tools such as sticks will not last the way stone tools will, so we may have a very misleading sample of primitive artifacts; moreover, it is often debatable whether any alleged tool was really used for hunting an animal or preparing it for eating, rather than used for preparing some vegetation for consumption; and finally, inferences from the behavior of contemporary primates and gatherer-hunter humans to the nature of our ancestors who lived two to twelve million years ago is a huge leap.

None of these considerations should be dismissed out of hand; each provides evidence of some sort, though rather weak. We have a case of under-determination — there simply isn't enough evidence to pick out a winner from these two rival theories. Nevertheless, our ability or inability to find the right theory is not important. The moral of this example is that it displays how background beliefs, many value-laden, can affect scientific choices. If one is already inclined to think of males as the inventors of tools, then some chipped stone will be interpreted as, say, a tool for hunting. This will then become evidence in the man-the-hunter account of our origin. On the other hand, if one is a feminist then one might be inclined to see some alleged tool as an implement for the preparation of vegetable foods. On this interpretation the tool becomes evidence for the woman-the-gatherer account of our evolution.

We may hope, with Helen Longino, that "In time, a less gender-centric account of human evolution may eventually supersede both of these current contending stories." (1990,

111) It will not, however, be a value-free view of the matter. Values will always play a role in any scientific theorizing; it's just a matter of getting clear on this and making the operative values visible. The great merit of the woman-the-gatherer theory is that its very existence made manifest the previously hidden assumptions of the prior man-the-hunter theory. Until the existence of the rival, the "evidence" for the man-the-hunter account was "dependent upon culturally embedded sexist assumptions." (1990, 111)

The moral to be drawn from this example is simple enough, though many-sided. Science with a bias needn't be bad. The "man-the-hunter" theory is not stupid. It's elaborate, thoughtful, and plausible. It's rival, the "woman-the-gatherer" theory, was the product of a different social outlook. But that doesn't make it silly either. It, too, is elaborate, thoughtful, and plausible. And to its extra credit, it provided a relief for the older theory. Background assumptions and values became visible by contrast. There is even a next step which suggests itself. Are the categories "man-the-hunter" and "woman-the-gatherer" the right ones? They arise in our own sexual division of labor, but are they justly imposed on the very distant past?

The example of rival origins theories is a stock example for feminists. It illustrates the point that science is not only more democratic by including within its ranks a greater variety of theorizers, but it also has the potential to produce better science. The set of biases we bring to scientific inquiry is harmful when the whole community shares them. But when they are open to inspection and challenge, they can be rendered harmless, perhaps even useful.

8.2 Medical Research and Intellectual Property Rights

A second example where political involvement might be helpful concerns current medical research.

Richard Davidson (1986) found that in his study of 107 published papers that compared rival drugs, the drug produced by the sponsor of the research was found to be superior in every single case. The Davidson study is typical; there are many like it coming to similar conclusions, though not quite so dramatically. For instance, Friedberg *et al* (1999) found that only 5% of published reports on new drugs that were sponsored by the developing company gave unfavorable assessments. By contrast, 38% of published reports were unfavorable when the investigation of the same drugs was sponsored by an independent source.

Stelfox *et al* (1998) studied 70 articles on calcium-channel blockers. These drugs are used to treat high blood pressure. The articles in question were judged as *favorable*, *neutral*, or *critical*. Their finding was that 96% of the authors of favorable articles had financial ties with a manufacturer of calcium-channel blockers; 60% of the authors of neutral articles had such ties; and only 37% of authors of unfavorable articles had financial ties. Incidentally, in only two of the 70 published articles was the financial connection revealed.

With these cases in mind, we should naturally become worried about who is funding the research. Whether we attribute these kinds of results to the theory-ladenness of observation, or to outright fraud, or to some new and subtle form of corruption doesn't really matter. The big question is: What can be done about it?

The editors of several leading biomedical journals got together recently to forge a common editorial policy that was published simultaneously in several journals. They were concerned with the commercialization of research and wished to protect their journals from being a "party to potential misrepresentation." The editors especially oppose contract research where participating investigators often do not have access to the full range of data that play a role in the final version of the submitted article. The new guidelines are part of a revised document known as "Uniform Requirements for Manuscripts Submitted to Biomedical Journals," a compendium of instructions used by many leading biomedical journals. The breadth of requirements in these guidelines is considerable, from double-spacing to respecting patients' rights to privacy. The editors, collectively, have added specific new requirements concerning conflict of interest. Here in outline are some main points:

- Authors must disclose any financial relations they have that might bias their work. For example: are they shareholders in the company that funded the study or manufactures the product, are they paid consultants, etc.? At the journal editor's discretion, this information would be published along with the report.
- Researchers should not enter into agreements that restrict in any way their access to the full data, nor should they be restricted in contributing to the interpretation and analysis of that data. (Often research is done by several small independent teams providing partial data; no one except the company sponsor has a sense of the global picture.)
- Journal editors and referees should similarly avoid conflicts of interest in the peer review process.

These guidelines, if rigorously enforced, should go a long way in helping improve the situation, and the journals should be warmly applauded for instituting them. More recently, the same journal editors have taken another big step. They now require every clinical trial be registered at the outset. No results would be accepted for publication unless they resulted from a registered trial. The point of this requirement is to prevent selective reporting and the suppression of negative results. Thus, if negative results are discovered but not published, others can raise appropriate questions (*//jama.ama-assn.org/cgi/content/full/292/11/1363*).

These problems can be very serious. We can briefly consider a couple of troubling cases. Celebrex, which is used in the treatment of arthritis, was the subject of a year long study sponsored by its maker, Paramacia (now owned by Pfizer). The study purported to show that Celebrex caused fewer side effects than older arthritis drugs. The results were published in *JAMA* (*Journal of the American Medical Association*) along with a favorable editorial. It later turned out that the encouraging results were based on the first six months of the study. When the whole study was considered, Celebrex held no advantage over older and cheaper drugs. On learning this, the author of the favorable editorial was furious and remarked on "a level of trust that was, perhaps, broken."

Selective serotonin reuptake inhibitors, known widely as SSRIs, have been central in the new generation of anti-depressants. Prozac is the most famous of these. There are several drugs in the SSRI class, including fluoxetine (Prozac), paroxetine (Paxil, Seroxat), sertraline (Zoloft), and others. They are often described as miracle drugs, bringing significant relief to millions of depressed people. The basis for the claim of miraculous results is a large number of clinical trials, but closer inspection may tell a different story.

There are two related issues, both connected to non-reporting of evidence from clinical trials. Whittington *et al.* (2004) reviewed published and unpublished data on SSRIs and compared the results. To call the findings disturbing would be an understatement. The result was favorable to fluoxetine, but not to others. They summarized their findings as follows:

Data for two published trials suggest that fluoxetine has a favorable risk-benefit profile, and unpublished data lend support to this finding. Published results from one trial of paroxetine and two trials of sertraline suggest equivocal or weak positive risk-benefit profiles. However, in both cases, addition of unpublished data indicates that risks outweigh benefits. Data from unpublished trials of citalopram and venlafaxine show unfavorable risk-benefit profiles. (Whittington, *et al.* 2004, 1341)

The related second point is illustrated in a GlaxoSmithKline internal document that was recently revealed in the *Canadian Medical Association Journal*. They were applying to regulatory authorities for a label change approving paroxetine (Seroxat) to treat pediatric depression. The document noted that the evidence from trials was "insufficiently robust," but it stated: "It would be commercially unacceptable to include a statement that efficacy had not been demonstrated, as this would undermine the profile of paroxetine." Perhaps they had lots to worry about from a commercial point of view, since annual sales of Seroxat are close to $5 billion. Imagine two ways of approaching a health problem. One way involves the development of a new drug. The other way focuses on, say, diet and exercise. The second could well be a far superior treatment, both cheaper and more beneficial. But obviously it will not be funded by corporate sponsors, since there is not a penny to be made from the unpatentable research results. It should be just as obvious that a source of funding that does not have a stake in the outcome, but simply wants to know how best to treat a human aliment, would happily fund both approaches to see which is superior.

To get a sense of what might be at issue here, consider a comparative trial carried out on patients who were at high risk of developing diabetes. Over a three year period, 29% of the placebo group went on to develop diabetes; 22% who took the drug metformin developed diabetes; but only 14% of those who went on a diet and exercise program developed the disease. (Angell 2004, 170) This trial, by the way, was sponsored by the US National Institute of Health, not by commercial interests.

In a study of the effects of exercise on depression Dunn and co-researchers found significant results. "In summary, aerobic exercise in the amount recommended by consensus public health recommendations was effective in treating mild to moderate MDD [major depressive disorder]. The amount of exercise that is less than half of these recommendations was not effective. Rates of response and remission with a PHD [public health dose, commonly recommended amount] dose are comparable to the rates reported in trials of cognitive behavioral therapy, antidepressant medication, and other exercise studies.”

Of course, there is also the problem that the public wants a quick solution and are not that keen on diet and exercise. Yet, if they were not so bombarded with industry propaganda or got an equal amount of publicly sponsored information about the relative benefits (perhaps presented in a humorous way like the Viagra ads), then we might well see more people opting for the better solution. Public funding is clearly the answer to several aspects of this epistemic problem.

Even within patentable research, some areas will be less profitable than others. Consequently, diseases of the poor and the third-world (malaria) are going relatively unexplored, since the poor cannot afford to pay high royalties. We’re also in danger of losing a genuine resource in the form of top-notch researchers who don’t do patentable work. One can consider an example outside medical research. At Berkeley there was a Division of Biological Control and a Department of Plant Pathology, but neither exists today. Why? Some people close to the scene speculate that it is simply because the type of work done in these units is not profitable. Typical research in these units involved the study of natural organisms in their environments carried out with a view to controlling other natural organisms. This type of work can’t be patented. Is it valuable? Yes. Is it profitable? No.

Trends being the way they are, top graduate students won’t go into the field. Fewer and fewer people will work on agricultural and environmental problems through biological control. Perhaps the petrochemical industry will be able to solve all our agricultural problems. It’s not the job of a philosopher to speculate on this possibility. But it is the job of philosophy of science to make the methodological point that without seriously funded rival approaches, we’ll never know how good or bad particular patentable solutions really are.

The epistemic point is a commonplace among philosophers. Evaluation is a comparative process. The different background assumptions of rival theories lead us to see the world in different ways. Rival research programs can be compared in terms of their relative success over the long run. But to do this, we need strong rivals for the purposes of comparison.

Getting good regulations in place is vitally important. Nevertheless, the problems regulation truly solves are the lesser ones, and often they can be solved in a different way. More serious is the problem that there is no incentive to do research on medical solutions to health problems that cannot be patented. It is the crucial generation of a wide class of rival theories that is totally lacking in for-profit research. And that is the main reason, according to the author, for wanting political involvement in research. The following should be adopted.

- Eliminate patents in the domain of medical research.
- Adjust public funding to appropriate levels.

What can be said in favor of these two points? It might be thought that patents are necessary to motivate brilliant work. Nonsense. The most brilliant work around in mathematics, high energy physics, evolutionary biology, etc. is all patent free. Curiosity, good salaries, and peer recognition are motivation enough. What about the problem that a great deal of medical research is simply drudge work, i.e., massive clinical trials? This may be true, but clinical trials are going to be needed for some types of research that are clearly not patentable and just as clearly are of great use to society. If we can carry out clinical trials for the influence of broccoli on health, where nothing is patentable, then we can do it for drugs, too.

It might seem that the author's insistence on eliminating patents in medical research, on being involved in policy decisions, and on the particular policy advocated is a mere reflection of various social and political values. Perhaps this is so. But there is another way to consider this issue, a way in which social and political values don't play any determining role at all. In fact, we could consider the whole business as just a question of good methodology.

Scientific method is not fixed for all time, but rather seems to evolve, often under the influence of scientific discoveries. For instance, as mentioned above, the discovery of placebo effects lead to the introduction of blind and double blind tests. That is, the discovery of a fact can lead to the institution of a norm: In situations of such and such a type, you *ought* to use blind tests. Though this is a norm, it is not what ordinarily would be called a value. That is, it's not a social or moral value, though it is certainly an epistemic value. The practice of blind testing, as others have noted, is best seen as science simply adopting what it has itself established as appropriate methodology.

The current situation can be viewed in a similar light. We have learned *empirically* that research sponsored by commercial interests leads to serious problems, so serious that the quality of that research is severely degraded. The switch to public funding solves many, if not all, of these epistemic problems. Therefore, as an epistemic norm, public funding for medical research should be adopted. This is no different than, first, discovering the placebo effect, next, discovering that blind testing can overcome the difficulties that the placebo effect entails, and then, as a result of all this, adopting the methodological norm of employing blind tests.

Is there not a danger in inviting political considerations in scientific research? Certainly. But nothing new is advocated here in that regard, since there is already political involvement in the form of establishing intellectual property rights.

9. Social Constructivism

Because of the presence of various social factors in science, a significant body of opinion takes the view that all of science is a manifestation of social factors. Collectively, this goes by the name "social constructivism." There are many versions, some embrace "naturalism," others "postmodernism." More or less common to them all is the view that science does not give us an objective account of how things are, but rather provides us with theories that are intimately linked to social groups and promote their interests. Some historians, for example, have claimed that non-deterministic quantum theory was adopted in Germany during the Weimar period in the 1920s because of prevailing social conditions. Germany had subsequently lost the Great War and German scientists were held in low esteem. The German public was in an irrational, anti-deterministic mood, happy to embrace the views of Spengler's *Decline of the West*, a spectacular best seller at the time. Rejecting classical, deterministic physics and adopting the new theory was the scientists' way of currying favor with the German public. The choice of a non-deterministic theory was based on social factors, not evidence, as we normally understand it. This way of seeing intellectual history is typical of social constructivism.

Only the very naive could deny that there have been social factors at work in the sciences. The real fight is between social constructivists who deny that science can be a genuinely rational activity and their opponents who claim that there are such things as good reasons and that objective evidence can support or discredit particular theories, independent from our interests. (This can be a confusing point, since social constructivists certainly allow that talk of evidence plays a role in science; but on close examination, this is not what is meant by evidence in the sense of having objective normative force.) According to the opponents of social constructivism, scientists can believe and often do believe on the basis of reason and evidence. Reason and evidence are the causes of belief. This need not always be the case. But the mere possibility is enough to upend a consistent social constructivist.

The debate is on many fronts, but two are central. Individual case studies are strenuously debated. Rival accounts are presented of particular episodes in the history of science. Forman's account of the Weimar scientists wanting to regain lost prestige is met with a rival account saying they were reacting to various empirical problems, such as the anomalous Zeeman effect, which had nothing to do with their social standing. The respective plausibility of these rivals helps to make the larger case for or against social constructivism.

The second front focuses on philosophical matters. David Bloor, for instance, has famously proposed his "strong program" and it has proved to be very influential. Bloor declares himself to be a naturalist (as characterized above) and a great fan of science; he embraces what he takes to be the scientific method. As a student of science, he claims that the best way to understand science itself (or anything else for that matter) is to understand it scientifically. Thus, we must look for the *causes* of a scientist's beliefs. We must *impartially* explain those beliefs regardless of their truth or falsity. Whether a belief is true or false, rational or irrational, we must, *symmetrically*, use the same type of explanation for each. And we must do all of this *reflexively*, that is, we, too, are subject to the same sorts of considerations we are applying to the beliefs of other scientists.

As Bloor see it, the upshot is obvious: We must understand science (or anything else for that matter) in sociological or other naturalistic terms. The appeal to reason and evidence that is made by most philosophers, traditional intellectual historians, and other champions of objective science is scientifically illegitimate nonsense.

Champions of objective science, of course, don't agree. They will have no trouble accepting some of Bloor's principles, such as impartiality and reflexivity. The demand for causal explanations is acceptable, too, provided that reason and evidence can count as causes of belief. Implicit in Bloor's discussion is the assumption that evidence as normally understood couldn't cause anything. On the other hand, social factors (such as a desire to regain lost prestige) or neural hard-wiring (a product of the evolutionary process) are prime examples of causes of belief. Without going into why, let us just note that the vast majority of philosophers today firmly believe that reasons can be the causes of belief. Understood this way, they would have no quarrel with Bloor over his causality principle.

Perhaps the real difference between Bloor-type social constructivists and their opponents turns on the symmetry principle. We call on an engineer to tell us why one bridge is standing and another has fallen down. We get the same type of explanation in each case, explanations that involve the stability of the ground, the strength of the building materials, and so on. This is symmetry at work. What Bloor wants to fight is the common older view (he calls it the "weak program" in contrast to his own "strong program") that calls on sociology to explain false, irrational beliefs, and that calls on reason and evidence to explain true, rational beliefs. Symmetry demands the same type of explanation for both cases, which he takes to be properly sociological.

There is a serious ambiguity about "same types of explanation," that renders the principle highly problematic. It certainly can't mean identical, since then we would have to cite an earthquake as the explanation for a bridge standing, if we used it to explain why another fell. If we liberalize the notion enough we can make every belief explainable by reason and evidence. All we have to do is change the goal sought. If the goal is truth, then we cite reason and evidence as the cause of the true belief. If the goal is regaining lost prestige, then we cite reason and evidence as the cause of the adoption of non-deterministic quantum theory as the best way to achieve that goal. That would be symmetry in action, but the author dares to say it's not what Bloor had in mind.

The rationalist alternative is not to claim that all of science has been pure of heart. Lots of past science and no doubt lots of current science is racist, sexist, stupidly wrong-headed, and corrupted by various social factors. The belief of any rationalist is that these factors can be uncovered (though often with difficulty) and science put on the right track. Seeing "man-the-hunter" (above) as serving a male dominated society or commercial interests corrupting medical research are obvious examples. Rather than shrug them off as inevitable (as is too often the case with social constructivists), the scientific community can face them head-on. Alternative theories can be generated and tested. In this way bias can be exposed and overcome. But it requires an assumption that objectivity is within our grasp. Without this assumption, quietism is inevitable. The relativism of social constructivist approaches to science has been one of its more unfortunate consequences. It was one of the main reasons Alan Sokal perpetrated his infamous hoax.

10. Science and Values

Are there values in science? Should there be? Before trying to answer these questions, let's make some distinctions.

The first is between *facts* and *values*. It's a very basic distinction, reflected linguistically as declarative and imperative sentences. Often the distinction is put simply as *is* versus *ought*, or as *descriptive* versus *prescriptive*, or as *factual* versus *normative*. A common claim by those who stress the distinction is that science is “value-free”, that is, it deals with facts, not values. At least, they often maintain, good science is value-free; when values are injected, the facts are besmirched. A simple explanation is often provided for this claim: facts are objective, but values are subjective. So, now we have two additional questions to answer: Is there a genuine fact-value distinction?, and second, Are values objective or subjective? But first, another distinction.

There are different types of values: *moral* and *epistemic*. Let us forego further distinctions, though they are certainly possible. Instead let us lump cultural, political, and ethical norms under the single heading “moral values.” And by “epistemic values” we mean all those norms having to do with the justification of belief. The idea is best explained with examples: “Reject any theory that is self-contradictory”, “Accept theories on the basis of explanatory scope and power”, “Successful predictions of unexpected phenomena should count heavily in support of a theory.” Notice that these are all prescriptions, ought-sentences. They are clearly values, not facts, given that distinction. (Are epistemic values really different from moral values? This is a deep and difficult question which we won’t try to answer here. For now let us simply assume that they are.)

We can now answer the first pair of questions. Are there values in science?, Should there be? Both get a resounding Yes. Science would be impossible without epistemic norms. Science needs guidance and that is exactly what methodological or epistemic norms provide.

There are moral norms at play in science, as well. For instance, “Inflict no unnecessary pain” is a rule that governs laboratory experiments using animals. Animal rights have long been a very contentious issue in science. One side claims that the use of animals is justified by the benefits it brings to humans. The other side claims that animals have rights that should not be violated under any circumstances. We won’t pursue this issue, but merely mention it to show what’s at issue.

There is an important class of norms that embodies both epistemic and social-political aspects. For instance, the presence or absence of intellectual property rights (mentioned above) is clearly a political matter, but it is one that affects the practice of science. Robert Merton (the American sociologist of science), described what he called the “ethos of science.” The four principles that he thought were at work in the practice of science are a mix of epistemic and social norms.

- *Universalism: the evidence is open to all; there are no privileged observers*
- *Communism: knowledge is collectively arrived at and is owned by all*
- *Disinterestedness: we approach nature without prior wishes that it be one way or another*
- *Organized skepticism: nothing is immune from doubt*

The question of the objectivity of values (moral or epistemic), is one of the hardest in all philosophy. Some champions of subjectivity go so far as to make our values out to be as subjective as our preferences for a particular flavor of ice cream. That’s ridiculous. A preference for consistent theories, or for empirical adequacy, or for broad explanatory scope is hardly a personal whim. Nevertheless, it is not easy to establish the claim that values are independent of all human interests. Indeed, the concepts of health and disease are shot through with values, some of which are clearly not objective. It suffices if we mention “the disease of masturbation” or the “disease of homosexuality” to make the point. One of the most interesting examples involves an isolated group of people who typically pick up a particular parasite at a young age and this has the effect of mottling the skin. It is very common and much admired within the group. Those few in the group without mottled skin (because they didn’t pick up the parasite) are considered diseased. People outside this community would view the situation the other way around and see those with the parasite as diseased. In a case such as this, it is hard to see the difference between health and disease as anything other than subjective.

The author certainly won’t make the claim of complete objectivity, but will make the assumption that some values are relatively objective. It should be further stressed, however, that even if values are perfectly objective, there is still the question of our knowledge of which ones are the right ones. There are, after all, conflicting values being proposed by different groups — not all can be right. Making progress here can be as difficult as making progress in discovering facts.

There would be near universal agreement that much of Nazi science was grossly immoral. Hypothermia experiments, for instance, were carried out on prisoners who were immersed in ice water to see the effects. A great deal was learned about what the human body can endure before death sets in, about the effectiveness of different methods of recovery, and so on. The interesting thing about this case is that we condemn the method, but not the result. Even if we decide not to use the information — because it dishonors the victims — we nevertheless allow that the result of these experiments is a body of objective facts. The cruelty of the methods and the horrible ideology that motivated the research can somehow be separated from the results of that research. But is this true in all cases?

This leads us to a final question: Can moral values affect the content of science? This is by far and away the most important question. Obviously, epistemic values determine the content of science. If we changed those values, we likely would end up with different theories. The question we are now concerned with is whether our social-political-ethical values play a role in choosing which theory is eventually adopted.

No, says one very influential school of opinion. The negative answer is based on a very popular distinction between *discovery* and *justification*. Values, according to this camp, play a role in thinking of theories in the first place. They are what motivate us. It could be a desire for a certain type of beauty, or a wish to promote racist theories, or merely a wish to become rich by finding a theory that is patentable. These are the factors that lead to discovery. However, to be acceptable, there must be evidence for a theory. Someone’s theory will stand or fall on evidential grounds, according to this view, not on what made her think of it in the first place. This is the justification side of the process. Champions of the distinction say it doesn’t matter what values, or psychological quirks, or accidents (like an apple falling on your head) are present, they all come out in the wash of justification. The upshot of this view is clear: when a theory has been properly tested and justified, it has no value content.

The fact-value distinction and the discovery-justification distinction fit together very well. It's not surprising that they have been widely accepted. But there is a serious problem that has been overlooked. The discovery-justification distinction assumes that we can evaluate a theory by direct comparison with nature. It assumes that in isolation from everything else, an objective assessment of its truth can be made. This is almost certainly false. Real evaluation of theories is to a very significant extent comparative. That is, theories are not only compared with nature, but with rival theories. This can still be perfectly objective, but it is much more complex than the model of testing one at a time. Rivals highlight features that would otherwise go unrecognized; contrasts bring out aspects that are subsequently realized to be crucial. Without rivals we can no more objectively evaluate a theory than we can objectively evaluate a tennis player who has never faced an opponent. This is a point made above, but cannot be overemphasized.

If all reproductive biologists were male, then we could objectively rank their rival theories, but we couldn't eliminate a sexist bias that might be common to them all. If all anthropologists were of European decent, then we could objectively rank order their rival theories, but we couldn't eliminate a common cultural bias that might be present in each theory. Being common, it becomes invisible. The way to deal with this problem (as was stressed earlier), is to diversify the scientific community, that is, to make sure the class of scientists has as wide a range of representatives as possible. Female biologists and non-European anthropologists will not be free of bias, but they will at least have different prejudices and they will produce different theories. The pool of rival theories will be the richer for it and the process of evaluation will be greatly improved. Scientific objectivity is best promoted by the diversity of scientists.

11. Values and Understanding

What relationship does understanding have to values? A variety of answers are possible, though none have been made explicitly.

Someone of an anti-objectivist, social constructivist turn of mind might be tempted by the following argument. Scientific theories are not an objective account of reality; they reflect our values and interests. Since there are no objective facts about, say, racial and sexual equality or inequality, we should simply insist that any acceptable theory adopt racial and sexual equality as a fundamental tenet. Though this is not an objective truth about the world, it is a value that makes for a better world.

Such a view may be inspired by morally praiseworthy motivations, but it abandons any hope of understanding. It would leave us in that state of craving which was mentioned at the outset. We want to know how things are and why they are that way. If we came to believe, as social constructivists would like us to believe, that what we crave does not exist, then we might be slightly pacified, but we could never be completely content.

The flip side of this is the view that says intellectual honesty trumps everything. It is a popular view among some very dishonest or easily deluded people. The facts about either racial or sexual inequality of cognitive ability may be unpleasant, they tell us, but honesty demands that we face up to them and not try to sweep them under the carpet. This is usually the first phase of a campaign that ends in undermining some affirmative action program. They see it as a simple matter of truth and understanding trumping mush-minded sentimentality. But the laughable history of racial science is, for example, replete with grotesque dishonesty and an infusion of values of the worst sort. If faced with this dichotomy, it is easy to see why many opt for a socially progressive version of social constructivism: we make the facts, so let's make morally decent facts.

Neither of these is a very satisfying account of science. Fortunately, there is another possibility. Perhaps what is objectively true and what is objectively of value coincide. Perhaps the races and sexes are equal, as a matter of fact, not because we want them to be and somehow impose this value on our theories. There is no necessity for this; it's just a happy accident. But such evidence as there is, supports equality of cognitive ability. The author is inclined to say that previous research that lead to theories asserting inequality were not merely false, they were corrupt pseudo-sciences. There is strong historical support for this outlook. We need only compare such theories with other theories that we also now consider false: Newton's mechanics, the caloric theory of heat, and Phlogiston chemistry. Though these are false, we can in hindsight see that they were very reasonable beliefs in their day. The same cannot be said of any theory that asserted sexual or racial inequality. They aren't just false the way Newton's theory is now seen to be false. Rather the evidence offered at the time in their support was deeply flawed and, unlike the Newton case, should have been seen at the time to be deeply flawed.

We can't prevent the prejudices of individual scientists, but we can protect science from their ill effects by ensuring the most diverse community of researchers with their diverse backgrounds and biases. They stand as a check on one another. If the scientific community were organized this way, it would make the paradigm wars something like a fair fight, one that would result in the most objective results we could obtain. This is our best hope, it seems to the author, to have a science that satisfies the demands of morality and the craving we have for genuine understanding.

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