

## Constructive Empiricism and the Role of Social Values in Science

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To accept a theory is to make a commitment, a commitment to the further confrontation of new phenomena within the framework of that theory . . . and a wager that all relevant phenomena can be accounted for without giving up that theory. Commitments are not true or false; they are vindicated or not vindicated in the course of human history.

—Bas C. van Fraassen,  
*The Scientific Image*

In her book *Science as Social Knowledge*, Helen Longino argued not only that social values are in fact ineliminable from theory choice in science but also that we ought to rewrite our ideals in such a way as to incorporate this fact. One of the most common criticisms one hears of this idea of granting a legitimate role for social values in theory choice in science is that it just doesn't make sense to regard social preferences as relevant to the truth or to the way things are. "What is at issue," wrote Susan Haack, is "whether it is possible to derive an 'is' from an 'ought.'" One can see that this is not possible, she concludes, "as soon as one expresses it plainly: that propositions about what states of affairs are desirable or deplorable could be evidence that things are, or are not, so" (Haack 1993a, 35, emphasis in original). Haack does not provide an argument for the view that it is impossible to derive an is from an ought, but the intuition she expresses is strong and widespread. The purpose of

p. 164

p. 165 >

this chapter is not to determine whether this view is correct but rather to show that even if we grant it (which I do), we may still consistently believe that social values have a legitimate role in theory choice in science. I will defend this conclusion by outlining a view about social values and theory choice that is based on the constructive empiricism (CE) of Bas van Fraassen. Some questions about what role social values may legitimately play in science look different, I contend, depending on whether they are viewed from a realist perspective, according to which the aim of science is literally true description of reality, or from the point of view of a constructive empiricist antirealism, according to which the aim of science is empirical adequacy, the fit of a theory to the observables.<sup>1</sup> In Longino's account of the role of social values in science, she expressed what appears to be a realist view when articulating one of the goals she took science to have:

My concern in this study is with a scientific practice perceived as having true or representative accounts of its subject matter as a primary goal or good. When we are troubled about the role of contextual values or value-laden assumptions in science, it is because we are thinking of scientific inquiry as an activity whose intended outcome is an accurate understanding of whatever structures and processes are being investigated. (Longino 1990, 36)

Consequently, when Longino went on to argue that social values had to play a role in deciding between theories because one had to choose with insufficient evidence which auxiliary assumptions to adopt in order to have any view about which evidence was relevant to a theory, she appeared to step just where Haack insists we must not. She appeared to commit herself to the view that social preferences could play the role of reasons to believe an assumption was true, or at least could make adoption of an assumption legitimate.

As I develop CE in what I take to be the most natural way toward a view of the role of social values in theory choice, it will become clear that there is a sense in which the view I describe is different from, and not consistent with, Longino's 1990 view. Nevertheless, the conclusion I draw, that social values indeed may have a legitimate role in theory choice, is obviously a defense of part of Longino's overall claim. After defending CE's solution to the problem of social values in theory choice against some obvious objections, I will argue that an attractive realist

p. 165

p. 166 >

way of attempting to achieve something similar is not successful. This suggests that CE may be the only way to grant a legitimate role to social values in theory choice without falling prey to Haack's objection.

### 8.1 Limitations of the Objection

It goes without saying that social values are often involved in the decisions we make, consciously and unconsciously, about which subjects to inquire into and which questions to ask, and often legitimately so. Haack's objection that an ought does not imply an is does not undermine the legitimacy of social values playing a role in determining which things we learn about the world, nor does Haack think it does. But the relevance of Haack's criticism to the sorts of matters those writing about social values and science are concerned about is narrower still. The force of Haack's objection can be maintained only if it is stated pretty much exactly as she has put it: Preferences that a thing be so cannot be evidence that it is so, or a reason to think it is so. There are claims that sound similar but are found on inspection to be indefensible. For example, we might have thought Haack said that the way we want things to be cannot be relevant to the way things are (as I stated the matter in my opening sentence). But this is manifestly false if the "things" referred to are themselves social things, to take the easiest case; our preferences are the reasons many social things are as they are, though not alone reasons to think they are as they are. And the way we want things to be is clearly to some extent relevant to the way they will be in the future, even with nonsocial nature.

We might have thought Haack's objection was the same as saying that social values cannot be epistemically relevant. But if it were, then it would be wrong, because everyone knows that social values can introduce bias and that bias is relevant to whether we have good epistemic reasons. Finally, it must surely be acknowledged that the social structure of a community is relevant to how successful its members will be in finding correct answers to their questions. To take an extreme case, if the social structure of a community was such that everyone believed on authority the views of a certain individual rather than investigating any claims on their own, then it would be less likely that this community would find good answers to questions about the world than it is for a

p. 166

p. 167 >

community of independent and interacting investigators. Social structures can embody social values that are also epistemic values, though the way they function as epistemic values is in governing practices, not directly as reasons to believe a certain theory is true. Because they have an epistemic role to play, and epistemic questions are constitutive of science on anyone's view, that these sorts of social values have a function in science does not need to be defended by the sort of view I am developing. To summarize, some of the most discussed ways in which social values might be relevant to science do not fall prey to Haack's objection; I will focus on those that do.

## 8.2 Social Values in Theory Choice

According to the constructive empiricist, accepting a theory need not and should not involve believing that it is true. That is, it need not and should not involve believing that the theory's claims about all types of things—observable and unobservable—are correct. Accepting a theory should involve believing only that it is empirically adequate, that is, believing that it fits all observable phenomena, those we have actually observed and those we have not. The source of the flexibility about theory choice that I find in CE lies in the following two aspects of the view:

- (1) For a given domain, there is only one true theory, whereas there are in general many empirically adequate ones (whether we can imagine them or not). This follows roughly from the meanings of the terms.
- (2) On this view, virtues of a theory that go beyond consistency, empirical adequacy, and empirical strength do not concern the relation between the theory and the world; "they provide reasons to prefer the theory independently of questions of truth" (van Fraassen 1980, 88). On this admittedly controversial view, there isn't any kind of evidence we could get that would make it more rational to think a theory was true than to think it was empirically adequate. So, since when we might be wrong we should be wary of making ourselves wrong about more things by believing stronger claims, we are better off restricting our beliefs about theories to claims of empirical adequacy.

On standard assumptions about truth, there is at most one theory that is true of a given domain up to notational variation, and therefore at most one theory that it would be correct to believe, because believing,

for van Fraassen at least, means believing true. However, there are many theories about a domain, including many we have not thought of, that we would be within our epistemic rights to accept, in van Fraassen's sense of acceptance, because there are potentially many theories that are empirically adequate, true to all observables, in a domain.<sup>2</sup> For the constructive empiricist, social values can legitimately play a role in grounding choices among theories when these are choices among theories all of which are legitimately believed at a given time to be empirically adequate, because the choice of one among these theories is a pragmatic affair.

Constructive empiricism is not as it stands a methodological view that would tell us how to decide which theories are empirically adequate or how to choose between candidate theories. Nevertheless, it has some framing implications for these questions, because it involves a view about what we ought to be doing when we choose a theory, namely, believing that it is empirically adequate on epistemic grounds and preferring it to any rival in the same domain that we also believe to be empirically adequate, on pragmatic grounds that have nothing to do with correspondence between the theory and the world. It follows, crucially, that if we were faced with rival theories that according to our present evidence, both appeared to be empirically adequate, choosing between them would not be a choice of which theory we should believe to be true. Because we would not be choosing which one to believe true, a fortiori we would not be choosing which one to believe true on the basis of social values. Thus, if social values were among the pragmatic grounds we appealed to in choosing among theories we believed to be empirically adequate, we would not be committing the fallacy Haack inveighs against. And on this way of placing social values in the activity of theory choice, the possibility that social values (one sort of pragmatic factor) would be a reason to think a theory empirically adequate does not arise. It is assumed that the criteria for judging empirical adequacy are thoroughly epistemic, as they should be—taking the lesson of Haack's objection—because a theory is empirically adequate just in case what it says about observables is true.

It is illuminating to compare this view with Longino's (1990) picture of why social values are ineliminable from theory choice. There, social values enter into our judgment not after all current evidence is tallied in the columns of the appropriate hypotheses but before this

tallying and as a precondition.<sup>3</sup> Following the bootstrap model of confirmation, Longino concludes that confirmation is relative to auxiliary assumptions or, in words that even a nonbootstrapper could accept, that it is only by means of auxiliary assumptions that the relevance of evidence to a hypothesis can be judged. To require these auxiliaries to be “directly” confirmed—that is, confirmed without reliance on further auxiliaries—would be, according to Longino, to place unreasonable constraints on science that would disqualify much of the science we admire.<sup>4</sup> It would be feasible, she submits, only for “theories” that expressed nothing more than relationships between observables, and our sophisticated science

does not look like that. We should require, she thinks, only that auxiliaries be “indirectly” confirmed, an option that allows the influence of interests and values to enter into theory choice through the choice of auxiliary assumptions (Longino 1990, 51–52).

Constructive empiricism looks like a solution to at least part of Longino’s worry. It does not, and according to van Fraassen does not need to, place any restriction on scientific theorizing. Our constructed hypotheses may be as elaborate as we please; elaborate theories can have pragmatic significance, and searching for them may even be our most effective way of producing empirically adequate theories. However, because what we take from theories (and auxiliary assumptions) epistemically is only their observable consequences, their observable consequences are also the only parts of them that need confirmation. And according to Longino, as well as to CE, the observable consequences are more likely to be susceptible to “direct” confirmation than other parts of a theory are. There may be further questions about whether CE can sustain this focus on observables without undermining the rationale for elaborate theories, but maintaining both is the advertised view, and it should seem attractive to anyone who has been impressed by Longino’s reflections.

It seems to me that if Longino’s claim that we should require only indirect confirmation of auxiliaries can be defended in case the aim of science is truth, then it can also be defended in case that aim is empirical adequacy. Those who aim for empirically adequate theories face the incompleteness of their evidence no less than do seekers after true theories, because our actual observations do not add up to all of the observables. And if the seeker of true theories must appeal to auxiliary assumptions to show that evidence is relevant to a theory, then so must the

p. 169

p. 170 >

seeker after theories that are empirically adequate, because the latter is equally obligated to link the theories to observables. However, because empirical adequacy is truth for observables, we cannot accept a claim that social values can be reasons for believing in the empirical adequacy of a theory—on pain of the fallacy I began with—and likewise we cannot accept that they can be reasons for believing auxiliary assumptions (are empirically adequate or wholly true) either. Longino’s argument applies to empirical adequacy as it does to truth, but its conclusion is unacceptable if that conclusion is stated as I have just done. In this sense, the account I am sketching is at odds with Longino’s (1990) view.

This tension between the view I am describing and what appears to be Longino’s view arises from the impression her 1990 discussion creates that social values are being substituted for evidence and the consequent impression that we are relaxing an epistemic standard when we allow social values to play a role in theory choice. However, something of a rapprochement between the two views is possible, partly because, though I think the reading I have given Longino’s discussion is the natural one, she does not explicitly commit herself to the view that social values are to be reasons to believe auxiliary assumptions. Moreover, there is a constructive empiricist way of fleshing out some of what she does say that escapes that formulation and provides an ersatz version

of what she has claimed. Longino has rejected the constructive empiricist option I am presenting, for reasons I will elaborate and make some replies to.<sup>5</sup>

Notice that an auxiliary assumption is a claim about the world, just as a theory is, so constructive empiricists aim for any such assumption they accept to be empirically adequate. If two such assumptions have equal amounts of (incomplete) evidence for the claim that they are empirically adequate, and neither is falsified, then they can legitimately base a choice in favor of either on social values. Crucially, such a choice will not be an indication that they believe either assumption to be true, or even more empirically adequate, than the other. It will be a choice to accept and work with one rather than the other for pragmatic reasons, and it will be a choice that should be revised in light of new evidence that shows the assumption is in fact unlikely to be empirically adequate. Because empirical adequacy requires only fit with observables, it can be not only that more than one theory is empirically adequate for a domain but also that more than one auxiliary is empirically adequate

p. 170

p. 171 >

for a domain. (Of course, not all auxiliaries involve unobservables, but some do.) When our evidence is incomplete, more than one theory or auxiliary may appear to be empirically adequate. In such a circumstance, epistemic considerations would leave us indifferent, in a tie that pragmatic considerations like social values could legitimately break, in a decision that is nevertheless not about which theory or assumption to believe. Thus the tiebreaker use of social preferences works just the same for auxiliaries as it does for theories themselves. With Longino, I can say that it can be legitimate to use social values to decide which auxiliary to accept. Moreover, because which auxiliary one accepts decides which evidence is relevant to a hypothesis, this means that social values can have a role in the preconditions for evaluating evidence. However, that role is not reason to believe an auxiliary assumption, and on the CE view, it should never be the case that social values are deployed in deciding which statement to accept before our current epistemic resources on that question are exhausted. This is the precise sense of my commitment here to not condoning the entry of social values before, or as a precondition of, epistemic evaluation.

The ideal I describe demands pure epistemic judgments before social values can be legitimately taken into account in judging auxiliaries. Admittedly, there is a sense in which on the CE view the subsequent judgment of whether evidence favors a hypothesis will have involved social values, insofar as social values may have played a role in the choice of the auxiliary that determined which evidence we hold a hypothesis up to. Nevertheless, neither in that choice nor in the subsequent judgment of the hypothesis do social values act as a reason to believe a statement: The subsequent epistemic judgment is whether given the chosen auxiliary, the hypothesis is confirmed by the thereby chosen evidence. Thus, the view allows that social values have a legitimate role in theory choice, both in the choice of auxiliaries and in the choice of hypothesis, but at every stage it demands pure epistemic judgments first. The priority of pure epistemic judgments is both conceptual and temporal, because our epistemic resources at a given time must be exhausted for the



limits on our pragmatic options to be determinate.

Longino rejects this way of saving her view from the Haack objection, for two reasons. One is that she is not sympathetic to antirealism, a doctrine that, like other controversial aspects of CE, it is not within the scope of this chapter to defend. The other reason is disagreement with

p. 171

p. 172 >

the demand that epistemic and social considerations be strictly separated, that is, that epistemic judgments about auxiliaries (or anything else) be pure, and that only after we have exhausted our resources for them at a given time should social values have a role in our choice of hypothesis. The view I am describing does not accord a place to a claim that it is, in part, the very point of Longino's view to make sense of, namely, that it is unrealistic to expect epistemic judgments leading to theory choice to be devoid of social values. Thus, there is a sense in which the way that I have defended the overall Longino view, making it palatable via CE, is giving up her most important claim.

This claim needs to be taken seriously, I grant, at least for the human sciences, because it is a response to the way these sciences actually behave, and I think to a recognition that there are limits to the capacity of an individual epistemic subject to be conscious of, much less actively engaged in scrutinizing, the assumptions that come to him or her in virtue of participation in a given society, at a given place in that society.

However, I differ from Longino in my response to scientists' failures to actually maintain clear distinctions between evidence and social values as reasons to accept hypotheses. First, as mentioned earlier, if we are constructive empiricists, then a demand that we "directly" confirm assumptions that we make epistemic commitments to is more realistic than that same demand is for realists, because we make epistemic commitments only to claims about observables. At least, the demand in question is more realistic according to the constructive empiricist. This constructive empiricist response to worries about our capacity to confirm theories directly (i.e., without relying on social values) differs from Longino's in that it opts to restrict what we take as the epistemic goal of science to fewer truths, rather than alter or weaken any epistemic standard for achieving truths.

Second, while Longino is right that effective scrutiny of auxiliary assumptions requires more than that the individual think hard, have good intentions, and follow the best methodological rules, I do not think this claim is incompatible with maintaining the highest epistemic demands on individuals. Because the values that seep into our choices of auxiliaries can be social, their scrutiny requires that the scientific community meet a number of criteria that ensure that genuine criticism is happening at the level of social units, that is, communities. To have objective scrutiny of community-level social values requires interactions

p. 172

p. 173 >

between communities that qualify as critical, and so on. Let me call this

the socialist view of the regulation of epistemic choices. I will call the view that is implicit in the constructive empiricist's demand that epistemic reasons and commitments be kept distinct from pragmatic reasons and commitments the individualist view because reasons and commitments are had by individuals in the first place.

Political connotations of these terms notwithstanding, I regard the individualist and the socialist views as compatible and complementary, because the socialist view does not undermine the idea that epistemic purity should be demanded of individuals, and the individualist demand does not rule out the appropriateness of regulation at the social level. One may worry that accepting the socialist point does undermine the individualist ideal because making demands on the social aspects of a community appears to be motivated by an acknowledgment that the individual cannot be epistemically pure. Relatedly, one might think that if people actually followed the individualist ideal, then the socialist ideal would be otiose. It is not clear to me that the second claim is true because there are ways in which legitimacy can be conferred on a theory through actions or events that no individual person could be held accountable for; think of the verdicts of committees and funding agencies.

What of the first worry, that the socialist ideal undermines the individualist ideal? An easy way out of the bind is to regard ideals we want to impose on the social functioning of a scientific community as, in part, rules of application of the individualist ideal. The motivation for this would be the idea that the individual needs help, and can be helped, in achieving the individualist ideal, and this help is supplied by incentive structures at the social level. This scheme would have us regard the imposition of social standards as, at least, a method for achieving the individualist ideal, something we must do in order to improve our chances of achieving the individualist ideal. On this view, the legitimacy of the individualist ideal and an expectation of achieving it, far from being incompatible with the imposition of social standards, is one of the reasons for that imposition.

If the second claim, that proper behavior at the individual level would render social regulation otiose, is not true, then that provides a further, independent reason to call for social ideals in addition to individual ideals. The idea here would be that we can imagine every individual's behavior as being above reproach and yet the results of research

p. 173

p. 174 >

turning out prejudiced because of effects that can perhaps be regulated only at the level of the larger group. For example, it is hardly an actionable offense for an individual male committee member on a given occasion to ignore an individual female committee member when she speaks. We all daydream once in a while. However, if all male committee members ignored all female committee members whenever they spoke, then I think it is fair to say we would have a potentially prejudicing phenomenon that might require action at the level of the group.

To avoid apparent tensions between individual and social ideals, one might also further refine individual ideals. For example, the requirement that an individual's epistemic judgments be pure might be refined to the



weaker requirement that individuals have a disposition to regard their views as illegitimate upon becoming aware that they have confused epistemic and pragmatic reasons in them, as well as a disposition to become so aware. This ideal would arguably be achievable by an individual, but its achievement would not make social ideals otiose, because what an individual can become aware of is quite limited. In general, it seems to me that far from social and individual ideals being incompatible or mutually undermining, individuals actually need favorable social structures to have any hope of achieving individual ideals.

However the details may come out, it seems to me that a high probability that people will not in fact attain a (certain formulation of) an ideal is not by itself reason to give up the ideal. And an ideal is especially worth keeping if what people do achieve by means of aspiring to it is worthwhile. What we achieve by means of what I have called the individualist ideal is admittedly a topic of disagreement. Some feminists have counseled rejection of this ideal for what I think is the following reason: The existence of this ideal lulls people into believing they have achieved it when they are aware of no blatant prejudice, and this makes the ideal actively detrimental because prejudices that have a social life are often invisible.

This concern is quite legitimate, but I think that the best response to this concern is to strengthen awareness of the social aspects of a scientific community that make it critical or prejudiced, and relatedly to educate people about the fact that socially conditioned prejudice will not necessarily be obvious and may not be visible even to the well-meaning individual. So, I do not think that this concern gives us reason to give up an ideal that demands epistemic purity of the individual. The

p. 174

p. 175 >

reason not to give up the ideal is clear: There are plenty of prejudices that are visible or easily accessible to an individual's consciousness, and there is no excuse for not demanding that each of us contend responsibly with those, and keep our eyes open to others.

### 8.3 Objections to the Constructive Empiricist Solution

The CE conception I have described as a response to Haack's objection may sound fine in abstraction, but we may wonder whether it fits the way we actually see social values at work in science. Given how difficult it is to find even one empirically adequate theory for a domain, how often are we faced with two such theories between which pragmatic factors that we can recognize as the social values authors like Longino have lately called attention to will have the opportunity to be tiebreakers?

First, the need for a tiebreaker does not depend on two theories actually being empirically adequate but rather on our having some evidence that they are and (in the best case) no evidence that they are not. Second, the CE-based view I am articulating does not limit the situations in which we may appeal to pragmatic factors to cases of tiebreaking, a topic I will postpone to the last part of this section.

For now, note that apparent ties do happen in just the domains that have been discussed as most susceptible to the influence of social values. One of the most discussed cases of the intrusion of social values into theory choice at the time of Longino's book involved the "man-the-hunter" and "woman-the-gatherer" hypotheses of how the most distinctively human traits evolved in our species. To the question both perspectives regarded as pivotal about how the use of tools developed in the species, the hypotheses each have a plausible answer. On one account, male hunting provided the conditions under which having tools and cooperation gave an evolutionary advantage, and the advantage afforded by spears provided the reason that the size of the canine teeth could decrease at a certain point in time and allow humans to take advantage of diets requiring more effective molars. On the gynecentric account, the development of tool use was a response to the nutritional stress women faced as abundant forests were replaced by grasslands in which food was further afield, and as the conditions of reproduction changed to include

p. 175

p. 176 >

longer human infancy and dependency. The nutritional stress of females was greater than that of males because females fed their young through pregnancy, lactation, and beyond. Tool use on this account developed much earlier than the stone implements used in hunting, as women fashioned organic materials into objects for digging for, carrying, and preparing foods. As for the changes in human teeth, female sexual choice of more sociable partners can explain the decline in the number of males with the most aggressive-looking canines. Thus ingenuity began with the women of the species (Longino 1990, 106–8).

Both hypotheses speculated beyond the data we had, but I see no obstacle to our understanding them as possible accounts of the data we had. I do not have to be a partisan of the man-the-hunter view to be able to see that that hypothesis, along with its auxiliaries, will predict much of the evidence we have found, and I do not have to be a partisan of the woman-the-gatherer view to admit that, taken with the auxiliaries that sustain its relation to evidence, this hypothesis fit the observed observables at least as well as (and probably better than) its rival. What we have in this case is two hypotheses that the evidence at the time suggested were both empirically adequate. Thus, on the CE account, one might have legitimately thrown one's commitment behind one or the other of these two hypotheses on the pragmatic grounds that one or the other was in line with one's social values. One would not thereby have believed that the hypothesis preferred was true, and this last step would not have provided the grounds one had for believing the account to be empirically adequate. But one might have made a legitimate choice to accept one or the other hypothesis all the same.

I will consider the next two objections together. Although they come from opposite sides, my answers to the two are related. First, I can imagine someone worrying that although I have incorporated social values into theory choice, the fact that the choices I allow to be based on social values come after, and not as integral to, epistemic judgments and have nothing to do with truth, whether about unobservables or about observables, trivializes the role of social values in theory choice. The second objection, which Haack has made to Longino's account,

says that the situation I consider, where two rival hypotheses are running neck and neck as far as empirical adequacy judgments are concerned, is one where we have no right to make a choice at all because the evidence is insufficient to distinguish the rivals (Haack 1993a, 35).

p. 176

p. 177 >

The first thing to say about the first of these objections is that choices made on the basis of social values with the purpose of furthering social ends are trivial only if the social ends are trivial. A theory need not correspond with reality for its ideas and stories to have social effects—effects that we may or may not think it is good to promote. Moreover, people may regard the pictures and stories that a theory contains about unobservable items as true when some part of the scientific community accepts the theory, even if (or, on the account I am sketching, though) that is not the enlightened attitude to take toward an accepted theory, and even if it is not the attitude scientists actually take. It is legitimate, and not trivial, that we weigh the consequences of such takings to be true by the society at large when we make a pragmatic decision about whether to accept a theory that passes epistemic muster as far as we know.

The first thing to say about Haack's objection—that we have no right to make a choice at all in case of ties—is that although it would be well-suited to address someone whose claim entailed that social values could be a reason to think that one of these tied theories was true, it can gain no traction against the position I am describing. Constructive Empiricism grants that choices about whether one theory is closer to the truth or even more empirically adequate than another must be based on evidence and that we have no right to make a choice about such an epistemic matter when the evidence is (roughly) equal for two rivals.

However, the choice between two epistemically tied rivals that according to CE we legitimately make after the evidence is in is not a choice about which theory is closer to the truth, is more empirically adequate, or has more evidence in its favor. It is a choice about which theory serves better our practical goals, including social goals. And I see no reason to think that two theories whose evidence makes them equally compelling will necessarily be such that which theory we decide to work with makes no difference to social goals, or such that no one could find social reasons to prefer the one to the other. Whether a theory makes much difference to any social goals does depend on its subject matter but surely not in general on how it stands epistemically when compared with its rivals.

It may be that Haack's intention was to submit that when two theories are tied epistemically we have no legitimate grounds to do anything—including promotion of social goals—with either one. However, this

p. 177

p. 178 >

claim would be far harder to defend than the claim that social preferences cannot be grounds for epistemic choices, and it is not a claim she even tries to defend. Surely, if the positive evidence in favor of the empirical

adequacy of the theories is sufficiently great, then we have a right to use either theory on observables to serve our practical ends, where what is “sufficiently great” will depend on the level of reliability the practical goals demand. We are well acquainted with the shortcomings of Ptolemy’s model of the heavens, but astronomers use the model to this day because it is the best tool for many calculations of things around the solar system. However, even leaving social goals aside, we would have to reject the claim that we have no right to do anything with epistemically tied theories because of its long-term consequences for achievement of the epistemic goals of science, a concern that Longino’s call for a more realistic epistemology has taken very seriously.<sup>6</sup>

This is because two theories that are tied according to our present evidence may not actually be empirically equivalent, even if our evidence so far suggests that both are empirically adequate. Obviously, the two theories may coincide in what they say about the things we have in fact observed, without coinciding in what they say about everything that is observable. (I am assuming that our evidence comes only from observables.) It can be, further, that we simply do not know whether the two theories coincide in what they predict about all observables. A general proof of the empirical equivalence of two theories is sometimes possible when the theories are axiomatized and mathematical but not as readily available with the sorts of hypotheses that tend to have the most relevance to social values. In such a case, only subsequent development of the theories would tell us whether they are empirically distinguishable.

Further research may be the only route to finding out not only whether two hypotheses are empirically distinguishable but also exactly how each can be linked to observables. Kant thought the question whether the universe was finite or infinite in space and time was an unresolvable antinomy of reason, but now we understand how these questions are linked to claims that are empirically testable. Some seem ready to believe that nothing that could be observed would distinguish the man-the-hunter from the woman-the-gatherer view of human evolution (Haack 1993b, 35), but this is something we do not know and should be wary of assuming. I doubt that most of us thought we would be able to distinguish summer and winter seasonal temperature variations

p. 178  
p. 179 >

for periods 34 million years ago, that is, to find observational evidence that could be linked to a cooling trend in the winters that was not present in the summers. (Weather, like behavior, is evanescent after all.) But recently a method has been found to make such distinctions via variations in the ear stones found in a certain species of Gulf Coast fish that survived the mass extinction of that era (Ivany et al. 2000). Human behavior of the past is a matter particularly difficult to link tightly to observational evidence, but cannibalism on the part of some prehistoric native Americans has recently been claimed on the basis of chemical analysis of their preserved feces (Wilford 2000).

In any of the sorts of cases I have described, research on both rival theories will be required if we are to determine what further empirical consequences each has and how to test those predictions. If Haack’s claim is that during this period of research everyone should remain agnostic about which, if either, of the two theories is true, then CE agrees

(though that is because, according to CE, we should always remain agnostic about that). However, to demand that no scientist work with either theory would make advance on either of them impossible. And to demand that no scientist work on one theory to the exclusion of the other seems an unwarranted restriction on the division of labor. Further, though some scientists will be able to work on the development of both theories by keeping an open outlook, others will not find this psychologically sustainable. This tendency should not be viewed entirely as a weakness, either, because advocacy and competition can bring discoveries in ways that we should not want to impede. I conclude that it is not impermissible to adopt an attitude of advocacy, or what van Fraassen calls “commitment” or “acceptance” for one theory over another on the basis of social values in cases of epistemic tie.

If we are ever to know whether two theories apparently equally supported for the status of empirically adequate are indeed both empirically adequate or rather empirically distinct, and even what their empirical import is, then the scientific community usually has to do more research, and it will be to the positive good of that research if some people make a commitment to one theory while others accept its rival, more to the good than if no one makes any such commitment. If the commitment is made on grounds of social preference, so be it. The fact that commitments to one or the other of the epistemically tied theories can serve the epistemic interests of science in the long run also addresses

p. 179

p. 180 >

the former objection, which worried that my view had trivialized the theory choice that is allowed to be made on the basis of social values. Here we see that not only can such a choice have social consequences but also making such a commitment at all to one or the other theory can serve the epistemic ends of science by motivating more research.

The view I am sketching has the feature that we can acknowledge a legitimate role for social values in theory choice while not admitting that social preferences can be reasons to believe a theory true (or empirically adequate) and that we can do this without rejecting the distinction between facts and values, or between the contexts of discovery and justification, and without denying that theories have truth values. One may wonder, finally, whether the price of these features—features that I regard as advantageous—must be as extreme as some take antirealism to be. In particular, one may suspect that the epistemic tie situation in which I find a tiebreaker role for social values is a situation that the realist could find just as easily in any case where insufficient evidence is (roughly) equal for two rival theories.

There remains a salient distinction between this and the antirealist’s tiebreaker situation. The antirealist’s tied theories can both be believed to have met all the antirealist’s epistemic goals, namely, empirical adequacy (and consistency and empirical strength), because it is possible for more than one theory to do all of these things. For the antirealist, an evidence tie can be an epistemic tie in the sense just described. In contrast, we can know a priori that it is not possible for the two theories to have met all of the realist’s epistemic goals, because no more than one theory can be true. For the realist, an evidentiary tie cannot

be an epistemic tie in the sense I described for any theories that go beyond the observables, because such theories will make reference to unobservables, in the interesting cases they will differ in what they claim about unobservables, and which of two such theories is true is what it is the realist's aim to find out.

The realist's aim is to believe the true theory, and it could not be right to believe true both of two distinct theories of the same subject matter. Thus, if we appealed to pragmatic factors as a tiebreaker in such a case, we would be illegitimately substituting pragmatic factors for missing evidence. Because the antirealist's epistemic goals are more modest, it is possible for a full epistemic tie between two rival theories to arise, both in the sense that both theories in fact meet all epistemic

p. 180

p. 181 >

goals and, even more commonly, in the sense that we could have reason to believe that both meet our epistemic goals. This is why appeal to other kinds of reasons for theory choice is permissible for the antirealist. One may object to the steps I have just outlined: If we accept van Fraassen's permissivist conception of rationality, according to which rationality rarely compels us to choose uniquely among possible beliefs and we are permitted to believe anything we are not rationally compelled to disbelieve, then why must we not admit that the realist can believe whichever of the two tied theories she likes better? (van Fraassen 1989, 171ff.). Even though the realist knows both theories cannot be true, that does not mean neither can. Why is she not permitted to pick one, just like the antirealist? I think that we can be permissivists about rationality and still come to my conclusion that in the situations I have described, the constructive empiricist is in a position to appeal to social values for theory choice, whereas the realist is not.

The issue for the claim I am making is not whether the realist is permitted to believe one of the tied theories. On the permissivist account, he is allowed, but what matters to my point is on what basis the realist or antirealist may believe or accept one of the two theories. Permissiveness about rationality will allow the realist to believe one of those two theories but only on the basis that nothing rationally compels him to reject it. This does not change the realist's epistemic goal, a goal that makes evidence (broadly construed) the only sort of positive reason to believe a theory. One might put it this way: Permissiveness about rationality allows one to believe without a reason but does not change things in such a way that the realist may believe a theory citing his social values as the reason. This is because for the realist the choice of a theory is not a choice of which theory to accept but a choice of which theory to believe true. This is a purely epistemic goal, and pragmatic grounds for such a choice are inappropriate.

Since in the case I have described, the realist definitely still has epistemic questions to address—those two theories cannot both be true, and truth is the epistemic goal—other sorts of positive reasons cannot decide between them because the realist has not discharged her epistemic duty. For the antirealist, when the two theories are tied, that does not mean she knows that both are empirically adequate; for any interesting theory, there is always evidence still out. Nevertheless, it is not irrational



to believe that they are both empirically adequate on the basis

p. 181

p. 182 >

of evidence suggesting that, because it is possible that they are. (Here I not only acknowledge but also rely on permissiveness about rationality. 7) And once one believes this, other sorts of reasons, pragmatic reasons, become legitimate positive reasons for choosing which theory to accept. The realist, by contrast, can never get to the point where other reasons become legitimate positive reasons for acceptance. This is because if the realist ever got to the point where she had discharged her epistemic duty, there would be only one theory left to choose from.

What I should really say is that the realist can never get to this point where nonepistemic factors become positive reasons for theory choice unless that is where he started. I am assuming that both the constructive empiricist and the realist are committed to the priority of epistemic goals over pragmatic goals, and therefore epistemic criteria over pragmatic criteria, for theory acceptance. If we assume that both the constructive empiricist and the realist regard epistemic criteria as having to be satisfied before pragmatic criteria can become relevant, which I think is an assumption commonly made on either view when thinking of pure science, then the constructive empiricist can be let off the hook and allowed to consider pragmatic factors as relevant to theory choice, whereas the realist never can. Put differently, the constructive empiricist can allow pragmatic factors to affect theory choice while never giving up the priority of epistemic over pragmatic criteria, whereas the realist cannot.

As I mentioned earlier, the constructive empiricist does not need an epistemic tie between two theories that both appear to be empirically adequate to be allowed to appeal to social values as a reason to accept a theory. Let us consider the worst possible case: The theory he favors is significantly less well confirmed than some rival; it is an epistemic dark horse. Let us assume, to make things cleaner, that his theory has no clear counterinstances. The darkness of its prospects comes simply from a lack of positive evidence or positive success. What may the realist do? What may the constructive empiricist do?

If we assume the permissivist conception of rationality, the realist may believe the theory is true. What she may not do is believe it is true because of her social values. May her social values ever enter her choice of theories? Arguably not, stemming from the fact that her epistemic goal is to believe true theories, and I am taking epistemic goals to be primary for both the realist and the constructive empiricist. Permissively

p. 182

p. 183 >

speaking, the realist may believe the theory is true on slim positive evidence and for no other reason, but her epistemic task can never be believed to be done, except in a circumstance that leaves her with no more choices to make, that is, when she believes one theory to be true. Thus in the circumstance where the realist's epistemic task is believed to be completed (for the moment), she is left with no more choices to

make and so no choices to make on the basis of social values.

The constructivist empiricist fares differently for reasons that are already familiar. Assuming permissiveness about rationality, when the constructive empiricist is faced with two theories and one has much more evidence, though neither is certainly falsified, he may believe that the one with less evidence is empirically adequate. Given that the other has more evidence, it would be odd, then, not to grant that it probably is empirically adequate, too. (If there is a reason not to grant that, then the constructive empiricist's epistemic criteria would be enough for him to decide to accept the theory he likes, so it would not be a case of interest.) For the moment, then, his epistemic work is done, but he is still left with a choice because both theories are believed to meet his epistemic goals. So, he may choose which theory to accept on the basis of his social preferences.

To recap, there is a way of accommodating the influence of social values on theory choice without falling into the fallacy Haack warns against, an option that is open to CE but not to realism. Is there any other strategy for achieving this end that a realist could accept? Of course, I do not know all the strategies that may be open to a realist, but I will close by discussing why I think one option that looks attractive is not successful.

In this option, we accommodate Longino's claim that social values are ineliminable from theory choice in science by granting that the role of social values is significant but maintaining that it should be, and largely is, played out entirely within the context of discovery. Such values will be among the things that lead people to articulate certain theories rather than others, but rigorous testing devoid of social values is the only way a choice to believe a theory true can be justified. Maintaining in this way that social values have no legitimate role in the context of justification sidesteps the Haack objection. Constructive empiricism has no reason I can see to give up the distinction between contexts of discovery and justification, but the view now under consideration has it that this distinction

p. 183

p. 184 >

all by itself can resolve the tension that I described CE as resolving, thus making antirealism unnecessary. There are signs in Longino's text that she is confident that social values are ineliminable from the choice of auxiliaries only while a theory is under development, and they are encouraging for this proposal. After theories are developed, she admits, independent confirmation of auxiliaries is at least sometimes available (Longino 1990, 51–52).

This strategy does not sit well with Longino's dim view of the purported distinction between discovery and justification, but I think part of her disapproval can be addressed. It is true, as she points out, that the context of discovery has often been defined in terms of the psychological histories of individuals; through mysterious processes involving dreams, guesses, and other parts of mental life, people light on novel ideas (Longino 1990, 64–65). This is why the distinction is often assumed to run parallel to the distinction between psychology and logic. It tends to be assumed that the mental processes leading to discovery are

randomizing because various and unknown. Longino is fully justified, I think, in complaining about this conception of the distinction, because to define it so is implicitly to deny that which ideas and theories come into existence tends to be systematically related to the culture, social structure, or socioeconomic interests of the context in which an individual scientist works. This is to deny something that is true, in the human sciences at least.

However, the context of discovery can be defined in more careful terms: This is the context in which the genesis of ideas and theories takes place, the context composed of the causal or temporal history of the genesis of ideas and theories. The genesis of ideas has many aspects, including social ones; all aspects count. This account of the context of discovery allows us to reject as we should the presumption that the ideas and theories generated represent anything like a random sample of the possible ideas and theories. With this understanding of the distinction in mind, the proposal under consideration is this: that we regard the use of auxiliaries that have not yet been “directly” confirmed as something that takes place in the context of discovery. This means we require that auxiliaries be “directly” confirmed at some point, and we keep in mind that an auxiliary is not epistemically justified, and belief in any theory it is necessary to the confirmation of is not justified, until the auxiliary has been “directly” confirmed.

p. 184

p. 185 >

If Longino is right in her descriptive claims, then a theory’s auxiliaries often do not get “directly” confirmed until after at least the theory’s development. Those auxiliaries may not get scrutinized or justified, and hence the theory is not fully scrutinized or justified, until some time after the theory has taken on a certain life with the auxiliaries’ as yet unjustified help. This suggests an alternative diagnosis for cases in which social values have affected theory choice in science: Auxiliaries get used in the context of discovery (i.e., without “direct” confirmation) for so long and with such robust effect on the development of a theory that the attention a theory has received creates a presumption in the theory’s favor that is mistaken for epistemic warrant. This suggests that the best prescription for science is not to abandon the requirement that epistemic judgments be pure and primary but to reiterate the requirement that a theory not be considered justified until after its auxiliaries are “directly” confirmed. What Longino has observed, on this view, is people overstating their cases for their theories, a phenomenon we should speak out against and not condone.

This warning not to regard a theory as justified until its auxiliaries are “directly” confirmed, regardless of the attention it has received, is reminiscent of Haack’s insistence that when “available evidence is not sufficient to decide between rival theories . . . [t]he proper response is that, unless and until more evidence is available, scientists had better suspend judgment” (Haack 1993a, 35). However, the strategy just described that Haack’s stricture here seems to be allied with fails to resolve the problem at hand: If we refrain from regarding a theory as justified, it does not follow, as this view needs to assume it does, that everything that happens concerning that theory is rightly assigned to the context of discovery.

First, there obviously exist questions of legitimacy about a theory, even when we are not at the point of choosing whether to believe that it is thoroughly epistemically justified. Second, we give answers, even positive answers, to questions of justification, even when the answers are not full-blown belief that a theory is true, and we are obligated to give these provisional answers to evaluative questions about a theory's prospects as we understand them. The context of justification is not the context merely of the justified.

This is because development of theories is not blind to justificatory questions about the theories' worth. It could not afford to be because development requires resources, and resources must be differentially

p. 185

p. 186 >

allocated due to their scarcity. Moreover, judgments about allocation of resources for development of theories often have to be made on the basis of slim positive evidence; following the recommendation to suspend all judgment in such cases would close down science, and refraining from regarding a theory as fully justified in such a case is good but irrelevant to the evaluative questions we do need to answer. (We can even imagine a case where a judgment is necessary despite the slim evidence for two theories being tied, simply by imagining that developing either at all would require more than half of the available resources.) It is thus no surprise that explicit arguments about the prospects and worth of a project are demanded of scientists who seek funding even for development of a theory. The winning theories are judged to be to some degree, in some way, more justified than their competitors, and their consequent ability to be developed due to the funding won is an indication of that judgment.

It is wrong to think that there is no justificatory question that we ask or answer except the question whether a theory is fully justified and thus mistaken to think that development of theories is an activity that can be assigned completely or even primarily to the context of discovery. The context of discovery is a place where things just happen. Development of theories is not something that just happens. To think so looks like a way of sweeping under the rug decisions that require justification. Thus, the strategy open to the realist of acknowledging social values by confining them to the context of discovery is unsuccessful. Constructive empiricism, by contrast, allows social values to be answers to justificatory questions about which theory to choose. The questions are not epistemic, but they do not have to be.

## Notes

1. Constructive empiricism is a restricted form of realism because it grants that theories have truth values and takes the aim of science to be finding theories that fit the truth about observables. I have called it antirealism in recognition of the fact that many realists wouldn't regard this much of their doctrine as worthy of the name.

2. I say there is "at most" one true theory to accommodate the possibility that the true view cannot be expressed in what we regard as a theory. If the truth about observables could not be expressed in a theory, then there would also not be many theories that are empirically adequate, which is why I say there are "potentially" many such theories.

3. I am using the terms hypothesis and theory interchangeably.

p. 186

p. 187 >

4. "Direct confirmation" in Longino's usage does not appear to require independence from all theory, only independence from the influence of social values and freedom from the regress that threatens once we take the role of auxiliary assumptions into account.

5. Discussion at conference "Value-Free Science: Ideal or Illusion?" in Birmingham, Alabama, February 2001.

6. Though this will not be the basis of my argument, note that a theory is always epistemically tied with its notational variants, but that does not mean we refrain from using any of these theories.

7. I seem to rely on a strong dose of it: One might object that we know a priori that the probability of our coming up with two empirically adequate theories for a domain is nearly zero. We may think this, but it is too speculative to be compelling. Given permissiveness, it is enough if the antirealist does not have compelling reasons to think the two theories in question are empirically inequivalent, which he often will not when they are under development or nonmathematical.

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