

LONGINO'S THEORETICAL VIRTUES

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Kuhn (1970) provided a list of criteria that he identified as being used by scientists to aid in the choice of one theory over another. Helen Longino (1995) has offered a feminist alternative to this list, which I determine in the following paper to be a failure. Longino's errors are explained as a product of her misunderstanding of both the general function of Kuhn's list and the specific function of simplicity and related values. Broader lessons are then suggested for evaluating the feminist philosophy of science.

SCIENCE, IT APPEARS, is not a pretty business. While it had previously enjoyed an image as a paragon of epistemic virtue, as the most trustworthy and effective way of producing knowledge, that picture of science has faced serious challenges from the philosophy of science. The creation of scientific hypotheses, for example, is peculiar and unpredictable, and the criteria defining the discipline's boundaries are notoriously problematic. In 1962, Thomas Kuhn further complicated the picture by suggesting that even the processes by which scientists choose between competing theories are far from ideal, as their decisions appear to be governed in part by external – that is, non-scientific – factors (Kuhn 1962). This final attack could be considered the most potentially damaging to the status of science as a rational and objective enterprise, as the epistemic merits of science reside not in the discipline's ability to produce flawless explanations of the world – no one expects our accounts to be perfect – but in its role as the objective and effective arbiter determining which theory better represents the world. The common expectation of science is that even if the discipline fails to explain the world around us perfectly, it should be able to tell us which explanation of those currently available is most likely to be right, and this evaluation should be arrived at objectively.

While his ideas provided a definite threat to this aspect of our image of science, Kuhn himself did not mean to suggest that science was failing entirely in this respect (Kuhn 1970). Indeed, he stressed that theory choice is traditionally performed according to five major objective criteria. These are accuracy, simplicity, internal and external consistency, breadth of scope, and fruitfulness. Thus, when choosing between any two competing explanations for how the world works,

Kuhn suggested that scientists often behave appropriately, choosing one theory over another on the basis of objective and epistemologically-relevant considerations. These criteria generally were relevant for competing theories of any scale, grand or minute: whether they addressed the major questions of a field or were attempting to explain some particular detail, Kuhn's criteria provided a rational basis by which to decide which account was more likely to be right.

Kuhn was not the first or the last to attempt to define how scientists go about their business in respect to theory choice; but, perhaps for reasons more to do with the prestige of the author than the merits of his particular formulation, the criteria of theory choice as defined by Kuhn's exposition appear now to be taken as fundamental, remaining the most commonly cited criteria for theory choice despite their apparent lack of originality or significant novel insight. However, I believe that despite their elevated status (or perhaps because of it), the criteria are widely known but poorly understood. Helen Longino (Longino 1995) may be an example of this, as I believe her proposal for additions (or perhaps outright replacements)¹ for Kuhn's list of criteria show that she has misunderstood the logic behind the criteria on Kuhn's list, and has made suggestions that are untenable upon an appropriate understanding of the criteria.

Longino

In "Gender, Politics, and the Theoretical Virtues," Longino (1995) offers six criteria that, "like the elements in Kuhn's list function as virtues, qualities of a theory, hypothesis, or model that are regarded as desirable and hence guide judgments between alternatives" (385). Her suggested criteria – empirical adequacy, novelty, ontological heterogeneity, complexity of interaction, applicability to human needs, and diffusion of power – are thus clearly intended to function in the same fashion as Kuhn's criteria, as outlined above. Scientists are to compare two or more competing theories on each of the proposed criteria and declare one the victor, which is then deemed to be "right," or, more cautiously, "more representative of reality." And, as was also mentioned above, this process occurs at all levels of abstraction: in the choice between two explanations for how one particular protein turns into a different one, as well as in the evaluation of two competing accounts explaining the orbits of all celestial bodies.

Longino offers her revised guidelines for theory choice as an antidote to androcentric science. Where the values on the standard Kuhnian list are seen as inhibiting the development of a feminist science, Longino's replacements are tailored to promote just such a

body of knowledge. “Feminist,” here, has a very particular meaning: while often bandied about in philosophy as referring to “a feminine orientation to the world” or an “outsider perspective,” Longino defines these values as feminist because of “the work these virtues can do for specifically feminist inquiry” The aim of revealing gender and/or the activities of women is, I propose, what makes inquiry feminist. Feminist theoretical virtues will be those that serve this aim” (Longino 1995, 391). Kuhn’s virtues, then, are being rejected for their perceived failure to remove the stains of the West’s androcentric past from the current body of scientific theories. (Though at times Longino appears to be suggesting that many of Kuhn’s values are the stains themselves, representing an androcentric way of viewing the world – we’ll see how “simplicity” may be under attack on precisely these grounds.) The feminist revisions are here to clean up the house of science – perhaps staying on as permanent guests, as Longino appears convinced of their general utility in scientific reasoning. She is not alone in this conviction; indeed, her paper is largely a summary and defense of the theory choice criteria initially proposed by other prominent feminist philosophers of science. While this paper will focus on Longino’s claims in particular, then, the discussion also doubles as an evaluation of the general feminist position on theory choice.

Before proceeding to analyze Longino’s list, there are two issues that must first be settled. One is the point that Longino’s list is quite clearly not an index of qualities that are useful only in theory choice as an epistemological enterprise. In determining which questions in science deserve fiscal priority, for example, the “applicability to human needs” is obviously considered. Similarly, in theory creation we value those researchers who break new ground – in Longino’s terms, suggest novel ideas – instead of just working within established theories. The usefulness of Longino’s values in these more pragmatic areas, however, does not entail that they are productive when brought into a discussion of purely epistemological theory choice. The question, then, is not the general productivity of these values or criteria, but rather their worth in the specific domain in question.

The second issue concerns an alternative meaning of “theory choice.” While Longino is clear in stating that she intends the criteria she describes to function “like the elements in Kuhn’s list” in guiding epistemic judgments between competing theories, she introduces her final two criteria as being oriented instead around a more pragmatic version of theory choice. “Theory choice” in this context refers not to epistemic evaluation of competing theories with the aim of determining which theory is “right,” but to the pragmatic evaluation of which problems in science deserve more attention than others.² An example

of the former is a biologist choosing between group selectionist and gene selectionist accounts for altruistic behavior; the latter process occurs more often in the boardrooms of grant foundations deciding whether to fund, say, research on ozone depletion or the treatment of AIDS.³

This distinction between the two varieties of theory choice produces the second part of my thesis. My first claim, as described above, is that Longino's values are not relevant to the epistemologically-concerned version of theory choice,⁵ and that one would only think them to be so by misunderstanding the logic behind some of Kuhn's criteria for theory choice; my second is that Longino's criteria, while unproductive in this first version of theory choice, are in fact perfectly suited for the feminist practice of the pragmatic version of theory choice. I end with the suggestion that Longino's work may be representative of much of the feminist philosophy of science, both in its particular errors and in the path for its eventual rehabilitation.

Longino's Values

The first value on Longino's list, empirical adequacy, is defined as "agreement of the observational claims of a theory or model with observation and experimental data" (Longino 1995, 386). This appears to be a simple rephrasing of Kuhn's value of accuracy, the requirement that "within its domain" consequences deducible from a theory should be in demonstrated agreement with the results of existing experiments and observations" (Kuhn 1970, 260). Both are simply the claim that your data should match your predictions. The value is feminist only insofar as it is read as the suggestion that the criterion of accuracy/empirical adequacy has been ignored at times as a result of androcentrism, and that this has allowed sexist theories with poor empirical support to survive for an inappropriately long time. But while Longino's discussion of empirical adequacy does suggest she sees some particular merit for the criterion in a feminist context, her

conception of the value still appears otherwise identical to the traditional idea of “accuracy,” and as a simple rephrasing of an established criterion we have no cause to discuss it further here.

Novelty, Longino’s second value, is also discussed approvingly by other philosophers of science, but Longino’s application of it to theory choice is, I believe, original, and, as we shall see, probably incorrect. To be sure, novelty’s place as a virtue of some sort in science is well established. Novelty is, after all, a large part of what makes science the apparently progressive program that it is: assuming we did not explain the universe correctly on the first try, new and different theories are the only way to improve our picture of the world. The reward system in science, recognizing the merits of novelty, has honored its occurrences accordingly: the more novel a successful theory is – the more it revolutionizes the field of play – the greater the payoff reaped by its creator(s). However, the virtues of novelty do not, in the conventional thinking, apply to the theory itself, but only to the theory’s creator: a theory is no better or worse off, in our epistemic judgment, for its novelty or lack thereof.

The powerlessness of novelty to argue for a theory is shown easily enough. If two theories were to offer incompatible explanations for the same phenomena, and these theories were agreed by all evaluators to satisfy other criteria to exactly same extent – they both matched the data equally well (if in different ways), they were equally simple and broad, etc. – we would not proclaim the novel theory the victor; the order of discovery surely counts for nothing when we are considering the epistemic merits of competing theories. Other criteria are not similarly powerless: for example, two theories equal in accuracy and scope are traditionally not considered equally acceptable if one is simpler than the other. (We will explore this particular scenario in greater detail later.)

It is true, of course, that we would prefer that one theory succeed over its competitors in more than one category (e.g. simplicity *and* accuracy) before dismissing the alternatives – indeed, this is generally the case in scientific practice. But all that must be demonstrated for our purposes is that novelty would provide *no* reason for choosing between two theories when considered by itself, while other factors are not similarly powerless. If novelty cannot function alone to any extent, it appears reasonable to assume it would not suddenly gain power when combined with any other values – that is, if it cannot function singly in theory choice, we have no reason to suspect that it could function at all.

Without having any reason provided us for how novelty can provide support for a theory, we might ask why we would even expect it to. Even within a feminist context, in fact, novelty appears to be

entirely unnecessary as an added virtue for theory choice. If, as Longino suggests, many of our current theories show deficiencies as a result of being shaped by androcentrism, and are thus in need of replacement, novel theories are clearly needed to clean up the house of science. But these theories will not prove themselves to be superior to their predecessors by their mere recency or new point of view; instead their victory will come through the traditional criteria. They will simply explain the world better.

We are not finished with “novelty” quite yet, however; for while we already have good reasons in hand for dismissing the criteria as unproductive in the context of epistemological theory choice, examining why Longino proposed the value at all may help us to understand her original motivations for attempting to revise the process of theory choice.

Longino intends “novelty” to refer to “models or theories that differ in significant ways from presently accepted theories” (Longino 1995, 387); some might read this as an indication that “originality” and not “order of discovery” is what is intended, but surely the concepts are not so easily separated. Originality, after all, has temporal considerations built into its very meaning: a theory is “original” to the extent that it departs from the *previous* theory. Imagine the relations between theories X and Y: if X is the predecessor and Y the replacement, then Y is the theory with “originality.” Reverse the order of discovery, and now the difference between the two theories is chalked up to the originality of X. To praise a theory for its “novelty,” then, necessarily is to reward it for having come later in the game: there is simply no way to reward any theory but the one with the later birth date when considering this value, an asymmetry that is suspicious in its own right.

An attempt to revive the value by removing its temporal connotations could be made by renaming it “difference;” the obvious weakness of this approach is the trouble defining which theory succeeds over the other in respect to this category. In praising “novelty” the solution was simple – the new theory scores points over the old – but with “difference” the closest preservation of Longino’s intent with “novelty” seems to be to count the currently accepted theory as the one from which theories may differ in order to satisfy this value. This leads to the absurd result that any theory that has earned disfavor with the scientific community (presumably because of its failings relative to other values, such as accuracy) is now seen, at least in one sense, as superior to the accepted theory. Those theories with the greatest difference from accepted theories will generally be those with the greatest empirical failings:⁸ to fail in other categories is thus

necessarily to improve in this one, and the more spectacular the empirical failings, the more drastic the improvement in this consideration.

“Difference,” then, is probably not what Longino had in mind, and our previous discussion gives us little reason to suspect she would really think all that highly of other readings of “novelty.” The problems in identifying the appealing features of this proposed value suggest that it may not be “novelty” that Longino is looking for after all. Within her discussion of novelty, hints of what she’s really after come from her approving citations of Sandra Harding’s discussions of a “successor science,” a term that refers not simply to any “succession” of one scientific theory by another but to the very specific replacement of our current body of scientific knowledge (thought to be infected by a whole host of –isms) with a new body of knowledge produced by feminist scientists. We can see that “novelty” would be certainly be a characteristic of such a science, but a purely incidental one: the replacements for our purportedly androcentric, etc. theories aren’t gaining prestige because they’re new or different (a vastly more androcentric science, after all, might depart from our current science as much as would the non-androcentric science Harding envisions) but rather because these new theories are not androcentric. It is unclear, then, whether Longino truly cares for “novelty” in and of itself, or whether she is looking for reasons to endorse a type of theory that, given the current state of the world, would happen to be novel. It is very possible that she has simply mistaken novelty, an incidental characteristic of the types of theories she hopes to see a feminist science develop, for a feature of some significance.⁶

It was mentioned above that novelty is commonly mentioned as a virtue in discussions of science, even if Longino’s placement of it in the context of theory choice was more original. The next two of Longino’s values, ontological heterogeneity and complexity of relationship, are less frequently discussed – their promotion, as far as I can tell, is found primarily in a portion of the literature on science by feminists and Marxists.

Discussing ontological heterogeneity is an unfortunately complex enterprise, as Longino appears to have three uses in mind for the value, two that are possibly acceptable in other contexts but are irrelevant to theory choice, while the other is topical but seriously flawed. Ontological heterogeneity, according to Longino, requires that one “grants parity to different kinds of entities” (Longino 1995, 387); as the current context is epistemological theory choice, it’s clear that the value is not concerned with scientific practice – scientists are not being exhorted to make attempts to “grant parity” – but rather with truth:

theories that meet this criteria are, according to Longino, more likely to be right.

From the somewhat nebulous definition offered one could interpret Longino as suggesting one or more of the following three criteria:

i) Scientific descriptions of different entities, when interpreted by her (or our) ethics, should suggest ethical parity between the two entities,

ii) Scientific theories should not themselves declare one entity to be more valuable or ethically better than another entity, and

iii) Scientific theories should treat individual differences as causally and explanatorily important.⁷

The first and second values are the two with no relevance to theory choice, unless theory choice is reconceived as an entirely different enterprise. If theory choice is to remain a process concerned only with epistemology and not ethics, the first two values simply have nothing to say about the process. For example, if the public would see homosexuality as morally inferior to heterosexuality if biologists discovered that the former is caused by stress experienced by the mother during gestation, that counts for nothing against the maternal-stress hypothesis: ethical interpretations of scientific statements of fact (using “fact” as in the fact/value distinction, not in the lay sense of “indisputable truth”) are irrelevant to the truth value of these statements. Ontological heterogeneity functions equally poorly in its second reading, as the scientific theories themselves (if properly stated) can never make statements of value. For example, even the scientific positions of the Nazis are read appropriately only as making factual claims (however dubious they may be). Values may infuse scientific writings, and they may even inspire scientific claims, but the claim itself is always one concerned purely with fact – any theory of which this is not true is simply not a scientific theory. Thus, the claim:

Golfers score lower on valid measures of intelligence than bowlers, which is to say that golfers are dumber than bowlers, and therefore are inferior people

is not itself a scientific (i.e. factual) claim, as the conclusion relies on the values of the claimant – in this case, his determination that greater intelligence makes one “superior.” With the clause concerning inferiority removed, however, the claim is now a scientific one, even

with the seemingly derogatory “dumber” clause included: while many may personally translate the factual term “dumber” into the value-laden “inferior,” this does not change the purely factual nature of the former description. Ethics, then, would seem to have no business involving itself in epistemological theory choice: claims that themselves involve ethics are not scientific, and the ethical interpretations one could draw from purely factual claims are irrelevant to the claim’s truth value.

The first two readings of ontological heterogeneity, then, cannot function as criteria for theory choice without substantial revision to the meaning and purpose of the process. To incorporate the third reading of the criteria would not require similar revisions to the process of theory choice, but instead would make the process disastrously less effective, perhaps to the point of paralyzing epistemological theory choice.

This reading of ontological heterogeneity amounts to the suggestion that theories are better when they have more entities than their competitors, a necessary product of Longino’s commitment to treat “individual differences as important” for their own sake (Longino 1995, 387). Longino’s position apparently suggests that one encourage additions even when the extra entities do not improve explanations or predictions. Her statements appear inconsistent with the much milder and more defensible claim that “individual differences often end up being valuable in our theories (say, for the greater accuracy they impart), and we should investigate whether or not our current theories take individual differences sufficiently into account,” because this claim is no longer about *epistemological* theory choice, but about more general scientific practice and theory creation – that is, more pragmatic considerations. It is one thing to value the development of a research program in a particular field that looks more carefully for individual differences among seemingly similar objects – and this is exactly what many feminists do encourage in fields like genetics, where Barbara McClintock’s careful attention to individual corn kernels produced important discoveries. It is quite another to compare two competing accounts of the same phenomena and suggest that the theory postulating a greater number of causally significant entities (say, epicycles, deferents and the like, rather than a simple heliocentric orbit) is superior to its competitor *because it postulates the extra entities*, rather than as a result of the greater predictive accuracy such postulations would hopefully induce. As Longino offers ontological heterogeneity as a value for the process of choosing what theory better represents reality, she is tied to the latter suggestion concerning the nature of good theories.

We can reuse the approach we employed to evaluate the merits of

novelty once again to consider the merits of this reading of ontological heterogeneity. Simply consider a situation in which two theories provided equally accurate and broad accounts of some feature of the natural world: the only difference between the two is that one is a simpler explanation – that is, invokes fewer objects and processes. When considering novelty, the results of this same exercise suggested that novelty was an unproductive criterion: there was no reason to think that employing it improved our ability to choose the theory that best represents the workings of the world, which is the goal in scientific theory choice. For ontological heterogeneity, however, we get even more extreme results: using this as a criterion does not simply fail to add anything, but rather it actually hurts the process of theory choice. The theory that explains less with more is traditionally seen to be at a disadvantage to more efficient theories, but valuing ontological heterogeneity turns this maxim on its head.

Individual differences, contra Longino, *are* to be ignored whenever possible, as to fail to do so allows the unnecessary multiplication of entities in one's explanations of the workings of the world – that is, one's theories would become unnecessarily complex. Furthermore, not only will this value of heterogeneity cause problems by encouraging unnecessary complexity, but it also brings no benefits to the table to balance the score. Any theory that ignored significant individual differences would *by definition* perform less well in other categories (particularly accuracy) than a theory that took account of these differences⁵: therefore, ontological heterogeneity is superfluous in all situations in which it would score points for the right theory, and counterproductive in situations where the simpler theory was the better one.

Longino's fourth proposed value for theory choice, complexity of relationship, suffers from the same weakness as ontological heterogeneity. Longino intends this value to argue for "theories that treat relationships between entities and process as mutual and as involving multiple rather than single-factors;" theories taking this criterion into account will produce "models in which no factor can be described as dominant or controlling and that describe factors in which all active factors influence others" (Longino 1995, 388). Satisfactory examples of theories that would perform well by this criteria are hard to come by, as advocates of this value focus on its worth in fields where the disentangling of cause and effect is notoriously difficult (e.g. neuroscience), while skeptics would point out that the application of the value would appear absurd in systems where causal relations are better understood (e.g. the movement of balls around a pool table). Generically, then, theories exhibiting complexity of relationship are

ones in which the causal arrows loop all over the place: A causes B; B causes C; C causes D sometimes, and sometimes loops back to affect A; D sometimes changes B so that it causes E instead of C; etc. As we shall see, valuing complexity of relationship is as problematic as seeing inherent worth in theories that exhibit ontological heterogeneity; unlike novelty, the valuing of which could be seen as relatively harmless (if still unproductive), valuing complexity of relationship in a theory actually damages the process of theory choice.

We may start on a conciliatory note: it is entirely possible that many natural processes work in complex, mutual fashions, as advocates of this value often insist.⁹ But even were we to assume this worldview to be the case, the practice of valuing theories for their invocation of complexity is simply mistaken, and faces the same problems as ontological heterogeneity. If relationships are complex in the natural world, then theories appropriately taking this into account will necessarily succeed over theories that oversimplify when they are evaluated by more traditional values of theory choice, like accuracy. If two compatible theories are on par as far as accuracy and other values are concerned, the one postulating extra relationships and objects is traditionally the one that loses, and advocates of this new value have provided no argument for why this should not be the case.¹⁰ Any theory can have its relationships needlessly complicated, but if the extra complexity brings no benefits I can see no motivation for considering the apparently powerless additions.

Before completing our dismissal of ontological heterogeneity and complexity of relationship as possible additions to a list of values for theory choice, a brief examination of Occam's razor will both show the problems with accepting these as valid criteria and suggest a possible reason why Longino mistook them to be appropriate. The principle in question – that entities should not be unnecessarily multiplied – is at the core of both simplicity and breadth of scope,¹¹ the two values that Longino sets out as roughly opposed to ontological heterogeneity and complexity of relationship, respectively. At the root of Longino's defense of the latter two values seems to be her belief that the world works in complex ways, but the traditional acceptance of the descendants of Occam's razor is not similarly motivated. In fact, it is entirely consistent to be committed to a worldview where interactions are complex and individual differences all significant and yet still privilege the derivations from Occam's razor over those that, at least on the surface, appear to match better one's ontological commitments to complexity and heterogeneity.

A simple thought experiment demonstrates why the practice of valuing simplicity in one's theories is unrelated to any suspicions that

the world works in simple, straightforward ways (or, for that matter, in complex and confusing ways). Imagine a set of competing theories, A, B, and C, the first of which produces a picture that is overly simple and homogeneous, the second of which describes the world “as it actually is,” the third of which has multiplied entities beyond what B describes. Experimentation should be able to show the difference between A and B, but not necessarily between B and C. Other criteria (e.g. accuracy), then, can always do the work heterogeneity and complexity of relationship are supposed to do (they can differentiate between the inadequate A and the correct B), but the latter two criteria actively interfere with simplicity when it tries to do the work accuracy cannot – specifically, differentiating between B and C.

The value of simplicity, then, is not motivated by any ontological commitment – in fact it is entirely and equally consistent with any worldview – but rather by the epistemologically concerned realization of the dangers of unnecessary complexity. We are now prepared for the first part of my thesis, then: Longino, in proposing values that oppose simplicity, may have simply mistaken the logic for the criterion. If preferring simplicity made sense only for those with commitments to a simplistic ontology, then those with opposing commitments would be free to propose opposing values. Indeed, Longino describes “a metaphysical certainty that this is the way the world is” (Longino 1995, 388) as the grounds that many authors have taken to defend their support of complexity of relationship; in the minds of traditional scientists these feminists doubtlessly see a similar commitment, but to a simplistic rather than complex picture of the world, and these authors have apparently assumed that science’s employment of “simplicity” as an epistemological tool derives from this opposing metaphysical worldview. The ontological commitments of Western scientists throughout the ages is indeed an interesting topic for investigation, but it should be clear by now that the results of such inquiries are entirely unrelated to the justification for values like simplicity in an epistemological context. Neither the motivation or function of simplicity is to promote any particular ontology, and thus the criterion simply cannot be attacked on ontological grounds: the question of whether or not the causal interactions in the world are complex or not is simply irrelevant to the worth of simplicity as a virtue in theory choice. That Longino and other feminists may differ from traditional scientists in these ontological commitments, then, is no reason for them to abandon the traditional value, especially having seen the apparent counterproductivity of their proposed replacements.

Longino’s final two suggested additions for theory choice are applicability to current human needs and diffusion of power. It is these

two values that suggest to me that Longino's use of "judgements between alternatives" is a problematic mix of the epistemological and pragmatic version of the concept, as the application of these two values to epistemological theory choice seems impossible to render sensibly. It is hard to imagine a person of scientific mind suggesting that one theory of a natural phenomenon – one explanation of how point mutations occur, say – is correct because to believe so would lead to political equality. Furthermore, the products of a theory in terms of meeting current human needs depend entirely on the theory's being right; wishful thinking helps not a bit. (Imagine the following reasoning: "Sugar pills are cheap and easily distributable; if they functioned as a cure for AIDS, the world would be a much better place, and these considerations give some measure of support to the thesis that sugar pills do in fact cure AIDS.")

This seemingly uncharitable interpretation – Would a philosopher seriously suggest that our AIDS researchers should incorporate considerations of what they *wish* were the case in their attempts to decipher what truly is the case? – really does appear to be what Longino, at times, has in mind. As she puts it, "when faced with a conflict between [my political commitments] and a particular model [I] allow the political commitments to guide the choice" (Longino 1990, 190-191). This quotation comes from her discussion of the problems of brain research conducted by males: she justifies her willingness to reject the results of any brain research that suggests innate cognitive differences between males and females (results which she finds politically objectionable) by offering as a general rule the idea that political commitments can legitimately guide choices between alternative accounts of reality.¹² From this general rule we can derive the example of our confused AIDS researcher: his political commitments suggest the desirability of equal and immediate access to a cure for AIDS for all victims of the disease, regardless of income, but he realizes that the treatments are currently both expensive and inadequate. If a sugar pill were to effectively counter the disease, though, this politically undesirable reality would not be the case. Because of these considerations, if we are to take Longino's claims at face value, the AIDS researcher has now produced some (which is not to say "sufficient") justification for believing it to be the case that sugar pills cure AIDS – justification of the same, epistemological form (though not necessarily the same strength) as a successful clinical trial.

The utter implausibility of such a claim, I trust, will be readily apparent to the reader. However, the values at the base of the seemingly incorrect claims described above may in fact work much better in a different context, in which we read "judgments between

alternatives” to refer to the pragmatic choice of which scientific theories and fields to invest in. Indeed, Longino’s other three original values (novelty, ontological heterogeneity, and complexity of relationship) are much more intelligible in this light as well. For a feminist (in the peculiar, philosophical usage), at least, it makes perfect sense to ask grant foundations to encourage research from novel viewpoints in established fields, specifically searching for ontological heterogeneity and complex relationships. If the current body of scientific knowledge is sexist, particularly in the way it ignores heterogeneity and complex interactions, then this research should produce new theories that, in having corrected the overly simple theories of the past, better represent the world.

Longino quite possibly had something like this in mind all along, but mistook the appropriate way to go about getting feminism’s “successor science.” While such theories would certainly be new and original, their novelty is a purely incidental characteristic: we needn’t suggest that valuing novelty in a theory itself is a way to get better theories, as Longino does. At the same time, if feminism’s metaphysical presuppositions are correct, we might also expect these new theories to be more heterogeneous and complex than our current stock; but we have seen that taking heterogeneity and complexity as epistemological values is nevertheless counterproductive, and relies on a misunderstanding of the logic for taking simplicity as such a value. There is no reason, then, why developing a “successor science” requires a radical reworking of standard epistemological theory criteria.

Roughly, then, our appraisal of Longino’s project thus far runs as follows: Longino and other feminists believe they have differing ontological commitments than do traditional scientists – namely, feminists are inclined to see the world as working in more complex ways than scientists have previously supposed. They see science as having both favored research programs that assume a simplistic, direct world (pragmatic theory choice), and as having allowed these ontological assumptions of simplicity and consistency to be reflected in the set of criteria by which they choose between competing theories (epistemological theory choice). On the grounds of feminism’s differing ontological commitments, Longino and others have proposed that the set of criteria used in epistemological theory choice be replaced by values derived from their ontology. This suggestion, we have seen, was predicated upon a mistake: the criteria traditionally employed in epistemological theory choice were not derived from any metaphysical picture, and are equally valid for those with any and every ontology. But if epistemological theory choice is unaffected by metaphysical concerns, pragmatic theory choice is not, and this is where Longino’s

criteria find a more appropriate application.

The relevance of metaphysical beliefs to the choice of a research program can be demonstrated by considering a case study. In evolutionary biology, adaptationist researchers generally share an ontological picture in which natural selection is seen as a remarkably effective agent in providing adaptations for organisms. This ontology has led them to follow a research program focused on picking out various characteristics of living things and determining adaptationist histories and functions for them. Opponents of adaptationism – those who think natural selection was a relatively weaker force in evolution – see this research program as a waste of time, and unlikely to produce significant results. Different ideas of how the world works can thus lead very legitimately to support for very different kinds of research programs.

Longino's values, then, have potential when read as suggestions for directing research instead of for evaluating theories. This is not, of course, to say we must already concede their worth in this pragmatic context – I, for one, do not see our current stock of theories as reflecting sexist or overly simplistic presuppositions, and so would oppose focusing resources on the development of an alternative research program. (As was the case with adaptationism, then, we see how differing ontological commitments provide very good reason for disagreeing over how to direct research, even if they do not argue for one criteria of epistemological theory choice over another.) Still, if I have been at all successful in my project, it should be clear by now that the values on Longino's list (with the exception of empirical adequacy, which is also on the traditional list) function only in this second, pragmatic version of theory choice. Her values simply cannot function as Kuhn's do, in helping choose which account to accept as the correct explanation of a given phenomenon.

This result, I believe, is neither surprising nor unique. If my thesis is correct, Longino has proposed unworkable revisions as a result of her mistaking the motivations and logic for various aspects of the standard scientific project. Her misunderstanding has led her to believe erroneously that the criteria of theory-choice should have a basis in one's ontology. In this misunderstanding, and in the type of changes required to save the project, I believe that the essays in question are representative of their genre: recent works in the feminist philosophy of science are often directed at revising the scientific process, and I believe them to be universally mistaken in this aim. Science, it would seem, works wonderfully as it is, and its incredible instrumental success licenses a strong skepticism towards any claims to have discovered a better way of figuring out how the world works.

Contributions like Longino's, I hope to have demonstrated, are of no worth when considering topics like effective scientific practice; but they have a much more reasonable claim to a role of some sort in general research direction, similar to the case of adaptationism in evolutionary biology. Like other similar positions, this position would be subject to empirical review: adaptationism will stand or fall on whether its researchers are successful in discovering new functions, and the feminist oversight process should be discarded if it fails to turn up any sexism-induced flaws.¹³ Nonetheless, for the moment, these values are at least tenable when taken as overseeing considerations that scientists ought to keep in the foreground of their minds, rather than as suggestions for a more direct revision of the scientific process.

NOTES

¹ I remain unclear as to exactly how she intends her criteria to function. Her criteria could simply be added to the traditional (i.e. Kuhn's) list, perhaps replacing a few of the original values, but I believe she may intend her list to completely replace Kuhn's. The only original criteria to go unchallenged by Longino is accuracy, which she preserved on her list under the name of "empirical adequacy," a point that we will get to shortly.

² A distinction between epistemic and pragmatic virtues was also made by Van Fraassen (1980); readers familiar with his account will recognize that I am using the terms in an entirely different sense. Instead, I hold to Longino's terminology, where pragmatic theory choice is a question of how to direct research. "Epistemological virtues," in my discussion, are all those values which, when employed, would regularly lead scientists to discover more true theories. Van Fraassen rightly points out that values like breadth of scope (concerned with how many areas a theory's relevance reaches), fruitfulness (favoring the discovery of new phenomena and relationships rather than simply explaining previously existing data) and simplicity "do not concern the relation between the theory and the world," strictly speaking (Van Fraassen 1980, 88). Inconsistencies within a theory, or between it and experimental evidence, are more obviously related to such a relation, and for this reason Van Fraassen separates logical consistency and accuracy from the previous values. But Van Fraassen is wrong when he claims that simplicity and like values can only "provide reasons to prefer the theory independently of questions of truth" (88), for the nature of the scientific process is such that scientists employing these values will in fact get a greater number of true theories. These values are necessary to license our disapproval of endlessly modified theories, a disapproval that is not motivated by aesthetic or pragmatic concerns, but by the realization that endlessly patched-up theories have simply never turned out to be right. While Van Fraassen could thus maintain a distinction between values directly and indirectly related to the truth of theories (the latter gaining relevance only because of the nature of scientific investigation), I claim that both categories exist as subsets of "epistemological values."

³ It could be objected that what is being chosen between here are not "theories"

per se, but rather particular problems or areas of research; “pragmatic problem choice” would perhaps more accurately reflect the nature of the process, but I have chosen “pragmatic theory choice” to keep with Longino’s focus on her proposed values’ relevance to theories.

- ⁴ Again, I am using pure “epistemology” to mean that pragmatic factors do not come of account.
- ⁵ We may take gradualist evolutionary thought as our paradigmatic “accepted theory:” the strong version of punctuated equilibrium shows a moderate extent of difference, and its empirical deficiencies are relatively small compared to standard evolutionary theory; literalist creationist theory differs far more dramatically from accepted theory, and with this difference comes a concomitant radical empirical failure. The claim, I believe, is not an artifice of the examples chosen: it is fairly intuitive that theories that are similar in meaningful ways will get relatively similar results. While there are, no doubt, some situations where there are empirical peaks and valleys – theory A will achieve empirical success, as will the vastly different theory C, while theory B of intermediate difference between A and C will be a dramatic failure – I suspect that these situations are less common than that of theories moving gradually closer and closer to a single empirical peak. To differ greatly from a theory that has already climbed a good way up one of the relevant empirical peaks, then, is most probably to have fallen off the peak altogether into a valley of empirical failure.
- ⁶ The idea that Longino is proposing unworkable modifications to theory choice solely for their perceived ability to bring about a particular kind of science will be raised again later.
- ⁷ Longino apparently has some conflation of two or three of these criteria in mind: her attention to McClintock’s work demonstrates her support for a value along the lines of the third reading, but she also wants ontological heterogeneity around to beat back “theories of inferiority;” her discussion suggests “inferiority” here has an ethical reading, which implies support for either of the first two readings.
- ⁸ The idea is that if any individual difference is significant, the difference must be one that could be exposed through empirical examination; if the difference can be so examined, then the criterion of accuracy is able to separate the good, difference-acknowledging theory from the inadequate, over-generalizing theory. (McClintock’s theories that took good account of the differences between individual kernels of corn succeeded in just such a fashion.) Any difference that could not be discovered upon empirical examination is therefore labeled insignificant.
- ⁹ Though the problems in such a concession are apparent enough – the terms just don’t seem to have much meaning in this kind of context, due to the limitless nature of complexity (how much mutualism is enough?) and the difficulties inherent in attempting any objective determination of the extent of complexity.
- ¹⁰ This is not entirely true: Longino notes that other authors proposing this value often defend it on the grounds that it “expresses a female quality of apprehension,” or claim that it derives from their “metaphysical certainty that this is the way the world is” (Longino 1995 388). The first argument very obviously requires a considerable amount of work to be taken seriously – even if the claim concerning the nature of female cognition is

correct, we're still missing reasons for why this pattern of thinking is superiorly conducive to finding scientific truth. The second argument is addressed later in this paper.

- ¹¹ Breadth of scope is defined by Kuhn as the quality where "a theory's consequences extend far beyond the particular observations, laws, or subtheories it was initially designed to explain" (Kuhn 1970, 261). This is essentially a combination of a Popperian appreciation for surprising predictions, and simplicity at a multi-field level: if one proposes a theory that helps explain so much that its power extends outside the discipline in which it was created, the theory is obviously explaining a lot with relatively little, which is to say it is simple.
- ¹² Perhaps the most developed part of Longino's contributions to the philosophy of science are her arguments on this topic, where she claims that "contextual" values, such as political commitments, influence theory choice as legitimately as do "constitutive" values (e.g. accuracy and simplicity). Evaluating these claims on theoretical grounds requires more discussion than space permits here (but cf. Couvalis 1997), but the brief examination of the consequences of the claim that follows below leads quickly to a *reductio ad absurdum*, which I believe licenses a general skepticism towards the merits of her position.
- ¹³ See *Higher Superstition* (1994) by Gross and Levitt for a good discussion of the problems in some stock examples of sexism in science.

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