"Empiricism all the way down": a defense of the value-neutrality of science in response to Helen Longino's contextual empiricism

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A central claim of Longino's contextual empiricism is that scientific inquiry, even when "properly conducted", lacks the capacity to screen out the influence of contextual values on its results. I'll show first that Longino's attack against the epistemic integrity of science suffers from fatal empirical weaknesses. Second I'll explain why Longino's practical proposition for suppressing biases in science, drawn from her contextual empiricism, is too demanding and, therefore, unable to serve its purpose. Finally, drawing on Bourdieu's sociological analysis of scientific communities, I'll sketch an alternative view of scientific practice reconciling a thoroughly social view of science (such as Longino's) with a defense of its epistemic integrity.

Introduction

Objectivity, in the sense of value-neutrality, is commonly taken as the hallmark of scientific knowledge. But this claim to objectivity has been recently under great stress. Numerous historical and sociological studies focusing on fine-grained details of scientific practice have drawn a portrait of science quite different from the smooth image of an enterprise largely immune to the specificities of its historical and social context. "Contextualize!" has become the new motto.

"Contextual" (i.e. external, non-"truth-seeking") values may shape scientific knowledge to the extent that they play a role in the definition of research programs, in the choice of questions deemed scientifically inter-

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esting, in the way scientific results might be applied, etc. This contextualization of the *goals* of science does not in itself threaten objectivity.[1] More epistemologically challenging is the distinct charge that the very *content* of scientific knowledge is shaped by contextual values. What is at stake here is the epistemic integrity of science, that is to say, its capacity to screen out the influence of contextual values on its content. A radical version of the attack against epistemic integrity contends that rules of "good" scientific practice, *even when properly applied,* fail to do so. Otherwise put, cases of scientific research producing non value-neutral results cannot always be seen as (unfortunate) departures from properly conducted science: non value-neutral science may sometimes qualify as "good" science, hence the alleged loss of epistemic integrity.

I will focus in this paper on one of the most influential and worked-out versions of these 'radical content' critiques, to wit, Helen Longino's "contextual empiricism" (Longino 1990, 2002). The reason for this choice is twofold. First, Longino's contextualizing approach fits squarely into the empiricist tradition, thereby making her critique an "insider" critique more challenging for proponents of epistemic integrity: " [. . .] observational and experimental data, writes Longino, constitute the least defeasible grounds of theory assessment". (1996, p. 39) Second, not only Longino provides a *precise* mechanism by which the influence of contextual values is supposed to operate, but she draws from it important political conclusions by attending to how science should be organized, given the impassable influence of contextual values on its very content, to maximize epistemic success and minimize biases. For anyone concerned with issues of value-neutrality and epistemic integrity in science, Longino's thesis is thus challenging on both epistemological and practical grounds.

My discussion of Longino's contextual empiricism will be both critical and sympathetic. The critical part of it will operate at two different levels. I will first argue that even if Longino is right to claim that non value-neutral science may sometimes qualify as "good" science, this is not sufficient to dismiss the epistemic integrity of science, given the ambiguity of the notion of "good" science. Second I'll explain why Longino's practical proposition for suppressing biases is too demanding and why there are serious grounds to be skeptical about its ability to serve its purpose. Finally,—and this is the sympathetic part of my discussion—I'll suggest an alternative way of keeping biases at bay in science that incorporates some of Longino's important insights.

1. See for instance Kitcher 2001, chap. 6 on how scientific significance may depend on contextual values.

Longino's contextual empiricism

According to Longino, what is not immune to contextual values is the *acceptance and justification* of scientific hypotheses. More precisely, Longino argues that "there are standards of rational acceptability that are independent of particular interests and values but that satisfaction of these standards by a theory or hypothesis does not guarantee that the theory or hypothesis in question is value- or interest- free." (1990, p. 12) How could that be? The punch line in Longino's answer is that evidential relevance of data depends on background assumptions or beliefs. By background assumptions or beliefs, Longino means "beliefs in light of which one takes some *x* to be evidence for some *h* and to which one would appeal in defending the claim that *x* is evidence for *h*". (1990, p. 44) These beliefs are not general principles of inference but contextually located assumptions that play the role of "enabling conditions of the reasoning process in much the same way that environmental and other conditions enable the occurrence of causal interactions". (1996, p. 44) Complex examples of the influence of background assumptions in the choice between competing hypotheses are presented in a detailed analysis of several models on the biological bases of sex difference in temperament, behavior and cognition. (Longino 1990, chapters 6 and 7 ; these examples will be discussed in detail in the next section) All cases are supposed to illustrate the fact that "no purely formal relations can be established between [data and hypotheses]. Evidential relevance of data is secured instead by background assumptions, with the consequence that the same data can in different contexts serve as evidence for different hypotheses". (1996, p. 39) The mediating role of background assumptions is thus what opens up space for the influence of contextual values in theory choice: "background assumptions are the vehicles by which social values and ideology are expressed in inquiry and become subtly inscribed in theories, hypotheses, and models defining research programs." (1992, p. 204) Note that this mechanism is not the only way by which contextual values may shape scientific knowledge. Longino lists four other ways, involving 'practices', 'questions', 'data' and 'global assumptions'. (1990, p. 86) But as regards the issue of epistemic integrity, the mediating role of background assumptions is the most challenging and the only one directly relevant.[2]

It is important to note that the charge is essentially about the *theoretical*

2. I leave aside for the moment another, indirect way by which contextual values may shape scientific knowledge, to wit, the fact that traditional epistemic values such as empirical adequacy, consistency, breadth of scope, etc., may have political valence. This point will be discussed at length in the last section of this paper.

impossibility of value-neutral results. It is not only about what doesn't happen *in practice* in science, it is about what cannot happen even *in principle,* in ideal cases of properly conducted research. Proponents of the epistemic integrity of science may grant that perfectly value-neutral results are never or very rarely obtained in the actual development of science, for all that, value-neutrality remains the aim, and the way to reach this aim would be to stick as much as possible to canons of scientific research whose sources are exclusively epistemic values (i.e. "truth-seeking" or, in Longino's terminology, "constitutive" values). This normative stance is usually the one endorsed by practicing scientists. The normative stance adopted by 'radical content' critics such as Longino differs as regards both the aim of scientific inquiry and the way to achieve this aim.

Satisfaction of traditional epistemic values and, of course, elimination of biased research are still on the agenda, but value-neutral research is not any more. Why? Because *aiming* at value-neutral research by sticking to traditional methodologies is not for Longino the most efficient way of increasing empirical success, nor is it the most efficient way to get rid of biased inquiries. Recall Longino's essential claim that background assumptions play a role in evidential relations, thereby making the acceptance and justification of scientific hypotheses not immune to contextual values. For Longino the efficiency of any remedy against biased research hinges on this insight: "good" science does not imply value-neutral science. Cases of biased research may qualify as "good" science. At first sight, this considerably complicates the task of getting rid of them. If simple dismissal as "bad" science is of no avail, what should be done?

Longino's answer is mainly built on her analysis of the role of background assumptions in evidential relations. In a nutshell, since background assumptions are what permit the expression of ideologies in scientific inquiry, they should be subjected to appropriate criticism. This criticism can only operate at the level of a community. As we shall see later in this paper in more detail, a crucial point is that Longino allows for the expression of political interests in what counts as "appropriate" criticism of background assumptions. But let us first see if acknowledging the existence of "good", non value-neutral results leads to the abandonment of the epistemic integrity of science.

What is "good" science?

To back up her contention that biased research may qualify as "good" science, Longino gives us a detailed analysis of several cases of research on sex differences in behavior, temperament, cognition and evolution. To count as an effective support of her contention, these examples must meet two obvious conditions: i/ the content of the results of the inquiry must indeed

be shaped by contextual values (in the examples discussed by Longino these values are mainly sexist ideologies) and, ii/ such inquiries must nevertheless meet the criteria of "good" science. The first condition is hardly problematic and the case studies analyzed by Longino meet it quite convincingly. The second condition is much more challenging and complex, for the notion of "good" science—as attested by the widespread use of commas in the expression—is thoroughly an ambiguous one. Let us spell out why by focusing first on one of Longino's key examples of research on sex differences.

The example belongs to the field of study of human evolution. I chose it since its relative simplicity most clearly puts to the fore the ambiguity of the notion of "good" science. To put it briefly, the scientific issue at stake is to come up with an account of how anatomical and behavioral development contributed to the emergence of the human species by the process of natural selection. Two accounts compete, reflecting two very different assessments on the relative contributions of males and females to the evolution of human species. Not surprisingly, in the androcentric "man-the-hunter" account, the changing behavior of males is the key factor, whereas in the gynecentric "women-the-gatherer" account, the changing behavior of females plays a crucial role.[3] Both accounts claim evidential support from fossil records, but from a close and critical look at these evidential supports, Longino concludes that: "none of the admissible data [. . .] provides any sort of decisive or even unequivocal evidence for or against either of the two accounts." (1990, p. 109) Why is that so? Because "how the data are read depends on whether one is working within the framework of man-the-hunter or woman-the-gatherer." (1990, p. 109) Her complete diagnosis is the following: "Each perspective assumes the centrality of one sex's changing behavior (or "adaptative strategies") to the evolution of the entire species. Neither assumption is apparent from the fossil record or dictated by principles of evolutionary theory. Each is an example of a contextually driven background assumption facilitating inferences from data to hypotheses." (1990, p. 107)

The punchline in Longino's argument is thus to say that satisfaction of the criterion of empirical adequacy involves empirically unwarranted, contextually driven background assumptions. In other words, contextual values are what enable a verdict based on constitutive values to be reached. Hence the possibility of "good", non value-neutral results: the "man-the-hunter" account did fit the data, had explanatory power, etc., thereby qualifying as "good" science, but since its evidential support relies upon

3. For more details and scientific references, see Longino 1990, pp. 106–107 and Tuana 1995, pp. 453–457.

male-centered background assumptions, its value-neutrality is not guaranteed by its status of "good" science. Longino's analysis shows that "good" science is indeed an ambiguous notion, having both an epistemological and a social dimension, since what counts as sufficient epistemic warrant for a community actually depends on value-laden background assumptions (often implicitly) adopted by the community.

The key issue now is whether or not the relinquishment of epistemic integrity follows from this diagnosis. I will argue it does not. Recall that the epistemic integrity of science consists in its capacity to screen out the influence of contextual values on its content. Does the existence of "good", non value-neutral science is sufficient to deny this capacity? In fact, Longino has only established the *weaker* claim that satisfaction by an hypothesis of constitutive values such as empirical adequacy does not always guarantee its value-neutrality. But what she needs to establish to deny epistemic integrity is the *stronger* claim that background assumptions cannot be critically assessed *on constitutive grounds.*[4] Let us see if the examples provided by Longino succeed in backing up this stronger claim.

Consider first the fact that day and night alternate. In itself, Longino argues, this observation does not count as evidence for any definite hypothesis about the motions of the sun and the earth (1990, p. 45). Evidential relevance of this observational fact depends on which of the two theoretical frameworks, the heliostatic or the geostatic, is considered. In the heliostatic framework, it counts as evidence for the hypothesis that the earth is spinning around its axis at a steady state. In the geostatic framework, it does not. On the contrary, it corroborates another hypothesis, to wit, the hypothesis of the motion of the sun around the earth at a steady state. So far so good. But the background assumptions involved here—the heliocentric and the geocentric hypotheses—are factual claims, and as such, they are open to assessment on empirical grounds. And indeed, the phenomenon of stellar parallax for instance was supposed to provide such grounds: the failure to observe it in the XVIIe century was taken as evidence for the geocentric hypothesis. But Longino would reply that evidential support for background assumptions, when available, relies upon further background assumptions. In her example just discussed, the evidential support in favor of the geocentric assumption provided by stellar parallax depended on another background assumption, to wit, the estimation of the distance of the stars (as it turned out, this estimation was later

4. Note that Longino also calls for criticism of background assumptions. As we shall see in the last sections of this paper, such criticism is at the center of the normative picture of scientific communities she proposes to get rid of biased research. But what matters here is to show that Longino's examples of biased research can be dismissed *on constitutive grounds,* once the evidential role of background assumptions has been put to the fore.

revised, so that the failure to observe parallax—given the resolution of the telescopes at that time—could no longer provide evidential support for the geocentric hypothesis). But what are the lessons we should draw from this threat of "infinite regress" when assessing background assumptions? How does it affect the capacity of science to screen out the influence of contextual values?

One can argue first that this threat is merely a purely theoretical threat, largely irrelevant to the *actual* settlement of scientific debates. What are the *actual* cases in the development of science showing that the dependence of evidential support on background assumptions makes the resolution of a controversy on constitutive grounds impossible? Historical cases such as the one just discussed won't do the job: estimations of stellar distances did rely upon further background assumptions (calibrations of astronomical distances are never definitive), but the fact is that this dependence did not prevent the debate on the motion of the Earth to be eventually settled in favor of the heliocentric hypothesis (background assumptions could be assessed independently of the hypotheses at stake; other independent evidential support for these hypotheses was provided). And I am at loss to see how one could contend that it has not been so on constitutive grounds only. More generally, one can argue that the evidential role of background assumptions in the confrontation between data and hypotheses does not in itself entail the impossibility of screening out contextual values, since background assumptions may themselves be open to independent assessment on constitutive grounds (further illustrations and justification of this point are given in the next sections).[5]

At that point Longino may reply that this line of defense of epistemic integrity is tenable only for background assumptions for which empirical evidence is available, that is, background assumptions resting on factual claims. But what about background assumptions that do not rely on such claims? The assumption of the centrality of male's behavior in the "man-the-hunter" account is a case at hand. My rejoinder is the following: Longino may be right to contend that no direct empirical evidence is available for it (1990, p. 111). But my point is that we do not need *direct* evidence in favor or against background assumptions to make a case for

5. And once again, the fact that this independent assessment may involve further background assumptions does not entail that contextual values necessarily step in; it only entails that the process of evidential assessment is, in principle, an open-ended game. Actually we know that at least since Duhem's insight on the role of auxiliary hypotheses in confrontation between theory and observation ([1906], 1954). To defend the capacity of science to screen out the influence of contextual values in evidential assessment is thus not tantamount to defend the goal of *definitive* evidential assessment (epistemic integrity and underdetermination are not incompatible).

the capacity of science to screen out contextual values. All we need is to be able to discuss and assess the epistemic merit of background assumptions indirectly, so to speak. How can that be done? To get an answer, just look at what feminist criticisms have successfully done in many fields: by bringing different background assumptions to the assessment of various theories, feminist studies have revealed the *empirical, constitutive* weaknesses of these theories. In other words, what feminist studies have established is that the evidential role played by sexist background assumptions led to *constitutive* failures of the dominant theories.[6] Realizing, thanks to feminists, that the "woman-the-gatherer" account was as much empirically grounded as the "man-the-hunter" account did weaken the epistemic value of male-centered background assumptions, since scientists became aware of the following constitutive weakness: the data available could not be taken anymore as *conclusive* evidence in favor of the "man-the-hunter" account, for an alternative account based on different assumptions was as much supported by the same data. Consequently, the "man-the-hunter" account could not be seen anymore as an epistemically successful and warranted account, but rather as a mere working hypothesis, among others.

Similar conclusions can be reached about other examples of research on sex differences given by Longino to back up her charge against the epistemic integrity of science. These examples are in the field of behavioral neuroendocrinology. The general issue is to examine possible effects of sex hormones on anatomy and physiology, on temperament and behavior, and

6. In a review of feminist studies of science,Wylie writes for instance that, famously, studies of reproductive physiology "were long structured by assumptions that arose from attribution of stereotypically masculine traits to sperm (as active agents) and feminine traits to eggs (as passive), *sometimes at considerable cost to empirical adequacy and explanatory power.*" (My italics) (Wylie 2000, p. 170).

Tuana's in depth look at physical anthropology in the mid-nineteenth century points at constitutive failures as well. Commenting on the classification used in craniology to study physical differences between sexes, she writes: "the classification indices of three nineteenth century craniologists [. . .] offer an illustration of the way in which selection of the characteristics employed to prove female inferiority actually *presupposed* it. [. . .] Faced with recalcitrant facts, Ecker [one of the three craniologists] simply shifted the model of the inferior skull from that of a primate to that of an infant to bolster his argument that a larger facial angle was a mark of inferior development." Hardly indeed an attitude embodying a high respect of basic standards of good research satisfying constitutive values . . . (Tuana 1995, pp. 443–444).

In her review of works in neuroendocrinology on hemispheric lateralization of human brain, Nelson cites several critiques developed by feminist biologists (1995). Here again, one finds stories of scientists failing to report contradicting studies as well as pervasive unwarranted hypotheses (as for instance the hypothesis positing a relationship between lateralization and thickness). All of which appear clearly as empirical, constitutive weaknesses.

on cognition. Longino is quite right to emphasize that these topics have broad and serious sociopolitical implications. That certainly explains by the way why discussions of these topics are not restricted to professional scientific journals: it is never long before the general public is informed of a new study explaining why men are allegedly better at mathematics than women, or showing that sexual orientation or level of aggressiveness is determined by some cascade of hormonal events in the first months of your life in the womb of your mother. If the journalist (or the scientist for that matter) is serious, she won't omit to say that such studies are still controversial in the scientific community, and not only because their conclusions may be disliked, but essentially because scientists disagree on the soundness of these conclusions, *and my point is that they do so on constitutive grounds.* That research on these topics can be criticized on constitutive grounds is actually convincingly demonstrated by Longino's own review of it. Longino notices that scientists disagree for instance on the appeal to rodent experiments to learn about humans.[7] Another controversial topic is the widespread adoption of a bivalent ("male"/ "female") classification for gonadal hormones. Here's how Longino phrases the objection: "The assumption of behavioral dimorphism parallel to anatomical dimorphism [. . .] results in a bivalent classification system for gonadal hormones that mirrors their postulated effects on sexual differentiation, *regardless of the studies showing that their effects vary depending on other physiological factors.*" (My italics) (1990, p. 123) But surely a case can be made to challenge the background assumption of behavioral dimorphism in light of the contradictory empirical results mentionned by Longino.

Longino discusses at length other weaknesses of research studies on sex differences, conveying the ongoing controversies in the field and adding her own critical analyses of background assumptions expressing sexist values. This is all quite convincing and very useful but the point is that nowhere does Longino establish that these weaknesses or disputable assumptions cannot be dismissed on constitutive grounds. On the contrary, on many occasions she points (in a somewhat self-defeating way) to doubtful experimental protocols, scarce data, unwarranted leaps from correlations to causations, or ignorance of contradictory studies, all of which reveal constitutive weaknesses.

Longino's critical assessment of works on prenatal hormonal determination of behavior is complemented by a detailed analysis of "explanatory models in the biology of behavior". By "explanatory models" she means "a normative and somewhat general description of the sort of items that can figure in explanations of a given sort of phenomenon and of the relation-

7. See Longino 1990, p. 120 for arguments discussing the limits of rodent experiments.

ships those items can be said to bear to the phenomena being explained." (1990, p. 134) Their role in scientific research is to "serve as background assumptions against which data are ordered, in light of which data are given status as evidence for particular hypotheses and as a context within which individual studies gain significance." (1990, p. 135) Longino contrasts two competing explanatory models, the "linear-hormonal" model and the "selectionist theory of higher brain function". The details of these models don't matter here. What matters is the conclusion that can be drawn from Longino's review of their evidential status. In this review Longino points to the existence of assumptions about causation and human nature underlying these models and emphasizes their lack of evidential support. That contextual interests and values motivate these models seems quite convincingly established. But as for the previous examples, what is lacking at that point is an argument showing that the epistemic merit of the background assumptions underlying those models cannot be assessed on constitutive grounds. And here again, Longino's own analysis seems self-defeating. Commenting for instance on the second model (the selectionist theory), she concludes that this approach "[. . .] is subject to a variety of criticisms driven by *internal epistemic considerations.*" (My italics) (1990, p. 160) To back up her claim, she then lists three of these considerations, all pointing to features of the approach indeed clearly open to criticism on purely constitutive grounds. (1990, p. 161) So that the conclusion is as simple and straightforward as for the other examples: on a close look at Longino's own analysis, such works turn out to be problematic on constitutive grounds.

A brief comment may be in order here. Acknowledging that the cases of research on sex differences discussed by Longino involve unwarranted background assumptions that can be challenged on constitutive grounds does not mean that this is all deliberate sloppy science, not worth of any consideration. One should certainly not expect scientists to suspend their judgment and hold their conclusions every time their work relies upon such unwarranted assumptions. After all this is research. The key issue as regards epistemic integrity is whether or not responsiveness to peer criticism based on constitutive values is able to get rid of biased results (or at least is the best way to limit them)? Longino's answer is a ringing "no": she holds background assumptions to be immune against criticism on constitutive grounds only. The problem is that *empirical* support for such a negative answer has proved to be very shaky: all the cases put forward by Longino to back up her thesis have turned out to be dismissible on purely constitutive grounds. And they've turned out to be so according to Longino's own criticism of them.

Let us briefly sum up what has been dismissed so far in Longino's the-

sis. Longino's charge against the epistemic integrity of science rests on the claim that some cases of biased research can nevertheless qualify as "good" science. I've argued that, given the ambiguity of the notion of "good" science, the existence of "good", non-value neutral science is not sufficient to dismiss the capacity of science to screen out contextual values on its content: what needs to be established is the impossibility of assessing background assumptions on constitutive grounds. I've shown that Longino does not provide any good reason to believe that this assessment is impossible: all her examples of biased theories, including those qualifying as "good" science according to Longino, can be shown to fail on constitutive grounds when the evidential role of unwarranted background assumptions is brought to the fore. The instrumental role of feminist (or other contextually valued) criticisms in this recognition by scientists of the constitutive failure of their theories is indisputable and cannot, I think, be overstated. For all that, value-neutrality can remain on the agenda.

At this point 'radical content' critics such as Longino may reply that my line of defense of epistemic integrity turns too much on a rational reconstruction of idealized evidential relations: in the actual ongoing process of evidential assessment—they would argue—critical practice exclusively based on constitutive grounds remains a wishful thinking. The last two sections of this paper, which address the practical issue of how scientific community should function to get rid of biased research, will hopefully show that this objection does not hold.

But before turning to these issues, I would like to offer a brief digression in Bachelardian land. This is just another way of framing the challenge that, I think, 'radical content' critiques fail to meet.

Epistemological obstacles

A precision may be in order first. Thanks to the hard work of feminist historians of science, the influence of sexist ideologies on the very content of science is now well-documented. I will focus in this section on this kind of biases (those are also Longino's central concern, as attested by her choice of examples of biased inquiries), but the discussion would go along the same lines for other kinds of biases, based on race, social class, sexual orientation, etc.

Disciplines traditionally investigated by feminists are disciplines whose subject matter are humans or primates and/or whose theories deal with processes in which sex or gender is a key variable. Most cases discussed in the feminist literature belong thus to archeology, anthropology, sociology, biology, medicine, ethology, or primatology. But it is worth noticing that patent influence of contextual values is not restricted to these disciplines. A similar assessment can be made in fields that appear, on the face of it,

very unlikely to be prone to such an influence. Astrophysics, for instance, is certainly one of the best candidates for the mythical category of contextual value-free science. It deals with objects quite remote from our lives and interests, whose study is obviously very unlikely to have any social or political import. For all that, the history of astrophysics is far from being free from episodes that bring to the fore the permeability of this discipline to contextual values.

A telling episode is the genesis of stellar spectral classification. Here's a very brief and simplified account of it.[8] By the 1860s and 1870s, the two main elements of stellar spectroscopy had become available. On the theoretical side, G. Kirchoff and R. Bunsen (building on previous work by J. Foucault) had laid down in 1861 the two basic principles of spectrum analysis.[9] On the observational side, progress in photography had permitted the collection of a significant number of stellar spectra (several thousands were available in 1880). A first qualitative classification was proposed by the Jesuit Angelo Secchi. His observations, as well as observations by other astronomers (H.C. Vogel, J. N. Lockyer and W. Huggins) had led to the conclusion that all stars could be grouped together in a very limited number of categories. Secchi established three classes, depending on the color of the star (blue, yellow or red). Then arose the issue of the origin of this spectral diversity. Was it due for instance to differences in chemical composition, or differences in temperature? Did the three classes correspond to a sequence of stellar evolution?

A look at the intricate arguments exchanged by the main protagonists of the debate, (Secchi, Huggins, Lockyer and Vogel mainly) shows clearly that the interpretation of spectral diversity was very much hindered, first by an ideological reluctance to give up the idea of cosmic uniformity, then by general ideas of evolution directly (albeit loosely) imported from the newly born theory of evolution.[10]

This early episode of the history of stellar classification constitutes a nice example of an "epistemological obstacle". An epistemological obstacle is generally defined by Bachelard as what, in our current knowledge, hampers the access to new knowledge. As Bachelard nicely puts it: "When we contemplate reality, what we think we know very well casts its shadow

8. A detailed historical study of this episode is provided by De Vorkin 1978. A classical historical source on early stellar spectrography is Pannekoek 1961.

9. In short, the first principle stated that solids and liquids typically produce continuous spectra in the visible, whereas gases produce characteristic emission lines. The second principle stated than when a source producing a continuous spectrum is seen through a cold gas, the wavelengths of its absorption lines correspond to the wavelengths of the emission lines produced by the gas when heated.

10. For more detail on these arguments, see Ruphy 1994, 1997.

over what we ought to know. Even when it first approaches scientific knowledge, the mind is never young. It is very old, in fact, as old as its prejudices. When we enter the realms of science, we grow younger in mind and spirit and we submit to a sudden mutation that must contradict the past." ([1938] 2002, pp. 24–25) Science comes forward against ordinary knowledge and common sense, but also against its own tendency to conservatism and inertia. According to Bachelard, the human spirit tends to become unduly attached to certain reasonings and images. Facing new situations and questions, scientists get into the habits of appealing to forms of explanations and arguments they have used already in specific cases. Habit is thus what creates obstacles to new, fruitful questionings, by perpetuating fixed images that have been excessively valorized and become accepted as evident. The same inertial effect is produced by images or ideas functioning as unconscious collective symbols. Metaphorical references to psychoanalysis are omnipresent in *The Formation of the Scientific Spirit.* Certain types of explanations are valorized, not so much by habit, but mainly because of their unconscious affective charge. Hence the necessity for the scientist to overcome all these obstacles by fighting stereotypical images, ossified ideas that have accumulated by habit, pre-scientific concepts based on unconscious images. Scientists must always shake themselves free of the answers they have already obtained to resolve new problems. Research is thus conceived as a true process of liberation—Bachelard talks metaphorically of "psychoanalysis of knowledge". When dropping references to individual psychology, isn't it a process that, appropriately transposed as a collective process, should appeal to feminists? Isn't Bachelard's charge against abuse of fixed images, explanations and reproduction of stereotypes external to science very relevant to cases of gender-biased science investigated by feminists?

I want thus at this point to raise the following question: are projections of gender biases—being at the level of descriptions or at the level of mediating background assumptions—of a different kind than these epistemological obstacles that are part of any scientific enterprise (physical sciences included), in particular at an early stage of its development? My claim is that there is no essential difference between the two. So that there is no reason to grant to physics for instance the capacity to overcome epistemological obstacles by critical assessment made on constitutive grounds, but to deny it, say, to behavioral biology or studies of human evolution.

At this point 'radical content' critiques may first deny that there is no essential distinction. But what is the distinction then? What is this essential difference between reproduction of gender-biased stereotypes and more traditional instances of epistemological obstacles *à la Bachelard* that would entail a resistance of the former to methodological remedy working

for the latter?[11] As noticed earlier, Bachelard's description of epistemological obstacles in terms of reproduction of stereotypes external to science, fixed images, etc., seems on the contrary perfectly appropriate to cases of gender-biased science as described by feminists themselves.

Another rejoinder would be to refuse to grant to disciplines such as physics the capacity to overcome epistemological obstacles by constitutively based critical practice. But this claim is certainly very hard to maintain in light of the historical development of the discipline—*no serious attempt has been made so far*—, at least much harder than in usual cases of gender-biased science. To come back to my astronomical example, that would mean denying that early spectral classifications, admittedly shaped by contextual values, were simply dismissed on constitutive grounds, via in particular criticism of background assumptions. But as a self-proclaimed empiricist, Longino would certainly concede that the change in the way data were organized and interpreted was triggered by the acquisition of more refined data and the concern to come up with a coherent and inclusive account.[12] Constitutively based critical practice was thus enough to overcome epistemological obstacles set by background assumptions such as the idea of cosmic uniformity or the evolution dogma.

To sum up, 'radical content' critiques face the following challenge: they need to show, *across the whole range of scientific disciplines* (physical sciences included), that epistemological obstacles cannot be overcome by constitutively based critical practice. *Or* (since that would release them from the previous challenge) they need to make a case in favor of an essential difference between cases of gender-biased science and epistemological obstacles *à la Bachelard* (a difference that would justify that only the latter can be overcome by constitutively based critical practice). On both points, the

11. Note that this difference cannot be that epistemological obstacles *à la Bachelard* are not cases where background assumptions mediating the relation between evidence and hypotheses are widely (and often implicitely) held within a community. Recall that for Bachelard a source of epistemological obstacles is ordinary knowledge and common sense, widely shared assumptions *par excellence.* Moreover, my brief incursion in the history of astronomy aimed at showing that, at its early stage, spectral stellar classification was precisely a case where widely shared beliefs did influence the way data were organized and interpreted. Besides, a radical difference in social consequences should not be confused with a difference of nature. A caveat that leaves unchanged by the way the fact that domains where gender biases may occur certainly call for much more critical scrutiny than cases where the influence of contextual values lead to clumsy spectral classifications, an unfortunate but admittedly harmless outcome.

12. Here again, I can only refer to the works cited in footnote 9 to back up this claim, for the story is quite intricate.

burden of proof rests on their shoulder. Meanwhile, the challenge remains unmet.[13]

Longino's ideal scientific community

Let me turn now to my second level of attack against Longino's thesis announced at the beginning of this paper. It is not independent of the first; it just focuses on another part of Longino's thesis I haven't said much about so far. This is the part that complements her attack against epistemic integrity by dealing with what should be done to get rid of cases of biased science if, as she claims, simple dismissal as "bad" science is of no avail. As briefly announced earlier, the remedy elaborated by Longino draws directly on her analysis of the role of background assumptions in the shaping of scientific knowledge. Since background assumptions play a role in evidential relations and are what permit the expression of ideologies in scientific inquiry, the remedy to biased research will be appropriate criticism of these background assumptions. The key issue is then what controls these assumptions. Longino's answer is built on her viewing scientific practice essentially as a social, rather than an individual practice.

13. A precision might be in order here. To claim as I do that there is no essential distinction between cases of projection of gender biases and epistemological obstacles à la Bachelard does not entail that no specificity exists at all, on the contrary. Undoubtedly, domains scrutinized by feminist are particularly vulnerable to abusive uses of images, reproduction of common sense ideas and stereotypes. Let me draw on the typology of scientific objects proposed by Daston to shed some light on this specific vulnerability. Daston discerns four notions to characterize the historicity of scientific objects: salience, emergence, productivity, embeddedness. (Daston 2000) Only the first two categories are directly relevant to my purpose here, by suggesting an interesting difference between phenomena that are part of our reality before they become subject of scientific inquiry, and phenomena that do not possess such a mundane reality. As Daston puts it: "Salience, be it cultural or economic or epistemological, silhouettes extant objects; scientific reality might be said to intensify their reality but not to create them ex nihilo. Emergence posits a more radical form of novelty." (2000, p. 9) Dreams, personal identity are the kind of objects whose existence is intensified by scientific inquiry. On the other hand, quarks, black holes are typical examples of objects that lack this quotidian prehistory that dreams or self possess. Being part of our reality before becoming objects of scientific inquiry, it should come as no surprise that salient scientific objects are more vulnerable to abusive uses of images, reproduction of biases than emergent objects. Importing stereotypes external to science when describing the behavior of a Bose-Einstein condensate is surely more unlikely to happen than when describing the behavior of male gorillas before mating. Domains traditionally investigated by feminists being domains whose subject matter are humans or primates—salient objects if there are—such domains are clearly the most challenging for the scientific enterprise that has, as Bachelard nicely puts it, to struggle against original images, that is to say, to break the immobility of the archetypes contained in the seat of the soul. Admittedly a difficult task in the case of gender-biased research. But difficulty is not impossibility, and Longino's argument fails to establish the latter.

Acceptance or rejection of a hypothesis, a theory or an experimental method is the outcome of a social process of interaction among scientists. Criticism of background assumptions is also taken as operating at the level of a community.[14] But of course, not any process of mutual criticism will do. One needs "critical interactions among scientists *of different points of views* [. . .] to mitigate the influence of subjective preferences on background assumptions and hence theory choice." (my italics) And to transform the subjective into the objective, "[. . .] those interactions must not simply preserve and distribute one subjectivity over all others, but must constitute genuine and mutual checks." (1990, p. 40)

Longino comes up with four features that a scientific community must possess to reach this aim. In a nutshell: i/ there must be publicly recognized forums for the criticism of evidence, of methods, and of assumptions and reasoning; ii/ there must be uptake of criticism; iii/ there must be publicly recognized standards by reference to which theories, hypotheses and observational standards are evaluated and by appeal to which criticism is made relevant to the goals of the inquiring community; iv/ community must be characterized by equality of intellectual authority.[15] It is not my purpose here to assess in detail the general merits and shortcomings of Longino's sketch of an ideal scientific community. Let us just notice that i/, ii/ and iv/ are hardly disputable for anyone committed to a view of science as a collective rather than an individual practice (even if iv/ certainly calls for more elaboration since equality of intellectual authority, as noticed by Longino, cannot mean simple equality of voice). In the remaining of this section, I'll be solely concern with the third feature, for an understanding of the nature of these public standards is directly relevant to our concern in this section, to wit, the grounds on which criticism of background assumptions should occur.

So what do these public standards by reference to which criticism may be formulated contain? Longino's set of public standards is rather packed: it includes "cognitive values, pragmatic values, and substantive assumptions grounded in either the metaphysical commitments or the social and political commitments of a society, i. e. metaphysical or value-laden substantive assumptions." (1996, p. 41) Such inclusiveness would not be problematic if it only reflected the fact that public standards, as defined by Longino, seem to include both standards about the goals of scientific in-

14. This goes hand in hand with a social account of objectivity in science: " [. . .] objectivity is analyzed as a function of community practices rather than as an attitude of individual researchers towards their material or a relation between representation and represented." (Longino 1990, p. 216)

15. For a more detailed exposition of theses four features see Longino 1996, p. 40 or Longino 2002, pp. 129–131.

quiries and standards of epistemological acceptability. As mentioned at the beginning of this paper, contentions about the contextualization of the *goals* of scientific inquiries seem now solidly established. So yes, public standards, when including standards about the goals of scientific inquiries, do include standards reflecting social or political commitments of a society. But what about the public standards of epistemological acceptability? Longino contends that they should include non-cognitive values as well. This is the interesting and highly disputable part of her thesis: Longino's normative picture of science "admits political considerations as relevant constraints on reasoning, which through their influence on reasoning and interpretation shape content." (**1990, p. 193**) Otherwise put, the satisfaction of a political agenda, the commitment to certain values may constrain the choice of a theory or model *via* the adoption or criticism of background assumptions.[16] Going back for instance to her examples of explanatory models in the biology of behavior, Longino writes: "our political commitments, however, presuppose a certain understanding of human action, so that when faced with a conflict between these commitments and a particular model of brain-behavior relationships *we allow the political commitments to guide our choice.*" (my italics) (1990, p. 191)

This does not mean of course that political commitments should have the last word. The empiricist Longino is always at pain to distinguish her position from relativist standpoints: "acceptance of the relevance of our political commitments to our scientific practice does not imply simple and crude impositions of those ideas onto the corner of the natural world." (1990, p. 191) Her position may be described as a multiple-constraint picture of theory choice: given the role of background assumptions in theory choice, both traditional constitutive values and contextual values shape public standards of epistemological acceptability. Hence the title of her book *Science as Social Knowledge:* science is *social* knowledge not only because scientific practice is essentially a collective rather than an individual process, or because contextual factors may constraint the goals of scientific inquiries, but also because contextual values may "serve as cognitive values" (Longino 1996, p. 41), that is to say, play a role in inquiry that is epistemically acceptable and desirable.

But why should the stepping in of contextual values in theory choice be desirable? What ensures that it has a *positive* role in scientific inquiries? In fact there is I think an unfortunate ambiguity in Longino's use of the term 'positive'. Does 'playing a positive role in scientific inquiries' mean playing a *cognitively positive* role, or a *politically positive* role? Strangely enough, Longino seems to think the two go together. But why should that be the

16. For more details on that point see Longino 1990, pp. 185–194.

case? Surely, it is much easier to make a case for the latter than for the former. Rejecting certain models of brain-behavior relationships on the grounds that their unquestioned background assumptions run counter to the understanding of human action presupposed by your political interests will evidently serve these political interests. But what about your "cognitive interests"? What ensures that your own politically-oriented choice of background assumptions have any superior *cognitive* merit?

Let us take stock here. Longino's undeniable merit is to have put to the fore the role of often invisible background assumptions in evidential relations and hence, in the shaping of scientific knowledge. Her demand for appropriate criticism of these assumptions (rightly taken as expressing sometimes harmful contextual values) is undoubtedly appealing. As appealing (if not sufficiently worked out) are the social, organizational norms she proposes to impose on scientific communities to ensure that such a criticism occurs: heterogeneous scientific communities are surely more likely to be able to question and revise their background assumptions than monolithic ones. So yes, diversity of points of view may be epistemically advantageous. But what about the *grounds* for such criticism? Longino's answer, admitting both constitutive and contextual values, should not please proponents of the epistemic integrity of science, for it means acceptance of social or political considerations in choice and justification of theories, *via* adoption or criticism of background assumptions. Of course, to justify one's reluctance to such openness, one can always content oneself with brandishing the spectre of a science completely under the heel of political power (remember the Soviet joke about an apparatchik asking impatiently how much 2+2 is and the cautious answer of the mathematician "how much do you want it to be?"[17]). But that would be a bit lazy and moreover, it would miss the point. Recall that the empiricist Longino is not arguing for crude impositions of claims on the natural world but for *supplementation* of empirical constraints by contextual ones when it comes to the critical assessment of background assumptions. Why should this be resisted if it may contribute to promote progressive social and political agenda, without compromising cognitive achievements?

The main reason I offer is skepticism about the capacity of Longino's normative picture to reach its aim, that is, to cure science from ideological biases. How plausible is it that only *progressive* values will play a role in theory choice when political considerations step in? Otherwise put, how plausible is it that in a scientific community, interests and values of members of all groups of the larger society will be fairly taken into account and play a role in theory choice, rather than interests and values of a dominant

1 LINE SHORT
REGULAR
1 LINE LONG

17. I got this joke from Anderson 1995.

group? Well, it is as plausible as is plausible fair representation in the larger society and equal access of all groups to jobs in scientific communities, which, on the face of it, really does not seem very plausible.

As anyone who has worked in a research laboratory or has attended scientific conferences knows too well, the social origin of scientists is not very diverse or, at least, is far from reflecting the social diversity of the larger society. Not surprisingly thus, political or social interests of dominated groups usually find very weak support in scientific communities. An ideal democratic society where fair representation is achieved in decision-making authorities would thus not be enough. One needs also that the recruitment of scientists would ensure fair representation in scientific communities as well. Nothing less than this kind of notoriously difficult social and political achievements is required by Longino's picture of an ideal scientific community. But Longino (understandingly) does not give any clue on how that might be achieved. I would not blame her for that, though. After all, isn't that the privilege of philosophers to propose normative pictures and to leave to others the task of coming up with practical ways to implement them? Perhaps. But one can at least worry that a normative picture is too *demanding.* This might not be a sufficient reason to reject it altogether, but it is certainly a reason good enough to investigate whether a less demanding normative picture could achieve similar goals.

An alternative picture

In the alternative picture I'll propose, my aim is just to show that, once one has acknowledged the role of background assumptions put forward by Longino, appeal to constitutive values is still the best one can do to get rid of ideological biases, for it is much less demanding than Longino's picture just discussed earlier. I see thus the advantage of my alternative picture in terms of relative efficiency toward the fulfillment of Longino's and other "radical content" critics' agenda, to wit, the suppression of the influence of harmful ideologies in science.

My starting point will be a contrast that can be drawn between Longino's views on science as a collective social process and the view of science proposed by Bourdieu. Contrasts can appear only on a minimal common background. And indeed, Bourdieu also endorses the general contention that the production of knowledge is a social process and that objectivity is the outcome of this social process whose key principle is mutual criticism. In his 2001, Bourdieu provides us with a very rich and compelling description of science as a social activity, one of whose essential properties is the existence of censorship resulting from the sacrosanct process of peer-reviewing. Being based on the "arbitrage of constructed reality", this collective censorship is what allows a process of "departiculari-

zation and universalization" to take place, by which scientific truth may be produced (Bourdieu 2001, pp. 141–149). In short, this social process "irons out", so to speak, social values: what you end up with is knowledge that is not marked any more by the specificities of the social and historical context of its production. In Longino's scheme, on the contrary, we have seen that social values, far from being "ironed out" by mutual criticism, may intervene as legitimate grounds for such criticism. Bourdieu's appeal to "the arbitrage of constructed reality" as grounds for intersubjective criticism excludes such political and social considerations.[18] The contrast is thus clearly on the legitimate grounds for mutual criticism. Let me now attempt a hybridization of Bourdieu's and Longino's schemes.

I'll take from Bourdieu his general, restrictive view of legitimate grounds for mutual criticism and I'll draw on Longino's analysis of processes of justification to elaborate on what a social process of mutual criticism solely based on the "arbitrage of reality" could look like. The result is rather straightforward, at least in its basic features: social, organizational norms of scientific communities should be designed to ensure that scientific communities are best equipped to produce results that meet traditional epistemic norms. This implies in particular (and this is Longino's important insight) the capacity of mutual criticism of background assumptions. The key point is that social or political considerations should be kept at bay in this process: appeal to constitutive values is sufficient. Or, at least, *we have no good reason to believe that it is not,* as hopefully demonstrated by the arguments developed in the first sections of this paper: just recall the empirical weakness of Longino's claim that background assumptions are immune against criticism on constitutive grounds (her examples of biased research have all turned out to be dismissible on such grounds). Moreover, Bourdieu's detailed sociological analysis suggests that the "ironing out" of contextual values can and does sometimes occur in science.[19]

At this point one may reply that my restrictive view of legitimate grounds for mutual criticism is actually not quite enough to guarantee the epistemic integrity of science, since appeal to constitutive values may em-

18. As noted in Ruphy 2001, one may certainly be frustrated by the fact that Bourdieu leaves his key notion of "arbitrage of constructed reality" rather unspecified (especially the "constructed" part of it). But I'm not sure, though, that one can blame him for that. Recall that Bourdieu, as a sociologist, is primarily concerned with the social mechanisms at work in scientific communities rather than with pure epistemological issues.

19. I am very aware of the fact that my hybridization of Bourdieu's and Longino's schemes remains very sketchy but it should suffice to point to the *possibility* of reconciling a thoroughly social view of science with a defense of its epistemic integrity.

body social or political interests.[20] And indeed, Longino has come to deny that constitutive values are value-neutral grounds of judgment, by arguing against the dichotomy constitutive/contextual values (that is, cognitive/non-cognitive values) (Longino 1996). The punch line of her argument is to contend that the valorization and the adoption of certain constitutive values are laden with socio-political interests. Let us see if the charge is solid and how it affects the alternative picture I've just sketched.

To make her case, Longino focuses on the five cognitive values identified by Kuhn as guide for the judgments of the scientists in their choosing between theories. Accuracy, simplicity, internal and external consistency, breadth of scope and fruitfulness are all taken to constitute rational, objective grounds for theory choice (Kuhn 1997). What Longino wants to challenge is their independence from social or political considerations. Here's how she sums up her charge: "[My aim is] to cast doubt on the very idea of a cognitive value or virtue, where we mean by that a quality of theories, models, or hypotheses that can serve independently of context as a universally applicable criterion of epistemic worth." (1996, p. 42) Her strategy is to contrast the Kuhnian set of values with a set of feminist values and to show that the former are not more value-neutral grounds for theory choice than the latter. The set of "feminist theoretical virtues" considered by Longino includes empirical adequacy, novelty, ontological heterogeneity, mutuality of interaction, applicability to human needs, diffusion of powers.[21] Take for instance simplicity *vs.* ontological heterogeneity.[22] She notes that in neoclassical economic theory, the social word is

20. Another line of criticism could point to the fact that contextual and constitutive values are often inseparable *in practice,* and that the selection of background assumptions as acceptable or unacceptable cannot always be fully thematized. I recognize that in such cases, criticism of background assumptions is much more difficult to implement. But this is a difficulty both for Longino and I. Restricting as I do the grounds for such criticism to constitutive values does not make it worse. Therefore, acknowledging the inseparability, in practice, of contextual and constitutive values does not weaken my claim that my alternative picture is superior to Longino's in terms of *relative efficiency* toward the fulfillment of Longino's own agenda. Just recall that I do not contend that constitutive values are always decisive, I only contend that appeal to constitutive values in processes of mutual criticism is *the best one can do* to get rid of ideological biases in science.

21. See Longino 1996, pp. 45–48 for a detailed presentation of theses standards and how exactly they serve a feminist agenda. Note that Longino does not claim that these values are uniquely or intrinsically feminist, just that "theories exemplifying them would be more likely to satisfy feminist cognitive aims (which are also socio-political aims)—namely to make women and female-identified phenomena as well as gender relations more visible." (1996, p. 51).

22. This is one of the three pairs considered by Longino, the two other pairs considered being external consistency or conservatism *vs.* novelty, and fruitfulness *vs.* feminist prag-

supposed to be composed of a very limited number of basic entities. The head of the household is for instance considered as the main economic actor. Longino argues that this taste for simplicity has politically harmful consequences: "By erasing the independent interests of other household members from theoretical views, these models prop up an oppressive family structure [. . .]." (1996, p. 53) It is certainly hard to disagree with Longino on that point, but what does that tell us about the cognitive virtue of simplicity? Not much I'm afraid.

Longino rightly points to a case where the criterion of simplicity is patently politically laden (the reader has unfortunately to content herself with one single example—in economics—of such political valence). She also emphasizes the take of metaphysical views on the simplicity of the universe as a common ground for valorizing simplicity. Longino is certainly right to be unsatisfied with these kinds of justification, as any empiricist should be. All this indeed shows that there are cases where simplicity is valorized on non-epistemic grounds. So yes, embracing simplicity as a *universal* criterion for epistemic worth is unjustified.

My point is that this demanding view on the epistemic worth of "theoretical" values such as simplicity is actually not what is needed to maintain the possibility of value-neutral mutual criticism. All is needed is the possibility to critically discuss, on a case-to-case basis so to speak, the epistemic worth of a value *on empirical grounds.* When should one favor ontological heterogeneity and mutual interaction in a given line of research? Well, just when commitments to such values have led to empirically successful results. This is for instance exactly how the evolutionist Stephen J. Gould justifies his commitment to heterogeneity and interaction: this is just "a matter of good science", and he adds explicitly that this has nothing to do with gender or feminism.[23] My view is that the epistemic worth of values such as simplicity or ontological heterogeneity can and should be assessed on empirical grounds, by appeal to inductive arguments. This contention is directly inspired by Mc Allister's general analysis of the role of aesthetic values in science (1996).

McAllister is interested in the reasons why scientists often trust their

matic virtues (such as applicability to human needs or diffusion of power). What I have to say on simplicity *vs.* ontological heterogeneity applies also to external consistency *vs.* novelty. I will not discuss the third pair, for I am at loss to see how a purely pragmatic virtue such as 'applicability to human needs' can be treated on a par with candidates to the status of cognitive virtue.

23. Gould 1986. This is actually Longino herself who reports Gould's justification in a footnote of her 1996 paper. Longino also mentions the work of the biologists Levins and Lewontin (1985) as an example of valorization of ontological heterogeneity and interaction, *not* grounded on feminist commitments.

aesthetic judgments in their appraisal of theories. Cases where aesthetic criteria played an influential role in scientific judgment are not exceptional, especially in physics and astronomy. Kepler's laws of planetary motions were not very well-received mainly because ellipses were seen as imperfect curves, less beautiful than circles. Paul Dirac's enthusiasm for the newly born theory of general relativity was not so much grounded in the empirical success of the theory (rather scarce at the beginning) than in its aesthetic virtues. Dirac is famous for the extreme trust he put in aesthetic judgment: "It is more important to have beauty in one's equations than to have them fit experiment. [. . .] It seems that if one is working from the point of view of getting beauty in one's equations, and if one has really a sound insight, one is on a sure line of progress." (1963, p. 47, quoted in McAllister 1996, p. 15) Most scientists may be convinced, like Dirac was, that there is some intimate link between beauty and truth. On the philosophical side, the inference from beauty to truth is less popular. One of Bachelard's chapter of his *The Formation of Scientific Mind* is entitled "Unified and pragmatic knowledge as an obstacle to scientific knowledge".[24] As for McAllister, the belief in a link between beauty and truth is only a kind of unwarranted epistemological variant of the old doctrine of unity of virtues expressed for instance in the Greek notion of *kalos kagathos*[25], later reincarnated at the Renaissance in the motto *Pulchritudo splendor veritatis* ("Beauty is the splendor of Truth").

In fact, scientists have much better reasons to trust their aesthetic judgments. Rather than appealing to intuition or some kind of *a priori* considerations, McAllister suggests approaching the issue in an empirical manner, by raising the following question: how is it that at a given time, some aesthetic properties of a theory are taken as good indicators of its epistemic worth? The bottom line of McAllister's answer is to appeal to an inductive mechanism: when a theory has turned out to be empirically successful, then its aesthetic properties are granted truth-conducing value. Otherwise put, the properties of empirically successful theories become

24. Far from having any epistemic virtue, the search for unity is even characteristic, for the French philosopher, of a pre-scientific spirit: " For the pre-scientific mind, unity is a principle that is always desired and always cheap to achieve. Only one capital letter is needed for this to happen. The different natural activities thus become the varied manifestations of one and the same Nature. Experience cannot be conceived as self-contradictory or as compartmentalized. What is true of something large must be true of something small and vice versa. Error is suspected whenever there is the slightest duality. This need for unity poses a multitude of false problems." (2002, p. 94)Then follows a rather diverting list of historical cases—drawn mainly from the physics of the XVIIIe century, demonstrating that indeed, when unity is the goal, truth is rarely the result.

25. An approximate translation would be: "both nice to be looked at and good in its actions".

aesthetic canons that may guide scientists in the elaboration of new theories. An immediate consequence of McAllister's view is that correlations between aesthetic properties and epistemic worth are never definitive. And indeed, styles of scientific theories come and go out of fashion. Just recall how in the XVIIIe century physicists favored very abstract mechanical theories, whereas in the next century, physicists like lord Kelvin and Ludwig Boltzmann praised theories that would enable scientists to visualize phenomena. I can only refer to McAllister 1996 for many more historical examples of changing correlations between aesthetic properties and epistemic worth. What interests me here is what McAllister's analysis suggests as regards values such as simplicity or ontological heterogeneity, to wit, the possibility to defend their cognitive virtues on empirical grounds, hence to defend their value-neutrality without having to commit oneself to some intuitive or *a priori* thesis on their universal epistemic worth.

I propose thus to distinguish between a very minimal, stabilized set of values whose cognitive virtues are universal (this set would be restricted to empirical adequacy and internal consistency), and a larger, unstabilized set that would include values whose cognitive virtues are not universal. Simplicity may be a good guide to empirical success in certain branches of physics at a certain stage of their development, but not in economics or biology today, where ontological heterogeneity is a better bet. Requiring external consistency is sometimes epistemically rewarding, sometimes not.[26] And the same goes for novelty. In any case, I see no good reason why the epistemic worth of these values could not be established (by inductive arguments in particular), or challenged, on empirical grounds. *Let us then be an empiricist all the way down:* not only background assumptions can and should be criticized by appealing to constitutive values, but what counts as constitutive (i.e. cognitive) values (except of course the minimal set mentioned earlier) is also susceptible of being revised on empirical grounds. Defending the epistemic integrity and the value neutrality of science thus turns out to require a much more sophisticated picture of "properly conducted" science than the "traditional" one briefly presented at the beginning of this paper, which boils down to a simple call for satisfaction by scientific results of "traditional" cognitive values.

A last remark may be in order to conclude: I am not denying that most of what counts traditionally as cognitive values have political valence and,

26. Requiring consistency with some basic features of Newtonian physics (such as its determinism or its ontological commitment in favor of point particles) turned out to be quite fruitful for a while: just think of the success of Boltzmann's statistical mechanics for instance (see McAllister 1996 for more examples). Quantum mechanics is on the contrary a clear case where conservatism was not the right option.

consequently, that appeal to these values may be motivated by non cognitive purpose. But recall we are discussing here the respective merits of *normative* pictures of scientific practice. What I'm contesting thus is only Longino's claim that the appeal to constitutive values in an ideal process of mutual criticism could not free itself from social or political motivations. It can. And not because there are such things as "universally applicable criterion of epistemic worth", but because the actual epistemic merit of what is taken as constitutive values by a scientific community is itself susceptible to be critically discussed on purely empirical grounds. And this requirement of "critical empiricism all the way down" seems to me much less demanding than Longino's picture of ideal scientific practice, and hence much more efficient to get rid of biases in science.

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