



# Social exclusion in academia through biases in methodological quality evaluation: On the situation of women in science and philosophy



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## ARTICLE INFO

### Article history:

Received 11 June 2015  
Received in revised form  
29 August 2015  
Available online xxx

### Keywords:

Feminist philosophy of science;  
Women in philosophy;  
Methodological standards;  
Theory appraisal

## ABSTRACT

Empirical studies show that academia is socially exclusive. I argue that this social exclusion works, at least partly, through the systematic methodological disqualification of contributions from members of underrepresented social groups. As methodological quality criteria are underdetermined their interpretation and weighting can be biased with relation to gender, race, social background, etc. Such biased quality evaluation can take place on a local or global level. The current situation of women in academic philosophy illuminates this. I conclude that only mechanical solutions can effectively change the situation.

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When citing this paper, please use the full journal title *Studies in History and Philosophy of Science*

## 1. Introduction

"I have never seen a first rate woman philosopher." This was a statement a professor made to Sally Haslanger when she was an undergraduate student (Haslanger, 2008). One might simply be stunned about such a remark and wonder why a philosophy professor would make such an obviously sexist claim. However, the statement from Haslanger's professor matches the poor situation of women in academic philosophy quite well. During the last few years, philosophers from all directions have provided anecdotes and data that paint a dismal picture: a rising number of current investigations show that women in academic philosophy have to face strong disadvantages (e.g., Haslanger, 2008; Jenkins & Hutchison, 2013; Saul, 2013), and empirical data confirm not only the underrepresentation of women in academic philosophy, but also that women philosophers are less likely to be employed (at least in full time and tenure positions) in high-ranking departments and to publish in high-ranking journals (cf. Bishop, 2013; Bishop, Beebe, Goddard, & Rini, 2013; Jenkins, 2013, p. 82; West, Jacquet, King, Correll, & Bergstrom, 2013). What are the attitudes, norms, and standards that are at work here?

In the last few years, the role of social behavior in philosophy has been investigated thoroughly. Academic philosophy is characterized by a combative working environment, and women, being underrepresented in this scientific area, find themselves in a situation in which their performance attracts particular attention; their sheer isolation makes them feel observed and "deviant from the norm" (Fehr, 2011, pp. 151–152; cf. also Beebe, 2013; Haslanger, 2008). This intensifies the role that gender schemas play in philosophy, "hypotheses shaping expectations about performance and behavior that vary for men and women" (Jenkins & Hutchison, 2013, p. 2). Such schemas influence the behavior of women which has been robustly confirmed by social psychological studies<sup>1</sup>: the schemas are usually reinforced by stereotype threats, "ways that a person's (awareness of their) own group membership may negatively affect their performance" (Saul, 2013, p. 40), and by implicit biases "that affect the way we perceive, evaluate, or interact with people from the groups that our biases 'target'." (Saul, 2013)

However, in contrast to the thorough analyses of gendered attitudes in social norms and standards shaping the atmosphere and conversational tone in philosophical discussions, debates, and

<sup>1</sup> For a good overview of these social psychological studies cf. Beebe (2013), Hutchison (2013), and Saul (2013).

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academic social life, it is largely unclear how shared *philosophical* norms and standards contribute to an exclusion of women from the philosophical establishment. As Katrina Hutchison stresses, “we ought to pay more attention to our interpretative methods and the role of interpreting, understanding, and explaining in philosophy.” (Hutchison, 2013, p. 117) In this paper, I wish to take a step in this direction and transcend any merely social-behavioral explanations by investigating whether social exclusion in academia is also generated through methodological evaluation practices and processes—despite methodological quality criteria which are meant to avoid unjustified exclusion of valuable contributions. Using ideas from the philosophy of science, I will argue that the application of methodological quality criteria can transport and reproduce gender-related biases. This rests on the premise that gender-related prejudices can unconsciously influence the evaluation of theories via methodological criteria for interpretation and weighing. While the issues for women are certainly not identical in different academic environments, the problem I am about to analyze is one that might be relevant in other areas of academia wherever women are underrepresented, be it in the natural sciences, social sciences, or the humanities. I will, thus, speak of “contributions” or “theories” most of the time without further specification of the exact kind that these contributions or theories are.

In Section 2, I will draw upon Thomas Kuhn’s insights into the underdetermination of methodological criteria. In light of Helen Longino’s conception of background assumptions that allow the influence of personal interests and value preferences to creep into the context of scientific justification I will suggest that this “Kuhnian underdetermination” presents a severe problem for scientific theory evaluation as implicit (e.g., gender-related) biases might be shared by a scientific community and subtly influence how methodological criteria in that community are interpreted and weighed. This problem can be a *local* as well as a *global* one, as I will show in Section 3. In Section 4, I will demonstrate how the general ideas from Sections 2 and 3 apply to the specific situation of women in academic philosophy: after having shown how biases can lead to a systematic exclusion of the contributions of women in science, I will proceed by arguing that the same sort of exclusionary biases can operate in philosophy. In light of these findings, I will ultimately, in Section 5, criticize some idealistic improvement suggestions and call for pragmatic mechanical solutions instead.

## 2. How biases work in the methodological evaluation of scientific theories

In this section, I wish to illuminate how scientific quality evaluation is often biased in such a way as to lead to the exclusion of the contributions from certain social groups. At first glance, this might seem a controversial claim as there are standards guiding scientists in evaluating theories, standards which are thought to ensure the impartiality (neutrality/objectivity) of scientific quality evaluation. Such standards are, on the one hand, formal criteria such as the general fit into the thematic focus of a journal or conference, the length of a contribution, or speech intelligibility. I do not consider such formal standards to be particularly problematic as they are transparent and quite unambiguous. On the other hand (and the core of our problem), there are methodological criteria.

There has been a long-lasting debate on the role of these criteria (just consider the amount of literature on simplicity or the inference to the best explanation). Even though I cannot go into detail here, I would like to stress that I am convinced that traditional methodological criteria are in fact truth-conducive and, as such, epistemically significant. When scientists refine theories, construct and improve experiments, classify data, etc., methodological

criteria help them to distinguish signal from noise, and how this works has been shown convincingly by inductive arguments (cf., e.g., Churchland, 1985; McMullin, 1983; Sober, 2004). However, the application of methodological criteria is not without problems. Thomas Kuhn, discussing a list of traditional methodological criteria (accuracy, external consistency, simplicity, breadth of scope, and fruitfulness), which are employed in scientific theory choice in order to decide which of a number of competing theories is epistemically best, wrote that these criteria are by no means unambiguous, but are in fact imprecise. They have to be interpreted and weighed in a given context of application and are often even conflicting:

“When scientists must choose between competing theories, two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions. Perhaps they interpret simplicity differently or have different convictions about the range of fields within which the consistency criterion must be met.” (Kuhn, 1977, p. 324)

As Kuhn argues, this underdetermination of methodological criteria allows for the influence of personal preferences of scientists within theory choice. In Kuhn’s view, this is not detrimental to science, but rather epistemically fruitful: if the methodological criteria were not underdetermined

“all conforming scientists would make the same decision at the same time. [...] What from one viewpoint may seem the looseness and imperfection of choice criteria conceived as rules may, when the same criteria are seen as values, appear an indispensable means of spreading the risk which the introduction or support of novelty always entails.” (Kuhn, 1977, p. 332)

So the underdetermination of methodological criteria leads to diversity and dissent in scientific theory evaluation, and this is doubtlessly decisive for scientific advancement—on the condition of the methodological criteria being applied by a socially heterogeneous scientific community. As Helen Longino in particular has pointed out, this is because scientists’ non-epistemic (moral, political, economic, etc.) preferences influence—consciously or not—the evaluation of scientific theories via background assumptions that depend on his or her personal situation such as individual experiences, gender, race, social background, etc. Such background assumptions can influence, as Longino and others have shown convincingly, a scientist’s decisions not only within the context of discovery but also within the context of justification (e.g., Douglas, 2000, 2009; Longino, 1990, 2002). In light of the underdetermination of methodological criteria, this means that in a socially homogenous community the chosen interpretations and weightings of methodological criteria may be too narrow to sufficiently warrant epistemically fruitful diversity and dissent. Consequently, for Longino only critical discussion among scientists with different backgrounds can make biases in theory choice and evaluation visible; and social plurality in scientific communities is vital to ensure that criticism comes from all possible angles so that biases are kept in check and the impartiality (neutrality/objectivity) of scientific theory evaluation is reached (at least to a certain degree).

However, if biases are shared by an entire scientific community (or by the majority of members of that community) specific theories are highly likely to be disqualified from the outset, and such disqualification can be properly justified with recourse to methodological criteria. This mechanism is likely to work not only against voices that criticize established theories, but, more generally, against all kinds of deviant contributions: assuming that Longino’s idea of background assumptions transporting social idi-

osyncrasies into the context of scientific justification is correct, contributions from social outsiders (i.e., members of social groups that are underrepresented in a scientific community) are likely to be deviant *per se*.

In summary, although methodological criteria are actually meant to ensure objectivity in scientific theory evaluation, there can be a specific interpretation or weighing of methodological standards, depending on a scientist's background assumptions. Thus, scientific theory evaluation can be biased, and if there are specific background assumptions that are widely shared in a community, theory evaluation in that community will have a general tendency towards biases. Therefore, within a socially homogeneous community, the underdetermination of methodological criteria can, quite in contrast to Kuhn's emphasis on its epistemic merit, allow for the preservation of an epistemically detrimental scientific homogeneity; it does not guarantee the emergence of scientific plurality as biases can exclude specific contributions from the outset.

Not only is the evaluation of scientific theories biased in such a manner, but the biases are also reproduced. As James Robert Brown explains,

"the evidence forces (or at least inclines) us to a rational, objective choice. But the choice that the available evidence forces us to is the best among the *available* theories. So now we face a big problem: What happens if the set of rival theories is skewed in some systematic way?" (Brown, 1997, p. 385, emphasis in original; cf. also Brown, 2008, pp. 198–199; Büter, 2015; Okruhlik, 1994, p. 204)

Kuhn and Longino both replied to this problem. Kuhn emphasized that theory choice is not a matter of taste since any scientific choice can be discussed within the community: "scientists may always be asked to explain their choices, to exhibit the bases for their judgments." (Kuhn, 1977, p. 337) From this, he apparently concludes that scientific theory choice cannot be arbitrary (or, in his words, "subjective"). However, while it is right that, most of the time, scientific theory choice is not completely arbitrary, scientific rationale can be biased, and biases can be shared within a community. The evaluation of a scientific theory might be discussible, but the discussion itself is directed and framed by (technical, conceptual, interpretative, and stylistic) conventions. These conventions can be justified by good, explicit, and explicable reasons. However, conventions can also include unconscious biases that keep scientists from being open to unconventional, yet helpful and sometimes even innovative contributions and ideas.

Longino argues similarly that shared biases in methodological theory evaluation can be made transparent through critical interaction between scientific (sub)communities. This works, she argues, whenever subcommunities have different sets of methodological criteria because that way, methodological criteria themselves can become object to mutual criticism: if they transport any biases, this will become transparent. For instance, for making gender biases in science discernible, Longino suggests giving a set of alternative methodological criteria such as novelty, ontological heterogeneity, and complexity of interaction "equal status with the more traditional" criteria as these alternative criteria "(do or could) serve feminist cognitive goals." (Longino, 2008, p. 72 & p. 77). The traditional methodological criteria to which these alternative criteria are opposed

"are mainstream precisely because they stand in reinforcing relations with values and relations in the social context of science. [...] The practice of science is too materially dependent on its socio-political context for significant change to be possible without changes in that context." (Longino, 1995, p. 396)

While I do not think that the traditional methodological criteria are inherently gendered, but in fact truth-conducive, I consider this point important, albeit for a different reason. Longino stresses that it is not possible to implement alternative methodological criteria as long as the social context remains unaltered; in contrast to this, I believe that it would not be required to have alternative methodological criteria if the socio-political conditions changed. Longino's call for alternative criteria is misguided insofar as methodological criteria (both the traditional and the alternative ones) are underdetermined and, as such, open to interpretation and weighing in the context of their application. Consider, for example, ontological heterogeneity which contrasts with simplicity (Longino, 2008, p. 73). A contribution that is evaluated with respect to ontological heterogeneity can still be devalued as too complex, as a scientific non-finding, or as not complex enough and therefore inadequate.

Moreover, it is unclear in what respect Longino's alternative methodological criteria really differ from the traditional methodological criteria; at least, it seems that those qualities identified as novelty or ontological heterogeneity are pretty much the same qualities as those identified as fruitfulness or accuracy. This gains additional support from Kuhn's note that the traditional methodological criteria often conflict (Kuhn, 1977, p. 322): fruitfulness (like novelty) can be in conflict with external consistency, accuracy (like empirical adequacy or ontological heterogeneity) with scope or simplicity.

What I wish to stress here is, once again, that the problem is not with the criteria *but with their application*. As long as there are biases shared by an entire scientific community (or at least by the majority of members of that community), specific contributions are highly likely to be disqualified from the outset, no matter how well-founded they are, and such disqualification can be properly justified with recourse to (any) methodological criteria. Thus, while Longino is right to stress that the practice of science depends on the social context of science, her call for alternative criteria seems to miss the point.

This becomes even clearer when Longino argues that methodological criteria (as a decisive part of "shared standards" within a community) are required to identify relevant, significant criticism (cf., e.g., Borgerson, 2011; Intemann & de Melo-Martín, 2014; Longino, 1990, 1995, 1996, 2002, 2008; Miller, 2013). Consequently, methodological criteria would also be needed for identifying relevant intercommunal criticism. However, that way, community-wide shared biases could hardly be eliminated by criticism from other (sub)communities as any such criticism would be disqualified with recourse to a biased methodological evaluation of that criticism (Leuschner, 2012).

In sum, methodological criteria, although required in order to avoid chaos in the evaluation of theories, can transport and reproduce biases, and are to this extent, and in contrast to Longino and Kuhn's assumption, insufficient in guaranteeing methodological self-regulation of scientific communities (cf. Longino, 1995, p. 385). In light of this finding, I will now propose an analytical framework that will be helpful to understand how exactly biased methodological quality evaluation works.

### 3. Global and local methodological disqualification of women's contributions in academia

What counts as a relevant, significant scientific contribution is subject to interpretation in ways that may allow for systematic subtle biases in evaluating the work of social outsiders in academia. Now, how exactly does such systematic methodological disqualification of outsiders' contributions work? I suggest that it functions

in two ways to which I will refer as ‘local and global disqualification’.

*Local* disqualification means—in a situation in which the referee knows the submitter’s identity—the tendency to interpret and weigh methodological criteria *ad hoc* so that the criteria serve to disqualify the contributions of women (or other members of disadvantaged groups). When judging a scientific contribution, one can, voluntarily or not, consider the contribution of a simple theory a brilliant clarification of complex relations or dismiss it as a naïve simplification; a highly accurate theory can be regarded as perfectly adequate, or as not innovative, and external consistency as a quality feature, or as boring and unoriginal depending on the submitter’s gender.

That such local methodological disqualification in fact exists is indicated, for example, by Rossiter’s “Matilda-effect”, a specification of the Mertonian “Matthew-effect”, i.e., the fact that scientists who have already established their professional reputation often reap profits from lesser known colleagues (Merton, 1968, 1988). As Rossiter shows, this means that the achievements from unknown scientists are often wrongly ascribed to more prominent colleagues. As there are disproportionately more women scientists who are unknown, their achievements are often ascribed to male colleagues (Rossiter, 1993).

Recent empirical studies also show that the work of male and female students is evaluated differently and that women in academia have to work harder than male counterparts to gain acknowledgment as productive and competent (Graf, 2015, p. 107; Moss-Racusin, Dovidio, Brescoll, Graham, & Handelsman, 2010).

Moreover, the representation of female authors in scientific journals increases significantly when review procedures become anonymous (Saul, 2013). For instance, in the journal “Behavioural Ecology”, the implementation of anonymous review procedures led to a 33 percent increase of publications from women (Budden et al., 2008). However, this does not suffice for gender equality (i.e., equal quotients of acceptances and rejections of the submissions from men and women to a journal) as *global* methodological disqualification still remains.

*Global* disqualification means that the respective scientific community tends to interpret and weigh methodological criteria *habitually* so that the criteria generally serve to disqualify contributions from women. Such a tendency in the interpretation and weighing of methodological criteria excludes contributions of women even in anonymous review processes. And, indeed, empirical research indicates that scientific contributions from women tend to differ methodologically from male contributions. This might sound unlikely at first glance since it seems to presuppose that there is something similar about the work of women scientists, in virtue of them being women. However, there are no biological, but rather social reasons for this: studies have revealed that the work of women scientists has specific gender-related methodological characteristics due to the social situation of women in many scientific areas. Carla Fehr, drawing on surveys on the impact that the given disadvantages have on “the way women conduct their research”, writes that

“one third of elite women scientists report that their gender influences their methodology [...]. However, in the interviews respondents rarely reported that they used ‘feminine methods,’ or even methods different from those used by men. Interviewees rather report differences in the application of traditional methods in terms of using a greater degree of caution, carefulness, attention to detail, and perfectionism.” (Fehr, 2011, p. 151)

Particularly, Fehr draws on a study by Gerhard Sonnert and Gerald Holton who found that

“[r]ather than being iconoclasts, women tend to uphold to a particularly high degree the traditional methodological standards of science [...]. As a group, women, as relative newcomers to science, adopted—or were taught to adhere to—an extra-high measure of conformity to the formal norms of conducting research.” (Sonnert & Holton, 1996, p. 152)

This appears plausible indeed. In contrast to Fehr, I wish to underline, however, that this means there *are* methodological differences between the work of male and female scientists. After all, upholding “to a particularly high degree the traditional methodological standards” does shape the methods in use. Being particularly cautious and careful characteristically influences the technical, conceptual, interpretative, and stylistic choices a scientist makes, choices within the construction of experiments, the selection of instruments, the interpretation of data and other evidence, or the analysis of findings.

That women in physics, for example, in fact do more detailed work is confirmed by the fact that women physicists are oftentimes used as support staff, as “calculators, scanners, unskilled assistants, cheap labor force”, while their male colleagues are responsible for “key decisions and [...] shape] laboratory technologies” (Rentetzi, 2003, p. 174) That women do such underrated work because they (feel they) have to give extra proof of their competence and skills is confirmed by a recent study on the effects that racial disparities have on women in STEM disciplines: “77 percent of Black women reported having problems with ‘prove-it-again’ patterns of bias, such as feeling compelled to justify their work more frequently than their colleagues. [...] two-thirds of non-Black women reported similar patterns of bias.” (Morris, 2015) Apparently, (black) women scientists (feel they) have to exceptionally justify the content, form, and mere existence of their work, which characteristically shapes the methodology of their work.

The inclination towards being particularly cautious and careful also manifests in a tendency to prefer methods that allow for a more comprehensive study of contextual complexity. Consider, for example, that women in the social sciences prefer qualitative to quantitative methods (Plowman & Smith, 2011). They prefer to investigate objects of high complexity and avoid making strong hypotheses and claims. As shown above, it is possible to disqualify an ontologically heterogeneous theory as a scientific non-finding, an accurate one as not innovative, or a theory that is particularly consistent with other available approaches as unoriginal. It is likely that there is a global tendency to disqualify contributions when shaped by cautious, careful, detailed, perfectionist methodological choices,<sup>2</sup> given the tendency to publish challenging, bold claims and the general disadvantage this poses for modest studies (e.g., Dickensin, 2005; Ioannidis, 2005; Schekman, 2013).<sup>3</sup>

#### 4. Biases in methodological quality evaluation in academic philosophy

So far, I have argued that biases can lead to the systematic methodological disqualification of contributions from certain social groups in science—not despite, but actually via the use of (under-determined) methodological criteria. The interpretation and weighing of these criteria can transport biases both locally

<sup>2</sup> Of course, such global disqualification can affect male scientists as well if they happen to work in a “female” way (just as one can imagine bold female scientists). But given that the majority of male scientists tend to be confident while the majority of female scientists tend to be cautious, the evaluation of the contributions from male and female scientists will be globally skewed under the given socio-political conditions.

<sup>3</sup> Thanks to Michael Hartmann for this point.



(situationally, *ad hoc*) and globally (habitually, generally). In what follows, I will argue that such systematic distortion of the methodological evaluation of outsiders' contributions also happens in the humanities such as philosophy.

Recent empirical studies show that within today's academic philosophy, the evaluation of contributions from women is biased in mainly two ways: contributions from women appear to be systematically disqualified as (1) thematically irrelevant or (2) methodologically insufficient.

The first bias concerns the issues that are addressed in the respective work, i.e., it only pertains to the context of discovery. Still, it decisively complements the mechanism of methodological exclusion, as I will show below.

There is a common division into "hard" and "soft" areas of philosophy, the former like epistemology, philosophy of mind, and philosophy of language supposed to be done by men, the latter like ethics, applied ethics, social and political philosophy by women (cf. [Dodds & Goddard, 2013](#), p. 159). As, for example, Haslanger writes, there seems to be a general thematic evaluation that leads to a global preference of contributions on "hard" topics and to a discrimination of contributions on "soft" topics (cf. [Haslanger, 2008](#); [Jenkins, 2013](#), pp. 91–92; cf. also [Jenkins & Hutchison, 2013](#)). Elisabeth Lloyd's work on discrimination of specific areas of science studies sheds additional light on this: feminist science studies have been judged (e.g., at the Nobel Conference 1989, and also in various discussions, monographs, and reviews, as Lloyd demonstrates) as "anti-science", relativistic and constructivist, indoctrinated by left-wing idiosyncrasies or simply irrational ([Lloyd, 1996, 2008](#)). This suggests that specific areas such as feminist approaches—in science in general and in philosophy in particular—are devalued from the outset when prejudices become corroborated and widely disseminated in society and scientific communities.

In contrast to these thorough insights into the systematic global disqualification of *topics* that are preferred by women philosophers, it is still largely unclear whether gender biases also distort the context of justification in academic philosophy, i.e., whether there is systematic *methodological* disqualification of contributions from women philosophers on the local and/or global level as well.

While there is certainly a strong need for further quantitative and qualitative research here, there has already been a growing amount of (anonymously provided) anecdotal evidence for the methodological exclusion of women from the philosophical establishment on the local level<sup>4</sup>: female students feel they have to work twice as hard as their male colleagues to get even roughly comparable marks and that their questions in discussions tend to be disqualified as irrelevant or simply wrong, or women faculty members report that their work is treated with respect only when it is evaluated blindly—experiences which are probably the exception for male philosophers while most women in philosophy are apparently familiar to them. Methodologically, the academic philosophy community seems open to many different technical, conceptual, interpretative, and stylistic approaches (cf. [Hutchison, 2013](#); [Zuckerman & Merton, 1971](#), pp. 76–77), and at first glance there is no reason to suspect methodological disqualification of the work of women philosophers on a global level. However, the mere fact of methodological plurality in academic philosophy does not mean there cannot be shared biases leading to the methodological disqualification of specific contributions. It does seem likely that, as in many scientific areas, women in academic philosophy characteristically tend to be cautious and careful. In philosophy, this might take the shape of an inclination towards the detailed reconstruction

of other's work and to being fastidious in recounting the exact history of their thoughts. At the same time, it is probable that such cautious and careful contributions in academic philosophy are methodologically disqualified in the manner I termed "global" since there are good reasons to assume a publication bias in philosophy exists, giving the advantage to challenging, bold claims. Philosophical quality, too, is often identified with arguing for a strong, original position or for refuting someone else's position ([Haslanger, 2008](#); [Hutchison, 2013](#); [Jenkins, 2013](#), pp. 87–88). Thus, the second bias—the tendency to disqualify the work of women in philosophy as methodologically insufficient—appears to function both on the global and the local level.

Finally, it is intriguing that both kinds of biases in philosophy, thematic and methodological, complete one another in a dilemmatic way: if women choose to contribute to areas dominated by men, they will tend to be careful and cautious and their work will be disqualified for methodological reasons. Conversely, if they choose to contribute to areas in accordance with their specific interests and can argue confidently, they will have to face thematic disqualification.<sup>5</sup> As long as the work of women in philosophy differs—methodologically or thematically—from the work of their male colleagues it is likely to be dismissed. And due to local methodological disqualification, the work of women philosophers is disqualified even if it does not differ at all.

## 5. How to tackle the problems?

I have argued that social exclusion in sciences and humanities is caused not only by aggressive, combative, disregarding social behavior, but also by unconscious biases that are deeply embedded in academic practice, namely in the methodological evaluation of theories. If there are biases that are widely shared by an academic community, a systematic disqualification of contributions from outsiders is likely, and if the shared biases are gender-related, they can lead to the systematic exclusion of the contributions of women. If these considerations are correct, the prospects for change appear difficult.

With respect to the case of philosophy, many suggestions have recently been made on how to make a change in the social and cognitive practices in academic philosophy: more women should be invited to conferences and colloquia; in classes, the work of women philosophers should be discussed on a regular basis ([Saul, 2013](#), p. 50). Women philosophers should experience direct and indirect encouragement more often, indirect encouragement meaning *micro-affirmations* such as "opening doors to opportunity, gestures of inclusion and caring, and graceful acts of listening [...], providing comfort and support when others are in distress, when there has been a failure at the bench, or an idea that did not work out, or a public attack." ([Brennan, 2013](#), p. 194) I consider these and related suggestions good and important. Still, the question remains "why in the face of goodwill and political commitment [...] philosophers [have] failed to meet goals to which we have collectively committed". ([Brennan, 2013](#), p. 182).

As Saul points out, "[m]any admissions and hiring committees have a commitment to improving gender balance and perhaps even to choosing a woman over an equally qualified man—but implicit biases may well prevent them from seeing which women are equally qualified." ([Saul, 2013](#), p. 52) Indeed, the problem is that biases can "influence our choice of keynote speakers and literature (even if we are committed to equality); they can prevent us from being encouraging and micro-affirming (even if we actually want to be supportive); they can influence our behavior and shape the way

<sup>4</sup> Cf. <http://beingawomaninphilosophy.wordpress.com/>.

<sup>5</sup> Thanks to David Ludwig for this point.

we reflect the standards of our discipline (even if we intend to be critical in an egalitarian sense).” (Leuschner, 2015, p. 248) And Jenkins points out that meritocratic selection, although it “should lead to the best being chosen [...] has supported] the self-reproduction of elite groups [...] and limited] the value of work produced relative to other, more pluralistic standards [...]. Meritocratic judgments are made within a field that is profoundly shaped by gendering operations” (Jenkins, 2013, p. 82 & p. 84).

Therefore it appears plausible to call for additional solutions, which Samantha Brennan appropriately names “mechanical” (Brennan, 2013). Imposed on academic processes by policy-makers or academic communities themselves, mechanical solutions stand in contrast to idealistic commitments from individuals. They are, as such, immune to biases—also to methodological biases of the sort I have analyzed here—and can, as such, provide a frame in which theory assessment becomes more responsible, accountable, and inclusive.<sup>6</sup> In what follows, I will discuss three kinds of mechanical solutions.

(1) Mechanical changes in publication procedures: It is important, for instance, to strictly adhere to alphabetical order in co-authored publications. Otherwise it is likely that contributions from less prominent researchers are marginalized (West et al., 2013).

Also double blind review procedures are helpful for women to get their work published, as we have seen. But editors or conference organizers can still decide to desk-reject or to reject in light of conflicting reviews and these decisions are prone to be biased in the way described. Triple-anonymous review procedures could be helpful here, i.e., submission systems that provide authors’ anonymity not only with regard to referees, but also to editors (or conference organizers).

There is one standard objection against the effectiveness of (all kinds of) anonymous review procedures, uttered, e.g., by a journal editor: “Removing the name and affiliation of the author does not make a manuscript anonymous. A competent reviewer can tell at a glance where the work was done and by whom or under whose guidance.” (quoted in Zuckerman & Merton, 1971, pp. 86–87) However, while it seems likely that competent referees are familiar with their research community and will often be able to identify another established community member by the content and style of his or her work even under anonymous conditions, a referee will mostly not be able to identify authors that are not so well-established and so he or she cannot be completely sure who the author of that work is. This alone could be helpful for women to get fairer reviews.

Another problem arises from my own argumentation above: if my argumentation is correct, even triple-anonymity remains ineffective to some degree as there is still global thematical disqualification, and the methodological differences (modesty, cautiousness) of women’s works would remain. Still, triple-anonymous review procedures would certainly encourage women to be more confident in what they do in their research and what they aim to discuss. The thematical bias, again, can be tackled in (2).

(2) Mechanical changes in evaluative standards: The standards used in theory evaluation can be (partially) changed. While I did not consider Longino’s suggestion of alternative methodological criteria convincing, I think it would help to bring criteria of another sort into play. As Janet Kourany suggests, social criteria should be taken into consideration when it comes to the evaluation of scientific contributions (Kourany, 2008, 2010), a point Longino also makes, suggesting to add two social criteria to the list of

methodological criteria: applicability to human needs and decentralization of power (e.g., Longino, 1995, p. 389, 1996, p. 48, 2008, p. 72). I agree with Kourany and Longino on this point, as these criteria are not methodological but social-political values. They are, as such, not open to interpretation and weighing, but transport a specific political agenda into the arena of theory evaluation. For example, this would support having feminist contributions not only “in journals devoted to this special topic, but in a plurality of journals, posing robust challenges to uncritical disciplinary norms and boundaries.” (Jenkins, 2013, p. 99) In sum, including social-political values would help to tackle thematical bias.

(3) Mechanical changes in the employment situation: Finally, I want to take a stand for hiring quotas, which I expect to be the most controversial sort of mechanical solutions. I decided to include this point as the problem of systematic methodological disqualification of women’s contributions is reciprocally connected to the social exclusion of women scientists in academia: methodological disqualification of contributions from women in academia entails the trivialization and ignorance of their theories and approaches in the communities. Women in academia have a harder time to get articles accepted, they do not receive invitations to conferences and institutional colloquia as often as men, and these disadvantages lead to them not getting (good) jobs. As a consequence, they do not get into positions that would allow them to become more confident in both their behavior and their work. I consider affirmative action programs helpful.<sup>7</sup> As Adriane Rini writes:

“For a start, we can advertise positions as open rank and open specialty in order to catch the widest possible interest. We can then make diversity an explicit priority in our appointments. It may be that at least in the short term, these must be enforced by some sort of quota system. I am aware that some will resent having quotas. Indeed I am myself unhappy that it seems the only solution. But surely everyone must agree that we have a problem. We don’t want the hiring ratio to become 25:1, or 30:1, or 35:1, or ..., with everyone still agreeing that there is a problem” (Rini, 2013, p. 142).

As soon as it is empirically proven that there is systematic discrimination in the hiring procedures of a community due to widely shared, self-reproducing biases, quota regulation appears to be a justified measure to improve the situation.

Of course, there are challenges to an implementation of quotas in academia. To start with, the pool of qualified candidates has to be sufficiently diverse. Quotas could merely ensure that mainly those women get ahead who share the traditional biases, even at the cost of sophisticated, unbiased men. However, these problems have turned out to be not too severe; in contrast, we know from studies on affirmative action programs in academia that the requirement that

“[...] institutions had to find out the number of women who might be available for any given job opportunity [...] has caused the higher education community to know itself in a way it never had before. Further, the increasingly sophisticated and extensive data on candidate availability had a strong impact on many open-minded, research-oriented faculty members who had been genuinely convinced that adequate women candidates did not exist. The requirement to advertise positions created an additional method, often outside the traditional networks, of

<sup>6</sup> Thanks to an anonymous reviewer for this journal who asked me to clarify this point.

<sup>7</sup> It is vital that an implementation of quotas means numerical requirements not only for hiring, but also for promoting women; the problem that women are hired but not tenured is so wide-spread that it is considered a “revolving door” phenomenon (Chamberlain, 1991, p. 178).

bringing women to the attention of search committees.” (Chamberlain, 1991, p. 176)

In light of these findings, I consider it likely that quotas in academia would be required not only for social and moral reasons (equality and fairness), but also for epistemic ones. In academic philosophy, for example, there are a number of fields that have been introduced or substantially developed by women philosophers such as care ethics, ecofeminism, feminist bioethics, embodiment, intersectionality, standpoint epistemology, and feminist science studies (Friedman, 2013, p. 32; cf. also Fehr, 2011, p. 137). Not to exclude the work of women philosophers is desirable for the sake of philosophy itself as there are “talented philosophers not being encouraged to continue, not getting grants, not getting jobs, not getting promoted, and not getting their work read. Moreover, talented and committed female philosophers are producing less good work than they otherwise would.” (Saul, 2013, p. 50) Carla Fehr puts it in a nutshell:

a simple “diversity promotes excellence theory [such as Longino’s] can be consistent with the exploitation of members of marginalized groups and with inequitable employment patterns in the academy. This can provide a misleading answer to the ‘what is in it for me?’ question, because it seems to show that one can reap the benefits of epistemic diversity without employing women, or members from other underrepresented groups, and in fact without even rewarding them for their epistemic diversity work.” (Fehr, 2011, p. 152)

Such “diversity free riding” doesn’t work in the long run since such exploitation leads to the loss of social diversity, which is why Fehr concludes that “theories such as Longino’s must be further developed.” (Fehr, 2011, p. 139) This paper is meant to be such a further development.

## 6. Conclusion

As studies have shown, the situation of women in philosophy requires substantial change. There is overwhelming evidence—provided by data on employment, salaries, publications, invitations—that women philosophers are severely disadvantaged vis-à-vis their male colleagues. Alongside these hard facts, the number of anecdotes and qualitative research results about the climate in the philosophical community being hostile to women has increased. This paper has sought to fill a gap in the understanding of the mechanisms that lead to the underrepresentation of women in particular sciences such as in philosophy.

I have shown that social diversity in science would not be achieved sufficiently through epistemic self-regulation, as the methodological standards that are used to evaluate scientific theories are open to interpretation and weighing. This Kuhnian underdetermination of methodological criteria allows for the systematic methodological disqualification of contributions from underrepresented social groups when these contributions share specific characteristics. Biases that are broadly shared in a scientific community allow for the systematic methodological disqualification of contributions from outsiders. Under gender-related biases the contributions from women—tending to be cautious and detailed—are systematically disqualified. Systematic methodological disqualification can be local (the tendency in a scientific community to interpret and weigh methodological criteria situationally *ad hoc* in such a way as to disqualify the contributions of women) or global (the respective scientific community tends to interpret and weigh methodological criteria *habitually*, so that the criteria generally disqualify the contributions of women).

This is not a problem exclusive to the natural sciences, but can be found in all academic disciplines, also in the social sciences and humanities, also in philosophy. Empirical data indicates that women’s contributions in philosophy are globally disqualified by topic and, both locally and globally, by the methods that tend to differ from male ones. Both kinds of discrimination, thematic and methodological disqualification, complement one another.

If this argument is correct and (gender-related) biases in academia are so deeply embedded in the practices of methodological quality evaluation, we have good reason to be doubtful that well-intentioned commitments will suffice to substantially improve the situation. Therefore, it seems reasonable to call for mechanical solutions to make a real change in the respective scientific fields. Such a change would be desirable not only for social and moral reasons but also for epistemic ones. In philosophy, the equal representation of women would lead to an upvaluation of contributions that have been unwarrantedly devalued, hence systematically disadvantaged, and, consequently, underdeveloped so far; it would be good for the sake of philosophy.

## Acknowledgments

I wish to thank the participants of the “Feminist Philosophy of Science” workshop in Ghent in November 2014 and of the Philosophy of Science colloquium at TU Darmstadt where earlier versions of this paper were presented. Thanks also to two anonymous reviewers for this journal as well as to Gregor Betz, Stefan Böschen, Angela Graf, Michael Hartmann, and Laszlo Kosolosky for their helpful and supportive comments. And very special thanks go to Stefan Gärtner, David Ludwig, and Robert Mitchell for thoroughly reading and commenting the manuscript.

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