

Gatekeeping Science – How Methodological Critiques Police Feminist Research.

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Abstract

In this analytical essay, we examine critical receptions of our recently published study on gendered inequality.

Our study received numerous extremely critical comments about the biological properties of gender, assumptions about the political stance, and the scientific integrity of the authors, and most importantly of whether the paper was even scientific. We identify two types of criticism: dismissive and scientific criticisms, and discuss the latter using Skov (2022). On closer inspection, our paper does in fact adhere to the criteria put forward by Skov.

We therefore argue that as this paper was exposed to the very same criticism most feminist research is exposed to, such critiques should not be understood as methodological critiques, but instead a misogynistic policing of science and scientific practice. The critiques mentioned above are not about ensuring scientific quality, nor about methodological standards, but rather about gatekeeping science, and preventing scientific studies that expose privilege and make claims aligning with feminist positions, from obtaining legitimacy. We draw here on the theory of misogyny as the policing of gender roles, as proposed by feminist philosopher Kate Manne.

KEYWORDS: gender research, inequality, positivism, antifeminism, misogyny

[W]hen it comes to your professional peers, I can almost guarantee that many of them will not entertain the idea that you are engaged in a different intellectual enterprise with different epistemological underpinnings. They will simply assume that you are doing what they are doing, only badly. (Luker, 2010, p. 40)

Introduction

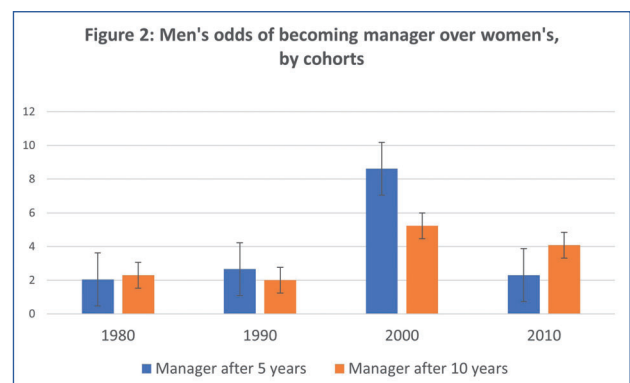
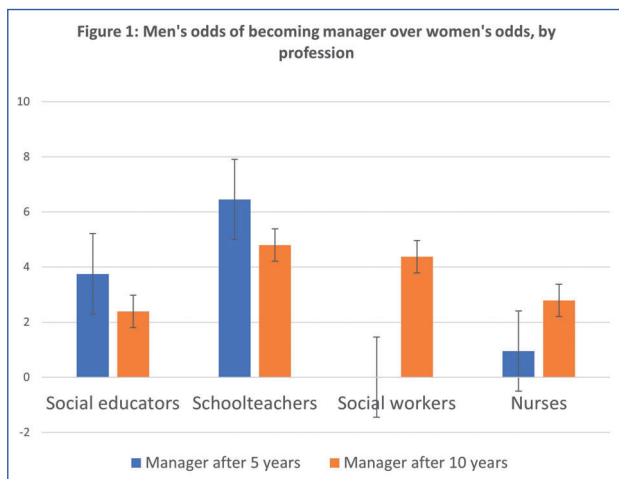
Feminist research often comes under attack for not adhering to scientific standards. In recent years, both individual Danish researchers, and entire methodologies (e.g. autoethnography) have come under such attack. In 2021, after a discussion on research into gender and migration, and their methods, a majority of the Danish parliament voted to impress upon Danish universities the need to ensure that political positions were not “disguised as research” (Forslag til vedtagelse V137, forespørgsel F49, 2021). One might then be tempted to assume that if only one did research in more traditional and quantitative ways, one might avoid such attacks. This, unfortunately, is not the case. In this analytical essay, we will exemplify this with a paper we published recently. We argue that these attacks are less concerned with methodology than they make out. Firstly we briefly present the paper, and then present an overview of the responses that this paper was met with in print and social media. We then examine the underlying scientific criteria, and discuss both whether these

are universal, and whether they apply to our study. Finally, we propose that this criticism is best understood as a pretense, and the intent is an antifeminist one, namely the delegitimization of feminist research.

A study in gender inequality

In 2021, we published an analysis of gendered inequality in access to manager positions within Danish welfare professions (Frederiksen & Poulsen, 2021). This study used data from four decades and followed welfare professionals career trajectories post-graduation. The data was extracted from Statistics Denmark, and besides data on employment, we used data on the professionals’ gender, social class origins, and citizenship¹. Through logistical regression, we estimated the odds-ratios of different social groups becoming managers after five and ten years, respectively². Our findings were relatively unsurprising and in line with similar research, confirming the persistence of gender inequality. We show the main findings in the figures below.

Figure 1 shows the odds-ratio of men versus women of obtaining a position as manager for each profession³. This plot should be interpreted for e.g. schoolteachers: men have 6.5 times better odds than women of becoming managers five years after graduation, and 4.8 times better odds after 10 years. Essentially, figure 1 demonstrates that men have significantly better odds of becoming managers, for all four professions, and only in the case of nurses, five years after graduation, do we see equality (an odds-ratio of 0.95). This equality disappears when we examine nurses after ten



years, where men have 2.8 times better odds of becoming a manager.

Figure 2 shows changes in odds-ratios over time. The plot should be interpreted the same way as figure 1, so in the case of the welfare professionals who completed training in 2010, men have 2.3 times better odds than women of becoming managers after 5 years, and after ten years, men's advantage increases to 4 times better than women's. Figure 2 thus demonstrates that men's advantage has remained stable from 1980 until today, with a massive increase in 2000.

In summary, our study showed that men in welfare professions are significantly more likely to obtain management positions than women educated at the same time, which is evidence of gender inequality in access to management positions. In the following, we will look at some criticism of the study, and its scientific merits.

Dismissive and scientific criticism

Our paper got a quite harsh reception, on social and print media, emails sent to the authors, etc. We collected more than a hundred critical comments and found them to fall into these categories:

Dismissive criticisms:

- a. "women and men prioritize differently"
- b. "women are naturally disinclined to become managers"
- c. "women are emotionally unsuited for leadership"
- d. "women are not victims of inequality, since [examples of women in power]"
- e. "this is not a real issue, you should focus on [other issue]"

Scientific criticisms:

- f. "managers are selected by merit, not gender, because in other professions women prevail"
- g. "if you didn't talk to hiring committees, you can't know whether gender played a role, and have no evidence of inequality"
- h. "since it is feminism, it isn't scientific"
- i. "this is wokeness, and not scientific"

We note that there are two different criticisms here. Points a, b, c, and d claim that differences between men and women do not express inequality, but merely preferences or natural differences. These claims have all been dismissed by research, and much of that research is discussed in our study (Corsun & Costen, 2001; Green & Cassell, 2008; Guillaume & Pochic, 2009; Kagan, 2021; Schoen & Rost, 2021). Point e claims that this inequality issue is unimportant compared to e.g. men's suicide rates, discrimination of persons below-average height, and issues women in the Global South are faced with. This is known as "what-aboutism" and is an attempt to derail the discussion rather than an actual argument (see, e.g. (Curtis, 2022) for a discussion of what-aboutism).

These five points of criticism we term dismissive criticism because they dismiss inequality, rather than question the evidence of it.

But the latter four points deserve a more substantial discussion, since they attack the scientific status of our paper, either by claiming we are wrong (f), that we employ an incorrect methodology (g), or voice suspicions about our political stance (h,j). These points state that we, either through negligence or malicious intent, have not delivered sufficient evidence to support our claim. They are voiced in a manner which seems to state that inequality would certainly be problematic, but fortunately, no such inequality is documented here. We will term this scientific criticism.

The dismissive criticism is not interested in debating gender inequality, but rather in silencing troubling and unwelcome claims, perhaps most evident in points d and e. The scientific criticism, at the very least, feigns an interest in the debate but then abandons it, lamenting the scientific inadequacy of our study. Several commenters in this vein underscored their research experience or affiliation with universities, invoking scientific authority. We do think that this scientific criticism is worth engaging with, albeit in an analytical way. We will first examine a more extensive criticism of this kind, and whether it really does apply to our study. We then present a feminist analysis arguing that this criticism in fact serves a completely different purpose, no different from dismissive criticism.

Canonical social science and positivism

Sociologist Kristin Luker has proposed the term *canonical social science* to describe the assumptions embedded in the dominant professional culture of social science. These assumptions describe scientific practice, and what procedures ensure that analyses and findings produced are valid, and consequently, also what procedures do not. Luker argues that canonical social science is nowhere near as universal as it is taken to be, but warns her reader that practitioners of canonical social science will not be aware of this, with the quote prefacing this essay. The four points of scientific criticism above are an example of this; that is, they are a case of the hypostatization of the specific criteria of scientificity that canonical social science subscribes to.

These four points of criticisms described above did not lend themselves to an analysis of their criteria, of what makes good science. But in the extensive Danish debate of gender research mentioned earlier, pundits have written extensively on this. One such pundit who was cited by Danish Right-wing politicians in relation to this debate was epidemiologist Torsten Skov (Ringgaard, 2023). Skov has also, online and on his personal website (begrund.dk), participated in attacks on feminist research on numerous occasions. Skov did not participate in the criticism of our paper, but his position aligns with the scientific criticism above and exemplifies the position in much more detail. In "Pseudo-science – 20 essays in defense of science" (Skov, 2022), he is very much in line with the scientific criticism above, as will be seen. All references in the following refer to Skov (2022).

In the preface Skov states plainly: "[...] great parts of [...] feminist science does not satisfy the most basic requirements of science[...]" (p.9). Specifically, Skov underscores the following issues with feminist science:

- it uses statistics wrongly (p. 9, 23)
- theoretical concepts are ill-defined and used unexplained, without explicit operationalizations (p.9, 115ff.)

- there is no evidence of the claims made (p.17, 49ff.)
- it makes invalid arguments (p.67ff.)
- it (repeatedly) draws on claims that have no scientific basis (p.49ff., 55ff.)
- it does not operate from scientific rationality (p.23), even claiming that there can be no common rationality (p.26)
- it proposes alternative (sic) epistemologies and ontologies (p.24)
- it is subjectivist, ideologic, and activist (p.95ff., 127ff.)

This list is incomplete but sums up the main points of Skov's position⁴. Invert these criteria, we arrive at Skov's implicit criteria, for what constitutes science:

- science uses quantitative methods correctly
- science uses clearly operationalized, well-defined concepts
- science provides evidence for its claims
- science makes logically coherent arguments
- science only draws on other science satisfying these criteria
- science operates rationally
- science has only one correct epistemology and ontology
- science is objective, neutral and does not meddle in politics

These criteria are all associated with positivism: claiming that science is a matter of disinterested examination of an outer world adhering to deterministic or stochastic laws. Science must strive to produce objective and logically coherent descriptions of the world, through theoretically informed and testable hypotheses. In short, the argument Skov makes is that science equals positivism, *sine qua non*.

In the rest of this essay, we will demonstrate the shortcomings of this position, and suggest that it is in fact motivated by another, less savory intent. First, positivism is not a universal position, but rather has obtained a privileged position for historical and political reasons. Secondly, our study does not, in fact, fall victim to scientific criticisms

proposed by Skov. We argue that this shows that both scientific criticism and dismissive criticism are attempts at policing science misogynistically.

Positivism is not universal

Sociologist George Steinmetz has examined why “U.S. sociology still [operates] according to a basically positivist framework” (Steinmetz, 2005, p. 276). Steinmetz answer by examining how the position of *methodological positivism* came to prevail in American sociology post-World War 2. Steinmetz extrapolates this position from writings, and practices of leading American sociologists and journals. It is a cluster of empiricist ontological assumptions, positivist epistemological precepts, and scientistic naturalism (Steinmetz, 2005, p. 281). *Empiricist ontology* denies that phenomena can be caused by underlying structures or mechanisms that elude human experience, implying that whatever exists does so independently of human observers (Carnap, 1950). This connects well with *positivist epistemology* in the understanding of Carl Hempel, wherein scientific explanation must refer a particular case to a general *covering law* (Murphey, 1986). Finally, methodological positivism also entails subscribing to *scientistic naturalism*, claiming that “the social world can be studied in the same general manner as the natural world” (Steinmetz, 2005, p. 283). This has two implications: First, methodological positivist science overlooks concept dependency, i.e. that “social practices and structures are inextricably bound up with peoples interpretations of the world” (Steinmetz, 2005, p. 283). An epistemology that does not take account of concept dependency assumes that things and ideas are completely different – a problematic position when applied to subjects of research who are themselves aware of being researched. Secondly, the scientistic naturalist position also implies a dichotomy between facts and values. Both dichotomies are problematic if one is to study gender inequality – is it possible to study inequality without those who are affected by this inequality having an opinion about it? Can inequality be merely factual or is the value

of gender equality intrinsically embedded in that object of research?

Comparing methodological positivism to Skov’s position, and the scientific criticism above, both clearly subscribe to the methodological positivism position. Demands for objectivity, neutrality, absence of political positions, and complete rationality follow from the dual dichotomies implied by scientistic naturalism; that concepts should be well-defined also makes sense if concepts are independent of both researchers and informants; that epistemology and ontology can only be respectively positivist and empiricist also leads to the demand for objective evidence, logical arguments drawing only on similarly logical and evidential research, and, of course, also to the position that other epistemological or ontological positions must be wrong. And finally, the demand that statistics be used correctly reiterates the point that methods are technical procedures, their correct application derivable from the epistemology and ontology of methodological positivism. Similarly, the scientific criticisms of our study are derived from the dichotomies of facts and values, and the denial of underlying structures. When there are no underlying structures, only conscious, discrimination can cause inequality. It follows from these arguments that methodological positivism is in complete opposition to any form of social constructivism and poststructuralism.

These positivist positions are not explicit in most sociological education – rather these positions are implicitly embedded in how methods and in particular statistics are being taught, as a matter of technical procedures (Steinmetz, 2005, p. 280). This helps us understand the claim that feminist research is not science – after all, if one understands there to be only one way of conducting scientific research, then any deviation from those procedures surely must be an error – or, paraphrasing Luker, bad science by a bad scientist. However, in most of the history of epistemological discussion, numerous other positions have appeared, and indeed the constructivist and poststructural positions, excluded by methodological positivism, are in fact very much on the rise in much of social science and humanities.

Steinmetz's analysis demonstrates why methodological positivism has come to occupy a privileged position, from which it may dismiss other epistemological positions, and he argues that this position of privilege stems from the social-epochal development in society at large. Leading journals and debates in sociology in the 1920s and 1930s harbored much more diverse epistemology and methodology. Referencing Merton, Foucault and Haraway, Steinmetz argues that the explanation for the dominance of positivism can be found in post-World War 2 capitalism, industrialism, and Fordism in the US (Steinmetz, 2005, p. 287). In other words, the dominant position of methodological positivism has not come about through its scientific qualities, but through its congruency with the political and economic developments in the US, and the global position of the US after World War 2. The assumption of universality of methodological positivism stems from this position of dominance, and not from researcher consensus about the qualities of methodological positivism. The arguments that non-positivist positions are scientifically inadequate originate (as is the case with Skov) from natural sciences, where the dichotomies between facts and values, and between things and ideas, as embedded in methodological positivism, are often considered less problematic.

An inverse strawman

Having made the case that Skov and scientific criticism erroneously assumes the universality of methodologic positivism, we now return to our study. Our study does in fact satisfy the scientific criteria presented by Skov. We use well-established statistical techniques, commonly used to determine inequality. Our concepts are clearly operationalized and draw on validated international schemes of operationalization of class and occupation. We also provide explicit quantitative evidence of our results, we argue from this evidence and from the research we address in the paper, and, while we subscribe to a constructivist epistemological position, this does not affect

the statistical analysis. We do suggest that our findings should have political consequences, but this is not part of the actual study. In short, Skov's position is not at all incompatible with our study. This leaves us with the question of why our study was ever targeted by scientific criticism. We can imagine two possibilities: The (unfortunately likely) possibility that our critics did not bother to read the paper, or that they did in fact read the paper, but made the criticism, ignoring that it did not apply. Either way, the criticism does not enter into a serious debate with how we conducted the study, how the data were analyzed, etc. That is perhaps the crux of this criticism: It appears to make a nuanced methodological call, taking exception not with the question, nor findings of our study, but rather with the intricacies of our procedures. But on closer inspection this turns out not to be the case; rather, the scientific criticism appears to be no different than dismissive criticism: It is an attempt at dismissing our findings, only now superficially draped in the vernacular of methodology.

This is an inversion of the disingenuous strawman-argumentation, wherein one knowingly misrepresents the proposition made by one's opponent. This fallacy serves to either make the impression that said proposition is much more radical, than what is actually the case, or to present the proposition in a form that is easier to defeat in argumentation. In our case, it is not our proposition, but the criticism of it, which is being presented in a disingenuous form. The criticism is presented as a form of red herring fallacy, or smokescreen, and hides the more nefarious strategy of delegitimizing our study, without doing so through explicitly antifeminist statements.

Gatekeeping science

We cannot conclusively show that scientific criticism does not differ substantially from the dismissive criticism. Yet it is puzzling that such criticism does not aim at substantial scientific debate – if they did, they would have learned that this criticism does not apply to our paper. Yet, as most of this criticism appears online, in public

fora, and in connection with our findings being disseminated through public media, it does seem reasonable to assume that the authors of the criticism want to influence the discussion, and possible consequences, drawn from our findings. We are then at somewhat of a loss to determine the genuine intent of the authors of the criticism. We suggest that the answer to this conundrum of not really examining the study, and at the same time publicly decrying it as unscientific is to understand the criticism as a misogynistic form of gatekeeping.

Feminist philosopher Kate Manne has suggested that we understand misogyny as “one strand among various similar systems of domination” that “serv[e] to uphold patriarchal order” (Manne, 2019, p. 19). More specifically, Manne argues that “[a] woman’s perceived resistance to or violation of the norms and expectations that govern [women’s] roles would naturally tend to provoke [misogynistic] reactions” (Manne, 2019, p. 49). Misogyny is the policing of patriarchal gender norms, and Manne proposes a definition of misogyny, from which one point is very salient:

Constitutively speaking, misogyny in a social environment comprises the social forces that [...]

- b. serve to police and enforce a patriarchal order, instantiated in relation to other intersecting systems of domination and disadvantage that apply to the relevant class of girls and women (e.g. various forms of racism, xenophobia, classism, ageism, transphobia, homophobia, ableism, and so on.) (Manne, 2019, p. 63)

When our findings are faced with dismissive criticism, this is a way of attempting to silence a certain class of voices, perceived to represent women, and transgressing patriarchal norms. Manne identifies misogynistic norms as linked to the perception of women as demanding or *taking* something, rather than sticking to their role in patriarchy as *givers* (Manne, 2019, p. 279ff.). Kristin Luker arrives at a similar point in her analysis of abortion debate where pro-life positions serve to restrain women from straying too far from their perceived roles:

The [pro-choice position] can therefore be seen as an attack on a symbolic lynch-pin that held together a complicated set of assumptions about who women were, what their roles in life should be, what kinds of jobs they should take in the paid labor force, and how those jobs should be rewarded. “Equal pay for equal work” was already a revolutionary demand in this context, but until women could get equal work, even this demand was irrelevant. And women could not get equal work until they could challenge the assumption that their work activities were, or ought to be, or might be subordinated to family plans. (Luker, 1985, p. 134)

What Luker says about the liberation of women through the rights to abortion, and the pro-life responses, is comparable to the criticism of our study. Statements about the unequal opportunities for becoming a manager challenge the subordination of women’s work positions to their position in family. Thus, dismissive criticism emphasizes the choice not to apply for a position as manager as voluntarily and related to innate characteristics or values of women; this reinforces the congruency between not being a manager, and staying in one’s place, according to patriarchal norms.

There is then no real difference between scientific and dismissive criticism when it comes to the effect. But the scientific draping of the intent to dismiss and silence, serves to delegitimize not just our work, but the entire project of feminist research, of documenting the existence, nature and experience of gender inequality and challenging it. This is why it is unimportant whether the scientific criticism really applies to the study at hand – the scientific criticism is a synecdoche of the patriarchal stance towards feminist research *tour court*, and so it is irrelevant whether the criticism does apply *in casu*, because the issue is the general disallowance of research challenging patriarchy. This is also why this criticism is applied to feminist research regardless of methodology; the issue was never the differences between quantitative methods and autoethnography etc. In scientific criticism, transgressions against methodological

positivism substitute for transgressions against patriarchal norms, as the action requiring a misogynistic policing of norms.

Conclusions

We have shown that the scientific criticism of our paper erroneously assumes universal acceptance of methodological positivism. These criticisms turn out to be irrelevant and misapplied, and do not intend to debate the scientific merits of our study, but is a pretense to dismiss its findings. Such scientific criticism is an attempt at gatekeeping science, barring the feminist endeavor from recognition as science. Our statements purporting to be scientific, while challenging patriarchal norms,

thus elicits misogynistic responses trying to police and enforce those norms. Patriarchal norms are being tacitly upheld through such faux scientific criticism, and any attempts at methodologically accommodating such criticism will most likely be futile. This then raises the question of whether it then is worthwhile responding and taking such criticisms at face value? We will conclude by letting Luker answer that question, as she does in direct continuation from the quote that prefaces this essay:

You must always be prepared to defend your research method at the outset, and you will have to be much more thoughtful and better prepared than any of them will ever be. (Luker, 2010, p. 40)

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Notes

- ¹ We used data from Statistics Denmark and are thus limited by how data on gender, income, occupation, and ethnic origin are being registered in Denmark. This hampers the analysis in terms of gender and ethnicity, being limited to binary gender, and parental citizenship for ethnicity. We recognize that in using these categories, we are also reproducing them, and contributing to analytical erasure of other identities of ethnicity and gender.
- ² For further details, please consult the original study.
- ³ Odds-ratios for social workers cannot be calculated after 5 years, due to small numbers of managers.
- ⁴ One point, omitted from this list, is Skov's radical suggestion for screening criteria when vetting publications for pseudo-science, the first of which is whether the research rests on French post-structural philosophy (p.187).