Abstract:

I have argued that political values are beliefs informed, more or less well, by the evidence of experience and that, where relevant and well-supported by evidence, the inclusion of political values in scientific theorizing can increase the objectivity of research (e.g., Clough 2003, 2004, 2011). The position I endorse has been called the "values-as-evidence" approach (Goldenberg 2013). In this essay I respond to three kinds of resistance to this approach, using examples of feminist political values. Solomon (2012) questions whether values are beliefs that can be tested, Alcoff (2006) argues that even if our values are beliefs that can be tested, testing them might not be desirable because doing so assigns these important values a contingency that weakens their normative force, and Yap (2016) argues that the approach is too idealistic in its articulation of the role of evidence in our political deliberations. In response, I discuss the ways that values can be tested, I analyze the evidential strength of feminist values in science, and I argue that the evidence-based nature of these values is neither a weakness nor an idealization. Problems with political values affecting science properly concern the dogmatic ways that evaluative beliefs are sometimes held—a problem that arises with dogmatism toward descriptive beliefs as well. I conclude that scientists, as with the rest of us, ought to adopt a pragmatically-inclined appreciation of the fallible, inductive process by which we gather evidence in support of any of our beliefs, whether they are described as evaluative or descriptive. (250 words)

1. **Introduction:**

Feminist and other philosophers of science attentive to the social and historical context of science and epistemology, have argued that there is nothing in the practice of science that guarantees the results will be free of evaluative elements and that we should not be surprised that even the more straightforwardly descriptive elements of scientific research embody, endorse, or appeal to, at least implicitly, any number of values (e.g., Longino 1990, Okruhlik 1994, Douglas 2000, Wylie 2003, Intemann 2005, Rolin 2012, Crasnow 2019). Building on earlier twentieth-century analyses of the theory-

ladenness of observation, the underdetermination of theories by evidence, the analysis of inductive risk, and the influence of auxiliary hypotheses and background assumptions, they have offered careful analyses of the array of conceptual roles played by values of all kinds in scientific research.

Some of these values, such as accuracy, consistency, scope, fruitfulness, and simplicity, are so well-accepted that they are seldom recognized as values at all. These are sometimes referred to as epistemic or cognitive values; Helen Longino refers to them as constitutive values for the role they are thought to play in guiding what constitutes good scientific practice (Longino 1990), though she argues that for all their constitutive virtues, these values may not always be purely cognitive (Longino 1995). Longino's concern takes us to the more controversial point about the role in science of what are typically called non-epistemic or non-cognitive, contextual values, most often identified in terms of worldviews, cultural norms, and politics. In this essay I focus on political values, understood not in terms of formal partisan politics, but in terms of the more informal arrangements of power in families, communities, and societies, and the formal structures and practices in settings such as law and criminal justice, health, science, and education that arise in response to and reinforce those informal power arrangements. On this understanding of political values, we can identify anti-racist, feminist values, for example, as well as racist, sexist values. (To be sure, those who endorse these latter values tend not to label them as "sexist" or "racist" but instead as "traditional" or "white nationalist" values, for example. On the assumption that readers of this journal accept my values more or less, I will continue to use the terms "racist" and "sexist," but when trying to persuade folks with different values that they ought to adopt ours, we should probably consider that our ability to be persuasive is very much related to the terms we use.)

One implication of the recognition that values of all kinds play a ubiquitous role in science, is that we can no longer use the mere presence of values to discriminate between science done well and science done poorly; between scientific research that is evidentially well-supported, and research that is weakly-supported. What is needed

then is a mechanism for explaining how we can distinguish between value-laden scientific research that is well-supported by evidence, and value-laden scientific research that is not well supported. Another way of putting the question is to ask what notions of objectivity in science can we salvage, once we take seriously the value-laden nature of even our best science?

I have argued that values, including political values, are themselves beliefs with empirical content, derived, more or less well, from the evidence of experience that generated the beliefs; that where relevant and well-supported by evidence, the inclusion of values in scientific theorizing can increase the objectivity of research (2010, 2012); and that where *irrelevant* and *poorly*-supported, the inclusion of values can decrease the objectivity of research (Clough and Loges 2008). In this paper, I use examples of feminist political values to argue that scientists, as with all of us, can increase the objectivity of research by adopting a pragmatically-inclined appreciation of the fallible, inductive process by which we gather evidence in support of *any* of our beliefs, whether they are described as evaluative or descriptive. I concur with Elizabeth Anderson (2004) that the main danger to objectivity in science is not evaluative content itself, but the implicit and dogmatic role that this, or I add, any kind of content plays. I acknowledge that what counts as "relevant," or even as "dogmatic," is also a question of evidence, and any evidential answer we give to that question will be contingent, historically- and culturally-shaped, and in need of updating in the face of new evidence.

My response to the question whether and how we can salvage notions of objectivity in the face of value-laden science is part of an ongoing conversation within feminist philosophy of science that has coalesced around two kinds of explanatory mechanisms designed to answer the question, namely: feminist standpoint theory and feminist empiricism. This latter approach has been further divided into two types: the community-based political knowledge approach, and the values-as-evidence approach (the nomenclature for these two empiricist strategies was developed by Goldenberg [2013]).

Proponents of all of three of these positions acknowledge the role of values in

science *and* maintain a commitment to objectivity as a goal of research (Crasnow 2013). I use the term "objective" here to refer to scientific research that is "empirically adequate" by which I mean research that "captures as much of the available evidence as possible, obscures or discounts as little of the available evidence as possible, is based on as representative a sampling of the relevant evidence as possible, and explains as much as possible of the variation in the evidence at issue" (Clough 2020b, p. 125).

Feminist standpoint theory considers objectivity to be maximized by those communities of scientists that include, or at least appeal to, the expertise of people who occupy marginalized social positions relevant to the research on which the scientific community is focused, and who are themselves critically engaged in interrogating the nature of their marginalization (Harding 1991; Wylie 2003; Intemann 2010). Standpoint theorists argue that appeals to the standpoint of relevantly marginalized researchers will help introduce those values the presence of which increases the empirical adequacy of the research, and will help identify and mitigate the effects of those values that negatively affect the empirical adequacy of the research. As Intemann explains, "members of marginalized groups are more likely to have had experiences that are particularly epistemically salient for identifying and evaluating assumptions that have been systematically obscured or made less visible as the result of power dynamics" (Intemann 2010, 791). Sandra Harding describes the methodological appeal of standpoint theory in terms of its "strong objectivity" (Harding 1991).

Turning to the two strands of feminist empiricism, the community-based social knowledge approach is the most well-developed, and is primarily associated with the work of Helen Longino (e.g., Longino 1990). On this account, objectivity arises out of communities of scientists adhering to a number of standards such as "tempered equality" which advocates for democracy in decision-making among relevant authorities, and addresses the need for a diversity of values among members of scientific communities. Research is objective to the extent that it is produced in such communities, where conflicting values, especially political values, cancel each other out, or where the effects of those values are managed or balanced. There is no mechanism

proposed for deciding between those values that increase empirical adequacy and those that decrease empirical adequacy, largely because on Longino's view, political values, feminist or otherwise, operate as normative background assumptions that are not themselves bearers of empirical content (Longino 1990, 75), though of course, she notes, we must still be responsive to the empirical evidence of the scientific hypotheses within which the values are operating.

Elsewhere I have discussed both standpoint theory and Longino's community-based social knowledge version of feminist empiricism (Clough 2013), in the present paper, I focus on and defend the second and most recent version of feminist empiricism, explicated and named by Maya Goldenberg (2013) as the values-as-evidence approach. The values-as-evidence view acknowledges Longino's point that values, as with any number of descriptive beliefs, are often implicit and assumed in scientific research, operating as auxiliary or background assumptions. On the values-as-evidence account, values are beliefs informed by the evidence of experience that can be expressed as propositions (from now on value beliefs and value claims will be used interchangeably). Value claims like descriptive claims have empirical content the accuracy of which can be assessed, or they are semantically-linked to claims whose empirical content can be objectively assessed. The contingency of any claim to objectivity is highlighted, but in general, objectivity will be increased to the extent that we make explicit and interrogate the background beliefs at work in our hypotheses, both descriptive *and* evaluative, in order to assess whether those background beliefs are relevant, and are well-supported by evidence.

In addition to reviewing the case for the values-as-evidence account, I also address concerns with the account as articulated by Solomon (2012) who questions whether values are beliefs that can be tested, by Alcoff (2006) who argues that even if our values are beliefs that can be tested, testing them might not be desirable because doing so assigns these important values a contingency that weakens their normative force, and by Yap (2016) who argues that the approach is too idealistic in its articulation of the role of evidence in our moral deliberations. In response, I discuss the ways that

values can be tested, I analyze the empirical strength of feminist values in particular, and I argue that the evidence-based nature of these values is neither a weakness nor an idealization.

2. Non-dogmatic Uses of Value Judgments in Science:

For many, those cases where political values have had a negative impact on the objectivity of science is a problem that, once acknowledged (and it is not universally acknowledged), needs to be managed using conceptual resources viewed as external to science. In the case of the negative impacts of sexist, racist values in science, Janet Kourany advocates for the introduction of feminist-derived policies and procedures to better regulate scientific practices (Kourany 2010). I think we must exercise caution when describing feminist values as external political policies designed to control or regulate practices internal to science (such as practices of evidence gathering and experimentation, say). This way of framing the solution feeds in to many scientists' concerns about political influence over their work because it is premised on (what I argue is) an incoherent view of political values as unavailable to empirical deliberation, as unrelated to evidence gathering and experimentation, and relatedly as (singularly) susceptible to dogmatic manipulation.

In contrast, because I argue that values, including political values, are beliefs that have empirical content that can stand in relation to evidence, this means that we can look to constitutive norms and methodological traditions *within* science to address the problems that arise when evaluative beliefs (just as descriptive beliefs) play an inappropriate role. By including values here in the evidential mix, I am, to be sure, extending the range of claims over which those norms and traditions are typically seen to be responsive and over which scientists typically see themselves as responsible. In other words, I am not saying that the norms and traditions of scientific evidence gathering and experimentation are typically if ever employed by scientists as a way to address the negative role of values in their research, but only that they *can* and should be so employed – a point about ideals vs. practice that I return to in section 7.

Feminist commentary on and within science has documented the terrible and ongoing harms caused when scientific research is informed by empirically-weak and irrelevant political values–Kourany's commentaries (2010) make powerful contributions here. The question remains how to address the problem. I argue that scientific research will improve in this regard when scientists adopt into their own research, evidentially well-supported and relevant political values (such as some feminist values). Such an adoption will empirically strengthen their research in any number of ways, including by helping them recognize more clearly what role if any is being played in their research by poorly supported and irrelevant values (such as sexist, racist values). Of course not all political values identified as feminist are relevant in all science contexts and even among those that are relevant, not all are evidentially well-supported—feminist political values too need to continue to be refined in the face of new evidence (more on this in section 6)

Another way to come at this point is to note that we should not conceive of political values as an external filter for or layer over the descriptive content of science, rather our evaluative and descriptive beliefs together form a large, messy web of beliefs (a metaphor introduced by Quine that I explore in some detail in "Fact/Value Holism, Feminist Philosophy, and Nazi Cancer Research" [Clough 2015]). Some of the beliefs in our belief webs play a descriptive and some an evaluative role in particular contexts. All of our beliefs are informed by our experiences in the world. Some of them go on to be part of scientific hypotheses. Of those beliefs that play an evaluative role (and those that play a descriptive role!) some will be more central to and connected with a number of other beliefs, which will make them hard to give up, because doing so disrupts so many of the other beliefs to which they are connected (a point I return to in section 6). But the process by which these central beliefs came to be so central, so well-connected, is experiential, and we need to be able explicitly to re-examine those experiential connections when necessary, and be open to the experiences that can disrupt or shift those beliefs. In other words we need to guard against dogmatism. What we need more of instead is epistemic humility (Tanesini 2016)–a pragmatically-inclined appreciation of the fallible, inductive process by which we gather evidence in support of any of our beliefs, whether they are

described as political and evaluative or scientific and descriptive. Admitting the fallible nature of our firmly-held beliefs (whether they be evaluative or descriptive) is a tall order in the current climate, but it is also, I argue, the only coherent way forward (Clough 2020a).

In her essay "Uses of Value Judgments in Science" (2004) Anderson argues that values aren't always held dogmatically (though of course they *can* be). She presents a case study involving a team of explicitly feminist social scientists that examined the effects of divorce on women and children, and published results that were at odds with the then prevailing view of divorce as a necessarily negative life-event (Abigail Stewart *et al* 1997, cited in Anderson 2004). Anderson notes that at one point in the research, Stewart's team reported data supporting the view that mothers should be the sole custodians of children after divorce, a nominally feminist view consistent with the researchers' expressed political values (Anderson 2004, p. 17). The children they surveyed routinely reported distress when being transferred to their non-custodial fathers. If the feminist researchers held their values dogmatically, that is without objective appraisal of the evidence in support of those values, they might have been content to stop there. However, the researchers chose at this point to extend their analysis, whereupon they discovered that, despite their nominally feminist commitment to supporting mothers in divorce cases, the results of their study needed to be changed in the face of new evidence. It turned out that the main cause of the children's distress was not the prospect of time spent with their fathers, but the prospect of their parents fighting during the moments of custodial transfer (Stewart *et al* 1997, p. 288; as cited in Anderson 2004). The researchers accepted results that conflicted with feminist support for mothers getting sole custody of children, and they were able to provide some good news for prospective custodial fathers.

Anderson warns against the dangers of dogmatism primarily as the danger inheres in cases of values, but of course dogmatism is a problem in cases of descriptive claims as well, and dogmatic affirmations and rejections of fairly straightforward descriptive claims abound in the history of science. In Clough (2015) I discussed the example of Arthur Eddington's work supporting general relativity. Of the telescopic data he collected in 1919, Eddington chose to publicize only the observations that supported Albert Einstein's theory and ignored the negative results. Although the details of

Eddington's case are controversial, he did not explain the evidence he had for down-playing the negative results; at the very least his rationale for doing so could surely have been made more explicit. Harry Collins and Trevor Pinch discuss the Eddington case and a similar problem in the work of Louis Pasteur who suppressed and explained away his negative results on tests of spontaneous generation (Collins and Pinch 1993).

Negative results are important in science and we can learn a lot from them, but only if they are made accessible. If we are to avoid dogmatism in our rejection of negative results, we need to acknowledge those results, and be much more explicit about the meta-evidence we have for ignoring them. Making our meta-evidence explicit can also allow it to be publicly assessed for the presence of confirmation bias or a variety of other cognitive biases that typically accompany the rejection of negative results. These kinds of biases are part of a broader phenomenon, known as the file drawer problem. Too often, negative results remain in a file drawer, they are not submitted for publication, and when they are submitted they are seldom published (this latter phenomenon is a problem having to do with biases in journal acceptance that discourage publication of negative results). A number of solutions to the file-drawer problem and biased journal practices are being considered by professional science societies, such as the development of on-line public repositories for negative results (see for ex., Dirnagl *et al* 2010).

Though it is an on-going challenge, scientists as with all of us, need to be objective rather than dogmatic in our assessment of the evidence brought to bear on our hypotheses—we need to be cautious and explicit about our rationale for ignoring negative or anomalous results (which means at a minimum acknowledging that we got those results) and we should model this caution wherever we can. These challenges to straightforward hypothesis testing are of course nothing new to scientists and philosophers of science. What I want to draw new attention to here is that *these challenges arise* and can be successfully addressed *whether the hypotheses in question concern descriptive claims about the theory of relativity or evaluative claims about feminism.* Dogmatism is not a special problem for values in science, but a problem more

generally, and avoiding it is a skill for which we get very little training. In "Charity, Peace, and the Social Epistemology of Science Controversies" (Clough 2020b) I discuss some of the skills such as epistemic humility, mentioned earlier, as well as trust-building, and listening with empathy, that would help address this problem, and that ought to play a greater role in scientific training.

3. What are (feminist) values and how can we test them?

If we can no longer use the presence of political values to discriminate between good and bad science, this means that some scientific hypotheses can be objective (understood as I have described it in section 1, as empirically well-supported by the relevant and representatively sampled evidence, etc.,) and *also* value-laden. How? I have reviewed different approaches from feminist epistemology that offer answers to this question, and promoted the values-as-evidence approach, but a fuller answer to the question depends a great deal on how we define our values, and on how we think about evidence and testing.

In her commentary "The Web of Valief: An Assessment of Feminist Radical Empiricism" (2012) Solomon uses the label "feminist radical empiricism" to describe what I have been calling "the values-as-evidence" approach. Solomon is critical of the values-as-evidence approach especially in terms of Anderson's 2004 essay, but Solomon has a very specific set of feminist values in mind and I don't think they are the kinds of feminist values under discussion in Anderson's work. For example, Solomon identifies as feminist the valuing of complex, holistic theories over reductionistic theories, or (following Longino) of a preference for novelty as against conservativism, and democratic as against undemocratic decision-making in science (Solomon 2012).

I think there is probably a way to think about what evidence would look like to help decide between the value of holistic accounts in science over reductionistic accounts, or the value of democratic decision-making processes over undemocratic processes, but we'd have to look at particular examples. These values, as stated, are too general – indeed too general to even have a clear relation to feminism. It is hard to know without more details, what these values *mean*. I return to the question of

meaning in section 4, but I note that this lack of clarity around the meaning of complexity, democracy, or novelty, especially as feminist values, is not unrelated, I think, to the difficulty of imagining what the empirical content of these values might look like, and how we might test them by examining their relation to evidence. Solomon describes this difficulty in the following:

> Making the claim that feminist values are well-confirmed in a manner that justifies their wider application requires showing their fruitful applicability to a range of domains (and occasions), as well as showing that they do better than sexist values in this range of domains (and occasions). However, no one has begun to think about how to individuate and aggregate the successes and failures of values in different domains (or occasions). Perhaps feminist values can be confirmed and shown to be more empirically successful than sexist values, but we are *nowhere on the way to doing that*. (Solomon 2012, p. 442, emphasis mine).

However, if you have more specific and precise feminist values in mind, then, it seems we are indeed well on our way to showing them to be more empirically successful than sexist values. Confirming any number of feminist values by testing for or appealing to their empirical success seems to be at least part of the point of the work of feminist scientists who make use of their feminist values as an intervention in science (e.g., Fausto-Sterling 1985, 2012; Tavris 1992; Jordan-Young 2011; van Anders 2013, van Anders, *et al* 2015). Indeed even on Solomon's reading of Anderson, Solomon describes Anderson's use of the case study as one where feminist values are *confirmed* (Solomon p. 444). I am not actually sure that Anderson uses the case study on divorce research to confirm the values she identifies as feminist. I think the more accurate interpretation of what Anderson is up to is to say that she uses the divorce case to show that the explicit inclusion of (some feminist) values can lead to more empirically accurate science in some cases, and that values don't always need to be held dogmatically. That said, I think the values that Anderson identifies as playing a role in the feminist research on divorce help increase the empirical adequacy of that research *precisely because* they are values that have been tested and confirmed (elsewhere) and for which we have gathered (in a number of settings), a great deal of evidence.

It might be helpful at this point to return to our definition of feminist values. In an earlier paper on the topic, I noted that the feminist values of relevance to Anderson's case study "do not concern preferences for general properties and processes such as complexity, democracy, or [novelty]" (Clough 2008, 276-277). In Anderson's example of feminist research on divorce, the feminist values at issue involve fairly particular claims such as the feminist evaluative claim that "women, just like men, cannot be adequately defined by exclusive attention to their relationships to their spouses and children" and that "both women and men can have needs, desires and concerns that focus on aspects of their lives other than their families and homes" (Clough 2008, p. 277). These feminist political claims about the desires and concerns of women as apart from their role as mothers and spouses are claims that have empirical content that can be objectively assessed, and indeed they have been the subject of such testing throughout the literature on the social psychology of gender roles, and have been found to be well-supported (for an early and classic review of this literature see Tavris 1992).

Now you might be thinking that these claims about women's needs and concerns are basic descriptive truisms rather than feminist political values, but if so, that is likely an effect of these claims being so well-supported by evidence from so many different domains, and not an effect of their being free of evaluative content. In fact more conservative readers might pick up on and resist the evaluative content precisely because they are unfamiliar with or critical of the vast literature in support of the claims. (Compare here the revelatory nature of Longino's argument that we should recognize as values our commitments to accuracy, simplicity, and scope – it was surprising to have them identified as values because they are so widely agreed upon). Insofar as the claims about women's needs and concerns seem to us like straightforwardly true descriptions, it is probably because we have had a lot of experiences in the world that support those claims. We either know about women who wanted more out of their lives than their roles as spouses and mothers allowed, or about women who when given the chance to expand their horizons beyond those roles, jumped at the chance. Many of us have studied the phenomenon further as it is systematized, tested, and documented in social

science research on gender roles. This is the evidence in support of these feminist values. It's also evidence *against* a sexist value that says women do not, or cannot have these kinds of desires.

Anderson showed that when these kinds of well-supported feminist values are relevant to scientific research (as in the case of divorce) then designing and testing hypotheses that foregrounded these values can (and did) *increase* the evidential strength of the research (Anderson 2004, pp. 12-18). It made the research more objective, as I have been using the term. In her example of Stewart's work on divorce, Anderson noted that by foregrounding the feminist value that women, just like men, cannot be adequately defined by exclusive attention to their relationships to their spouses and children, the feminist research team was led to frame questions for their participants that allowed for a wider range of responses to divorce. Rather than asking leading questions that assumed divorce was bad, they asked questions that allowed the respondent to frame divorce on her own terms. Rather than drawing participants exclusively from those who were seeking therapy after their divorce, they included participants who were not seeking therapy (Stewart 1997 p. 34; as cited in Anderson 2004, p. 16). Their feminist-informed research resulted in a more empirically accurate description of the phenomenon than had been available to researchers informed by more "traditional" value claims that, for example, women's roles as mothers and wives were naturally and ideally coupled, and that divorce was by definition a bad thing if it involved decoupling them. As I noted in my discussion of Anderson, "The feminist researchers were able to see what traditional researchers did not, namely that divorce might not always be seen by women as a negative life event" (Clough 2008, 277).

4. Where do our values come from?

According to the values-as-evidence approach that I endorse, claims are sorted as evaluative or descriptive (or typically some mix of these) largely as a function of context; our feminist and other progressive politics have empirical content that can be objectively assessed; and the objective assessment of any claims—in politics as in

science—will be contingent, historically- and culturally-shaped, and in need of updating in the face of new evidence.

One concern with this approach to thinking about values was articulated by Alcoff (2006) in a response to Anderson's 2004 paper. Alcoff argued that insofar as the values-as-evidence view depicts values as contingent facts that could change, then it fails to account for the importance of (feminist) values in our lives; that part of what we mean when we say we value something, is that our belief in that value should hold fast, the evidence come what may. She explains:

> Although feminism is based in a set of empirically revisable beliefs, that does not exhaust the meaning and implications of feminist values. What we know to be the case about women today will surely be revised; what we want for women, in broad strokes, will not be. What empirical facts could revise the commitment to take into account women's own subjective point of view, to accord women autonomy and self-determination, to value their needs and interests and desires and rights to bodily integrity at least as highly as we value all others? For that kind of claim, we need a value commitment that is not revisable (Alcoff 2006, p. 4).

Here, I think it is important to examine how it is that we arrive at our values in the first place, and what I mean when I say that values are beliefs informed by the evidence of experience. In order to develop our valuing of and commitment to women's points of view, autonomy, self-determination, interests, and integrity, we needed first to have experience in the world that gave those values and commitments meaning, experience that supported those values and commitments, and we needed to learn from experience that failing to hold those values and commitments led to massive inconsistencies with the data of people's lives, it led to hypocrisy, and it decreased human flourishing.

My claim that all our beliefs, whether sorted as descriptive or evaluative, arise from experience in the world, involves a pragmatist reading of Donald Davidson (e.g., Davidson 2001). Based on evidence we have concerning language acquisition in children and second-language acquisition in adults (evidence that is contingent and fallible as any

of course, see Ungureanu 2004 for a review) Davidson's work reminds us that to have meaningful beliefs at all, is to be experientially enmeshed in a physical/social/political relationship with the world around us, including other knowers. The meaning of our beliefs arises from a more or less complicated genealogical, web-like set of triangular relationships between ourselves, the fellow creatures with whom we engage in a host of communicative behaviors, and the shared bits of the world on which that engagement is focused. This process seems to accord with neurotypical experience in ways that might not generalize to folks on the extremes of the autism spectrum, so once again the contingency of the model needs to be acknowledged here (see Andrews and Radenovic 2006).

In Clough (2015), I discussed how we might use this model to explain how verbal language users learn the meaning of basic beliefs whether sorted as descriptive (e.g., "The stove is hot") or evaluative (e.g., "Maple syrup on waffles is awesome"). I argued that, for those of us who are verbal language users,

> we learn the meaning of these descriptive and evaluative claims by attending to the evidence of experience, that is, by attending to patterns in our communication with others about shared experiences in and with the world. Learning the meaning of a claim is to learn the circumstances under which it is true, and there is no principled distinction to be made between the way we learn the descriptive circumstances under which it is true that "the stove is hot" and the way we learn the evaluative circumstances under which it is true that "maple syrup on waffles is awesome" (2015, p. 4).

Because our ascriptions of descriptive features like heat or evaluative features like awesomeness are broadly empirical, the ascriptions are contingent and fallible. We can be wrong in both cases. And our wanting something to be the case does not necessarily make it so. Claiming that ice cubes are hot, or that lint is awesome will not make ice cubes hot or lint awesome. In most cases if we made these claims we would be revealing a failure to understand how the world works, what words mean, how evidence works.

There are some special circumstances I can imagine where we could successfully claim that ice cubes are hot or that lint is awesome, but what makes the circumstances special is that

we'd have to tell a story about how we were making the ascriptions ironically, or metaphorically ("the ice burned my skin, like the heat from a flame"). Or it would have to be a *really big ball* of lint. A helpful colleague shared recently a way of thinking about lint that builds on the awesomeness we sometimes ascribe to things that are really big, or in this case, processes that are unusual, claiming that we might reasonably "be amazed by the way bits of cloth can become something that is no longer part of clothing but turn into something that then coheres together into something new but very cloth like and – with work – could be made into a new cloth of some sort." In the space of this paragraph I now can think of a way that lint is awesome, but note, insofar as this shift in my thinking was successful—and I'm still not entirely convinced—it involved bringing new evidence to bear, coordinating my attention to features of our shared world, including lint, to which I had not previously attended, making comparisons with changes of state that can be counted on reliably to surprise us (that's how felt is made?!) even if not consistently to inspire awe.

When learning the meaning of beliefs sorted as descriptive or evaluative, context is key, the community of inquirers is key, and what counts as *relevant* evidence in support of either of these kinds of ascriptions will also be context sensitive and often in need of reappraisal and argument.

5. What counts as "relevant evidence"?

In the examples of informal ascriptions of heat and awesomeness, what counts as the relevant evidence will not typically be the evidence that results from formal science experiments where some conditions are manipulated while other conditions are held stable; though evidential appeals to this kind of classic experimental set up are not always practiced in science either. One reason for the limits on this experimental approach in science settings is practical (astronomers and geologists, for example, are seldom able to manipulate variables because of issues of time and scale); another reason concerns generalizability (even when sociologists and ecologists are able to manipulate variables in experimental settings, the evidence gathered in this artificial context seldom applies straightforwardly to more complex natural settings outside the

lab). Even in many science contexts, as with our more informal everyday cases, the relevant evidence brought to bear in our objective assessments of descriptive and evaluative claims both, is the evidence provided by our lived experience of and in our communities.

Moving from basic descriptive and evaluative beliefs about ice and lint, respectively, to more complex descriptive claims such as "The Earth orbits the Sun" or more complex evaluative claims like "Scientists should be honest about any financial interests that may influence their work," the learning process involves the same kinds of inferential, evidence-gathering patterns (Clough 2015). Regarding the experiential basis of these more abstract concepts, both descriptive and evaluative, I appeal to fairly standard versions of concept empiricism such as that articulated by Jesse Prinz (2004).

Of course we can and do make a variety of mistakes that we can (and sometimes do) identify, and correct. These concern overgeneralizations from the more complex examples discussed above, e.g., with respect to evaluative claims about science and honesty, there are of course exceptions to the maxim "honesty is the best policy." But in each kind of complex case, both descriptive and evaluative, the learning process involves the same kinds of inferential, evidence-gathering patterns.

Regarding the potential of this learning process, I have argued that,

> Just as with learning about concrete features of our world [such as planetary motion], we learn about abstract features [such as the importance of honesty] by deploying them in claims about our own relationships, with our families and friends, and building inferential associations to more complicated relationships, say, between scientists and funding sources. We improve on our understandings of these features, we fine-tune the objectivity with which we assess claims appealing to these features, as we move around our worlds, communicating with others (Clough 2015, p. 5).

If this argument about meaning and belief acquisition holds for beliefs sorted as descriptive *and* evaluative, (and I think we have good, though of course defeasible, reasons to think it does), then applying the tools of evidence gathering and justification makes as much sense when considering evaluative beliefs as it does when considering descriptive beliefs.

To return to an earlier point, when I suggest that we already have methodological traditions—constitutive norms—within science that would help us address any inappropriate influence of evaluative beliefs (or descriptive beliefs), I acknowledge that this requires expanding our understanding of what counts as the kinds of evidence to which scientists ought be explicitly responsive, i.e., expanding our understanding to include explicitly the experiential evidence that informs and is informed by political values, rather than having these values play an implicit role that makes those values less available for objective assessment. It is the main purpose of this paper to pragmatically nudge scientists and all of us in the direction of this kind of expansion, or at least to reflect very carefully on those occasions when we don't feel comfortable doing so.

I also acknowledge that regardless of the success of my nudging, we still might want to say that descriptive claims *function* in different ways from evaluative claims. We might think that we use these two kinds of claims in different ways, in different circumstances. As Alcoff pointed out, we sometimes think of our evaluative claims as those things that stand fast and around which other claims are organized (e.g., the value claim that "Gender identity and expression are irrelevant to our right to bodily integrity"), but, as Wittgenstein pointed out, basic descriptive claims can play this kind of role too (e.g., the descriptive claim we might utter when holding up our hand to our face— "Here is a hand"—can sometimes be a foundational claim, the certainty of which becomes the standard against which all other claims are measured [a point discussed in Michael Hymers 2003]). As I argued in section 2, it is largely the context within which claims are made that help determine their evaluative or descriptive function.

Whatever the differences between the two kinds of claims, I hope in this discussion to ease the burden of proof onto those who want to maintain that it is only descriptive claims and not evaluative claims that are related to evidence that can be objectively assessed and revised; that it is only evaluative claims and not descriptive claims that have a special normative hold on us, tempting us to hold fast to our convictions, resisting revision even in the face of new evidence. In section 7, I return to examples of descriptive claims that we sometimes find difficult to abandon.

6. Recap:

Perhaps in order to capture something important about the aspirational nature of feminism that might seem to go beyond the empirical content of any given feminist claim, Alcoff argued that we need to understand feminism as a value commitment that is not revisable. In response, I began by reviewing the question of how we arrive at our feminist values in the first place. Borrowing from de Beauvoir's insight that women are made, not born, so too with feminism: feminists are made, not born. We who would become feminists moved around our worlds in communication with others. We encountered sexist, racist, classist, ableist, and other arbitrary restrictions. We amassed evidence and analyzed patterns. For example, we learned through our interactions with people described as (trans*) men and/or women that there was nothing about their sex/gender that restricted their need for autonomy and self-definition. The evidence we've collected in support of many of the values most of us would call "feminist" is contingent, but fairly overwhelming, and is interconnected with the evidence we have for a large number of other beliefs similarly well-supported.

That broad connection and support is what gives our feminist values meaning, literally, and it is, I think, that broad connection and support that explains the centrality and importance of feminist values in our lives. While it is true that if our values have a relation to empirical evidence then they are revisable, it is worth noting that a lot of what we know about the world would have to be shown to be false, before all the values we identify as feminist could be shown to be false and in need of revision. But of course there are always the possibility of mistakes in our values, feminist or otherwise, that can and should be corrected in the face of new evidence.

Consider for example the feminist political claim associated with Betty Friedan in the 1960s that to be free from oppression, women need to be freed from the confines of their homes and to think of themselves as breadwinners (Freidan 1963). As bell hooks showed, this feminist evaluative claim was empirically weak insofar as it did not take into consideration the data provided by many women of color who suffered from oppression even though they were breadwinners who worked outside their homes (hooks 2000).

Feminist evaluative claims about the nature of oppression have had to change to take into account data from the complicated nexus of social hierarchies affecting people's lives, including race, disability, socio-economic status, sexual identity, and age. If we take the view that feminist values are immune to revision, then those values are less meaningful almost by definition.

I hope to have recast the characterization of feminism as a set of contingent empirical claims in ways that Alcoff would appreciate as a strength. The fallible, contingent, and evidence-based nature of feminist political values is not a weakness but an asset. And the evidentiary strength of (particular) feminist values means that they are as appropriate as any other relevant set of evaluative and descriptive claims for inclusion in scientific research. Indeed their inclusion may be critically important in particular contexts. The evidential superiority of many feminist claims needs to be acknowledged, not only by non- or anti-feminists, but with humility and nondogmatically, by feminists.

Of course, the claim that feminist political values are generally well-supported by evidence, and at least better supported by evidence than are sexist, racist, classist, ableist values, is itself an empirical, and therefore contingent claim, but so far it's held true (indeed the greater accuracy of a feminist worldview is the fuel that gives feminist political movement its normative force). When prescribing a choice among any given set of competing scientific theories for which questions of gender/race, say, are relevant, then we need to account for the normative force of feminist political beliefs as they support one or some of the theories but not others (I'm thinking here of the power of Sari van Ander's work, e.g., 2013). This normative force, where relevant and consistent with the evidence, can and should affect our choice of scientific theories, and doing so has produced scientific research that is more accurate than it would otherwise be.

7. Idealism and the values-as-evidence view

A final concern I'll address comes from Yap's recent essay "Feminist Radical Empiricism, Values, and Evidence" (2016). Yap follows Solomon in using "feminist radical empiricism" to describe what I have been calling "the values-as-evidence" approach. In Clough and Loges (2008) I argued that complex value judgments, such as those that

comprise anti-racism or even racism, are beliefs about the world and that we can objectively assess the evidentiary strength of those beliefs in ways that show anti-racist beliefs to be more accurate than racist beliefs. By way of illustration, I took on some of the racist evolutionary theories of the late Canadian psychologist Philippe Rushton (Rushton would not have called the values informing his research "racist," but he might have been happy to call them "white nationalist." For evidence for his allegiance to and participation in white nationalist causes, see Saslow [2018, p. 146]).

Yap raises a number of concerns with my discussion of Rushton, two of which I'll address briefly here. The first is that in discussions of the racist scientific views of Rushton, the problem is more than a lack of fit between his view and well-established empirical evidence. In the case of his views about the causal relation between race and IQ, there "is not a single empirical fact at the center of the conflicts, but a question of which model better accounts for the empirical data; one that attributes test score gaps to cultural factors or one that attributes it to hereditary factors" (Yap 2016, p. 9) and later "the problem is not with any individual empirical facts, but with the build-up of problems that those holding the hereditarian model should find problematic" (p. 9). She argues that we have here a case where an individual holds a racist view "in the face of contrary evidence" but that they haven't necessarily committed "errors in the treatment of that evidence" (p. 9).

I think a case could be made that Rushton did indeed commit a number of errors in the treatment of contrary evidence, but the more relevant point here is that in arguing for the empirical nature of values, and their potential for strengthening (or weakening) the scientific research in which they are implicated, we need to acknowledge the complexity of the process of evidential examination required. As feminist and other historically and contextually-minded philosophers of science have shown, we need to recognize that the issue is seldom as simple as gathering evidence, but also examining the historical and political conditions under which data become *taken* as evidence. Importantly, what counts as evidence, and for whom, is still a broadly empirical question of evidence, applied at a meta-level. When scientists discover cases

where epistemic values such as accuracy and scope conflict, their evidential examination doesn't stop. The decision about which values to prioritize itself becomes an investigative project that properly continues at a meta-level using roughly the same kinds of empirical criteria. They stumble on. And so should we. The level and kind of complexity of analysis required seems independent from the question whether we are assessing the evidential strength of an evaluative claim or a descriptive claim.

Returning to the complicated analysis of the evidence that Rushton uses to support his racist claims and any errors he committed in his treatment of contrary evidence, we must ask a number of more complex but not intractable questions. When we ask these questions, answers are available (and the answers are compelling enough to at least suggest some shifting in the burden of proof back on to researchers like Rushton). So, for example:

- Is the racist evidence gathered by Rushton relevant to the questions of IQ? (For some, more data is needed to say anything with confidence, though *decades* of research suggests the answer is *no*. The burden is therefore higher for these kinds of claims and Rushton does not recognize nor meet this burden).

- Is his racist evidence gathered in reliable ways and drawn from representative samples? (*No*. Rushton deployed and compared folk racial categories in ways that were completely insensitive to historical circumstance, claiming, for example that the racism experienced by "Orientals" and "Blacks" was equivalent such that IQ data from each of these "populations" could be used to make relevant and adequate comparisons).

- Are there other kinds of explanatory frameworks that account for more of the variance in the evidence observed than the racist frameworks he uses? (*Yes*. As I noted in Clough and Orozco [2016] with respect to his claim that black people and white people have evolved different parenting strategies for example, it is true that in a number of non-human animal species there is evidence for a negative correlation between gamete production and parental investment. However, it is not at all obvious that this pattern is relevant for explaining human

behavior and even if it was relevant, showing that the pattern does in fact hold for humans would require more empirical support than he ever provided. And even if he had robust empirical support that the correlation holds in humans, I noted further that "he failed to show that this pattern emerges from different evolutionary strategies, let alone that these strategies are determined by 'racial' categories" [Clough and Orozco 2016, p. 3]; and finally that "reliable evidence shows that when there are differences in human parental caring, the underlying causal role is more likely played by local responses to social variables such as poverty and violence, rather than evolutionary pressures tied to membership in any given 'racial' category" [Clough and Orozco, p. 3]).

These questions, for all their complexity, can and have been answered in ways that show Rushton to have been evidentially irresponsible.

An additional concern raised by Yap is that even if evaluative beliefs just as much as descriptive beliefs are capable of revision, we are seldom willing to revise our evaluative beliefs. She points out that we have research on cognitive biases of a variety of kinds (e.g., Gendler 2001) showing that most of us are inclined to continue holding on to our values even when we have evidence to the contrary. Yap makes use of Charles Mills' work on problems with ideal theory in philosophy (Mills 2005) to argue that the values-as-evidence view that I support presents a "problematic idealization of the open-mindedness of human agents" (Yap 2016, p. 58). I am not sure this is a problem for my approach so much as a problem generally wherever we need to trust that epistemic agents will be responsive and responsible to evidence regarding evaluative but also more straightforwardly descriptive claims. While it's quite likely that Rushton went to his grave believing both that his racist views were empirically adequate and that liberal academic presses refused to publish the truth, so too did Tycho Brahe go to his grave believing the descriptive claim that the Earth was at the center of the universe. The problem of dogmatic refusal in the face of mounting evidence against one's views is not a problem confined to the sphere of political values, instead the problem can be found across the evaluative/descriptive continuum.

Feminist philosophers of science know more than anyone else perhaps, about the various cognitive biases besetting scientists regarding even fairly mundane descriptive claims about the world. We know that scientists, just like the rest of us, sometimes find it hard to let go of their descriptive claims in the face of evidence against those claims. Knowing a piece of wire to be made of copper can lead scientists to expect certain results when they run an electric current through it, and can lead them to inappropriately discard results that don't fit those expectations.

Returning specifically to Yap's concern about the multiple ways that cognitive biases negatively affect the objectivity of evidential deliberations, her concern is representative of increased scepticism generally regarding the efficacy of using empirical evidence to get people to change their minds, especially if they are engaging in what gets called "politically motivated reasoning." The word "motivated" here is meant to indicate a kind of dogmatism whereby the politics in question drive the evidential conclusions inappropriately. As it was first documented (Lord, Ross, and Lepper 1979) the problem looks like this: "The result of exposing contending factions in a political dispute to an identical body of relevant empirical evidence may be not a narrowing of disagreement but rather an increase in polarization" (Lord *et al* 1979, p. 2098).

However, while few of us are as open-minded as we like to think—a variety of cognitive biases make us less likely to accept evidence that conflicts with our convictions, sorted as either evaluative, descriptive, or both—it turns out that much of the research on our collective cognitive failings offers a way forward (indeed work on epistemic injustice of a variety of kinds is aimed at diagnosing symptoms to single them out for treatment and melioration [e.g., Dotson 2011, 2014]).

In their paper "Science Curiosity and Political Information Processing," Dan Kahan and his co-researchers showed that this kind of resistance to new evidence is not a universal phenomenon (Kahan, *et al* 2017). According to their research using the "Science Curiosity" scale, "subjects high in science curiosity display a marked preference for surprising information—that is, information contrary to their expectations about the

current state of the best available evidence—even when that evidence disappoints rather than gratifies their political predispositions. This is in marked contrast, too, to the usual style of information-search associated with PMR [politically-motivated reasoning], in which partisans avoid predisposition-threatening in favor of predisposition-affirming evidence" (Kahan *et al* 2017, p. 197). And scores on the Science Curiosity scale are distributed in a bell curve across demographics, including, in the US, political party affiliations. Kahan also offers strategies for encouraging science curiosity. Those of us who teach science probably employ a number of these strategies or if we don't, we should. But the point here is that science curiosity is a cognitive virtue that can be developed. Earlier of Kahan's work also gives us strategies, most having to do with reframing evidence, and making the evidence less threatening. Resistance to new and conflicting evidence depends a great deal on the way that the evidence is communicated (Goldenberg, 2016). In cases of policy aimed at changing the minds of the vaccine hesitant, for example, so-called "uni-directional" messages seem ineffective, but other kinds of strategies have been shown to be efficacious (Goldenberg forthcoming). So we have lots of evidence-based strategies about how best to talk about evidence. Building trust, cultivating epistemic humility, and listening with empathy are among the strategies (Clough 2020b).

Acknowledging, engaging in, or recommending these strategies is not to participate in Ideal theory, it is to get into the muck of everyday conflict, and recognize efficacy and limits in both descriptive and evaluative contexts. But it is, to be sure, more complicated than we philosophers might have thought or hoped. And it will surely require more humility, compassion, patience, and empathy for ourselves and others than we might always have the patience for. Pragmatists will not be surprised to hear of these habits of mind as practices for which we need training, just like we needed training in reading and math. Taken together they form what Paul K. Chappell calls "peace literacy" (Chappell 2017), and our collective preliterate condition explains quite a few of the stumbling blocks we face when examining the evidence for our beliefs, whether those beliefs are evaluative or descriptive.

8 Conclusion

I want to re-emphasize that the point of highlighting the continuity between beliefs sorted as descriptive and beliefs sorted as evaluative is that insofar as any of these beliefs are meaningful, then they all have empirical content that can be assessed against evidence with more or less objectivity. Whether that evidence is properly and consistently assessed is an open question, and is not guaranteed by anything I've said here. But that's not a special problem for the assessment of evaluative beliefs. Our disinclination to change our minds presents a problem not just for our political values, but for more descriptive beliefs as well.

This means that while evidence supporting either descriptive or evaluative claims is something we can be responsible to, we are still susceptible to manipulation of that evidence or to other kinds of irresponsible epistemic behavior. However, the only way to recognize manipulation and irresponsibility to the evidence, is to return to an analysis of evidence—a pragmatically, contextually, complex, situated, contingent analysis of the evidence provided by and informing our inductive inferences. And this latter claim should itself be sorted as a meta-inference based on historical analysis of efficacy. No matter how hard philosophers of science have tried, we have not been able to provide a bright line that will tell scientists definitively when their support for their descriptive or evaluative claims against anomalous evidence is responsible and prescient, and when it's irresponsible and dogmatic.

My expectations regarding the ease with which scientists will let go of their false (racist, sexist) evaluative beliefs in the face of contrary evidence, is no higher or lower than my expectations regarding their attitudes towards their false descriptive beliefs. To be sure my standards for scientists are pretty high across the board. But I see our job as philosophers of science to help scientists meet those standards. My characterization of evaluative beliefs as responsive to evidence, and as playing an important and sometimes positive role in science is not a naïve idealization, but a recasting that sets standards for better science. And highlighting the (fallible, contingent) evidential parallels between

claims sorted as descriptive and claims sorted as evaluative is the best way I know how to provide this kind of help. Far from ideal theorizing, I mean to counsel instead a pragmatically-inclined muddling through with our fallible inductive inferences. These are the best practices we've got, in science as in politics, and perhaps especially in politically-informed science.

Finally, while my focus on the problems of dogmatism targets the importance of examining critically the genesis of even our most cherished beliefs, both descriptive and evaluative, and the ongoing scrutiny of our reasons for holding those beliefs, the characterization of our feminist values as empirical beliefs in particular is meant to highlight their evidentiary strength. They are connected to many other well-supported beliefs in a broader holistic web of meaning, painstakingly woven and appropriately difficult to disentangle. Recognizing the contingency of our feminist values involves a respect for the properly persuasive power of empirical evidence, while extending the range of what counts as empirical to include values. Because our feminist values stand in relation to evidence, this means that where they are relevant and well-supported by that evidence, their inclusion in scientific theorizing can increase the objectivity of research. This characterization is not meant to reveal a weakness but recognize a strength.

In sum, there is nothing epistemically unique about our evaluative claims that contrasts with our descriptive claims. Both are arrived at through the evidence of experience, and both can be clung to dogmatically and inappropriately in the face of new evidence. To guard against dogmatism, scientists and all of us ought to adopt a pragmatically-inclined appreciation of the fallible, inductive process by which we gather evidence in support of any of our beliefs, whether they are sorted as evaluative or descriptive, or some combination of the two.

References:

Alcoff, Linda. 2006. Commentary on Elizabeth Anderson's "Uses of value judgments in science." *Symposium on Gender, Race, and Philosophy* 2(1).

Anderson, Elizabeth. 2004. Uses of value judgments in science: A general argument, with lessons from a case study of feminist research on divorce. *Hypatia* 19(1): 1– 24.

Andrews, Kristin, and Ljiljana Radenovic. 2006. Speaking without interpreting: A reply to Bouma on Autism and Davidsonian Interpretation. *Philosophical Psychology* 19(5): 663-678.

Chappell, Paul K. 2017. S*oldiers of peace: How to wield the weapon of nonviolence with maximum force*. New York: Easton Studio Press.

Clough, Sharyn. 2020a. Rorty as liberal ironist peace warrior. In *The Ethics, Epistemology, and Politics of Richard Rorty* (Routledge Studies in American Philosophy) ed. Giancarlo Marchetti, New York: Routledge.

_____. 2020b. Charity, peace, and the social epistemology of science controversies. In *Social Epistemology and Relativism*, eds. Natalie Ashton, Martin Kusch, Robin McKenna, and Katharina Sodoma, New York: Routledge, pp. 122-142.

_____. 2015. Fact/value holism, feminist philosophy, and Nazi cancer research. *Feminist Philosophical Quarterly* 1(1):1-12. https://ojs.lib.uwo.ca/index.php/fpq/article/view/2993

_____. 2013. Pragmatism and embodiment as resources for feminist interventions in science. *Contemporary Pragmatism* 10(2): 121-134.

_____. 2012. The analytic tradition, radical (feminist) interpretation, and the hygiene hypothesis. In *Out of the Shadows: Analytic Feminist Contributions to Traditional*

Philosophy, eds. Sharon Crasnow and Anita Superson. New York: Oxford University Press, pp. 405-434.

⎯⎯⎯⎯. 2011. Radical interpretation, feminism, and science. In *Dialogues with Davidson*, ed. J. Malpas, Cambridge, MA: MIT Press, pp. 405-426.

⎯⎯⎯⎯. 2010. Gender and the hygiene hypothesis. *Social Science & Medicine* 72(4): 486- 493.

⎯⎯⎯⎯. 2008. "Solomon's Empirical/Non-Empirical Distinction and the Proper Place of Values in Science." *Perspectives in Science* 16 (3): 265-279.

⎯⎯⎯⎯. 2004. Having it all: Naturalized normativity in feminist science studies. *Hypatia: A Journal of Feminist Philosophy* 19 (1): 102-118.

⎯⎯⎯⎯. 2003. *Beyond epistemology: A pragmatist approach to feminist science studies*, Lanham, MD: Rowman & Littlefield.

Clough, Sharyn, and Bill Loges. 2008. Racist beliefs as objectively false value judgments: A philosophical and social-psychological analysis." *The Journal of Social Philosophy* 39 (1): 77–95.

Clough, Sharyn, and Julio Orozco. 2016. Scientific sexism and racism. In *The Encyclopedia of Gender and Sexuality Studies*, edited by N. Naples, Oxford: Wiley Blackwell, DOI: 10.1002/9781118663219.wbegss315

Collins, Harry and Trevor Pinch. 1993. *The Golem: What everyone should know about science*. Cambridge: Cambridge University Press.

Crasnow, Sharon. 2019. Feminist philosophy of science as social epistemology. In *The Routledge Handbook of Social Epistemology,* eds. Miranda Fricker, Peter J. Graham, David Henderson, Nikolaj J.L.L. Pedersen. New York: Routledge Press.

⎯⎯⎯⎯. 2013. Feminist philosophy of science: Values and objectivity. *Philosophy Compass* 8(4): 413-423. https://doi.org/10.1111/phc3.12023

Davidson, Donald. 2001 (1991). Three varieties of knowledge. Reprinted in *Subjective, Intersubjective, Objective*. Oxford: Oxford University Press.

Dirnagl, Ulrich, Editors in Chief, and Martin Lauritzen. 2010. Fighting publication bias: Introducing the Negative Results section. *Journal of Cerebral Blood Flow Metabolism.*30(7): 1263–1264. doi: 10.1038/jcbfm.2010.51

Dotson, Kristie. 2014. Conceptualizing epistemic oppression. *Social Epistemology* 28(2): 115-138, DOI: 10.1080/02691728.2013.782585

———. 2011. Tracking epistemic violence, tracking practices of silencing. *Hypatia: A Journal of Feminist Philosophy* 26 (2): 236–257.

Douglas, Heather. 2000. Inductive risk and values in science. *Philosophy of Science* 67(4): 559-579.

Fausto-Sterling, Anne. 2012. *Sex/gender: Biology in a social world*. New York: Routledge.

———. 1985. *Myths of gender*. New York: Basic Books.

Friedan, Betty. 1963. *The feminine mystique*. New York: W.W. Norton.

Gendler, Tamar. 2011. On the epistemic cost of implicit bias. *Philosophical Studies* 156: 33-63.

Goldenberg, Maya. forthcoming. *A Crisis of Trust? Vaccine Hesitancy, Science, and Public Debate*.

———. 2016. Public misunderstanding of science? Reframing the problem of vaccine hesitancy. *Perspectives on Science* 24 (5): 552-581.

———. 2013. How can feminist theories of evidence assist clinical reasoning and decision-making? *Social Epistemology: A Journal of Knowledge, Culture and Policy.* DOI: 10.1080/02691728.2013.794871

Harding, Sandra. 1991. *Whose science? Whose knowledge? Thinking from women's lives*. Ithaca: Cornell University Press.

hooks, bell. 2001. *Feminist theory: From margin to center*. London: Pluto Press.

Hymers, Michael. 2003. The dignity of a rule: Wittgenstein, mathematical norms, and truth. *Dialogue: Canadian Philosophical Review* 42(3): 419-446.

Intemann, Kristen. 2010. 25 years of feminist empiricism and standpoint theory: Where are we now? *Hypatia: A Journal of Feminist Philosophy* 25(4): 778-796.

———. 2005. Feminism, underdetermination, and values in science. *Philosophy of Science* 72 (5):1001-1012.

Jordan-Young, Rebecca. 2011. *Brain storm: The flaws in the science of sex differences.* Cambridge: Harvard University Press.

Kahan, Dan, Landrum, Asheley, Carpenter, Katie, Helft, Laura, and Kathleen Hall Jamieson. 2017. Science curiosity and political information processing. *Advances in Political Psycholog*y 38(1): 179-199. doi: 10.1111/pops.12396

Kourany, Janet. 2010. *Philosophy of science after feminism*. Oxford: Oxford University Press.

Longino, Helen. 1995. Gender, politics, and the theoretical virtues. *Synthese* 104: 383-397.

_____. 1990. *Science as social knowledge*. Princeton: Princeton University Press.

Lord, Ross, Lepper. 1979. Biased Assimilation and attitude polarization: The effects of prior theories on subsequently considered evidence. *Journal of Personality and Social Psychology* 37(11):2098-2109.

Mills, Charles W. 2005. "Ideal theory" as ideology. *Hypatia: A Journal of Feminist Philosophy* 20(3): 165– 84.

Okruhlik, Kathleen. 1994. Biology and society. *Canadian Journal of Philosophy* 20: 21-42.

Rolin, Kristina. 2012. A feminist approach to values in science. *Perspectives on Science* 20(3):320-330.

Saslow, Eli. 2018. *Rising out of hatred: The awakening of a former white nationalist*. New York: Doubleday.

Solomon, Miriam. 2012. The web of valief: An assessment of feminist radical empiricism. In *Out of the Shadows: Analytic Feminist Contributions to Traditional Philosophy*, eds. Sharon Crasnow and Anita Superson. Oxford University Press.

Tanesini, Alessandra. 2016. Intellectual humility as attitude. *Philosophy and Phenomenological Research*, doi: 10.1111/phpr.12326

Tavris, Carol. 1992. *The mismeasure of woman*. New York: Simon and Schuster.

Ungureanu, Manuela. 2004. Reading the minds of others: Radical interpretation and the empirical study of childhood cognitive development. *Dialogue* 43: 527-554.

van Anders, Sari. 2013. Beyond masculinity: Testosterone, gender/sex, and human social behavior in a comparative context. *Frontiers in Neuroendocrinology* 35(1): 198-210.

van Anders, Sari, Jeffrey Steiger, and Katherine L. Goldey. 2015. Effects of gendered behavior on testosterone in women and men. *PNAS* 112(45):13805-13810.

Wylie, Alison. 2003. Why standpoint matters. In *Science and Other Cultures: Issues in Philosophies of Science and Technology, edited by* Robert Figueroa & Sandra G. Harding. New York: Routledge, pp. 26-48.

Yap, Audrey. 2016. Feminist radical empiricism, values, and evidence. *Hypatia: A Journal of Feminist Philosophy* 31(1): 58-73.