

BAD APPLES: FEMINIST POLITICS AND FEMINIST SCHOLARSHIP

Abstract: Some exceptional and surprising mistakes of scholarship made in the writings of a number of feminist academics (Ruth Bleier, Ruth Hubbard, Susan Bordo, Sandra Harding, and Rae Langton) are examined in detail. This essay offers the psychological hypothesis that these mistakes were the result of political passion, and concludes with some remarks about the ability of the social sciences to study the effect of the politics of the researcher on the quality of his or her research.

Really free labour . . . is at the same time damned serious and demands the greatest effort. (Karl Marx, *Grundrisse*)¹

I. A BURDENED READER

I find myself as a reader having to do more work than is fair. Writers are less careful than writers used to be, and reading has become burdensome. Maybe I have been reading the wrong books, but there is more to the story. The publication industry, which caters to scholarly fads and chases the rewards of instant marketability, encourages negligent scholarship. The postmodern *Zeitgeist*, too, contributes to the decline of authorial conscientiousness. Mary Warnock bemoans this philosophy, according to which "there is an infinite variety of points of view" and "we each construct our own world" (1996, xxiv). Even if not every scholar subscribes to this ontology, postmodernism is seductive. It has made its way surreptitiously into our daily lives, our habits and practices, and into our scholarship. Listen to Roland Barthes (1974, 120):

[T]he meaning of a text lies not in this or that interpretation but in the . . . totality of its readings, in their plural system. . . . [T]he meaning of a text can be nothing but the plurality of its systems, its infinite (circular) "transcribability."

If anyone catches Barthes's singular meaning (*per impossibile*), she might presume, "My various readers, if by God's grace I have any, will read whatever they want into my words, so there is no reason to be careful, to explain my reasoning, provide evidence, or cite my sources. I hope only that the queer meaning they construct gets me good reviews, and that they do not take my ethical pronouncements as a call to arms against the feds (blacks, Jews, women, the disabled)."

Further, many academics publish just for the sake of publishing, concerned more that their books fatten their *vitae* than about the meticulousness of ideal scholarship. Or agitated writers publish speedily because their causes or pet peeves cannot wait, and must be conclusively handled by the academic pen *now*. Hence the extra work that readers must do, trying to fathom what the careless author is saying, to decipher the evidence (if any) the author provides for these claims, to make sense of incomplete citations and ambiguous bibliographies.² Evelyn Fox Keller, while defending the inscrutability of Barbara McClintock, suggests (1983, 143-44) that

To say someone is obscure is to put the burden of that failure in communication on the speaker. . . . [But] obscurity is, even with the most sophisticated audience, a relational attribute; it refers not simply to the properties of what is spoken (or written) but to how the message is heard (or read) as well. Looking at it in this way, we can avoid the futile argument about whose "fault" that failure of communication was.

We have no-fault automobile collision insurance and no-fault divorce. Now we have no-fault scholarly writing. To get the writer off the hook, Keller impeaches the "presuppositions, experiences, and expectations" of the reader--another case of blaming the victim.³

Feminism has been swindled by the *Zeitgeist*. Feminist treatises, along with the rest, are full of extravagant, equivocal propositions; reasoning and evidence are weak; conventions for quoting and citing sources are abandoned. Feminist scholarship has also been both a victim of and a contributor to the publication explosion. Feminist topics make for provocative titles and sexy dust jackets, and feminist books are stocked by every politically progressive library and crowd each other on bookstore shelves in America's malls. Eager, career-minded scholars have been quick to cash in on this bloated opportunity. All these social influences have undermined the quality of feminist scholarship, as they have undermined the quality of all scholarship.

Yet I have found in feminist literature scholarly carelessness that prompts a more specific worry. The mistakes in feminist scholarship I proceed to discuss cannot be attributed entirely to the *Zeitgeist*; nor do they seem to be due to publishing quickly for the sake of one's career; and these errors were not made because the scholars are women. The mistakes I found were made by well-educated and intelligent people, and hence were not caused by a lack of adequate training or brain power. Instead, the mistakes seem to have been made--an inference to the best explanation--because the political concerns and passion of these feminist writers induced them to ignore traditional standards of scholarship. Political fervor might lead scholars to see or find what they want to see or find, or helps them avoid seeing and finding what they do not want to see and find.⁴ Regardless of the psychological mechanism, the hypothesis is not implausible, that political commitment undermines scholarship. At least, it deserves to be investigated further, not to condemn feminist scholarship but to ferret out some causes of bad research, knowledge of which could improve scholarship in all areas. In claiming that some feminist scholars have been dramatically careless, I do not mean that they and others have not correctly exposed sexist bias in the humanities and sciences. But, as Susan Haack observes, "the antidote to pseudoinquiry, and to the loss of confidence in the importance of intellectual integrity it engenders, is real inquiry and the respect for the demands of evidence and argument it engenders" (1996, 61), rather than carelessness that further erodes confidence in intellectual integrity.

II. MISQUOTING THE BASTARD

Ruth Bleier received her undergraduate degree in political science from Goucher College and was trained as a physician at the Women's Medical College of Pennsylvania. Afterwards, she practiced medicine in Baltimore and did a neuroanatomy postdoc at Johns Hopkins. Bleier went on to be, until her death in 1988, a professor of neurophysiology and chair of the Women's Studies Program at the University of Wisconsin, Madison.⁵ Among Bleier's many publications in the feminist-critique-of-science genre is her 1984 book, *Science and Gender*.⁶ In his review in the *New York Times Book Review*, Stephen Jay Gould called *Science and Gender* an "excellent book" in virtue of the "firm stance" it takes against biological determinism (1984, 7).

Bleier devoted some of her feminist writing to a campaign against sociobiology as an account of *human* nature and culture. Early in *Science and Gender* (1984, 5-6), we read:

Sociobiology considers all human behaviors, characteristics, social relationships and forms of social organization to be biologically, genetically, and evolutionarily determined. Human characteristics and relationships are explicitly programmed in our genes, having evolved over millions of years because they were adaptive for survival. The very fact of their existence proves they have to exist, otherwise they would not have evolved. Not only do Sociobiologists claim to establish the

innateness of racism and wars, but also sex differences in social roles and position. Prominent in their writings is the specific attention they pay to the issues, concerns, viewpoints and goals of the contemporary women's movement. In fact, such issues become central to Sociobiological theory-making. "Sociobiology relies heavily upon the biology of male-female differences. . . . Ironically, mother nature appears to be a sexist" (Barash, 1977, p. 283). Sociobiology announces certain characteristics of the female and male "nature" to be universal and then explains why they are universal: why women are genetically predisposed to be "attached" to home and nursery and men to business and professions; why men are hasty, fickle, and promiscuous and women are faithful and selective; why men are aggressive and dominant and women nurturant and coy.⁷ [Bleier's ellipsis]

Is it not outrageous that David Barash would say this? We are supposed to believe that sexism in human societies, according to Barash, is genetically, evolutionarily inevitable.

Wanting to know whether Barash meant what he seemed to mean, I turned to *Sociobiology and Behavior* (1977, 283). This is what I found:

Sociobiology relies heavily upon the biology of male-female differences and upon the adaptive behavioral differences that have evolved accordingly. Ironically, mother nature appears to be a sexist,* at least where nonhuman animals are concerned. There may or may not be similar biological underpinnings of sexism in human societies; we do not know. Certainly, analysis and evaluation of male-female differences in human behavior should not be construed as supporting its propriety. Human sociobiology seeks to explore our nature, not to legitimize our foibles.⁸

A large difference exists between what Barash wrote and Bleier's truncated quotation; Barash's qualification "at least where nonhuman animals are concerned" eliminates the outrageousness and should have been quoted along with the rest. Barash emphasizes that he is speaking about nonhuman animals, not humans; and his "we do not know" shows that he is not offering a crude biological explanation of "sexism" in human societies. On the same page, Barash also writes: "Human social behavior is the product of many interacting factors and it is certainly unlikely that sexism is entirely biological. But it also may not be entirely cultural." This reasonable view about a complex phenomenon is far from Bleier's oversimplified characterization of sociobiology, according to which "human characteristics and relationships are explicitly programmed in our genes," are biologically "determined," and "innate."

Notice what Barash means by "sexism." In the asterisked footnote, which Bleier omits, Barash explains: "Sociobiology is sexist if sexism is recognition of male-female *differences*; however, it does not imply that either sex is *better*" (1977, 283n). He contends that sociobiology tries to explain male-female differences but does not pronounce on their worth. To claim merely that there *are* male-female differences that have *some* biological grounding is not to be "sexist" in an interesting social or political sense. In that sense, someone is "sexist" in advocating male supremacist sentiments or contributing to the existence of social policies that oppress women. Bleier's misquotation of Barash makes it look like he means that Mother Nature is "sexist" in the troubling sense and that he is jumping for joy over that. Barash wants to explain, not justify, sexual differences; it has always been a central goal of feminist scholarship to explain observed differences between men and women and the existence of sexism--without justifying them.⁹ Barash's finding some of the explanation of sexual differences in evolutionary biology makes him no more sexist than is the feminist Shulamith Firestone, who explains women's oppression biologically and calls for the "freeing of women from the tyranny of their reproductive biology by every means available," including "artificial reproduction" (1971, 206).

This piece of carelessness in Bleier's scholarship is ironic, given her accusation that "another problem in Sociobiological writings is the omission of unwelcome data that confound the stereotype" (1984, 34). Bleier's omission of Barash's qualifications forced his position to be indistinguishable from her stereotype of sexist, deterministic sociobiology. One need not be a defender of sociobiology to complain that Barash got a raw deal, and to suspect that Bleier allowed her feminist politics to overcome her scholarly responsibilities. In concluding an earlier critique of the sexist bias in biological determinism, Bleier had written: "No doubt, as feminist scholars reconstruct prehistory and history, as they offer their theories about biology and social behavior, they will continue to be accused of promoting their own bias. It is a

pity that the sensitivity to bias comes so late" (1978, 162). The rebuttal falls flat: I had been hoping for something better from feminist scholarship, not more of the same. Of course, the responsibility to quote opponents correctly, like the duty to tell the truth, is not absolute. Had it been possible for scholars to bring down Hitler by misquoting him, that would have been warranted. Do feminists perceive that Barash poses a threat of that magnitude to women?¹⁰

Bleier's mistake is ironic for another reason. What makes finding this kind of carelessness in Bleier astounding--discovering, by examining the material she reproduces, that she didn't quote it fairly--is that Bleier, as good scholars often do, has uncovered similar mistakes. While reading a scientific paper that argued for the existence of gender differences in brain-hemispheric dominance, Bleier noticed that the authors, in making their case, relied on a study of human fetal brains carried out by other investigators. When she turned to the latter paper, she discovered that it reported no sex differences at all. Bleier says about this experience: "[O]nly a highly skeptical reader of [the first paper] would bother, as I did, to find and read the [second] article, and thus discover the crucially withheld information" (1988, 95). I *was* skeptical that anyone would write what Bleier said Barash had written; that's why I turned to Barash (and found the "crucially withheld information"). But being skeptical, even if often psychologically sufficient for checking claims seen as suspicious, is not necessary; wanting to be thorough in one's research can supply reason for consulting relevant references. Bleier has also examined research on fetal androgenization; she claims that the investigators' conclusions gradually increased in confidence over the years even though their data remained the same (1986a, 150-52). About Bleier's exposing this sleight-of-hand, Haack (1992, 15) says: "A fine piece of detective work, this!" Bleier perceptively senses scholarly mistakes in the work of her foes. This ability should have made her less likely to make similar mistakes; that it did not succeed in doing so may indicate the effect of her political commitment on her scholarship.

Bleier is not the only feminist who has misquoted Barash. Ruth Hubbard, Professor of Biology at Harvard University (since 1990, Emerita)¹¹--six of whose books were reviewed by R. C. Lewontin in the *New York Review of Books* (1994)--committed a nearly identical mistake. This is the entirety of an indented quotation of Barash from an essay Hubbard co-authored with Marian Lowe (1979, 99; ellipsis in original):

Sociobiology relies heavily upon the biology of male-female differences and upon the adaptive behavioral differences that have evolved accordingly. Ironically, mother nature appears to be a sexist . . .

Lowe and Hubbard ended the quotation from *Sociobiology and Behavior* (1977, 283) with an ellipsis, indicating they were omitting something. But what they omitted ("at least where nonhuman animals are concerned") they should have included. Lowe on her own did the same thing in an earlier paper (from which the Lowe-Hubbard essay evolved): she quoted the sentence, replacing its closing qualification with an ellipsis.¹² In both essays, the paragraph that includes the truncated quotation also indicts sociobiologists, as Bleier did, for "selective use of data."¹³

In her own published writings, Hubbard has engendered yet other misquotations of Barash:

The [sociobiological] calculus goes like this: because women cannot produce as many eggs as men can sperm and, in addition, must "invest" at least the nine months of pregnancy (whereas it takes a man only the few minutes of heterosexual intercourse to send a sperm on its way to personhood), each egg and child represents a much larger fraction of the reproductive fitness a woman can achieve in her lifetime than a sperm or child does in a man's life. From this biological asymmetry, follow female fidelity, male promiscuity, and the unequal division and valuing of labor by sex in this society. As sociobiologist, David Barash, presents it, "mother nature is sexist," so don't blame her human sons. [1983, 6]

When I read this, I thought something wasn't right; I was remembering Barash's *Sociobiology and Behavior*, which states "mother nature appears to be a sexist." In Hubbard's endnote (1983, 8n3), I found "David Barash, *The Whispering Within* (New York: Harper & Row, 1979)," so I assumed that Barash had written something similar in his new book. I was annoyed--the burdened reader--that Hubbard failed to provide the number of the page containing the quoted sentence. A half-dozen years later, Hubbard twice

quoted Barash, supplying rough page numbers:

The calculus goes as follows. . . . From this biological asymmetry, they claim, follow female fidelity, male promiscuity, and the unequal division and valuing of labor by sex. As sociobiologist David Barash presents it, "mother nature is sexist," so don't blame her human sons (Dawkins 1976; Barash 1979, esp. pp. 46-90). [1988, 8]

Thus, from seemingly innocent biological asymmetries between sperm and egg flow such major social consequences as female fidelity, male promiscuity, women's disproportional contribution to caring for home and children, and the unequal distribution of labor by sex. As sociobiologist, David Barash, says, "mother nature is sexist," so don't blame her human sons (Dawkins 1976; Barash 1979, esp. 46-90). [1989, 124-25]

I went to Barash's 1979 *The Whisperings Within*, but did not find "mother nature is sexist":

To my thinking, sexism occurs when society differentially values one sex above another, providing extra opportunities for one (usually the males) and denying equal opportunities for the other (usually the females). As such, it has nothing to do with sociobiology. On the other hand, sexism is also sometimes applied to the simple identification of male-female differences, and on this count, sociobiology is, I suppose, sexist. No one would think it awful to state that a man has a penis and a woman, a vagina. Or that a man produces sperm and a woman, eggs. But when we begin exploring the behavioral implications of these facts somebody is sure to cry "Foul."

Nothing much different from *Sociobiology and Behavior*. Barash concludes with a hypothetical:

If male-female differences are sexist, we should put the blame where it really belongs, on the greatest sexist of all: "Mother" Nature! [1979, 90]

The sentence Hubbard claims is in Barash's text is not there. At best, she has paraphrased, so the sentence should not be enclosed in quotation marks, as high school students are taught (but do not necessarily learn).¹⁴ By truncating Barash's closing sentence, Hubbard hides the point of "if" from her reader. Barash doesn't assert that Mother Nature is sexist in an interesting sense; he is repeating the point made in the asterisked note in *Sociobiology and Behavior* (1977, 283n).¹⁵ Nor does Barash draw the conclusion Hubbard attributes to him, "don't blame her human sons."

The feminist epistemologist Jane Duran has expressed enthusiasm for these writers. Hubbard "is at her best when discussing the poverty of the sociobiology stance. Here she is able to use her accomplishments as a biologist to good advantage. . . . Since [Bleier] is a trained biologist, her criticisms penetrate in the same way that Hubbard's do" (1998, 103, 130). Of course, education in biology is helpful in comprehending sociobiology, and there is no doubt about the quality of much of what Hubbard and Bleier have written about it. What, then, is the explanation of the mistakes they made in quoting Barash? Mere sloppiness? Even if that is right, we should still want to probe the causes of sloppiness and ask why it occurs in some places but not others--that is, to develop a social science of slop. Indeed, this is part of Sandra Harding's project in feminist epistemology, although the slop she uncovers is men's sexist science (e.g., 1991, 39-46). Bleier and Hubbard have carried out a relentless scholarly attack on sociobiology, accusing it of sexism and racism, and their feminist writings are concerned with exposing the sexist biases of scientific research. This fact suggests the hypothesis that had they not been powerfully driven to destroy the credibility of what they took to be a threat to women, their misquoting Barash would have been less likely. Our science of carelessness should admit the possibility that Bleier's and Hubbard's feminist politics made quoting Barash accurately *appear to them* not worth bothering with (not necessarily that it induced them to be deliberately deceptive).

III. READING BETWEEN THE LEGS

Feminist scholar Susan Bordo is the Otis A. Singletary Chair in the Humanities and Professor of Philosophy at the University of Kentucky.¹⁶ Among her publications are *The Flight to Objectivity: Essays*

on *Cartesianism and Culture* (1987); *Unbearable Weight: Feminism, Western Culture, and the Body* (1993); the collection *Gender/Body/Knowledge: Feminist Reconstructions of Being and Knowing* (1989; edited with Alison Jaggar); and *Twilight Zones: The Hidden Life of Cultural Images from Plato to O. J.* (1997). Bordo is a productive philosopher who tackles novel topics. But I fear that her political commitment occasionally gets the best of her scholarship.

In the first essay of *Unbearable Weight* (the heavy, 42-page "Introduction: Feminism, Western Culture, and the Body"), Bordo discusses a stock way women are portrayed in popular media: as the "sexual temptress," as "continually and actively luring men to arousal." According to Bordo (1993, 6), these portrayals

work to disclaim male ownership of the body and its desires. The arousal of those desires is the result of female manipulation and therefore is the woman's fault. This construction is so powerful that rapists and child abusers have been believed when they have claimed that five-year-old female children "led them on."

In our society, there is a similarity between the responsibility of the actively seductive woman in cinema and the real life responsibility ascribed to the young victims of sexual abuse. Bordo does not explicitly claim a causal relationship between the two; that is not the issue I wish to pursue. Instead, I question Bordo's assertion that child abusers *have been believed* when they said that a five-year-old child led them on. Really? Who, exactly, believes them? Bordo thinks that such things have happened and that they are evidence for the "powerful" social "construction" of an ideology of the sexually predatory female. But Bordo does not document her astonishing claim that child abusers "have been believed." Shall we take her word for it? (There are, otherwise, four pages of endnotes for this chapter of *Unbearable Weight*.)

Bordo does the same thing in the next paragraph:

Conscious intention, however, is not a requisite for females to be seen as responsible for the bodily responses of men, aggressive as well as sexual. One justification given for the exclusion of women from the priesthood is that their mere presence will arouse impure thoughts. Frequently, even when women are silent (or verbalizing exactly the opposite), their bodies are seen as "speaking" a language of provocation (Figure 1). When female bodies do not efface their femaleness, they may be seen as inviting, "flaunting": just two years ago, a man was acquitted of rape in Georgia on the defense that his victim had worn a miniskirt.

I wondered whether this happened. I found it incredible that "just two years ago"--in the recent gender-conscious atmosphere of our society--a man achieved a rape-acquittal "on the defense" that his victim had worn a miniskirt. Again, Bordo does not document her claim. I wrote to her (the burdened reader), asking for help. She sent a photocopy of this newspaper article:

RAPE GUILTY PLEA,

AFTER ACQUITTAL

Man Gets Life Term in Georgia

--Florida Jury Freed Him

ATLANTA, Dec. 6 (AP) -- A man who was acquitted in a Florida rape case in which the jury foreman said the victim had "asked for it" pleaded guilty on Tuesday to raping a woman in Decatur, Ga., and was sentenced to life in prison.

The convicted man, Steven Lamar Lord, had kidnapped the woman at a 24-hour banking machine. Judge Hilton Fuller of Superior Court sentenced him to a concurrent 20-year term for the kidnapping. Under Georgia law, Mr. Lord, 26 years old, will be eligible for parole after seven years.

Mr. Lord told the judge that he entered his plea because he did not want to make the victim relive the trauma.

Court records show that Mr. Lord had reportedly told the victim: "It's your fault. You're wearing a skirt."

'Regrets What has Happened'

Mr. Lord's attorney, Bill Hankins, a public defender, told Judge Fuller that Mr. Lord "regrets very much what has happened to her."

Mr. Lord was acquitted of rape in October in Fort Lauderdale, Fla. At that time the jury foreman, Roy Diamond, said jurors believed the woman accusing him, who was wearing a white lace miniskirt without underwear when she was attacked, had "asked for it." Mr. Diamond later said he meant she had asked for sex and not rape.

The 24-year-old Georgia woman whom Mr. Lord pleaded guilty to raping on Sept. 24, 1988, had testified against the defendant in the Florida trial. She said then that Mr. Lord had raped her at knifepoint, as he had been accused of doing to the woman in Florida.

Other jurors in the Florida case said they had leaned toward acquittal because of the calm demeanor of the woman who brought the charge, compared with the emotional testimony of the Georgia woman.

"When the Georgia woman testified, my heart sank," said one juror, Dean Medeiros. "But when the other one testified, she didn't appear to be shaken up. Basically, we didn't believe her story."

'No Remorseful Awareness'

Betty Grant, president of the Georgia chapter of the National Organization for Women, said Tuesday night that she was delighted that Mr. Lord was finally sentenced. "There's no remorseful awareness here," she said. "I hope his sentence sticks because we know rape offenders do repeat the crime."

Alexander Siegel, the lawyer who represented the Florida woman, said in a telephone interview that Mr. Lord's guilty plea came because he had heard the Georgia woman's testimony in the Florida case.

"He knew he didn't stand a chance," Mr. Siegel said.

He said his own client was vindicated now that her accused attacker had been given a life sentence. Mr. Siegel said he had not been able to reach his client with the news of Mr. Lord's plea because her telephone was out of order.¹⁷

There are three mistakes in Bordo's presentation of the article. One mistake is that the man was acquitted in Florida, not Georgia, as announced in the headline and first sentence. Even if not very important (although Georgians might disagree), this mistake is baffling, except on the hypothesis that Bordo only browsed the article or took singularly sloppy notes on it.

Bordo's second mistake is misdescribing what the woman had worn: she "had worn a miniskirt," is the way she puts it. Bordo might have mentioned that the woman was attired (or not attired), specifically, in "a white lace miniskirt without underwear." This misrepresentation is important, for what the woman was actually wearing does not provide evidence for (as Bordo intends it to) Bordo's claim that "When female bodies do not efface their femaleness, they may be seen as inviting, 'flaunting'." Women who "do not efface" their femaleness are women who do not hide their sexuality or go out of their way to shroud their bodies. By contrast, wearing a see-through miniskirt *sans* panties is not the failure to conceal, but putting on display. I am not justifying or excusing the

attack. Perhaps Bordo was afraid--this is a possible psychological effect of her feminist politics--that other readers, had she told the truth, would have excused the attack. If so, she should not have used the example at all.¹⁸

Finally, Bordo claims that the man was acquitted in the Florida case *on the defense* that the woman had worn a miniskirt. Nothing in the article justifies that conclusion; nothing hints that this was his defense, asserted, say, by his lawyer to the jury in closing arguments. The jury's reasoning--which is irrelevant to the question of the man's defense--is, at any rate, not clear. The foreman said that the jurors (some? all?) thought the woman had "asked for sex," which suggests they attributed consent to her in virtue of her pantyless attire.¹⁹ These jurors undermine Bordo's exhibition of the case as one in which "conscious intention . . . is not a requisite for females to be seen as responsible." Other jurors said they were dissuaded from convicting the man in virtue of the woman's "calm demeanor" when testifying: "we didn't believe her story." The story, whatever it was, is more complex than Bordo's oversimplified account.

The worst of it is not that Bordo failed to document a fact that screams for citation. The worst is that Bordo's claim is not supported by her source and is likely false, and that Bordo's misdescription was, in effect, the manufacturing of evidence for her feminist thesis. As a result, Bordo's other undocumented claim, that child abusers "have been believed," cannot be accepted until it has been substantiated. But gullible readers, or those who relish believing gruesome things about patriarchal society, will let these "facts" sink into their brains and promulgate them. Scholars have a responsibility to forestall this process. In the "Notes" to *Unbearable Weight* (301), Bordo says that "portions of this essay grew out of a talk . . . delivered for the Women's Studies Inaugural Lecture Series at Bates College." Did she air these falsities in public? Scholarly carelessness is of more than mere academic interest; it can have undesirable social consequences.

IV. WOMEN'S WAYS OF FUDGING

Sandra Harding received the Ph. D. from New York University. After many years as Professor of Philosophy and Director of the Women's Studies Program at the University of Delaware, she was appointed Professor of Education and Women's Studies at the University of California, Los Angeles. Her *The Science Question in Feminism* (1986) won the Jessie Bernard Award of the American Sociological Association.²⁰ She has also written, in addition to dozens of essays on feminist epistemology, *Whose Science? Whose Knowledge?* (1991) and *Is Science Multicultural?* (1998).

It is the cause of enormous headaches, when studying Harding, that she is lax in providing accurate citations. I will discuss just one example. Here is the complete text of an endnote in Harding's essay "Feminism, Science, and the Anti-Enlightenment Critiques" (1996a, 317n7):²¹

M. F. Belenky et al. (1986) point out that a woman's claim that "It's my opinion . . ." means that it is just her opinion; a man's identical sentence means he's got a right to his opinion.

I gathered, when reading the note, that the comparison between what women and men mean by "It's my opinion" was made by Belenky et al., and that Harding was repeating their assertion. But I thought it odd that Belenky et al. would have contrasted these two groups, because their subject population consisted entirely of women. How was I to check Harding's note? She gave no page number. Her failure to supply a page number, which saved her some time, resulted in my having to search through *Women's Ways of Knowing*. Maybe Harding saved more time ("resources") by not supplying the page number than I lost by having to reread *Women's Ways*. Even so, I doubt that bibliographic practices should be governed by a calculus of time. Has the scholar's obligation to provide accurate citations, including page numbers (justifiable as being part of her defending what she writes), been abolished, replaced by a mere elective courtesy?

Eventually I found, in Belenky et al.'s description of *subjective knowers* (not of women generally, as Harding implies), this:

The form that multiplicity (subjectivism) takes in these women . . . is not at all the masculine assertion that "I have a *right* to my opinion"; rather, it is the modest, inoffensive statement, "It's *just my opinion*." Their intent is to communicate to others the limits, not the power, of their own opinions, perhaps because they want to preserve their attachments to others, not dislodge them.²²

The comparison between women and men was (barely) made by Belenky et al., even though they did not investigate what men, subjective knowers or not, might mean in using "It's my opinion."²³

Harding, who had earlier reviewed *Women's Ways*, knows that "the book is based on lengthy interviews with 135 women" (1987d, 6). At several places in her writings, Harding claims that "Research designs that legitimate having only men interview only men about either men's or women's beliefs and behaviors are bound to distort reality."²⁴ Yet Harding never complains about Belenky et al. that they, too, "distort[ed] reality" by committing this methodological mistake. Harding accepts a comparison between women and men that on her own account Belenky et al. had no warrant making. Indeed, the methodological criticism Harding does raise is that Belenky et al. did not interview certain *women*. Harding asks (1987d, 7) whether Belenky et al. would have reached their conclusions about women's highest way of knowing had they interviewed

women unionists, women working for welfare rights or reproductive freedoms, Black women political activists, women leaders of the peace movement. What if they had interviewed--dare I mention the word--*feminist* political activists?²⁵

Harding is suggesting that had Belenky et al. interviewed these women--including women like *herself*--they would have found an even *higher* way of knowing. The authors of *Women's Ways* agree in principle with Harding's criticism:

We did not discuss our findings in terms of class, racial, or ethnic differences among the women, a decision that seemed reasonable . . . , given our relatively small and nonrepresentative sample and qualitative research methodology that does not lend itself to comparative statements and conclusions.²⁶

This admission suggests that *Women's Ways of Knowing* is worthless as social science. By the way, Belenky et al. claim that they studied only women because William Perry had already studied men.²⁷ This reason is inadequate; for one thing, comparing women and men requires that both be interviewed by the same researchers who use the same techniques under the same conditions. The four researchers carrying out the *Women's Ways* project seem to have had enough woman power to interview some men. Carol Gilligan did it for *In A Different Voice* (1982).

In an essay written a few years later, Harding repeats the endnote (1993, 80n39). It contains an incomprehensible change:

Mary G. Belenky and her colleagues point out that the phrase "It's my opinion . . ." has different meanings for the young men and women they have studied. For men this phrase means "I've got a right to my opinion," but for women it means "It's just my opinion." Mary G. Belenky, B. M. Clinchy, N. R. Goldeberger [sic], and J. M. Tarule, *Women's Ways of Knowing: the Development of Self, Voice, and Mind* (New York: Basic Books, 1986).²⁸

I included Harding's reference (again no page number) to show that she didn't have another publication by Belenky et al. in mind, just *Women's Ways*.²⁹ From failing to criticize the study's gender comparison of "It's my opinion," on the grounds that Belenky et al. interviewed only women, Harding has progressed to asserting that in arriving at the comparison Belenky et al. *had* studied both men and women.

Writing, six years after having reviewed *Women's Ways*, that Belenky et al. had studied both men and women is an incomprehensible blunder; someone who gets such a fundamental fact about *Women's Ways* wrong is an untrustworthy scholar.³⁰ But even when the book was fresh in her mind, Harding misunderstood or misrepresented its content in a way that made it more congenial to her feminist politics. Consider these lines from Harding's review of *Women's Ways*, statements she makes in explaining what Belenky et al. found (1987d, 7):

[T]his study [finds a] gap between separate (characteristically male) and connected (characteristically female) thinking.

The book's thesis is that women's most developed perspective ["constructed knowledge"] is more complex and fruitful than the highest one found in men.

[T]he kinds of knowing favored by women and men differ.

The authors emphasize that this epistemological voice [connected knowing], like the others, is not limited to one gender though it may be more favored by one.

It would be surprising had the study found any of this for, to belabor the obvious, Belenky et al. did not interview men. At least in the context of discussing separate and connected knowing, they are extraordinarily candid, an admission Harding neglects to mention. "Connected knowing," Belenky et al. say (1997, 102-103), is not

exclusively a female voice. We all encounter men, in person and in print, who speak in this voice. Separate and connected knowing are not gender-specific. The two modes may be gender-related: It is possible that more women than men tip toward connected knowing and more men than women toward separate knowing. Some people, certainly, would argue that this is so, but we know of no hard data (to use a favorite separate-knowing term) bearing directly on the issue, and we offer none here because we interviewed no men.

Belenky et al. refrain from declaring that connected knowing is "characteristically female"; they admit they cannot contend that women and men "tip toward" (or "favor," in Harding's language) different ways of knowing; they do report, "we have in our sample very few highly reflective women who relied more heavily on connected than separate procedures" (1997, 124); and they never claim, nor is it the "thesis" of their book, that only or mostly women attain the highest way of knowing, "constructed knowledge."³¹ Harding is familiar with the passage I quoted, but quotes *only* its beginning (1987d, 7):

The authors emphasize that this epistemological voice, like the others, is not limited to one gender though it may be more favored by one: "We all encounter men, in person and in print, who speak in this voice. Separate and connected knowing are not gender-specific." But even though this epistemological voice is valued in some areas, the dominant tendency is to think of it as a less rigorous, less objective, less rational mode of thought--in short, I would say, as feminized.

Instead of quoting the rest of the passage--which shows that Harding's "more favored" was her own wishful extrapolation--Harding proceeds to claim that connected knowing, which *she* has made out to be favored by women, is socially less valued. Harding thereby "finds" confirmation of her view that in patriarchy that which is associated with women is of less value than that which is associated with men. Harding's misconstrual of *Woman's Ways* amounts, in effect, to the misuse of research to support one's political ideology. It is to do exactly what feminist scholars, including Harding, have accused mainstream patriarchal scholars of doing.³²

V. POUNDING PORNOGRAPHY

Rae Langton received the Ph. D. from Princeton, was lecturer at Monash University, and is now lecturer in philosophy at the University of Sheffield.³³ [As of Fall, 1999, University of Edinburgh.]

Langton has published papers on love (1997a), sexuality (1995), and pornography (1990, 1993, 1997b, 1998b; the first two appeared in the prestigious *Philosophy and Public Affairs*); she has also written on Kant (1992, 1998a). In some of her work on pornography, Langton explicates and defends the often turgid writings of the feminist legal scholar and anti-pornography activist Catharine MacKinnon. Despite her philosophical competence, Langton has a penchant for relying uncritically on popular feminist literature, which detracts from the quality of her scholarly work

Some of this mischief occurs in "Speech Acts and Unspeakable Acts" (1993), in which Langton defends MacKinnon's theses that pornography subordinates women and that it silences them.³⁴ When wrapping up her defense of MacKinnon's subordination thesis, Langton points out (1993, 311) that

to answer the question, "Does pornography subordinate?" one must first answer another: "Do its speakers have authority? " If they do, then a crucial felicity condition is satisfied: pornographers' speech acts may be illocutions that authoritatively rank women, legitimate violence, and thus subordinate.

Langton's defense of the subordination thesis turns on the question about the "authority" of pornography. She makes the point again when defending the thesis that pornography silences women. According to Langton, some men, when seeking sex, do not take a woman's "no" seriously (1993, 320). This happens in two ways. In one way, the woman's "no" is a refusal but he "fails to obey" (321). The woman experiences the "silence . . . of frustration" (323). The second involves illocutionary disablement. "Sometimes 'no,' when spoken by a woman, does not *count* as the act of refusal" (321). "[A]lthough the appropriate words can be uttered, those utterances fail to count as the actions they were intended to be" (299). How does it come about, through pornography, that women are silenced? "If . . . pornography legitimates sexual violence, then it follows that one of pornography's effects may be to prevent a woman's refusal of sex from achieving its intended purpose. If pornography legitimates rape, then it may silence refusal by frustrating its perlocutionary goal" (323). Langton here explains how pornography could bring about the first kind of silencing. What about the second? In pornography, says Langton, the refusal of women to sex "is absent altogether"; "consent is the *only* thing a woman can do with her words" (324). As a result, "Someone learning the rules of the game from this kind of pornography might not even recognize an attempted refusal" (324).³⁵ Langton concludes by observing, "the claim that pornography silences requires the same premise: pornographic speech must be authoritative if it is to engender the silence of illocutionary disablement" (326).³⁶ Her entire paper, the defense of the subordination and silencing theses, turns on whether pornography is "authoritative" (see also 1997b, 342-43, 347). She has a big stake in it.³⁷

Langton remarks that whether pornography is authoritative cannot "be settled from the philosopher's armchair" (1993, 312; see 1998b, 264). But in a convoluted passage that narrows the thesis that pornography is authoritative and describes the sort of person for whom pornography is authoritative, Langton tries to settle the matter (1993, 312):

What is important is whether it is authoritative in the domain that counts--the domain of speech about sex--and whether it is authoritative for the hearers that count: people, men, boys, who in addition to wanting "entertainment," want to discover the right way to do things, want to know which moves in the sexual game are legitimate. What is important is whether it is authoritative for those hearers who--one way or another--do seem to learn that violence is sexy and coercion legitimate: [1] the fifty percent of boys who "think it is okay for a man to rape a woman if he is sexually aroused by her," [2] the fifteen percent of male college undergraduates who say they have raped a woman on a date, [3] the eighty-six percent who say that they enjoy the conquest part of sex, [4] the thirty percent who rank faces of women displaying pain and fear to be more sexually attractive than faces showing pleasure. In this domain, and for these hearers, it may be that pornography has all the authority of a monopoly.³⁸

At this crucial place in her argument, Langton cites, as the source of her four claims about boys and

men, Naomi Wolf's *Beauty Myth* (1992) and Robin Warshaw's *I Never Called It Rape* (1988), instead of going to the social psychology research literature. Here is the footnote:

The first statistic comes from a UCLA study, Jacqueline Goodchild et al. cited in Robin Warshaw, *I Never Called It Rape* . . . , p. 120; the second and third from studies by Alfred B. Heilbrun, Emory University, and Maura P. Loftus, Auburn University, cited in Naomi Wolf, *The Beauty Myth* . . . , p. 166; the fourth from research done by Virginia Greenlinger, Williams College, and Donna Byrne, SUNY-Albany, cited in Warshaw, p. 93. [1993, 312n40]

In [1], Langton quotes someone as having written that the boys "think it is okay for a man to rape a woman if he is sexually aroused by her." In the footnote, Langton says the "statistic" is due to "Goodchild" as reported by Warshaw on page 120 of *I Never*. But Warshaw claims, correctly, that this study "did not use the word 'rape'."³⁹ Warshaw's reproduction of the results of the study bears this out: slightly over fifty percent of the boys (aged 14 to 18) agreed that "it's OK to force sex." Further, the boys meant it was OK to force sex in *these* circumstances: when "she is going to have sex with him and changes her mind" (54%), when "she has 'led' him on" (54%), and when "she gets him sexually excited" (51%). Warshaw's reproduction of the study does not include Langton's phrase "think it is okay for a man to rape a woman if he is sexually aroused by her"; neither does the original research report.⁴⁰ Some difference might exist between "rape" and "force." But there is a big difference between the study's "she gets him sexually excited" and Langton's "he is sexually aroused by her." In the context of the study, which probed attitudes about responsibility for date rape, the first implies some agency on the girl's part, similar to "she is going to have sex with him and changes her mind." The second is a weaker expression; it allows that she did nothing at all, except to exist as the passive object of his gaze by which he becomes aroused. Langton's [1] implies that boys think it is OK to force girls in *this* situation.

Why did Langton change the words? Maybe she ignored Warshaw's reproduction of the study and focused, instead, on the lesson that Warshaw herself later drew out of it: "More than half the boys . . . thought that it was okay for a male to force (that is, rape) a female if he was sexually aroused by her" (1988, 120). But the quotation in Langton's [1] is not even an accurate quotation of Warshaw's mistaken gloss on the study. Look at Wolf's gloss on the study (not cited by Langton in her footnote): "More than 50 percent of the boys . . . thought it was okay for a man to rape a woman if he was sexually aroused by her" (1992, 167). Langton's quotation in [1] is closer to Wolf's mistaken gloss (it differs only in tense) than it is to Warshaw's mistaken gloss: Langton and Wolf use "man" and "woman," whereas Warshaw uses "male" and "female"; Langton and Wolf write "rape," while Warshaw writes "force (that is, rape)." Langton, it seems, took the statistic and the mistaken gloss not from Warshaw, as she claimed in her note, but from Wolf.⁴¹ In any event, Langton's quotation in [1] is not a quotation of anyone, let alone the authors of the original research study.⁴²

In her footnote, Langton claims that the information contained in [2] and [3] is due to Heilbrun and Loftus (respectively?), as reported by Wolf, *Beauty Myth* (166). Wolf does say, on page 166, "[A]t Auburn University, 15 percent of male undergraduates said they had raped a woman on a date." On page 165 (rather than 166), Wolf reports [3]: 86.1% of 114 undergraduate men agreed to the statement "I enjoy the conquest part of sex." What were Wolf's sources? For [2], Wolf cites *only* Warshaw, *I Never* (13-14).⁴³ Wolf does refer (1992, 315) to Heilbrun and Loftus ("cited in [Warshaw], p. 97"), but as her source of [4], not of [2] and [3]. (Warshaw, on page 97 of *I Never*, does credit [4] to Heilbrun and Loftus.) For [3], Wolf provides (1992, 315): "Survey was conducted by Virginia Greenlinger, Williams College, and Donna Byrne, SUNY-Albany; cited in Warshaw, p. 93," which is the study Langton cites, through Warshaw (p. 93), for [4]. For [4], Langton says that the figure came from a study done by "Virginia Greenlinger" and "Donna Byrne," and she sends us to page 93 of Warshaw, *I Never*. But that study, as Warshaw says, resulted in the 86.1% figure of [3]. It seems that Langton did not go to Warshaw, as she said she did, but relied on her mistaken construal of Wolf's references. More evidence exists for this conclusion. Note something distinctive about Langton's and Wolf's references to Warshaw's *I Never*, p. 93. In her footnote, Langton writes,

"Virginia Greenlinger, Williams College, and Donna Byrne, SUNY-Albany," and Wolf, too, writes, "Virginia Greenlinger, Williams College, and Donna Byrne, SUNY-Albany" (1992, 315). Each contains the same two mistakes. On page 93 of *I Never*, Warshaw spells both names correctly: "Virginia Greendlinger" and "Donn Byrne"--a guy, a well-known social psychologist.

Langton's failure to read Warshaw carefully, or at all, is not inconsequential. Consider, finally, Langton's fourth item, in which she mentions "the thirty percent who rank faces of women displaying pain and fear to be more sexually attractive than faces showing pleasure." That's disturbing. But is it true? Wolf thinks so: "30 percent of male college students rated faces of women displaying emotional distress--pain, fear--to be more sexually attractive than the faces showing pleasure" (1992, 165). Wolf refers (1992, 315) to Warshaw, *I Never*, page 97. But in *I Never* (1988, 97), we find something different from what Langton and Wolf claim:

[S]ubjects were shown 36 photographic slides of women's faces expressing happiness, surprise, anger, fear, disgust, and sadness and then asked to rate the sexual attractiveness of each. . . . Heilbrun and Loftus found that 30 percent of the men rated the faces of women displaying emotional distress to be more sexually attractive than the faces showing pleasure.

Warshaw never mentions "pain" among the states displayed in the slides; and her summary of the study's results mentions only "emotional distress." Where did the "pain" in Wolf and Langton come from? Other questions arise. What emotions are included in the "emotional distress" that some men found more sexually attractive than pleasure? Further, did 30 percent of the men find "fear" more sexually attractive than pleasure, as Langton states in [4], or did each of the men in the 30 percent find *some distress or other* to be more sexually attractive than pleasure, so that only a few percent found any specific "distress" attractive? This is a less disturbing fact than that reported by Langton. But "pleasure," too, is *not* listed among the emotional states displayed on the faces in the slides; that was Warshaw's gloss on "happiness." If a small percent of the men found a face displaying anger or fear (or . . .) more sexually attractive than a face displaying "happiness," they might have been responding to the passionate intensity of anger (or . . .) and were not much aroused by the sometimes silly (and not sexually appealing) quality of happy faces. Langton, when writing her essay, could not have known the answers to these questions, for neither Warshaw nor Wolf provides them. But, clearly, Wolf illegitimately assumed "fear"; she created "pain" out of whole cloth; and she repeated Warshaw's "pleasure" gloss on "happiness."⁴⁴ Langton negligently allowed herself to be dragged into these fabrications.⁴⁵ "Aristotle was certainly a knowing man, but nobody ever thought him so because he blindly embraced, or confidently vented, the opinions of another. And if the taking up another's principles, without examining them, made not him a philosopher, I suppose it will hardly make any body else so."⁴⁶

Answers to these questions might be found in the original research report, to which I now turn.⁴⁷ The investigators never use the word "pleasure," only "pleasant" (320, 326), and they never showed their male subjects pictures of women whose faces displayed pleasure (324). The only facial expression in the "pleasant" category was happiness (326). The investigators ignored the data for "surprise" (326). They included the faces displaying fear, anger, disgust, and sadness in the "distressed" or "emotional distress" category (323, 325). The subjects were shown no faces displaying the pain of "physical harm" (323). Slides of faces displaying fear, anger, disgust, and sadness were used because "in different combinations [they] are typical of the female's reaction to sexual aggression" (325), that is, situations in which a woman is "dissatisfied" because a "male's excessive sexual advances" go beyond "the sexual limits that she would have preferred" (324). Faces displaying happiness were shown to the subjects because this affect "should be perceived as indicating satisfaction, not distress, by an observer. The ratings for facial expression of happiness provide an estimate of how sexually attractive the female models appeared to each subject given more optimal conditions" (326), that is, when sexual aggression is absent. (I think showing faces displaying sexual desire would have been more pertinent.)

Did the male subjects who perceived emotional distress to be more sexually attractive than happiness all respond to the same distress? We don't know: the investigators calculated, for each male subject, the *average* of the sexual attractiveness rankings for the four distressed faces (326). The next part is crucial: the average of the rankings for the distressed faces and the ranking for the happiness face were each "converted into a standard score distribution" (327). *Then* the happiness ranking for each subject was subtracted from his ranking for the distressed faces. Using this statistical procedure, the researchers found that 30 percent of the subjects, 15 of 50, "achieved negative *index* scores" (327; italics added). That is, it is *false* (contrary to Langton's [4]) that the investigators reported that 30 percent of the male subjects found faces displaying distress to be more sexually attractive than faces displaying happiness; the 30 percent figure is a relativized index score, not an absolute figure. Indeed, only 12 percent of the subjects (6 of 50) ranked distress as more sexually attractive than happiness. This fact emerges in a cautionary paragraph of the report (327):

It should be emphasized that the index scores refer to relative, not absolute, patterns of sexual attractiveness. Almost all subjects (44 out of 50) found the women registering happiness to be more sexually appealing than the same women portraying any other affect. The relative scoring of sexual attraction means that any given score reflects how sexually attractive the subject finds women expressing various distressed emotional states: (a) relative to his own baseline for sexual attraction of the same women when they are expressing a positive affect and (b) relative to other men.

The reason for relativizing the sexual attractiveness scores is that the investigators were *not* studying whether men respond differently to distressed faces than they do to pleasant faces. Instead, they were attempting to correlate scores on this measure (relativized to the whole subject group) with subjects' self-reported histories of sexual aggression (327-28).

The entire passage, and its footnote, in which Langton describes the results of research in social psychology, is sloppy. This carelessness is surprising: she was trying to make plausible for her readers the claim on which her paper depended, that "pornography has all the authority of a monopoly." But maybe Langton was sloppy *because* the claim was crucial in rounding out her case against pornography: she saw what she wanted to see, and did not look for what she didn't want to find.⁴⁸ Since the beginning of her career, Langton has been writing thoughtful essays against pornography; she is smart, well-educated, and well-read. Yet her pornography essays contain an abundance of errors that make it easier for her to make her case. These mistakes seem inexplicable unless we suppose that Langton's feminist-inspired determination to condemn pornography and defend MacKinnon led her to ignore traditional standards of scholarship. Langton's errors are especially regrettable because she writes about a social issue whose resolution has far-reaching effects. Errors in feminist social philosophy, in contrast to errors in, say, the exegesis of Thomas Reid, can adversely affect lives. A feminist scholar, as does any scholar writing about social issues, has a special obligation to avoid carelessness.

VI. STANDARDS OF SCHOLARSHIP

In the cases I have presented, a selective (not global) loss of objectivity was, plausibly, a psychological effect of political commitment.⁴⁹ I came across these examples of carelessness in feminist scholarship by accident, guided by my natural scholarly curiosity. That is, I have not carried out a controlled investigation of the relative incidence of carelessness in the academic work of feminists. Other scholars have found similar curiosities in feminist writings,⁵⁰ just as Bleier and other feminists have found mistakes in the research of mainstream science. I have exhibited here only what might be called the bad apples; there is, of course, good feminist scholarship. Harding has gone so far as to claim that feminist research is *better* than mainstream research⁵¹--a supremely ironic thesis, given the mistakes she has made. In light of the sorts of mistakes I and others have found in feminist scholarship, however, it is not clear that feminist-inspired research has been an improvement as scholarship over male, men's, or patriarchal scholarship. I suggest, then, that a controlled study is worth doing: let a team of social scientists search systematically for scholarly

blunders and try to correlate them with, for example, the type of feminism embraced by the authors, or its intensity. This project would add to our understanding of the relationship between politics and the quality of research. Does commitment to some political views, in contrast to others, more likely lead to scholarly carelessness? Let our team of social scientists examine the work in the humanities and the sciences of the various kinds of liberals, feminists, conservatives, Creationists, and neo-Nazis.

It is problematic, however, whether the social sciences could test the hypothesis that some scholars, as a function of their politics, are more prone to make mistakes than others. Can the social sciences actually illuminate the conditions that make the production of good feminist scholarship more likely? Can they even illuminate the conditions that make for good scholarship *simpliciter*? What seems required by any study of this sort is an uncontested (and true?) notion of "good scholarship." What counts as good scholarship, however, is not only debatable in itself but also an issue that might divide feminists and the mainstream. In claiming that the mistakes I found in the work of feminist scholars violate standards of scholarship, I have assumed a traditional notion of "good scholarship," a notion that might be rejected either by feminist theory or by the scholars I criticize. Perhaps there are no ideology-free standards of scholarship, in which case there might be standards of scholarship consistent with or derivable from feminism, standards consistent with or derivable from liberalism, and so forth. If so, how our team of social scientists would be able to avoid begging questions about the relationship between the quality of research and the politics of the researcher is unclear. Ruth Ginzberg has written about feminist scholars, "We have struggled . . . to be able to apply the term 'scholarship' to our work, even when much of that work didn't count as scholarship under the old androcentric language rules" (1996, 373-74). The feminist scholarship I have discussed, then, might not be bad scholarship at all, but good (feminist) scholarship. It is "bad" only by "androcentric" standards.

Furthermore, and politics aside, is there any such thing as a gender-neutral standard of scholarship? Are there gender-neutral standards of *anything*? No, if we are to believe a good deal of feminist theory. The idea of gender-neutral standards of scholarship may be an illusion, in the same way, as urged by MacKinnon, that the idea of gender-neutral standards of the "reasonable person" in the law of sexual harassment is an illusion (1989, 181, 183). Standards of scholarship might have been different--say, less abstractly rigorous--if women instead of men had been their creators. Alison Jaggar has suggested that had women been doing philosophy, instead of, say, John Locke, many things would have turned out differently: "Just as it is unlikely that women's experience would have led them to frame the liberal conception of human nature, so it is unlikely that women would ever have developed the conception of equality that is associated with abstract individualism" (1983, 46). It might be fair to extend this suggestion by saying that it is "unlikely" that women would have developed the particular, perhaps peculiar, standards of scholarship that were also the product of men's thought. Still, even if standards of scholarship developed by a women's intellectual tradition would have been somewhat different from those actually developed by men, it is implausible that standards of scholarship developed by women would have blessed misquotations. Why this particular standard of good scholarship--quotations must be accurate--seems both politics-neutral and gender-neutral is an interesting question.⁵²

NOTES

1. In McLellan (1977, 368).
2. Sometimes an incompetent copyeditor might have to share the blame with a sloppy scholar; see Michael Dummett (1997, 372-73).
3. Keller's excuse for McClintock turns out to be self-serving: much of Keller's work in the "gender and science" area is sloppy and obscure; see my 1994 and 1995.

4. In my 1995, I argued that feminist scholars wrongly found rape metaphors in the writings of Francis Bacon because doing so fit with their feminist politics. Bacon warned against this inclination. "[M]an prefers to believe what he wants to be true. He therefore rejects . . . sober things, because they restrict his hope. . . . [E]motion in numerous, often imperceptible ways pervades and infects the understanding" (*Novum Organum*, Bk. I, aph. 49 [1994, 59-60]). "[V]arious doctrines can be based and built upon the phenomena of philosophy. And fictitious tales of this kind also have this in common with those of poetic drama, that narratives written for the stage are neater and more elegant and more as one would wish them to be than true accounts drawn from history" (aph. 62 [1994, 67]).

5. Bleier (1986b, 205-206); Sue Rosser (1989).

6. Rosser, in her adoring obituary of Bleier, twice mistakenly calls her book *Gender and Science* (1989, 250, 252).

7. Bleier continues, "And as a final message to women everywhere, Sociobiologists explain the 'naturalness' of rape."

8. Unless otherwise stated, all material from Barash's *Sociobiology and Behavior* is from the 1977 edition, the one cited by the writers I discuss. The second edition appeared in 1982.

9. See Jaggar and Struhl (1978, Part II); Jaggar and Rothenberg (1984, Part 2).

10. Lynn Hankinson Nelson thinks that Barash *justifies* human rape (1990, 158-60). But her presentation of Barash's sociobiology of rape--"Birds do it . . . bees do it . . . many men find it stimulating to think about. . . . Now, *how could rape be bad?*" (160; Nelson's ellipses and italics)--is unfair. Barash does not draw the italicized conclusion; that is Nelson's extrapolation. See also Anne Fausto-Sterling (1992), 156-58.

11. Lynda Birke and Ruth Hubbard (1995), 281.

12. Lowe (1978), at 121-22.

13. Lowe and Hubbard (1979), 99; Lowe (1978), 122.

14. In her 1995 (8), Hubbard calls herself "a compulsive editor of my own and other people's work." That's doubtful, given her treatment of Barash. Further, in the references to the three passages I quoted from Hubbard, she incorrectly calls his book *The Whispering Within* (1983, 8n3; 1988, 14; 1989, 130).

15. Barash makes the point elsewhere (1978, 25). He also makes it in the 2nd edition of *Sociobiology and Behavior* (1982, 160)--in which I found no assertions about "Mother Nature" being sexist. In that book, he shows that he is no biological determinist: "[S]ome correlation always exists between genes and behavior, even human behavior. It may be precise, as in the nervous system wiring that produces the blink reflex in response to sudden loud noises. Or it may be diffuse and therefore almost entirely dependent upon environmental influences, as in the case of personality" (146). (My guess, having raised two children, is that personality is largely genetic, while beliefs and behaviors resulting from them are environmental in origin.) The sociobiological views criticized by Hubbard also cannot be found in Richard Dawkins, the other writer she cites (without page numbers). To see that Dawkins does not advance a biologically deterministic theory of human sexual differences, see his 1976 (177, 203, 215) or 1989 (164, 189, 200-201).

16. Ann Garry and Marilyn Pearsall (1996), 485. Bordo has also been elected to serve on The Executive Committee of the Eastern Division of the American Philosophical Association (1998-2001).

17. *New York Times*, 7 December 1989, B21. Reproduced with permission of the Associated Press.
18. The article's author, on seeing what the Singletary Chair did with his or her detailed description, would have good reason to complain, using Bordo's words: "There's nothing like reading interpretation . . . of one's own work to confirm the mundane reality of the postmodern pronouncement that the author is dead. The work . . . is handled and given shape by other people's psyches and concerns, other political and intellectual agendas" (1997, 173).
19. See Lani Anne Remick (1993), 1123. Remick refers to a briefer account of the case in Mary Nemeth (1992), 44.
20. Harding (1998), 243; Lynn Hankinson Nelson and Jack Nelson (1997), 309-10; Garry and Pearsall (1996), 486; flyer distributed by Spring Hill College, announcing that Harding would deliver its 1993 Suarez Lecture; Harding, "Excerpted Curriculum Vitae" (November 1994).
21. This essay was first published in Linda Nicholson (1990), 83-106. The note appears at 102n7.
22. Mary Field Belenky, Blythe McVicker Clinchy, Nancy Rule Goldberger, and Jill Mattuck Tarule, *Women's Ways of Knowing* (Tenth Anniversary Edition, 1997), 66.
23. I say "barely" because Belenky et al. do not contrast what men and women mean, but contrast what women mean and a "masculine assertion."
24. 1987b, 10; 1989b, 273; 1991, 57; 1996a, 305.
25. Harding repeats this methodological criticism in her 1996b (447).
26. Nancy Rule Goldberger (1996), 4.
27. Belenky et al. (1997), 9; see Goldberger (1996), 3, 7.
28. In this note, Harding refers to Belenky twice as "Mary G."; she had it right ("Mary F.") in both printings of "Feminism, Science, and the Anti-Enlightenment Critiques." This is *not* an isolated slip. In her 1990, Harding twice refers to Belenky as "Mary L." (141, 148n23). "Mary G." also occurs in Harding's 1991 (87n19, 118n12), while Harding's 1998 is undecided, giving both "Mary G." (208n14, 239 [the index]) and "Mary F." (211n3, 227 [the bibliography]).
29. The authors' anthology (Goldberger et al., *Knowledge, Difference, and Power*) wasn't published until 1996, and the Tenth Anniversary Edition of *Women's Ways*, with (only) a new preface, not until 1997.
30. Gonzalo Munévar (1993) has pointed out other mistakes in Harding's work. See also my 1992 and 1995.
31. See, for example, item 3 (Belenky et al., 1997, 15) and Chapter 7, which is about "constructed knowledge" in women and is perfectly silent about a comparison, in this regard, between women and men. (The middle paragraph on 150 doesn't come close.)

Patrocínio Schweickart, in her contribution to *Knowledge, Difference, and Power* (edited by the four authors of *Women's Ways*), states, without providing page numbers, that *Women's Ways* contains "the empirical finding that women show significant preference for the latter [connected knowing] and significant distaste for the former [separate knowing]" (1996, 310). The editors were asleep at the wheel. Equally wrong is this claim about *Women's Ways*, also presented without a page number: "*separate knowing* . . . is the traditional approach, but it was not the one that most women in the sample preferred. Instead, they opted for the other subcategory, called *connected knowing*" (Daphne

Patai and Noretta Koertge [1994], 164). There is not a shred of a sentence in *Women's Ways* to support this contention. Indeed, the opposite: The "subjectivist" women studied by Belenky et al., who occupy the middle level of their five ways of knowing, are products of the *Zeitgeist*; they "view truth as unique to each individual" (1997, 69). Belenky et al. report that "almost half [of their 135 subjects] were predominantly subjectivist in their thinking" (55).

32. See "The Sexist Misuse and Abuse of Science and Technology," Harding (1991, 34); also Harding (1986), 21.

33. Biographical and bibliographical information available at <http://www.shef.ac.uk/uni/academic/N-Q/phil/department/staff/Langton.html>.

34. MacKinnon's "subordination" thesis begins her definition of pornography: "We define pornography as the graphic sexually explicit subordination of women through pictures or words" (1987, 176; see 262n1). For MacKinnon's "silencing" thesis, see her 1987 (156, 193-95); her 1989 (205, 247); and her 1993 (6-7, 40-41, 72-73).

35. If pornography induces men to ignore a woman's "no," then, it seems to me, it would be because in pornography a woman's "no" means "yes" or because in many scenarios a woman says "no," the man ignores it, and it turns out she didn't mean it. Although Langton implies (I think) that this is the content of pornography, she never claims so explicitly, and it's a good thing, because it's false. What is characteristic of pornography is that its women are eager for sex and active initiators of sexual encounters; pornography does not portray women as playing traditional "hard-to-get" games, but as accommodating to men's sexual desires. There is a huge difference, however, between pornography's depicting women as readily saying or indicating "yes" and its purported causal effect, a real-life man's ignoring a woman's "no."

36. Daniel Jacobson (1995) argues, "The strange and troubling consequence of the argument from illocutionary disablement . . . is that *Langton cannot call this rape*" (77).

37. MacKinnon seems to think that pornography is authoritative: it "is not at all divergent or unorthodox. It is the ruling ideology" (1989, 205). She also thinks that men's ignoring women's refusals of sex is partially due to pornography (182-83). In commenting on Langton, Susan Dwyer is confident (she supplies no evidence): "It is at least plausible that pornography represents, for many young men, the only detailed information about these matters. In the absence of alternative views, such people would have no reason to doubt the 'truth' of pornography's pronouncements" (1995, 201). I do not find this "plausible." Neither does Lawrence Trostle, according to whose study the sources of information about sex for undergraduate men included, in descending order of importance: peers, books, media, school, pornography, parents, and church (1993, 410).

38. I added the numerals [1] through [4].

39. The researchers do use "rape" in addressing their readers. But the stories about male-female interactions that subjects had to read and evaluate and the questions presented to their subjects did not use "rape." Warshaw seems not to understand the reason for this, or just omits it (1988, 120). The researchers were studying attitudes about responsibility for acquaintance rape; they did not want their subjects to get entangled in the semantics of whether to call these events "rape." See Jacqueline Goodchilds et al. (1988), 246-48, 252, 264-65. Indeed, one segment of the research tested whether subjects would call date rape "rape" (264-69). The authors interpreted their data as showing that "Adolescents appeared reluctant to label nonconsensual sex within a dating relationship as rape, even when physical force was involved" (267). Both Langton, in her footnote, and Wolf (1992, 315), misspell Jacqueline's name; it is "Goodchilds." Warshaw also gets it wrong (1988, 120), although it is spelled correctly in her bibliography (212).

40. See Goodchilds et al. (1988), 254-56.

41. Wolf gives this citation as the source of the statistic in [1]: "See study by Jacqueline Goodchild et al., cited in Warshaw, op. cit., p. 120" (1992, 315).
42. Langton repeats the mistaken "aroused" gloss in her 1995 (186n56).
43. Regarding the 15% figure of [2], Warshaw claims (1998, 14) that it was produced in a study "coauthored by Auburn's [Barry R.] Burkhart," someone Langton never mentions.
44. On Wolf's unreliability, see Christina Hoff Sommers (1994), 11-12, 28-29, 233.
45. In a footnote in her 1995 (186n56), Langton changed her account of the study (citing Wolf): "Nearly a third of male college students perceived faces expressing distress to be more attractive than faces showing pleasure." She replaced "pain and fear" with "distress"; "sexually attractive" has become "attractive" (why?); the mysterious "pleasure" remains. In a later essay prepared for an undergraduate teaching anthology, Langton reverts to a more Wolfian reading: male subjects "rank faces of women displaying pain and fear to be more sexually attractive than faces showing pleasure" (1997b, 343).
46. John Locke, *An Essay Concerning Human Understanding*, bk. 1, ch. 4, §23: "Men Must Think and Know for Themselves" (1963, 79).
47. Alfred Heilbrun, Jr. and Maura Loftus (1986); page numbers in the text refer to this article.
48. While defending MacKinnon's subordination thesis, Langton says that "pornography does more than arouse": some men "have their attitudes and behavior altered by it in ways that ultimately hurt women." One such effect of pornography, according to Langton, is that men exposed to it "are more likely to say that they . . . would rape if they could get away with it" (1993b, 306; the claim is repeated on 310). In a footnote (306n33), Langton provides as her source Edward Donnerstein et al., *The Question of Pornography* (1987). She does not give any page numbers. In her earlier 1990 (335n70)--there, too, citing Donnerstein et al.--Langton had said the same thing: pornography "can change attitudes. . . . Subjects who are exposed to it can become . . . more likely to say that they would . . . rape if they could get away with it." And in her later 1998b (269), she asserts the claim yet again, still citing, without page numbers, Donnerstein et al. (281n23). This is what I found in Donnerstein et al. (1987, 101):

One finding in this study has often been misinterpreted. . . . For example, a document titled *Pornography: Its Effects on the Family, Community, and Culture* . . . mentions that even after brief exposure to pornography, particularly violent pornography, one-third of the subjects reported an increased willingness to actually commit rape. . . . However, there was no direct effect on the "likelihood-to-rape" measure from exposure to either of the first two stories read by the subjects [a story of a woman being raped; a nonviolent but sexually explicit story]. In fact, in none of the Malamuth studies, including a study with repeated exposure to aggressive pornography over 4 weeks, has there been a change in this rape measure as a function of exposure to pornography.

Langton's source for her claim *denies* it. See Neil Malamuth and Joseph Ceniti (1986), who recorded subjects' self-reported likelihood of rape (SRLR) before and after a four-week exposure to various kinds of pornography and found no effect on SRLR (134). Langton might have been better off appealing to the studies of James Check and Ted Guloien (1989). Check and Guloien did not record men's SRLR before and after exposure to pornography, but compared the SRLR of groups of men exposed to various kinds of pornography. They found that SRLR was higher for the group exposed to violent pornography and the group exposed to dehumanizing pornography (1989, 170-72). But when the men were sorted into "high psychotic" and "low psychotic," the effect on SRLR of exposure to pornography was found *only* for high psychotics (1989, 172-73).

49. Feminists have done interesting work on some esoteric aspects of the concept of objectivity; see,

for example, Donna Haraway (1988) and Sandra Harding (1992). Perhaps we need to be paying more attention to the failure of objectivity in its more mundane senses.

50. See Sommers (1996); Carol Iannone (1989); Alan Dershowitz (1994, 33-37); Kenneth Lasson (1992); and, on Martha Nussbaum's white-out escapade, Daniel Mendelsohn (1996).

51. For example: "Feminist standpoint epistemology was developed primarily to account for the more comprehensive and empirically adequate knowledge about social worlds that could be generated by starting off research from feminist understandings of women's lives instead of from dominant conceptual frameworks" (1997, 186-87); "some social scientists and biologists have made claims that clearly have been produced through research guided by feminist concerns. Many of these claims appear more plausible (better supported, more reliable, less false, more likely to be confirmed by evidence, etc.) than the beliefs they would replace" (1989a, 190); "the new feminist inquiry in biology and the social sciences appears to have produced less false (more reliable, better supported by empirical data, etc.) results of research than results that had been produced through purportedly value-free inquiries [T]he less false picture of the world emerging from feminist inquiry clearly has been created through research guided by the political goals of the women's movement" (1987a, 76-77); "the more objective, less false, etc. results of feminist research clearly have been produced by research processes guided by the politics of the women's movement" (1987c, 26). See also her 1987b (10) and 1995 (341).

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