

The History of Sexual Anatomy and Self-Referential Philosophy of Science

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In this essay I provide a case study of the self-referential self-destruction that befalls a social constructionist historian of science who espouses a radical philosophy of science. I focus on one of Thomas Laqueur's eminent texts in the history of biology in arguing that a social constructionist approach to the history of science aligned with a Kuhnian-Duhemian-Quinean philosophy of science is incoherent. I make the point by probing in detail this one text. I then turn, more briefly, to the phenomenon as it occurs in the work of the well-known feminist historian and philosopher of science, Evelyn Fox Keller. The social constructionist of the history of science cannot have his or her historical cake and eat it philosophically as well.

1. *Francis and Frances*

How many sexes are there in *Homo sapiens*? The obvious answer to this question, which every child knows before kindergarten, is "two." The female has a vagina, ovaries, fatty breasts, and whatever else anatomically and biochemically accompanies them. (Her long hair and Barbie doll do not count.) The male has a penis, testes, and is, in general, larger. (The grime under his fingernails and toy guns do not count.) Of course, there are unclear or ambiguous cases: some humans are anatomically deficient or abundant, and so members of neither sex or of both.¹ Despite these congenital anatomical oddities, the biological categories "female" and "male" are fairly solid, unlike the more contentious categories "gender," "sexual identity," and "sexual orientation." And even though the intellectual and moral similarities of the human male and female are greater than the differences between them, and the transvestite and transsexual confuse us, we usually have no difficulty recognizing two easily distinguishable sexes, the ordinary guys and gals that inhabit our everyday environment. Nor do we have much trouble, with a little coaching, picking out male and female hamsters and turtles.²

Or so it seems, to the unreflective--not meant as derogatory--layperson. In *Making Sex: Body and Gender From the Greeks to Freud* (1990),³ a history of the science of biological sex, Thomas Laqueur tells a sophisticated tale about the social construction (the "making") of the high scientific theory of human biological sex. Laqueur claims that there are, and have been, two pictures of human sexual anatomy in biological science, *neither* of which was grounded on empirical evidence; both were "made" by nonscientific factors. There is a "one-sex" model

that originated with the ancients (Laqueur starts his history with the Greeks) and still survives, but only barely. In this model, there is only one sex, the paradigm case of which is the male. Opposed to this is a "two-sex" model, a picture of human sex that emerged or ascended in the eighteenth century and eventually became our contemporary distinction between the human female and male. According to the two-sex model, there are two biologically different sexes: "the dominant, though by no means universal, view since the eighteenth century has been that there are two stable, incommensurable, opposite sexes" (6).

In the form of the two-sex model, biological theory, it seems, has finally converged with pre-analytic common sense. Or should we say, instead, that what was promulgated and established (or "established") by the biological sciences determined, or trickled down into, ordinary thought? In this case, it is not merely the scientific theory of biological sex that might have been socially constructed; our ordinary concepts of human sexual dimorphism might also have been, in part, socially constructed. Thus the easy question to which we think we know the obvious answer, "how many sexes are there *really* among humans," is not a fair question, but a prank. ("Have you stopped eating liverwurst on crackers?") The question has, for Laqueur, no answer. Maybe this is what Laqueur has in mind with his ambiguous remark, "The nature of sex . . . is the result not of biology but of our needs in speaking about it" (115).

On the one-sex model, there is only one human sex, the male sex. Beginning in classical antiquity and continuing through the Renaissance, "there was . . . only one canonical body and that body was male" (63). Despite what we today would take to be clear observational evidence for the distinctiveness of the female sex, viz., a woman's unique ability to become pregnant and to give birth to and feed new humans--or perhaps due to those facts, which were unconsciously frightening to male thinkers--male Greek scholars saw the female sex merely as an inferior modification or version of the male sex. How are male and the female the same? In the one-sex model, the vagina was just a penis projecting inward, a penis turned inside out, and the uterus was (seen as) an internal scrotum.

For thousands of years it had been a commonplace that women had the same genitals as men except that, as Nemesius . . . put it, "theirs are inside the body and not outside it." Galen . . . developed the most powerful and resilient model of the structural . . . identity of the male and female reproductive organs [and] demonstrated at length that women were essentially men. [4]

In providing evidence for the existence of this early one-sex model, Laqueur carefully dissects representative biological texts (Aristotle, Galen, and others); he reproduces hoary illustrations in which the vagina was diagrammatically rendered to resemble the penis, despite what was presented to the naked eye (I say that with raised eyebrows); and he pounces on revealing linguistic practices. The Greek *kaulos* was used for both the penis and the vagina (33-34), and

[f]or two millennia the ovary . . . had not even a name of its own. Galen refers to it by the same word he uses for the male testes, *orcheis*. . . . Herophilus had called the ovaries *didymoi* (twins), another standard Greek word for testicles. [4-5]

The one-sex model seems to persist in everyday English, in which the word "male" and the similar word "female" are used for the sexes. But I wonder. How are we to understand the "fe" prefix? Does it indicate a mere *version* of the male (a "one-sex" interpretation), or does it indicate *difference* from the male (a "two-sex" interpretation)? The English "man" and "woman" present the same problem: does the "wo" designate a modification of basically the same stuff, or does it mark a distinct difference? There seems to be no determinate answer to such a question. Laqueur would appreciate this linguistic tangle, for he makes much the same point about biological sex itself: whether *Homo sapiens* is a one-sex or a two-sex species is empirically indeterminate (viii).

2. *The Social Construction of the Science of Sex*

Laqueur argues that these two models of human sex in fact were not based on or derived from the observations of biological science. The ancients and their Renaissance successors who advanced the one-sex model did so not for empirical reasons, and at times in the face of contradictory evidence. For example, "the discovery of the clitoris," which should have, by virtue of its biological similarity to the penis, confounded the one-sex model's equation of the penis and the vagina, found "easy absorption by the one-sex model" (66).⁴ Laqueur draws a general lesson, from this sort of resilience of the one-sex model, about the insulation of theory from fact:

Why did competent observers, self-consciously committed to new canons of accuracy and naturalistic illustration, continue to think of reproductive anatomy and physiology in a manner that is manifestly wrong and egregiously counterintuitive to the modern sensibility?⁵ In the first place, much of what is at stake is not empirically decidable. Whether the clitoris or the vagina is a female penis, or whether women have a penis at all, . . . are not questions that further research could, in principle, answer. The history of anatomy during the Renaissance suggests that the anatomical representation of male and female is dependent on the cultural politics of representation and illusion, not on evidence about organs, ducts, or blood vessels. [66]

Similarly, the rise of the two-sex model in the eighteenth-century did not coincide with, in fact preceded, and is therefore not explainable by, advances in the accuracy of anatomical dissection and illustration and in the experimental powers of biomedical science (8-9). Allegiance to the modern two-sex model came about well before sufficient empirical warrant was available (as heliocentrism ousted geocentrism before full or adequate empirical warrant for doing so existed).⁶ Instead, "Sometime in the eighteenth century, sex as we know it was *invented*" (149; italics added).⁷ "The context for the articulation of two incommensurable sexes was . . . neither a theory of knowledge nor advances in scientific knowledge. The context was political" (152). That is, the new theory of biological sex, just like the older theory of biological sex, was "made" by cultural factors:

the social, the political, the religious, the metaphysical, and the philosophical. Empirical evidence was called upon by biological theoreticians to prop up a model of sex, either the one-sex or the two-sex model, that they had already accepted for other reasons. The scientific owl flies at dusk:

[T]here has clearly been progress in understanding . . . reproductive anatomy and physiology. . . . Any history of science, however much it might emphasize the role of social, political, ideological, or aesthetic factors, must recognize these undeniable successes. . . . Far from denying any of this, I want to insist upon it. . . . I hold up the history of progress in reproductive physiology . . . to demonstrate that these did not cause a particular understanding of sexual difference, the shift to the two-sex model. . . . Anatomists might have seen bodies differently--they might . . . have regarded the vagina as other than a penis--but they did not do so[,] for essentially cultural reasons. Similarly, empirical data were ignored . . . because they did not fit into either a scientific or a metaphysical paradigm. [16]

The insulation of scientific theory from an observational foundation was so powerful, according to Laqueur, that potentially good evidence for the otherwise bizarre (to many of us, at least) one-sex model had no impact:

[A]dvances in developmental anatomy (germ-layer theory) pointed to the common origins of both sexes in a morphologically androgynous embryo. . . . [T]he Galenic isomorphisms of male and female organs were by the 1850s rearticulated at the embryological level as homologues. . . . There was thus scientific evidence in support of the old view should it have been culturally relevant. [10]

A stranger surveying the landscape of mid-nineteenth-century science might well suspect that incommensurable sexual difference was created despite, not because of, new discoveries. Careful studies of fetal development would give credence not to new differences but to old androgynies, grounded this time not in myth or metaphysics but in nature. [169]

The one-sex model died, then, despite this scientifically therapeutic shot in the arm, because cultural forces had already signed its death certificate. Laqueur does not think the one-sex model actually croaked, but has been lingering on in a state of terminal illness: "While the one flesh did not die . . . two fleshes, two new distinct and opposite sexes, would increasingly be read into the body" (148). Alice Dreger, who agrees "wholeheartedly with Laqueur's major point that sex--anatomy and physiology--gets 'constructed' just as gender does" (Dreger 1998, 209n61), nevertheless thinks that nineteenth-century embryology undermines Laqueur's contention that the two-sex model had "completely displac[ed]" the one-sex model (Dreger 1998, 34-35). So whose factual historical thesis is right--or "right," as a social constructionist might put it--Laqueur's claim, that the one-sex model took second place despite the new, good embryological evidence for it, or Dreger's claim, that as a result of these advances in embryology new life had been breathed into the one-sex model? Perhaps what we should say, in the style of Laqueur, is that there is no determinate answer to this historical question. Maybe both theses are primarily the result of cultural influences, and the empirical data, about which both parties in this dispute apparently agree, is largely irrelevant. We would then have to unpack the cultural influences and personal agendas that caused Laqueur and Dreger to interpret the same data differently. (Or to discover

different, and perhaps incommensurable, facts?) Harvard's natural historian Stephen Jay Gould relies on the same embryology to argue, about the one-sex and two-sex pictures, that "neither model is 'correct'," although "both capture elements of anatomical reality" (Gould 1991, 11; are the scare quotes on "correct" a gratuitous tip of the hat to social constructionism?). For Gould, the one-sex model is right insofar as from an embryological perspective the external genitalia, the scrotum and the labia majora, "are the same organ," and the penis and female clitoris are "the same structure." But the two-sex model is right about at least some internal genitalia: the female's Fallopian tubes and the male's *vas deferens* are distinct organs, since they are produced along different embryological pathways (Gould 1991, 11). I would like to hear what Laqueur and Dreger think about this reasonable and friendly, even if insipid, reconciliation.

3. *Sex, Gender, and Politics*

Making Sex, then, is a history of how the theory of biological sex was socially constructed *in* the sciences *by* nonscientific factors. But what was taking place extrascientifically that was able to replace or oust the empirical in generating allegiance to these two models? Here is part of what Laqueur tells us about the excess baggage of the one-sex model:

This "one flesh," the construction of a single-sexed body with its different versions, attributed to at least two genders, was framed in antiquity to valorize the extraordinary cultural assertion of patriarchy, of the father, in the face of the more sensorily evident claim of the mother. [20]

The one-sex model can be read . . . as an exercise in preserving the Father, he who stands not only for order but for the very existence of civilization itself. [58]

In a public world that was overwhelmingly male, the one-sex model displayed what was already massively evident in culture more generally: *man* is the measure of all things, and woman does not exist as an ontologically distinct category. [62]

And about the two-sex model's excess baggage, or extrascientific impetus or rationale, Laqueur says:

By around 1800, writers of all sorts were determined to base what they insisted were fundamental differences between the male and the female sexes, and thus between man and woman, on discoverable biological distinctions. . . . There arose a shrill call to articulate sharp corporeal distinctions. . . . [T]he political, economic, and cultural lives of men and women, their gender roles, are somehow based on these "facts." Biology--the stable, ahistorical, sexed body--is understood to be the epistemic foundation for prescriptive claims about the social order. [5-6]

In both cases, a model of sex was employed to raise or keep the male above the female or, more precisely, to raise or keep men above women. The same "cultural work that had in the one-flesh model been done by gender devolved now onto sex" through the two-sex model (151). The goal of both the one-sex and the two-sex models was to justify and maintain gender inequality: the gender hierarchy in

which men were observed and described to be more intelligent, more effective in controlling the world and creating social institutions, more anything else good, and thus as properly having a wide range of exclusive rights, powers, and immunities.

We must wonder, then, whether a feminist, or gender-neutral, or nonsexist anatomical science would have done a better job of sorting out biological sex, whether instead of the science of biological sex following cultural prejudices about the abilities and status of men and women it could have rested more firmly on empirical observations. Why could not more determined and honest scientists, not committed to any of the sexual politics of the excess baggage, produce the best account of biological sex? Laqueur thinks this suggestion is pie in the sky:

[T]here is and was considerable . . . misogynist bias in much biological research on women. . . But it does not follow that a more objective . . . or even more feminist science would produce a truer picture of sexual difference in any culturally meaningful sense. . . [A]t stake are not biological questions about the effects of organs or hormones but cultural, political questions regarding the nature of woman. [21-22]

Laqueur implicitly suggests that feminist science would not have done any better, because like everyone else, feminist scientists are so concerned with what is really "at stake," the social relations of men and women, that they, too, would approach the scientific issue with social and political goals in mind. What is there to justify Laqueur's pessimism? Below we will find its roots in Laqueur's radical philosophy of science, according to which theory is always divorced from its possible factual basis, which necessary leaves a vacuum into which politics and philosophy, and so forth, enter.

There is a kind of Gestalt-switch that occurs in moving between the two models, or a Wittgensteinian duck-rabbit phenomenon: we are able to see one sex or two sexes in the precisely the same physical bodies:

In the absence of an Archimedean point in the body that assures the stability and nature of sexual difference, one sex is, and has always been, in tension with two: stark polarities poised on the edge of chiaroscuro shadings. Specific social, political, and cultural circumstances . . . favor the dominance of one or the other view, but neither is ever silent, neither is ever at rest. [114]

What divides the one-sex model of human sex from the two-sex model is, however, not merely that the former posits one human sex while the latter posits two. The discrepancy between the models go deeper. In Laqueur's history, biological sex was not always conceived of as an ultimate, natural, material base that interacts with cultural factors to produce gender; the social sciences did not always see gendered men and women, with their cultural traits, as arising out of the cutting and pasting of male and female biology by social practices, norms, and expectations. Today we do largely see biological sex, the male and the female, as substrates, while gender is seen as a cultural superstructure that assumes many forms in all the world's different cultures. But for the Greeks, according to

Laqueur, it was gender--the masculine with its social prerogatives, the feminine with its low status--that was both primary and natural (8, 134). The Greek cultural values of gender thereby played an important role in the Greeks' positing and finding only one sexed body, that of the male, in their biological science. "Destiny is anatomy," as Laqueur nicely puts it. The ancient one-sex theorists did not explain the well-entrenched gender hierarchy in terms of a biological hierarchy, but explained their biological hierarchy in terms of gender inequality. They concluded, on the basis of the obvious gender hierarchy in which men were superior, that the male sex was the perfect form while the female sex was an inferior version.

By contrast, in contemporary social and biological science the sexed body is conceived of as a natural, unchanging or only slowly changing, entity that contributes, along with the cultural, to the explanation of gender and of observed gender differences. Anatomy has finally become, at least to a greater or lesser extent, destiny. Thus, between the one-sex model and the two-sex model there is, for Laqueur, a conceptual or world-view gap the size of the gap that exists, on a Kuhnian view, between Newtonian and Einsteinian physics. The two-sex model didn't simply deny the one-sex model; it turned it on its head. In the one-sex model, according to Laqueur,

sex, or the body, must be understood as the epiphenomenon, while gender, what we would take to be a cultural category, was primary or "real." Gender--man and woman--mattered a great deal. . . . To be a man or a woman was to hold a social rank, a place in society. . . . Sex before the seventeenth century . . . was still a sociological and not an ontological category. [8]

Sex became a fundamental ontological category only with the two-sex model. Gender was no longer the ultimate category, the *explanans*, but was relegated to the realm of the epiphenomenal *explanandum*. The existence of this conceptual difference between the two models, how they viewed the relationship between anatomical sex and cultural gender, and not simply the difference between the number of sexes posited by the models, explains why Laqueur asserts that

anatomy, more than physics, provides the paradigmatic case of Thomas Kuhn's argument that one cannot translate between theories across the chasm of revolution. [96]

This is strong, if not grandiose, language, and I am not convinced by Laqueur's arguments that it is warranted. The transition from the one-sex to the two-sex model might not have been all that revolutionary, in some technical, Kuhnian sense of that term. For it seems quite easy to translate between the one-sex and two-sex models of sexual anatomy, in a way in which one might not be able to translate as smoothly between Newtonian physics and its concept of mass and Einsteinian physics. At least, one could make the case that even with the emergence of the modern two-sex model, the biology of sex was and still is in that messy (or "chiaroscuro") pre-paradigm stage in which "neither [view] is ever silent, neither is ever at rest." Perhaps work on the two models continues side-by-

side, as if neither view were predominant (see Gould, above), even if this state of affairs is not explicitly acknowledged by their practitioners.

The question does arise, though, why the two-sex model arose at all, and how it was able to challenge successfully its predecessor. What was the social need for yet another model of sex, for another and different way of speaking about the human body? Part of the answer might be that in the modern era, in which consciousness and appreciation of gender variability had been raised by travel to foreign, exotic lands, gender could no longer be taken as a primary category. Hence something had to be deployed, even "invented," to explain and justify the gender hierarchy. Two-sex theorists were seeking a new foundation for gender differences and their social and political correlates: "Two sexes," says Laqueur, "were invented as a new foundation for gender" (150). Further, even though the two-sex model, if properly deployed, could support traditional gender relations, it could also be wielded to support other and quite different social and political agendas, and it might have become attractive and popular for that reason. Once gender had been unseated as an ontological category, replaced by the sexed body as the material base of humanity, gender and gender relations, as epiphenomena, became in principle socially malleable, disputable, open to modification (or engineering) away from traditional patterns (152). The ambiguous and vague implications of the two-sex model made it an alluring alternative at a point in Western history when equality, freedom, and democracy had become central social and political values. The two-sex model, that is, could be employed, and was employed, both to assert the traditional gender hierarchy and to deny or undermine it:

[T]he reduction of women to the organ [the ovary] that now, for the first time, marked an incommensurable difference between the sexes . . . did not itself logically entail any particular position on the social . . . place of women. [216]

A biology of cosmic hierarchy gave way to a biology of incommensurability, anchored in the body, in which the relationship of men to women was not given as one of equality or inequality but rather of difference. This required interpretation and became the weapon of cultural and political struggle. [207]

Political and social arguments about the true nature or characteristics of gendered humans, and about the proper relations between men and women and their appropriate roles, dealt with what was really "at stake." The various positions could appeal opportunistically to sexual biology as their needs demanded. "Sex is everywhere," in this dispute, "precisely because the authority of gender has collapsed" (156). We have similar disputes today, in the areas of epistemology and moral development. If women and men have "different ways of knowing," does this imply that male knowers are superior to female knowers, that male knowers are inferior, or that there is a value-neutral difference in their respective cognitive powers, perhaps with some congenial male-female (or man-woman) complementarity on the horizon that would satisfy most sides? If men and women approach questions of morality differently--men thinking in terms of rights and justice, women thinking in terms of care and connection--does this mean that men

have better insight into the demands of morality, or that women do, or that each has its own special contribution to make to a terribly complex area of human life?⁸ The amorphous and flexible two-sex model both generated and was generated by the debate between those who defended a politics of "different-and-unequal" gender characteristics and those who defended a politics of "different-but-equal."

4. *The Self-Referential Shot to the Foot*

Laqueur's history of the biology of sex is unquestionably enlightening. But Laqueur's forays into the philosophy of science are self-destructive. He wants to put a substantial philosophical spin on his historical studies:

The notion that scientific advances alone, pure anatomical discovery, could account for the . . . nineteenth-century interest in sexual dimorphism is not simply empirically wrong--it is philosophically misguided. [169]

On Laqueur's historical account, the empirical endeavors of biomedical science had little to do, *as a matter of fact*, with support for and belief in the one-sex and the two-sex models. Yet it is another thing for Laqueur to claim, in addition, that *in principle* the empirical could have had little to do with supporting these models or resolving the dispute between them. There is "no scientific way to choose between them," Laqueur boldly asserts (viii); the "general shift in the interpretation of the male and female bodies cannot have been due, even in principle, to scientific progress" (9). This thesis is enticing but paradoxical. If the transition from the one-sex to the two-sex model could *not even in principle* been the result of advances in the empirical data, wouldn't that make Laqueur's historical conclusion, that the empirical was *in fact* irrelevant in this area, uninteresting? If something is impossible, then of course it did not happen. If, as a matter of the logic of science, empirical data are impotent or irrelevant, then we would seem to know in advance, by this philosophy of science, what any historical study of the theory of anatomical sex would eventually tell us about that specific history, that the empirical was in fact irrelevant. Laqueur, through his philosophy of science, "knew," prior to his research, that the empirical must have had little to do with theorizing in the area of biological sex. All he did was to gather, *ex post facto*, the "evidence" for this thesis, evidence that *had* to exist (as, in *Columbo*, Peter Falk already knows, before his detecting, who did the murder).⁹ But Laqueur thinks that his detailed examination of the history of anatomy has revealed something new and significant about the facts of the history of the theory of biological sex. To the contrary, Laqueur might be merely engaged in the drudgery of working out the particulars, of conducting a mopping-up operation whose conclusions he knows before he carries out his investigations.

Why does Laqueur believe that empirical data are impotent or irrelevant in deciding between the two models of sex? The central fault that Laqueur identifies is not exactly what we might have thought it was. The fault, for Laqueur, is not that the two models are too vaguely expressed by their advocates or contain

statements that are meaningless or untestable. Laqueur occasionally *does* mention such things as the reason for the impotence of the empirical in this specific area:

Further evidence will neither refute nor affirm these patently absurd pronouncements because at stake are not biological questions about the effects of organs or hormones but cultural, political questions regarding the nature of woman. [22]

Accordingly, Laqueur often proffers a modestly narrow thesis, one that focuses on problems of meaningfulness and testability only in this one area of the science of anatomical sex:

[M]y point . . . is that new knowledge about sex did not in any way entail the claims about sexual difference made in its name. No discovery or group of discoveries dictated the rise of the two-sex model, for precisely the same reasons that the anatomical discoveries of the Renaissance did not unseat the one-sex model: the nature of sexual difference is not susceptible to empirical testing. It is logically independent of biological facts because already embedded in the language of science, at least when applied to any culturally resonant construal of sexual difference, is the language of gender. . . . [A]ll but the most circumscribed statements about sex are, from their inception, burdened with the cultural work done by these propositions. . . . Two incommensurable sexes were, and are, as much the products of culture as was, and is, the one-sex model. [153]

By contrast, there *are* questions Laqueur believes can be answered empirically, both in the one-sex and two-sex models (66) and outside them, in biology generally. For example,

Unlike questions of anatomy and sexual difference, the question of whether women can conceive without orgasm . . . can be definitely answered. So can the question of whether female orgasm closes off the womb. Empirical evidence can address even more complicated and problematic matters: whether women generally have orgasms during intercourse, or whether they have strong sexual--I mean here heterosexual--drives at all. [182]

But Laqueur's blessing certain discrete regions of biology as predominantly or fully empirical seems to contradict what is, for him, his *favored* reason for urging upon us why the empirical was impotent or irrelevant in the anatomical sex question. For his favored reason for the irrelevance of the empirical is more radical: the place where blame is to be put is the epistemology or logic of science itself. Laqueur appeals to what he calls "the Quine-Duhem thesis" to explain why the empirical cannot adjudicate among different pictures of biological sex. But that philosophical artillery, as even Laqueur himself duly recognizes, would have us believe that the empirical cannot adjudicate far more widely in the sciences, if at all. To return to his own example, the debate about women's orgasms: if gender politics have, even must have, played an appreciable role in debates over anatomical sex, as Laqueur claims, then gender politics must, on his own view, surely play an appreciable role in the various moments of the orgasm question. Consider this remark made by the primatologist Sarah Blaffer Hrdy, one of the players in the women's orgasm game:

Nobody really denies that orgasm occurs sporadically among women. . . . [I]t is clear that the capacity for orgasm is universal. . . . [V]irtually all women, sufficiently prepared and

stimulated, do have orgasms, but not necessarily from intercourse or from intercourse alone. [Hrdy 1981, 166]

What a superb piece of well-protected unfalsifiability; what a culturally significant thesis that has all the appearance of being empirical, yet surely has political motives lying not far from its assertion. What is "at stake" here, as it had been at stake in the dispute between the one-sex and two-sex models, is nothing less than the incendiary matter of both the possibility and validity of women's sexual pleasure. Hrdy's "sufficiently prepared" is a poke at insensitive men who do not bother much, or at all, with foreplay--"insensitive," at least, and perhaps only, from the perspective of the woman's side in the battle of the sexes. And Hrdy's "not . . . from intercourse" is a poke at men who do not have the patience or taste for cunnilingus--which, from the woman's perspective in this same perpetual battle, men had better have or learn to have (or else!). Laqueur misses a place in biological science where his own historical approach--search for and find the cultural forces at work that disrupt the influence of the empirical--would certainly bear fruit, and where the students of Duhem and Quine (and the older Feyerabend) would have a field day. It is astounding that Laqueur, wearing Kuhnian glasses, fails to see this.¹⁰

The point, then, is that even though at times Laqueur restricts his Kuhnian reflections and the Duhem-Quine thesis to just the one area of biology that he studied, his favored move is to embrace Duhem-Quine in its most comprehensive, full-fledged version, in which no scientific theorizing or activity escapes the problem of the impotence of the empirical:

Evidence bearing on the empirically testable claims of the one-sex model failed to dislodge them not because such data were silenced but because these claims were part of a far more general, intricate, and many-stranded conception of the body which no observations, singly or in combination, could directly falsify. Willard Quine suggests why this should be the case on philosophical grounds. The totality of our beliefs "is a man-made fabric which impinges on experience only along the edges." . . . The ancient account of bodies and pleasure was so deeply enmeshed in the skeins of Renaissance medical and physiological theory . . . and so bound up with a political and cultural order . . . that it escaped entirely any logically determining contact with the boundaries of experience. . . . This is by now so standard [!!] an argument in the history and philosophy of science that it even has a name: the Quine-Duhem thesis. [69]

Laqueur is quite right to say, in virtue of Duhem-Quine considerations, that the "collapse [of the one-sex model] will not need to be explained by a single dramatic discovery or even by major social upheavals" (70). But that, in a different form, repeats, or is an admission of, my earlier point: if Duhem-Quine is generally true, as explicated by Laqueur, then there was no need for him to do his *detailed* historical study. There was nothing sensationally interesting waiting to be discovered, by him and his hard work and the labor of his graduate assistants, about the imperviousness of the biological theory of sex to the observational. What he "found"--that the debates in the science of sexual anatomy were not (and are not) empirically resolvable--he *must* have found. Laqueur thus fell into the same Kuhnian trap that made the one-sex anatomists draw a vagina as if it were

an inward penis: he found what he expected to find. Indeed, if Laqueur had not found what he found, *he* would have been a Kuhnian *failure*. In undertaking his study, all that Laqueur could do was to succeed at avoiding failure; he could not succeed at anything more significant than that. Laqueur, to his credit, engages in "full disclosure" about the vulnerability of his work to the operation of Kuhnian mechanisms:

Aline Rouselle . . . argues that, in the absence of opportunities for male doctors to examine women dead or alive, the quite precise observations regarding female pleasure and physiology were given to the doctors by midwives or female patients. Though there is *no direct evidence* for this view, I would like it to be true since it suggests that much of what I will say in this book reflects not just a high, male, medical tradition but the imaginative worlds of women as well. [255n37, italics added].

To some extent, of course, Laqueur's appeal to Kuhnian-Quinean-Duhemian considerations is a fascinating approach to the scientific theory of biological sex. For example, the ancient and Renaissance one-sex anatomists who drew the vagina so that it resembled a penis were not reporting brute observations gleaned from careful dissection, but were leaving for posterity proof of their theory-laden observations. They drew from the complex body what they wanted to see or what they thought they were supposed to see:

The new anatomy displayed . . . with unprecedented vigor . . . the "fact" that the vagina really is a penis. [79]

The more Renaissance anatomists dissected, looked into, and visually represented the female body, the more powerfully and convincingly they saw it to be a version of the male's. [70]

Ideology, not accuracy of observation, determined how they were seen. [88]

It matters little if the genitals of the female elephant . . . are rendered [in biological illustrations] to look like a penis because the sex of elephants generally matters little to us; it is remarkable and shocking if the same trick is played on our species, as was routine in Renaissance illustrations. [18]

Even though in the passage right above Laqueur calls the representation of a vagina as a penis a "trick," as a good Kuhnian he condemns none of this chicanery, and he repeats, nearly endlessly, the Duhem-Quine inspiration that scientific theory is *always* underdetermined by the facts, that the empirical does not *entail* any one theory (viii, 19, 153):

Arguments against the vagina as penis . . . are to the modern imagination stranger even than the claim itself. At the simplest level, an apparent failure to find equivalences between men and women could be saved by the sort of wishful thinking that daily saves phenomena in normal science. Except in moments of revolutionary crisis, there is always a way out. [90]

There is always a way out--for Laqueur, this is an appropriate explication of his global "Quine-Duhem" thesis, and indicates where Laqueur goes wrong. It is in moving from a narrow thesis, that a careful empirical study of the history of the

theory of biological sex reveals that the parties in the debate did not take empirical evidence seriously, to trying to understand his results in terms of more global Duhem-Quine considerations that apply not only to his tiny selected area of science but all science. Laqueur can get away with saying, at the very end of his book, that "basically the content of talk about sexual difference is unfettered by fact, and is as free as mind's play" (243), as long as he means to restrict this "anything goes" thesis to the theory of biological sex or other discrete portions of science. Laqueur is not satisfied with the narrow thesis, however, and thinks that he has uncovered an illustration of what is globally true in science. This goes too far, and endangers Laqueur's own historical-scientific project. For if we take Duhem-Quine globally, we can rephrase Laqueur's closing claim to read, "basically the content of (even my own) historical talk about the theory of sexual difference is unfettered by fact, and is as free as mind's play." What Laqueur says about the theory of biological sex, if true, must also be true, and even more powerfully, about the history of the theory of biological sex. For history is a discipline in which we have no brute observations of the events recounted, but only interpretations of the traces of these events (for example, the texts), and hence nothing but theory-laden observations. On his own view, there could not be any historico-empirical way to adjudicate between his reading of this history and other readings (for example, Dreger's and Gould's). *There is always a way out*. In embracing a global thesis about the power of cultural influences on science and the relative irrelevance or impotence of the empirical, Laqueur's historical work gets hoist on his own petard.

Laqueur does have a way out--a defense that he, however, firmly rejects. Laqueur could construe his history of science as more like "subjective" literary studies or literature itself¹¹ than hard core science that seeks brute facts and their unifying theories. Indeed, this is exactly what Laqueur does to biological science itself, changing it from an empirical discipline to a kind of phantasmic literature. Everything is a novel or a "socially constituted dream" (Keller 1992, 94; see the next section), and I would have been well advised to publish *this* essay either in *Sewanee Review* or, god forbid, *Social Text*. But Laqueur obviously takes his historical studies to be more serious labor and more cognitively sound than the writing of poetry.

Laqueur, that is, despite his obvious acquaintance with the philosophy of science, exhibits no sensitivity to the terrible threat of self-reference faced by a *historian* of science (or of anything else) who embraces Kuhn-Quine-Duhem. In describing his own project, Laqueur uses a type of language that seems inappropriate for a writer who is impressed by the breadth and depth of the applicability of Duhem-Quine, who already thinks he knows the fact that the social, the political, and the metaphysical trump the empirical when much is "at stake," and who thinks that theories are socially constructed, invented or concocted, to meet various social and personal needs:

My claims are of two sorts. Most are negative: I make every effort to show that no historically given set of facts about "sex" entailed how sexual difference was in fact

understood and represented at the time [i.e., for both models], and I use *this evidence* to make the more general claim that no set of facts ever entails any particular account of difference. [19, italics added]

[N]o particular understanding of sexual difference historically follows from undisputed facts about bodies. I *discovered* early on that the erasure of female pleasure from medical accounts of conception took place roughly at the same time as the female body came to be understood no longer as a lesser version of the male's (a one-sex model) but as its incommensurable opposite (a two-sex model). . . . Moreover, chronology itself soon crumbled and I was faced with the startling *conclusion* that a two-sex and a one-sex model had always been available to those who thought about differences and that there was no scientific way to choose between them. [viii, italics added]

If Laqueur truly believes that he is showing something, that he is relying on evidence to support his thesis, and that he is arriving at genuine factual conclusions about the history of biology, he had better give up any thought that Duhem-Quine applies globally, and he had better restrict the scope of his own ideas about the social construction of theory, that when a lot is "at stake" the social and the political trump the empirical.¹²

5. *Laqueur's Disease*

"Laqueur's Disease," as I call it, is infectious, especially, it seems, among historians. The major symptoms of Laqueur's Disease are a hurried eagerness to criticize the *bona fides* of science, the espousal of fantastic and seductive reasons for doing so, and the failure (through self-deception? false consciousness? bad faith? indigestion?) to recognize that this critique of science applies as well to the historical studies carried out by the inflicted person. Its causes are peer pressure from admired and similarly inflicted colleagues, a delusionary sense that something important socially and politically is "at stake" in the doing of the philosophy of science, and a bit of softness in the cerebral cortex. The disease has no rational cure, although a boot to the butt may be tried, and the patient (as the rest of us do) eventually dies. As evidence for the existence of Laqueur's Disease, consider the case of a prominent scholar, the feminist philosopher and historian of science Evelyn Fox Keller, who is now housed in the infirmary of the Massachusetts Institute of Technology.¹³

Keller finds fault with science, basing her worries on the fact that science is social produced or "made": to the extent that a scientific theory is, as it must be, influenced by the society that produced it, to that same extent the theory will not, inevitably, faithfully mirror nature. Science, says Keller,

gives to us in representation . . . a cultural transformation of nature. . . . [T]he particular instruments, theories, and values that scientists employ in their attempt to represent nature are reflected in the picture of nature that emerges from their desks and laboratories. [Keller 1991, 228]

Further, according to Keller, "words are far too limited a resource . . . to permit a faithful representation of even our own experience, much less of the vast domain

of natural phenomena" (Keller 1992, 29). Thus one central obstacle to obtaining faithful representations of nature lies in a specific element of culture, its language.

Since "nature" is only accessible to us through representations, and since representations are . . . structured by language (and hence, by culture), no representation can ever "correspond" to reality. [Keller 1992, 5; Keller puts "nature" in scare quotes because she is not sure there is any such thing.]

The Laqueur who critiques the theory of anatomical sex because it is embedded in the straight-jacket of language would be happy to read this. But it is odd, even bizarre, that Keller thinks that the Nobel prize winner Barbara McClintock did such wonderful, laudable, even true work in genetics (Keller 1983), somehow bypassing or avoiding the undermining, nefarious influence of culture, language, and values.

These considerations--that knowledge is in part socially produced by extrascientific factors, and that language must infect our representations of the natural world--must also apply to Keller's own historical theses. We could say, in Keller's mode, about the historical pronouncements she issues in her books *Reflections on Gender and Science* (1985) and *Secrets of Life, Secrets of Death* (1992), that because the knowledge she avows to have of the historical relationship between gender and science is accessible only through representations that are structured by language (and, hence, by culture), no representation of the relationship between gender and science can ever "correspond" to the reality of that relationship. "[E]veryone likes cultural relativism but wants to exempt what concerns him. The physicist wants to save his atoms; the historian, his [or her] events" (Allan Bloom 1987, 203).

Like Laqueur, Keller accepts a faddish philosophy of science that she takes to be indisputable:

In the past twenty-five years . . . [a] revolution has taken place in the history and sociology of science, and, on a smaller scale, even in the philosophy of science. With the exception of natural scientists themselves, few people in the academy still believe in the inexorability, inevitability, or even purity of scientific truth. . . . Historians of science have *demonstrated* that the very ideal of pure science is itself a historical construction. [Keller 1992, 86; italics added]

Keller, although challenging the "purity" of science, speaks favorably about the ability of *history* to demonstrate various things about science, forgetting (or is this acid reflux?) that history of science, if it is to be a credible discipline, is itself a science. Let's get real: Keller's history of science is precisely where she should be most worried about the interfering impact of culture, language, and values. If we have finally buried the "purity" of scientific truth, then the purity of everything else, including her own historical scholarship, is gone as well. Keller seems not to notice or care that her critique of science generates this vicious self-reference, nor does she find it peculiar to talk about what history, of all things, *demonstrates*. Neither does Laqueur. He, too, believes that his historical studies of the theory of

biological sex have demonstrated something true, that his results are not merely something "made" or concocted by his participation in a society that has caused him to advance his beloved brain-children. Alice Dreger, a historian who applauds the demonstrative power of history, also has Laqueur's Disease: "I . . . had always envisioned vaginas as holes and not as complicated, responsive organs. This was the case even though I had for years been thinking about Thomas Laqueur's fascinating critique of the . . . phallic-like, winged vagina imagined by the Renaissance anatomist Vesalius. . . . Laqueur demonstrated, using that example, that our concepts of sex organs are socially constructed" (Dreger 1998, 256n33).¹⁴

In a footnote attached to her biographical statement that when she rejected "naive realism" in favor of "relativism," she temporarily lost interest in questions about the goals of science, Keller writes: "suggesting, perhaps, that neither the history, philosophy, nor sociology of science are any more immune to sociopolitical interest than are the natural sciences themselves" (Keller 1992, 87). That's astoundingly backwards. We already had excellent reason for thinking that history, sociology, psychology, and philosophy were incessantly in danger of being infected with culture, language, and values. We hoped that hard-core empirical science would turn out differently. There is something, after all, about history that leaves it more vulnerable than the natural sciences to the social processes that make its assertions mere reflections of culture: history is not experimental. We cannot put its propositions on a Baconian rack to eliminate culture, language, and values. Keller, formally trained in physics and biology, now calls herself a historian and philosopher of science. Has she really ascended, in her life's work and in her own perception of it, from the horribly culturally infected to the merely possibly culturally infected?

In one place, Keller does admit that "It may not be possible for feminists . . . to 'tell the truth' about science, any more than it is possible for scientists to 'tell the truth' about nature" (Keller 1989, 152). That's an agreeable point, consistent with her general critique of knowledge. (I am not going to probe whether her statement undermines *itself*). Keller ruins her concession, however, by continuing, "Nonetheless, it *is* possible for feminists . . . to take on the obligation of avoiding 'untruths' about science as best they can." On the one hand, it seems that avoiding untruths could be accomplished, on her own account, only by remaining silent (taking *Tractatus* 7.0 to heart) by eschewing the language that culture distorts. On the other hand, if Keller's history of science gets off the epistemological hook by her promising to avoid false statements, natural science has that same defense against everything she (or Laqueur) has said about and against it. Unwittingly, Keller has moved from her post-modern philosophy (or Feyerabendian anti-philosophy) of science right back to the traditionalist philosophy of science of Sir Karl Popper, a patriarch if there ever was one. What else does Popper's methodology admonish us to do, but painstakingly expose and thereby avoid that which is false?¹⁵

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NOTES

1. See Alice Domurat Dreger 1998; Anne Fausto-Sterling 1993; Gilbert Herdt 1994, 1999; and Margaret A. Walsh 2001.
2. Alice Dreger, having finished her scholarly study of the hermaphrodite, can no longer approach anatomical sex (or anything else, apparently) with her earlier childish innocence:

The way I see the world has changed drastically. Besides being utterly unable to answer my teacher's question--"How can you tell if you are a boy or a girl?"--with anything but historical stories, I now think about nose jobs, "gay genes," amniocentesis, Huntington's disease, life support, prenatal medicine, "Siamese" twins, prescription drugs, and my father's portable wheelchair in ways I never could before. I realize how complicated life can be if one has a body or a being that stands out from the rest. And do not we all? Surely in some way every one of us is like a hermaphrodite, a being or body that won't quite fit the boundaries. [Dreger 1998, 14]

I have never seen a better case for why we should *not* do academic theory.

3. All otherwise unidentified page numbers in the text or notes refer to this book.

4. Laqueur discusses, *en passant*, "the controversy around who discovered the clitoris" (98; see 65). This comic question--think about male scientists arguing over who should get credit for the discovery of an organ that women (and observant men) had known about since Eve--must be the ridiculous *reductio* of the social constructionist approach to the history of the science of biological sex. By contrast, Thomas Kuhn's question (in his 1962) "who discovered oxygen?" is a serious one, and has both philosophical and historical significance. This is because, unlike penises and clitorises, "oxygen" had at the time no ordinary use in any language. Or, penises and clitorises are not theoretical items; they are part of the immediate ordinary world in a way in which oxygen is not.

5. Be on your toes here: Laqueur's emphasis is on "manifestly . . . to the modern sensibility," not on "wrong." Illustrations "which make the vagina look like the penis, are not incorrect because they emphasize a relationship . . . that anatomists since the late seventeenth century have chosen to deemphasize; nor conversely are eighteenth-century illustrations . . . more correct because they do not emphasize this relationship" (164-65).

6. See Thomas Kuhn 1959.

7. One piece of historical evidence confounds Laqueur's chronology of the two models. Early in *Genesis* one can find both the one-sex and the two-sex models: Eve's being created by Yahweh from one of Adam's ribs seems to be a one-sex story (Gen 2:21-22), while Adam and Eve's being created separately at the same time by Yahweh seems to be a two-sex story (Gen 1:27-28).

8. For discussion of these issues, with special reference to a supposed difference between men's objectivity and women's objectivity, see my 1994 and 2002.

9. See Slavoj Zizek 1997, 106-107.

10. Laqueur (288n78) does refer, but only briefly, to the women's orgasms debate between Hrdy and the sociobiologist Donald Symons.

11. I thank an anonymous referee of *Metaphilosophy* for this nice suggestion.

12. See how Larry Laudan makes the point (1990, 157-59).

13. For more extensive discussion of Keller, see my 1994, 1995, and 2002. The last two can be accessed on my web site, <http://www.uno.edu/~asoble> (click on the "Essays" link).

14. Here is more evidence for Laqueur's Disease in Dreger: "Historians of sexuality have convincingly *documented* that homosexuals--and by consequence, heterosexuals, too--as we generally know them came into being only recently, specifically in late nineteenth-century Europe" (Dreger 1998, 127; italics added).

15. This essay is a vastly expanded and revised version of a short review of mine (1991). Some passages from my 1998 (15-18) have also been reworked and included.