ANNE FAUSTO-STERLING
Translated by Justin W. Gibson

On the Critiques of the Concept of Sex. An Interview with Anne Fausto-Sterling

The following interview was conducted by Priscille Touraille and first appeared in French in the autumn 2014 issue of *Genre, sexualité, et société* 12 under the title "Autour des critiques du concept de sexe. Entretien avec Anne Fausto-Sterling."

Priscille Touraille: The "sexual continuum" argument is one that you have defended in the name of biology to maintain that "nature really offers us more than two sexes" (Fausto-Sterling, *Sex/Gender* 52). The continuum seems to be, however, a fairly vague notion that lacks precision on exactly what it describes. Do you still agree that the "continuum" argument is the best criticism of the concept of sex and the best strategy for defending a world where people who identify as intersex are no longer victims of social inexistence?

Anne Fausto-Sterling: I don't think it is the *best* response; I think it is one possible response that works well under some circumstances and for some arguments. It should be deployed according to context. For example, if speaking about reproductive sex in the biological world more generally, sex is neither a continuum nor is it strictly dimorphic. Some insects, for

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example, have three or four sexes, but these are discrete, not continuous. Some vertebrates (e.g., several large groups of fish) change from male to female or vice versa during the course of their life cycles. It is often social context that induces the change, and only during the transition period is there a continuum. Once the transformation is complete, reproductively, physiologically and anatomically, there is again dimorphism. Our data on humans show that anatomically and physiologically, humans are almost dimorphic with regard to genitalia and chromosomes but that when one considers intersex conditions, there are infrequent intermediate states. Strictly speaking, these are still discrete, not continuous.

On the other hand, when one is talking about sex differences in behavior, skills, and secondary sex characteristics, the concept of a continuum is more apt. Breast size and voice timbre are good examples. Women and men both range enormously in terms of breast size, and there is certainly overlap between these traits. The same is true for high and low voice pitch. Furthermore, women and men train their own voices toward lower or higher pitches to achieve a range that seems socially acceptable. In English, we sometimes say that a person is using his or her "chest voice," or speaks in a "falsetto."

PT: You give the example of breast size and voice pitch. Voice—like body size, too (Touraille)—falls more under the biological continuum, and I agree with you (one cannot determine the sex of another solely on the basis of size or voice). Perhaps we only draw that conclusion, regarding voice, for example, because the tone of one's voice is, as you say, manipulated by gender norms of differentiation (Azul). On the other hand, traits like permanent breasts (even if this varies greatly in volume and form intrasexually) or facial hair, for example, have a more clearly bimodal distribution among our species (one can more easily determine the genitalia of a person on the basis of the presence of breasts or facial hair). The concept of continuum does not, then, apply in the same way to all secondary characteristics. This weakens the argument of the continuum. What do you think?

AFS: I am not sure I agree that breast size and hairiness have greater intrasexual variation than voice pitch or height. Do you have data to share that we could discuss more specifically?

I do not think that breasts are a fixed trait. Women and men both have breast tissue. Overweight men and women have larger breasts, and breasts become smaller with weight loss. Breast shape and size change with age and hormone levels, which are not consistent in men or women. As you

know, some women have very small breasts, and it would be difficult to use their breasts to distinguish between male and female.

But regardless, the point is that these features, which exhibit mean group differences between populations of similar geographical background, are clearly not dimorphic. For example, you have to compare Dutch men and women for height and Italian men and women for height, but not Dutch women to Italian men. They are sex-related averages for traits that are continuous in distribution.

PT: One problem with the continuum argument is knowing what it really applies to. Sometimes it seems to describe the fact that all of the features defined as "sexed" in a single individual are not congruous (one individual can develop both testicles and breasts, have smooth skin—without noticeable hair—and a deep voice . . .). Other times, it seems to describe that there is not any perfectly bimodal distribution for the same characteristic, including external genitalia (which means that, for a given characteristic, there would not be any intermediary between "male" and "female" states). Which of these two definitions do you think is the most relevant?

AFS: I don't think of a lack of congruence between levels of sex in a single body (for example, a body with ovaries but also a penis) as an example of a sexual continuum. Rather, this is an example of the potential independence of different components (or organizational levels) of sexual development within an individual body. The term *continuum* applies to the lack of bimodality. It implies a continual gradation (and it applies to a single trait or characteristic, as you suggest).

PT: Then where does the continuum stop and where does bimodality begin? Does bimodality begin where there is absolutely no overlap? Or when is there only a very weak percentage of overlap? Could you clarify this point?

AFS: That is a very Hegelian question relating to the opposition between quantitative and qualitative difference! And of course, there is no perfect answer. We have to set limits. It could be said that there is bimodality in sex (at the level of the individual organism) when there is absolutely no overlap in reproductive anatomy. In this respect, humans are obviously not sexually dimorphic, but this goes against common sense. It is believed that no overlap is present in around 98 percent of the population, whereas in 2 percent there are intermediary anatomical forms. So our reproductive structures are *nearly* dimorphic, but not completely. At what point do we start thinking that reproductive structures of a species make up a continuum going from

male to female? The right answer is a social decision given that a statistical or scientific way of deciding does not exist.

If the same question is asked about sex difference relating to behavior, size, hormone levels, or a number of secondary sex traits, we find enormous overlap. For these traits, it does not really make sense to think in terms of male and female bimodality. In fact, classifying these traits as male or female only adds to the confusion, since these divisive labels imply bimodality where the continuum gives a better description (Joel and Fausto-Sterling).

PT: Could you explain how your idea that "our bodies are too complex to provide clear-cut answers about sexual difference" (Fausto-Sterling, *Sex/Gender* 21) is consistent with the concept of continuum? Is it "our bodies" that are too complex, or is it that the concept of sex is too simple? By simple, I mean that sex is the only concept we have to describe such a complex reality.
AFS: You make an excellent point by asking whether the problem is not that biology is too complex but that our notion of sex is too simple to describe biological complexity. When I lecture in biology classes, I restrict my use of the word "sex" to a discussion of the "nuts and bolts" of reproduction. Even this doesn't really work if you move beyond eggs and sperm to, say, consider reproductive hormones, since hormone levels are themselves influenced by environment—day length, temperature, behaviors of various sorts. So at heart, the language of sex, gender, continuum, and dimorphism is imperfect, and we always have to spend efforts qualifying and limiting the use of our terms.

PT: Your work leads some gender studies researchers to imply that the concept of "two sexes" is problematic in biology. But if it is possible to speak of "three or four sexes," as you were saying earlier, among insects or fish, why, then, would it not be logical to speak of "two sexes" among mammals relating to gametes?[1]
AFS: Yes, actually, in mammals we can talk about two sexes in terms of gametes—except in certain instances of intersexuality or for individuals who do not produce gametes at all. As for intersex individuals, some of them have an ovotestis containing both spermatozoon and oocyte precursors, but no mature gametes. Very rarely, an individual can have both an ovary and a testicle, but as far as I know, their gametes do not mature and they are, therefore, not fertile. XO women (Turner Syndrome) have what are called dysgenetic ovaries, for which histological evidence of an ovarian cellular

structure exists, but not oocytes. So even in this case, you are not dealing with absolute dimorphism, but with a "good enough" dimorphism to satisfy people's burning desire for classification.

PT: If the conceptualization of sex is imperfect, is it imperfect as an ordinary conceptualization or as a scientific one? For you, must we get rid of the idea of two sexes or rethink it?

AFS: The big problem with the term *sex* in both common and scientific usage is that it has many meanings. These are usually not clearly defined in discussion or in print, which leads to people talking at cross purposes, applying conclusions based on the idea of sex in one sense to the idea of sex in some totally other sense. So it is not so much that I want to throw away the word as I want to use it with specific, carefully defined meanings. As a biological term, we should mostly use it to discuss reproduction at the cellular and DNA level. Even here there are many types of sex, but these can be corralled so as to have a sensible discussion. When, however, we begin to use the term as shorthand for a huge variety—ranging from the physiological to the psychological—of sex-related differences (not even, strictly speaking, clear dimorphisms), we run into serious difficulty. (This is not even to mention what happens when we use the word to mean lovemaking or sexual intercourse.) In these circumstances, I think the term is best dropped in favor of naming the specific phenomenon under discussion. So, for example, instead of asking if there is a sex difference in voting patterns, we can ask whether men and women, on average, vote differently, and we can add in other characteristics such as race, class, age, and so on, to such an analysis without being pushed immediately into a deterministic account of our findings.

PT: There is perhaps also a problem in grouping organs that do very different things in the body under the same concept. According to its common definition, "sex" organs include vaginas, clitorises, penises, and so on, but also gonads, ovaries, testicles, and so on. If "sex," as you say, should strictly refer to internal genitalia and to their development, then clitorises and penises should not be defined as "sex" since they serve other purposes, such as the sensations produced by their stimulation. Do you think that such a reconceptualization is relevant? Let's even consider things in the opposite way: if clitorises and penises are defined as sex, selected for the sensations they produce, should gonads still be defined as sex since they relate strictly to reproduction?

AFS: Well, all of these (gonads, vaginas, penises, and so on) are involved with reproduction when it is done the old-fashioned way (as opposed to gametes meeting up in a petri dish). It is certainly plausible to claim that the pleasure involved with sex evolved in part to encourage reproduction. There are lots of debates among evolutionary biologists about how and why humans eventually acquired a suite of behaviors in which copulation became separated from reproduction.

Our contemporary abilities to use technology to prevent pregnancy but still engage in sexual activities accentuate the idea that clitorises and penises are primarily for pleasure, not for sex.[2] At any rate, it is important not to leave out scholarship on the evolution of mammalian sex and reproduction, that is, the historical (and prehistorical) accounts of the body when thinking about these matters.

PT: You wrote an article titled "La fin programmée du dimorphisme sexuel" ["The Programmed End of Sexual Dimporphism"] for the French journal *La Recherche* a few years ago. The topic of this article was clearly genitalia. I point this out because I think many people have a hard time understanding the "two sexes" critique. You just said, however, that human genitalia are discrete traits. For me, that means that the continuum argument, which works for the majority of secondary sexual traits very well, does not work for talking about genitalia.

AFS: First, the *La Recherche* article, if I remember well, was written not by me, but in conversation with me. I may not have edited it carefully enough to be consistent with the current conversation. That happens. However, I do think that secondary sex traits are more clearly described as a continuum than are genitalia. Again, this is a question of population frequency. Intersex individuals sometimes have genitalia that are in between the vast majority of males or females. And they thus break open the idea of absolute dimorphism that I elsewhere describe as a Platonic ideal form. However, in most populations, intersex individuals with intermediately shaped genitalia are fairly uncommon (about 0.1 percent is a decent "guesstimate"). So a bimodal distribution of genitalia still exists even though it is not absolute. And, of course, this form of bimodality does not demand that nonconforming individuals be "fixed" by surgery.

PT: A nearly bimodal distribution of genital forms does not allow for, then, according to you, speaking of "two sexes" in the case of genitalia?

AFS: Not absolutely, as explained in the previous answer.

PT: How can such a weak continuum really help to destabilize the general conception that there are two structural forms of genitalia and of sex?
AFS: Let's talk about the five sexes, the somewhat tongue-in-cheek essay I wrote about intersex (Fausto-Sterling, "Five Sexes"). I wrote this to introduce the idea that our physical and physiological sex involves much more than genitalia and that we notice this in intersex individuals because the different levels (chromosomal, genital, hormonal, etc.) of sex are incongruent. I was not talking there about secondary sexual characteristics, but I was including much more than just genitalia. I also wrote that essay to expose standard medical (mal)practice that aimed to hide developmental diversity even if it meant multiple harmful surgeries. I don't think I ever introduced the idea of a continuum, as the variability I discussed was still discontinuous; but if you take intersex development seriously, there are more than the two categories of male and female.

PT: Do you really think that eradicating the idea of "two sexes" for secondary sexual characteristics and external genitalia will help to show that gender is a binary social construction? People continue to see two forms of genitalia among individuals (and also two "roles" in reproduction). Along the same line, race is no longer a scientific concept, but people continue to define skin color differences in terms of "racial" difference.
AFS: No, I don't. I don't think I ever made this argument. I think that complicating bodily sex (genitals, reproductive organs, hormones) is disruptive. People do *assume* this, but they don't *see* it. They see the overlapping differences that comprise secondary characteristics. And in children, where secondary characteristics are less obvious, we take a lot of care to mark even infants so that adults can make assumptions about every child's genitalia. Nevertheless, there is a strong, commonsense account of sex as either male or female, and what people know about genitalia is part of that. Countering what is both an easy and reasonably accurate "go to" position in nonbiologists and people who are not experts on sex and gender requires a multitiered and long-range process, which will take time.

As for "two roles" in reproduction, this sounds so old-fashioned to me in these days of reproductive technology, same-sex parents, and surrogacy (there are at least two mothers in a surrogate pregnancy). On the one hand, I can't see how people will continue to hold on to a strictly dualistic, bimodal account of sex in the face of these many changes. On the other, and as I have indicated already, I find that the idea of a continuum offers a kind of mushy alternative.

I think it is best approached via specific issues: same-sex marriage, reproductive technology, transgender rights and politics, the rights of intersex individuals to keep the bodies they were born with, and so on. We should also not lose sight of the fact that even though male and female genitalia mostly do not overlap in structure, there is considerable variability within their forms. For example, the current fashion to use plastic surgery to perform labioplasty to reduce the size of the vaginal lips is nothing more than a profitable (to the doctors) way to insist that there are normal vaginas and that abnormal ones need to be fixed. Note that the women on whom these labioplasties are performed are not intersex. They merely have large vaginal lips, when the cultural fashion is to have prepubescent genitals (hairless, permitting fantasies of having sex with young girls). Nurka and Jones clearly show how this fashion emerges from racist colonial projects.[3]

PT: Did the idea of the continuum as it is defended in biological anthropology for undoing race initially inspire your critique of sex?
AFS: I have been engaged for decades with the analysis of both race and gender, and honestly, I cannot remember whether one came first. So I think it is safe to say that for me, the critiques are in constant conversation with one another and probably have been from the start.

PT: I don't believe you mention the parallel anywhere in your writing. Is that right? But has your critique as a biologist on "sex" been influenced by the critique that the community of biologists made regarding the notion of race, and did you intend to say that the notion of "sex" is—like the notion of "race"—a misconception in biology?
AFS: There is not a *direct* parallel between the critiques of the race and sex concepts in biology. To start with, race involves average phenotypic differences *between* populations. It is neither near-dimorphic nor discrete in the sense that human reproductive and genital sex are. Sex, on the other hand, concerns phenotypic differences *within* a population. The concept of a continuum better describes race in the world than sex in the world. However, there *are* analogies between critiques of race and sex when considering the social construction or structuring of race and sex. In both cases, the social context feeds back on the body's physiology. Thus, the social becomes biological. The critical intellectual task is to understand the relationships between social context and the body. And in both cases, the social context permeates the production of relevant scientific knowledge. My emphasis on understanding embodiment as a complex system in which bodies are

socially produced but are nonetheless material, biological entities applies equally to both race and sex.

PT: Has feminist criticism in the social sciences influenced your thoughts and in what way?
AFS: Think historically here. Rayna Reiter's *Toward an Anthropology of Women* appeared in 1975 and Rosaldo, Lamphere, and Bamberger's *Women, Culture, and Society* in 1974. These were the pioneering feminist critiques of the social sciences and profoundly shaped my thinking about the world. All feminist scholars developing a gender critique in this period (remember, I got my PhD in 1970) read these two books. Feminist critiques of science began appearing in the early 1980s. Of course, we built on what had gone before. We struggled with the dualism of nature/nurture and gender/sex (also framed by another anthropologist, Gayle Rubin, in "The Traffic in Women"). We also struggled with the question of objectivity in science, as did some of the social scientists who preceded us. If science and social science were objective, then how could they be sexist or racist? To answer that question, we built a picture of science as a cultural product rather than as an acultural form of knowledge. Here, another anthropologist's work, Sharon Traweek's *Beamtimes and Lifetimes*, was a crucial influence.

PT: Where did you come across, and how did you decide to use, the concept of "gender" when you wrote *Myths of Gender*?
AFS: Anke Ehrhardt and John Money applied the idea to individual psychology in their book *Man and Woman, Boy and Girl*, and I devoured that book when it first appeared. But its application in a political sphere came from Gayle Rubin, and [gender] was rapidly popularized in feminist political discourse as a tool to intervene in arguments about biology (like the idea that a woman can't be president because she might do something dangerous in a menopausal rage). I found it very useful in those early days when we were arguing against the idea that women were biologically incapable of participating in the public sphere.

PT: In *Myths of Gender*, you do not yet talk about the continuum. Was Thomas Laqueur's work, *Making Sex*, a source of inspiration for your own critical reflection and particularly in relation to the idea of the continuum that Laqueur invokes for the "one-sex model"?
AFS: I have never thought about this in terms of the timeline of my own work. I studied both Laqueur's book and the Gallagher and Laqueur article

and taught them in my women's studies and biology courses as soon as they became available. Since they postdated *Myths of Gender*, they were not relevant for that book. But his work, and that of other scholars (like Joan Cadden's *Meanings of Sex Differences in the Middle Ages*) that critiqued him and offered greater nuance to the history of sex certainly influenced me. In *Sexing the Body*, for example, I discuss premodern ideas that place sex on a quantitative continuum, more akin to height. So you are right that the ongoing scholarship on the history of difference influenced my thinking about contemporary conceptualizations of difference.

PT: In *Gender Trouble*, Judith Butler (in a passage where she gives a commentary on Monique Wittig) presents a critical argument on the notion of "sex" that resembles yours but is somewhat opposed to yours in her conceptual argumentation: "[S]ex imposes an artificial unity on an otherwise *discontinuous* set of attributes" (27, my emphasis). From a biostatistical point of view, you maintain that most of the biological traits conceptualized as "sexual" are *continuous* because it is impossible to render the male/female categorization operational (in a biological framework of thinking). Do you think that Butler's argument is incorrect? Is it her use of the mathematical concept of discontinuity that is wrong? If not, how do you reconcile Butler's critique of sex with your own?
AFS: I think Butler and I intend the same thing, but that the problem, discussed in a previous section, is ambiguity in the term *sex*. We could each be more specific in our usage and that would clarify matters (I think).

PT: Have you tried to publish your critique of sex in specialized journals of biological theory? Have you had the occasion to present this critique at biology conferences, and how have your biologist colleagues reacted to it?
AFS: Generally, biologists, especially developmental biologists, are aware of intersex and the independence of types of sex (chromosomal, hormonal, and so on). I don't think this aspect of my work is that controversial for biologists. I do think systems theory and the rejection of a nature/nurture analytic context is more difficult for biologists in some fields to grasp. I did, however, coauthor one article in the *American Journal of Human Biology* that calculated intersex frequencies in order to critique the Platonic ideal of the perfect male and the perfect female (Blackless et al.). That paper is cited a lot and has also been subject to critique on narrow methodological grounds.

PT: Does that mean that the way in which biologists think about sex is not, at its core, so biased by gender representation?
AFS: I think most biologists would have no idea what you mean by gender representation. They study the biology of sex from inside science, and so it appears to them that there are facts that describe reality, at least to the degree they can discern reality via observation and experimentation.

PT: Would you say today, like Butler in the first chapter of *Gender Trouble* ("Subjects of Sex/Gender/Desire"), that: "If the immutable character of sex is contested, perhaps this construct called 'sex' is as culturally constructed as gender; indeed, perhaps it was always already gender, with the consequence that the distinction between sex and gender turns out to be no distinction at all"? (69). In other words, sex is as constructed as gender, or, as the authors of the manual of gender studies in France maintain, "[S]ex appears as the product of gender" (Bereni et al. 54), which leads to the conclusion that "sex is no longer understood as a reality of nature" (30). What do you think of calling into question the idea of sex as a reality of nature?
AFS: I agree with Butler, and I think several of the chapters in *Sexing the Body*—especially those on the discovery of the so-called sex hormones—offer specific examples of what it might mean to say that sex "was always already gender." On the other hand, I balk at the phrase that sex is no longer understood as a reality of nature. I balk because it seems to suggest that our discussion of sex is in no way constrained or shaped by the materiality of the body. But material embodiment needs to be part of the theoretical discussion.

PT: If I am following you, you disagree with the ultraconstructivist posture that implies that scientific knowledge is a knowledge like any other: always constrained by a way of thinking and never by a reality separate from thought. As a scientist, do you think that there exists a reality independent of thought for which we can account (ideally in a way that accounts for this independence)?
AFS: I reject what you call "ultraconstructivism." It is important to distinguish between "reality" and "scientific knowledge production." I believe that there is a real, material world independent of human thought. Just for example, I believe in evolution and therefore that a world of plants and animals and rocks and ice and volcanoes all existed during geological eras prior to human existence. I see no contradiction between what I have just written and the idea that producing scientific knowledge involves framing

and interpreting empirical discovery. Furthermore, it is inevitable that our social context and beliefs form part of that interpretive and experimental framework. Nor do I think that all scientific knowledge is relative. Rather I adhere to the idea formulated so well and over many decades by Sandra Harding, Donna Haraway, and others, that scientific knowledge is always partial. Not all knowledge claims are equal or equally dependable.

PT: The posture I was speaking of goes beyond the idea of a relative trustworthiness of data. The criticism that comes out of it—no longer understanding sex as a "reality of nature," for example—suggests that reality itself described within the concept of sex is "invented" by biology.

AFS: Sex is obviously natural in the sense that it is material and circumscribed in various ways by the biology of what bodies do and do not do, how bodies do and do not develop.

I bristle a bit at this description because of the word "invent," which suggests a kind of disregard of experiment, data, and actual bodies that biologists would certainly reject. I think biological researchers who study sex (*sensu* reproduction) try very hard to understand and empirically test what they see. And they afford the organisms they study an essential materiality. I am very sympathetic to this. Social institutions such as gender, however, certainly provide the framework within which biologists do their research, so gender ends up shaping experiments, methods, descriptive language, and conclusions. The two are intertwined. Neither precedes the other.

PT: For you, *sex* and *gender* are two concepts that must be differentiated?

AFS: My answer to this has to be contextual. At a theoretical level, I believe that sex and gender are two sides of the same coin. As such they constitute one another in a manner that makes them inextricable. At a practical level, however, I often distinguish between them, either for political reasons—to break open a biological determinist argument or point of view—or for empirical investigation. Sometimes it can help to bracket the interconnections in order to more effectively examine some small part of one side of the coin. This approach can work if one remembers the need to reintegrate partial findings into the whole coin.

To briefly recap: I don't think we can define the concept of sex itself independently of gender because they are mutually constitutive. This does not take away from the importance of studying the body and sex/gender using the tools of biology. Biological and sociological approaches may be in tension with one another, but they are not inherently contradictory.

PT: Don't you think the "inextricability" that you are talking about is an effect of gender itself? Shouldn't the scientific objective (be it biological or sociological) be to show how the mechanisms of connection function? In order to show this, isn't there absolutely a need for these two concepts?
AFS: Yes, I think you are right about this.

PT: In your latest work, you support a "developmentalist" perspective, defending that nature and nurture are "inseparable." But isn't it important to distinguish the constraints imposed by the genome (genetic information) from those imposed by the environment, for example, by a system of thinking (gender)? Isn't that precisely the whole issue of the gender debate? Is saying that it is a wrong issue going to eliminate the issue?
AFS: I need to quarrel with how you have formulated the question. By talking about genetic and environmental constraints you suggest that these can exist independently of one another. But my point is that these are always interdependent. A gene may have one phenotypic effect under one environmental condition but a very different effect under another. And genes have no effect at all without an environment. DNA in a test tube is useless. It has no active meaning. Only inside cells can genes express themselves; that is, genes require environments to have phenotypic meaning. Similarly, environmental effects work through activating specific genes. Smoking, for example, is an environmental input. But cancer caused by smoking occurs when the externally inhaled carcinogens alter gene expression.

PT: I certainly agree with you, and the argument that you put forward here is absolutely fundamental in understanding how general thought perceives the opposition gene/environment. Nothing can be said about gene expression separately from the environment in which an organism develops; your position as a biologist is clear. And that allows one to get rid of a number of non-issues. But do you also agree that a fly's genome will never produce a mouse independently of its environment?
AFS: This is a little beside the point, but the biologist in me can't let this go. One of the astounding things that contemporary biologists have found is the degree to which genes (specific sequences of DNA) have the same functions in vastly different organisms. So, for example, if you take a mouse gene that is involved with the control of eye development and incorporate it into a developing fruit fly, it will induce the development of a fruit fly eye! The same genes are found in a wide variety of organisms and serve analogous functions. But whether they make a fly eye or a mouse eye, in

this example, depends on whether their environment is in a mouse egg or a fly egg.

PT: Okay, but there is certainly a sort of limit to the plasticity of genetic information, isn't there (a cell from a mouse or a fly is also a product of DNA)? Don't the proponents of a "biological basis" of the order of gender base their arguments precisely on the lack of clarity in biology on how to draw plasticity's limits?
AFS: You may be right here, that this is a critical area of ambiguity. It is why I am arguing with biologists and developmental psychologists that we use dynamic systems to examine the developmental process rather than think about fixity and the limits of plasticity.

PT: Isn't it for this reason that a distinction between the genetic and the social should be made by emphasizing the plasticity of the body and behavior? I think the important argument that you put forward with the developmental approach is that it should force biologists to systematically consider the reality of the social environment that produces physical and mental differences between human beings (here, gender). That would require, necessarily, an interdisciplinary approach. But the theoretical question of what a given environment can create from given genetic information remains the question . . .
AFS: Regarding the expression "social environment," I want to emphasize that I include the physical and sensory environment as well as the social. Further, the term *social* requires disassembly so that we can clarify when we are speaking of one-to-one personal interactions, when we speak of "mid-level" phenomena, for example, advertising, media images, and marketing, and when we speak of the structural, for example, laws requiring registration of sex at birth, on driver's licenses, and passports, laws governing inheritance, prohibiting workplace discrimination, and so on—in other words, the apparatus of the state with regard to sex/gender.

PT: I would really like you to explain why the theory of systems of development tries to exclude any distinction between these two mechanisms of information: DNA and information/production of the reality created by the brain. How is it that proving a gene has no effect whatsoever without an environment should necessarily lead us to affirm that the distinction gene/environment or sex/gender, or nature/nurture, as you mentioned earlier, has no heuristic worth?

AFS: The word *heuristic* means a shortcut that allows people to solve a problem more quickly or efficiently. My difficulty with using the gene/environment or sex/gender splits is that they lead us to draw incorrect conclusions, and each time we use these divides to resolve or investigate a problem, we run the risk of reinstating them as the correct analytical tools. Thirty years ago, as I was writing *Myths of Gender*, I was amazed to see the same claims about gender and racial inferiority reappear that had been made one and two centuries prior, and not by just anyone, but by the most important scientific figures of the time. In any particular era there were feminists and antiracists who refuted the claims, and the claims would die back for a while; but then they would reemerge in more modern form. I began to wonder why certain ideas seemed so impossible to kill. I expressed frustration with this problem again in the introduction to the 1992 edition of *Myths of Gender*. And at least one of the answers is that we did not have the right framework with which to analyze difference using the tools of experimental science. Setting up the problem as measuring the relative contributions of nature and nurture to the final outcome makes it always possible to conclude that one or another difference is inherent (genes, chromosomes, ovaries, and so on) and that therefore one or another inequality is natural and unchangeable. To break out of this bind, we need new language and new theories of development.

PT: In your book, *Sex/Gender: Biology in a Social World*, you write: "[J]ust because rats do gender one way, doesn't mean that prairie voles or Japanese macaques or humans do it the same way" (xiii). Are you using the word *gender* here as a synonym for sex? The phrase makes me wonder what is, after all, your definition of "gender"? Is it a concept applicable to nonhuman societies?

AFS: It seems you expect me to be totally and infallibly consistent whenever I use these terms! And, alas, I am not. For one thing, the title of the book emphasizes the slippage between sex and gender. That holds even for animals. The division of labor and patterns of behavior one finds in animals that are sex-related are not all biologically hardwired. Animals exhibit behavioral plasticity in different social and environmental contexts, and I give some examples of this in the book in question. So yes, I suppose gender is a concept sometimes applicable to nonhuman societies. But on the whole, it is probably better to use the phrase *sex-related* rather than *gender*, in part because animals don't have the kinds of social institutions (the state and legal apparatus) that structure and enforce gender for humans.

PT: What do you see as the most urgent research needs in the future of gender studies? Which research agendas are to be the most important in biology and in sociology?

AFS: This is hard to answer because there are different programs of research depending on the type of question one is trying to answer. For sociology, I suppose the important foci should be around inequality and violence. But which inequality (education, wealth, legal rights, and so on), what form of violence, and where it is found would have to be specified in order to develop a specific research question. In biology, there are loads of questions about animal reproduction, development, and behavior that fascinate me. But in terms of humans, I suppose the most important questions are medical. What accounts for different disease rates between men and women? What are some of the under-attended-to medical problems specific to either women or to men? Here, what is important in setting the research program is to use methods compatible with dynamic systems approaches. This means conducting a time series (longitudinal sampling) and collecting data on overlapping and multilevel systems suspected of contributing to the problem under study. Trying to understand how the social affects the biological to produce disease remains essential to my approach.

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Notes

1 See Kraus, on whose work I base this comment.

2 For biologists' debates on the evolution of the clitoris, see Lloyd.

3 See also Fausto-Sterling, "Gender."

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