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Reply to Miller and Costello; Kennelly, Merz, and Lorber; and Risman

FEMINIST CRITICS UNCOVER DETERMINISM, POSITIVISM, AND ANTIQUATED THEORY

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The comments of my colleagues are eye-openers for me, for which I am grateful to them. I consider it an honor to have inspired three such heated attacks on my article from such distinguished critics. First, let me summarize what I was trying to accomplish in my original article (Udry 2000).

It is widely noted among those who study higher mammalian species that males and females of each species have characteristic differences in behavior. These differences are influenced by a common biological process, but at least in primates they are also conditioned by environmental circumstances at crucial periods in development. I wanted to test on humans the implications that follow from these (mostly primate) models. Given the complexity of the human species, and the fact that research design limitations require a focus on what can be measured in a single study of a single sample, only a few elements of both the environment and the biology can be incorporated. The biological aspects studied were therefore limited to the theoretically relevant hormones to which I had access on a group of respondents: prenatal maternal androgens and sex hormone binding globulin (SHBG, their binding protein), and adult measures of the same substances in the daughters.

The environmental measures focused on the process of acquiring sex-typical behavior from parental socialization. I don't believe that parental socialization is the sole source of acquisition of sex-typical behavior, but it is a source that is generally believed to be active on everyone.

For the dependent variable, gendered (sex-typicality of) behavior, I wanted measures of behavior (and attitudes and personality) that generally distinguish male and female humans in the available population (living in the same culture at a particular time). To be comprehensive in my measure, I used the largest composite of measures ever assembled that constitute sex differences.

It is something of a miracle that I was able to find an existing sample of women on

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whom to build such a design. It was even more of a miracle that I was able to confirm a set of hypotheses as specific as mine (how specific hormones at specific developmental stages, specific hormone interactions, and specific socialization processes affect sex-typical or gendered behavior in a specific way). What I showed was that for my sample of women, the biological model worked as the theory specified. Both prenatal and current adult hormones affected gendered behavior, and there was a significant interaction between the prenatal and adult hormones. Further, parental socialization had the effects one would predict on adult gendered behavior. But the effect of the parental socialization on later gendered behavior was conditioned by the level of prenatal hormone exposure. No one else has done an empirical test of the full model.

No part of my paper pleased the critics. They call me a biological determinist. My critics found fault with my using an animal model as the basis of my predictions. They found fault with the concept of the dependent variable, gendered behavior, and with how I measured it. They found fault with the hormone theory, and fault with the measure of parental gender socialization. They also found fault with the *ASR* for publishing the manuscript at all. Above all, they were unhappy with the political and policy implications they and I drew from the research. In the space that the journal has allocated to me, I will try to come to grips with some of these problems.

CROSS-CULTURAL AND HISTORICAL UNIQUENESS OF GENDERED BEHAVIOR

More than 50 years ago, the most important thing I learned as an undergraduate from my anthropology professor, Melville Herskovits, is that sex differences in human behavior are unique in every culture and vary across time in the same culture. The view then was that patterns of sex differences in behavior were so variable that all patterns could be found, and no pattern would prevail.

This is still the view among social scientists today. We love to discover that "among the headhunters of New Guinea, seven genders are recognized." One of the major contributions of social science to modern culture is to convince us that there is no human nature, so that cultures can do whatever they want with the plasticity of human clay. One of the burdens of social science, however, is its exaggeration of the diversity of cultures and times, which causes scholars as well as laymen to miss entirely the universal elements of humanity. As anthropological guru Geertz (2000) says in his new book, *Available Light*, "Any sentence that begins, 'All societies have . . .' is either baseless or banal" (p. 135).

No wonder most social scientists find evolutionary psychology and sociobiology incomprehensible. Most have no problem with evolutionary biology and assume that humans as physical creatures have evolved from nonhuman ancestors. But this does not mean acceptance that our behavior is also biologically evolved in the same way. How could this be possible when humans have no pan-human patterns of behavior? As molecular genetics matures and we find more genes for similar behaviors that are shared by fruit flies, mice, and humans (Wiener 1999), our current belief system in the social sciences will be sorely strained.

DEFINING AND MEASURING GENDER

We can divide the history of measuring sex differences in behavior into three phases. Beginning in the 1930s, psychologists developed scales that measured masculinity/femininity or sex-typicality as a way of measuring the degree to which individuals were more like females or more like males in their behavior. Terman and Miles (1936) started with a large pool of questionnaire items and identified those on which males' and females' distributions were different. Individual scores defined the degree to which behavior was sex-typical; there was a continuous distribution for each sex. As expected and required, the sex distributions overlapped. Masculinity/femininity was then treated as a more or less stable individual personality trait. The metric was bipolar, with most masculine (or least feminine) at one end and most feminine (or least masculine) at the other. This way of measuring sex differences and sex-typicality prevailed in sociology and psychology for the next quarter-century.

The second phase of measuring sex differences began in the early 1970s, shortly after physician John Money introduced the concept of "gender" into our vocabulary. Researchers became politically uncomfortable with talking about "opposite" sexes and even with measuring sex differences in behavior. Bem (1974) introduced the Bem Sex Role Inventory, which defined two orthogonal behavior dimensions: masculine and feminine. Raters identified traits that were considered desirable for males, and traits that were desirable for females. These two dimensions were not correlated with one another. Obviously these two dimensions did not measure the differences in behavior between the sexes, or sex-typicality. Since the mid-1970s, self-ratings on Bem-type personality measures have dominated the measures of masculinity and femininity.

In the 1980s, feminist sociologists, including the current critics of my article, rejected the concept of personality-type measures of masculinity and femininity. They argued that gender was a part of social structure, not a stable dimension of personality, and that gendered behaviors were situation specific, defined by the structural positions occupied by men and women. These writers denied the existence of consistent gender-related behavior in individuals that transcended the specific structures in which their behaviors were situated (Lorber 1994; Risman 1998). The idea that childhood gender socialization by parents and other socializers created a stable pattern of gendered behavior in individuals was considered to have become out-of-date and non-sociological. It is this view that dominates the present critics' comments. This is the logic by which Risman (2001, henceforward Risman) finds me two revolutions behind and says that I am integrating the sex-role theory of the 1960s with equally outdated endocrinological theory from the same period—that I am about 40 years too late in getting this article into print. My article deserves the headline, "40-Year-Old Theory Confirmed." For Lippa (2001) and I (Cleveland, Udry, and Chantala forthcoming) have now returned to measuring sex-typicality and sex differences the old fashioned way—based on how males and females typically differ in their behavior.

The logic of the feminist critique is impeccable. If measures of individual behavior do not yield a stable personality trait with respect to sex-typicality, then both the Terman-type and the Bem-type measures of masculine and feminine personality measure something meaningless: There is no such dimension of masculine/feminine, whether bipolar or orthogonal, on the level of individual personality. Therefore it is senseless to say that some of this dimension is created by socialization and some by hormone experience. What in the world was I thinking of?

I am therefore somewhat surprised that the critics have found the contents of my measure of gendered behavior interesting enough to criticize. Here I must take part of the blame for not making crystal-clear how I constructed my measure of gendered behavior. I have somehow led Kennelly, Merz, and Lorber (2001, henceforward KM&L) to believe that I had a dichotomous measure of gendered behavior instead of a continuous variable ("more or less masculine" as they say), so that my subjects were defined as either "masculine" or "feminine." Since they think this is what I did, they must have found my tables, graph, and most of the description of methods and results to be incomprehensible. Small wonder they then assert that my "dichotomous conceptualization of gender cannot account for its interactive and developmental aspects, its cultural and temporal contexts, and its reflection of institutional pressures and conventionalized assumptions" (p. 600). Should I suppose that they have somehow assumed that a "bipolar" concept of gender led me to a "dichotomous" measure of gender? Since they believed that my measure of gender was dichotomous, the remainder of their discussion of my behavior content of gender goes down a track dominated by their misunderstanding on this point.

Not only did I lead KM&L astray with respect to my measurement of gendered behavior, but somehow I completely misled them into thinking that the purpose of my research was "[t]o determine whether prenatal androgens or socialization have had the greater effect on adult women's gendered behavior"

(p. 600). If they think this was my purpose, I wonder what they think I concluded.

My critics believe that gender is inherent in social structure and that it has no organization at the individual level of personality. They believe that I should have a measure of gender that takes into consideration cross-cultural differences, differences in behavior by specific elements in the social structure, age, class, and history. This is really tantamount to saying that the research problem as I framed it has no meaning, since, in their view, I am measuring a nonexistent entity (gendered personality) and asking how it is produced by biological forces and parental socialization (i.e., that there is no researchable problem here for which measuring individual behavior is a strategy). It is easy to find fault with (and even take offense from) my individual gender measures, since I had hundreds. But for those who believe that gendered personality is a trivial concept, critique of the gender measurement items should be irrelevant to their point.

Since the critics believe my dependent variable measures something that does not exist, they should find my independent variables (hormones and socialization) equally irrelevant. My theory is derived from animal models that presuppose the existence of some kind of stability of sex-typicality that differentiates individual animals on the basis of their pattern of hormone experiences and modified by environments. If humans do not have such a stable sex-typicality in their behavior, then the animal model is irrelevant to humans, however well it explains the behavior of other animals. Whether I am using an antiquated endocrine theory or the contemporary version that is most accepted in current endocrinology is beside the point. If socialization theory (that adult gendered behavior is in part shaped by childhood socialization) is discredited for the same reasons (there is no individual cross-situational consistency in gendered behavior), why criticize the details of its measurement?

I can think of only two reasons why my critics bother to critique my endocrine and socialization theories as applied to humans. (1) They really believe that there might be some individual consistency in gendered behavior across situations, age, and history. In this case I have misinterpreted them. (2) They really believe that there is no cross-situational stability to gendered behavior, but they know that many *ASR* readers do not share their views, and therefore that predicting individual gendered behavior from hormones and socialization is of possible interest to other readers.

There is a way at least to think about the theoretical integration of gender as personality (consistency in individuals across situations) and gender as social structure. Think of an (imaginary) analysis of variance of all the gendered behavior in society. What proportion of the gendered variance is organized at the individual level and what proportion by the social structure independent of individuals? We can interpret the measure of gendered behavior in my article as only measuring the small proportion of gender variance, say 25 percent, that is gendered personality. The remaining 75 percent can still be organized by structure, and not by individuals. Theoretically, some proportion of the individual variance may be associated with women and men as agents, selecting into situations and structures according to their individual predispositions.

So let me state clearly that the empirical part of my article assumes that some proportion of gendered behavior is organized at the individual level. That is the foundation of my dependent variable. Further, it hardly needs saying that I can only measure the dependent variable and the hormone levels in a sample of currently existing women in a particular culture and at this time in history, and not in some other time or place. So the empirical results can only specifically apply to this time and this place. All else is theory. From the point of view of my critics, the application of my theory and my methods should produce random results. The fact that it does not deserves serious attention.

BIOLOGICAL DETERMINISM AND MODERN ENDOCRINOLOGY

Miller and Costello (2001, henceforward M&C) believe my view is one of biological (endocrinological) determinism. I thought I knew what a biological determinist was, and was confident that I wasn't one. Now I am not sure. A biological behavioral determinist believes that the behavior in question is fundamentally caused by biological processes, and that social and other environmental causes and interactions are trivial or nonexistent. With respect to gendered behavior in humans, and sex dimorphism of behavior in mammals generally, there are no biological determinists, even among biologists. But evidently there are environmental determinists.

M&C say my framing of the issues is "evocative" of the seventeenth- to nineteenth-century biological determinists (e.g., Durkheim, a craniologist) who sought "to anchor patterns of gendered behavior to immutable biological roots" (p. 592). The only biological determinists today are the "boogy-persons" conjured by those who distrust *any* biological causes of social behavior. These folks use words like "immutable biological roots" and "hard-wired" to identify determinist belief structures—phrases never used by contemporary biologists. Today the biologist is more likely to say that environments activate genes.

M&C share the view of Risman and KM&L that patterns of gender are de novo to each culture and period and therefore cannot incorporate biology as a participant cause. Meanwhile animal ethologists show us that behavior patterns within a mammalian species differ according to the prevailing environmental conditions it faces. For example, hippos are congenial creatures with placid social relationships under conditions of plenty, but in conditions of scarcity they become hostile, territorial, and violent. E. O. Wilson (1975) explains this by hypothesizing that each species is evolved to display a range of behavior appropriate to the range of variance in the environments experienced over its evolution. Now that view really is close to biological determinism.

M&C don't buy the animal model as applied to humans and offer warnings from previous writers about how different each species is from others. These warnings are a good idea, of course. But M&C don't necessarily pay attention to this advice. They evidently believe they learned something relevant about the importance of environment-hormone interactions, not from my examples on humans but from studies of rats, cichlid fish, and orangutans. We humans do a lot of research on other animals, not because we have a consuming interest in fruit flies, mice, and rhesus monkeys, but because we hope to learn from them something about humans. I thought it funny that M&C tell the reader that I speak "without irony, if oddly, of the gender of garter snakes" (p. 595). I thought it funny, although not true, because these days I often hear perfectly mainstream scholars speak about the gender of dogs and about how monkey mothers teach different behaviors to their offspring according to offspring "gender."

In a spirit of comity, M&C say, "If in fact Udry's findings have some validity, we suggest that this is because variations in prenatal hormonal exposures affect physical development in small ways, and that these small variations in bodily form are given great social significance in our deeply gendered society" (p. 595). In this way, what looks like a biological mechanism might really be socially constructed, they suggest.

This is certainly true. I think that everyone will agree that prenatal hormones are definitely a cause of differences in external genitalia by sex, which are of course small variations. And of course many other tiny variations in male and female bodies, internal and external, are also caused by prenatal hormones. The external differences are picked up on by those sensitive to such things. But the story is more interesting than that. For both males and females in the fetal period and throughout life, the enzyme, aromatase, transforms testosterone into estrogen *after* it enters the brain cells, and this estrogen is thought by endocrinologists to *masculinize* (yes) the brain. Another enzyme—5-alpha reductase—transforms testosterone into dihydrotestosterone, which masculinizes the body. These two processes operate independently (Panksepp 1998:233).

Thus there may or may not be a congruence in particular individuals between body and brain masculinization. For various reasons (sometimes genetic) one of these enzymes may be deficient, or the individual may be insensitive. This can lead to a masculinized brain in a feminine body, or a femininized brain and a masculine body. Thus, the social process that M&C hypothesize might come about through social attribution also has a neurohormonal explanation. The lesson is that two separate processes masculinize the brain and the body—and that both are testosterone-based.

M&C are disappointed that the prenatal hormone → sex behavior theories treat the brain as a "black box," leaving the mechanism of action unexplained. This is hardly a fair criticism. For 30 years, Gorsky (1988) and others have been doing experimental work on how hormones actually change *prenatal* brain structures. Many important theories use "black boxes" and hypothetical entities as valuable holding patterns—temporary steps in the process of theory building. For example, for decades biologists used the concept "gene" as a hypothetical construct before anyone knew what genes looked like or even where they lived. So let's be patient as our knowledge grows.

M&C don't think girls with congenital adrenal hyperplasia (CAH) behave in a more masculine way because of prenatal hormones; they don't necessarily agree that they behave in a more masculine way at all. The fact that in most CAH studies, female patients were born with genitals that are slightly or grossly masculinized and have often had genital "corrective" surgery, leads them to a socialization explanation when CAH girls are in fact masculinized in behavior. Their description of the childhood experience of female CAH patients is indeed distressing. But let us not become confused and think that this is the experience of any of our respondents. I had no CAH subjects in my sample, and none was born with unusual genitals according to hospital records.

While we debate the usefulness of my "40-year-old theory" of the origin of sex-typical behavior, it is instructive to realize that my version of a theory of gendered behavior is being used to guide treatment of CAH as well as other clinical cases presenting with gender-body conundrums. The most celebrated case is of a male infant whose penis was destroyed in the course of a routine circumcision in the 1960s. John Money, the father of modern sex change surgery, was the physician in charge of case management. At that time, Money shared the consensus of the period—that corrective genital surgery and assignment as a girl and to feminine socialization would turn the boy into a girl. Early follow-ups by Money and others reported that the patient was doing perfectly well as a girl. Later follow-ups contradicted the previous findings and indicated constant identity problems from early-on. As a teenager, the patient discovered her early history, decided to live as a male, and has since married a woman. Now Money is castigated for his "error" in management (Colapinto 2000). But over the years the clinical community has changed its theory.

Individual case histories are never decisive. Textbooks in the 1970s, however, used this case to "prove" that we can shape people's behavior to any gender, since when they are born they were assumed to be psychosocially gender-neutral (Robertson 1977). In those days, social scientists still believed in gender as personality. What will the textbooks of the present century say?

KM&L are the only critics who explicitly accept that "hormonal input may indeed affect behavior" (p. 599). But they believe that "the pathways are reciprocal since behavior has also been shown to affect hormone levels" (p. 599). They call such models interactive. But the term "interactive" has taken on another statistical meaning in sociology and as used in my paper: The relationship between A and B varies as a function of C. To be clear, I use "reciprocal effects models" to describe the relationship of the form, hormone A affects behavior B, which in turn affects hormone A. These loop-back relationships are interesting but difficult to study. They usually are in the form of equilibrium systems, where changes in A and changes in B mutually regulate B and A respectively. (Furnace heats room, heat turns off thermostat, temperature drops, cold turns on thermostat, furnace heats room, etc.) Let us say instead, as Kemper (1990) does, that testosterone causes masculine behavior in a woman, but masculine behavior increases testosterone, which again increases masculine behavior. This is not an equlibrium system, if it exists, for there is no equilibrium mechanism, and both masculinity and testosterone continue to reciprocally augment one another until one or the other reaches toxic levels, or one reaches a level at which it becomes insensitive to the other.

In principle, I don't have a problem with this model, but it is not my model, and there is no data to support it. In my theoretical model, prenatal androgen exposure leads to a masculinized brain structure, which produces masculine behavior in childhood without further androgen support. The masculine behavior does not lead to increases in childhood levels of testosterone.

HOW DID THIS ARTICLE GET PUBLISHED?

Risman is thoroughly dismayed by the review process that led to the publication of my target article. She says the article is bad science because my theories are 40 years old, and long since discredited. She says it is "bad" science because it ignores the prevailing contemporary gender theory in sociology. Its publication, she says, indicates the review process failed. Her analysis of the review process is both thorough and interesting in itself (pp. 608–609).

A possible reason why the review process "failed" in Risman's eyes is that Risman's views as to the current consensus about discredited theories and "must-be-included" theories are distorted by her own position in the field. Each of us sees sociology through our own set of distorting lenses. It is easy for me to recognize that my hypothesis of biological influences on gendered behavior is not widely shared by my sociological colleagues. It may be less easy, for example, for Risman to understand that gendered behavior as personality is not discredited in the discipline. We all see gender as an integrated creation of social structure. That does not preclude us from simultaneously conceptualizing and measuring masculinity–femininity as an individual trait. The *ASR* reviewers had strong opinions about how to measure it. Even the critics published in this issue have strong views about what should and should not be included in the measurement of this supposedly nonexistent entity. In fact, Risman entertains the idea that my measure of gendered behavior actually measures something more like "degree of conformity to a locally specific norm of femininity" (p. 609). I can live with that. What gives such a measurement any meaning to Risman, given her views? Degree of conformity is an individual measure of gendered behavior.

Risman notes a polemical antifeminist tone of my conclusions. As an example she quotes from my article—"A social engineering program to degender society would require a Maoist approach: continuous renewal of revolutionary resolve and a tolerance for conflict" (Udry 2001:454)—except that she omits the phrase after the colon (Risman, p. 608). KM&L quote the same sentence (except they omit "a Maoist approach"). KM&L respond, "We quite agree" (p. 603).

Risman ridicules my statement that degendering society will create social malaise, and asks, "for whom?" (p. 609). But she titled her last book *Gender Vertigo* (Risman 1998), her term for the disorientation and sense of unease to be experienced by people in our society should we implement her recommendations for degendering society. Sounds to me like Risman and I are talking about the same thing.

In the title to her comment, Risman focuses us on what she wants us to take as her main message. She "calls my bluff" for pretending to be a value-free positivist, while I cloak behind this stance the value-system she perceives as having motivated my theory and my results. Now value-free positivist is not the worst thing I have been called. She evidently thinks that the scientific review process privileges work that purports to be value-free, that comes from heavily-funded, longitudinal studies, and that intimidates readers with biological data. I infer that she believes feminist scholarship is devalued in this process because feminists are "post-positivists" who believe that science never proves anything. (Where are the positivist feminists?) She thinks we should have a disciplinary discussion that would lead to equal privilege for science that includes value commitments to a more just world.

I think such a disciplinary discussion would disintegrate into name-calling. Risman thinks I am bluffing when I say, "I make no judgment here as to whether it is morally good to reduce sex differences, or to leave them alone" (Udry 2000:453). She sees dark footprints of my antifeminist values imbedded in every step of my research process. It does no good for me to argue that I made no judgment *in the article* about the morality of sex differences. If I say that I work toward reducing sex differences because I think this will lead to greater human dignity and the realization of our full human potential (if not greater happiness), am I just self-deluded? At least that is better than being accused of bluffing.

CONCLUSIONS

Sociologist are very diverse in their theoretical orientations. Some of us work within paradigms that are incompatible with paradigms used by other sociologists, even though we suppose we are working in the same domain—in this case, the study of gender. Sometimes we simply cannot understand what the other is saying. This causes us to think that the other guy is just thickheaded. It is hard for us to avoid saying that the paradigm we don't use is discredited, tainted with bias, and out-of-date. If we aren't careful we give the impression of arrogance. In fact, in the first review of my manuscript that eventually became the critics' target article, I provoked the descriptor "arrogant" from reviewers. In my reading of the first versions of the critics' papers, the word "arrogant" came to my lips. Now that we have all calmed down, both sides should be able to understand that I don't have to be wrong for them to be right, and they don't have to be wrong for my analysis to have something to say to feminists. Each culture has its own version of gender, but gender is not randomly created across cultures. Prenatal hormones constrain gender construction, but environments create gender. Gendered behavior is developed within structures, but parents socialize children to emerge in adulthood with personally characteristic levels of sex-typicality. Paradigms with different perspectives are not necessarily mutually exclusive. I can live with the critics' paradigm. But can they live with mine?

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