

# Brain Storm

*The Flaws in the Science of  
Sex Differences*

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## Preface

Most books set out to answer questions. This book sets out to question answers. The answers I question have to do with the nature and causes of differences between men and women, and between straight people and gay people. Specifically, I question what we “know” about male and female brains, or gay and straight brains.

When Simon LeVay reported in 1991 that he had found a difference in brain structure between gay and heterosexual men, which was trumpeted as the discovery of “The Gay Brain,” I found it interesting but also puzzling. How could gayness take a single identifiable form in the brain when it takes such varied forms in people’s lives?

At the time, I had already been engaged for several years in large-scale sexuality research related to the AIDS epidemic. In an outreach storefront in Washington, DC, I ran a project that focused on injection drug users. It was there that I first met a lot of gay men. These men were not the poster children of the gay rights movement, but were poor, struggling with addiction and recovery, and trying to avoid or outlive AIDS. And so were their heterosexual brothers, with whom I also worked. In fact, these men were so similar in demeanor, dress, and daily struggles that without our detailed interviews, it was impossible to tell the difference between the gay and straight men. In LeVay’s study, the homosexual men were a singular “type” unlike the (also homogeneous) heterosexual men, and they were also somehow similar to (presumably straight) women. Those are com-

mon enough ideas both inside and outside of science, but our research challenged these notions.

The reason for this was our research methods. Since our main questions didn't have to do with sexual orientation, our preconceptions about sexual orientation didn't shape the way we gathered our information. We asked people to join the study based on their drug use or their connection with drug users, not their sexuality—not their sexual practices or identities, not the gay or straight reputation of the bars or neighborhoods they hung out in, nor even how we perceived them as straight or gay. We asked everyone extremely detailed questions about their sexual practices with both same-sex and other-sex partners. Scientifically, this approach is a much more reliable way to get information about the nature of sexuality in a population than to go out looking for gay and straight people to compare. The number and variety of people who talked about same-sex relationships surprised all of us—including the gay and lesbian staff members who thought we had finely tuned “gaydar.” The bias in epidemiology at the time was to see same-sex behavior among people outside of well-defined gay communities as being “instrumental”—meaning that it was due to drug use, incarceration, or sex work. But as we spent hours and weeks and eventually years with our participants, growing to know and love many of them, it was clear that they had same-sex relationships for the same reasons they had heterosexual relationships: desire, affection, and love. The thousand or so sexual histories we gathered fed into an enormous pool of research that eventually included information on the sexual and drug-using behavior of tens of thousands of people from more than fifty cities. And on the basis of this research, it would be very hard to suggest that either men who desire and have sex with men, or women who desire and have sex with women, are a distinct type of person, or are somehow “like” heterosexuals of the other sex (Young, Weissman, and Cohen 1992; Young et al. 2000; Young and Meyer 2005).

So how could I make sense of what LeVay had found? What functions might relate to the brain structures that he suggested were somehow connected to both sex and sexual orientation? Where did the brain differences come from? And are they the *cause* of differences in behavior, personality, or desires, or the result of them?

Some years later, when I was in graduate school, one particular semester had my brain stretching in almost too many directions to bear. I was studying psychometrics, observational epidemiology, and biostatistics, and I decided to add a class on “the gay brain.” LeVay's 1991 study was on the syllabus, and by then, I knew enough to spot quite a few problems with it. But other studies seemed to point in the same direction as his: Dick Swaab

and Michel Hofman (1990) had reported another structural difference between the brains of heterosexual and gay men, and Laura Allen and Roger Gorski (1992) reported yet a third. Each of these research teams was also looking at male versus female differences in the brain. It turned out that none of the studies could answer my earlier questions about function, and none of them even entertained the idea that the structural differences might come from behavior and experience, rather than the other way around. Instead, through these studies, I learned about the largely unquestioned theory that was guiding the work of these research teams and many others. According to this theory, prenatal hormone exposures cause sexual differentiation of the brain—that is, early hormones create permanent masculine or feminine patterns of desire, personality, temperament, and cognition. Further, hormones later in life could “activate” behavioral predispositions, but the predispositions themselves result from the initial “organizing” effect of hormones very early in development, before birth. Intrigued, I began to look for other research related to this theory, which some scientists call the “organization-activation hypothesis,” and some call the “neurohormonal theory.” I was particularly interested in studies that explore the earlier, organizing role of hormones, the time when hormones presumably cause sex-typed predispositions. I think the clearest way to refer to that work is by the term *brain organization research*, so that is the term I use in this book.

Once I began looking into this theory, I couldn’t look away, and I have now spent thirteen years exploring brain organization research. I was not initially interested in using my analysis to reflect back on how well or how poorly brain organization theory was supported by the evidence from these studies. I was more interested in methods, particularly in how scientists resolved the problem of measuring something as complex as sexuality or gender in such a way that these in turn could be associated with brain structure or hormone exposures. I was also curious to see how scientists differed in their approaches, and what kinds of methods they used to translate research findings across different study designs. Focusing on original, peer-reviewed research in English-language scientific journals, I used a combination of strategies to identify studies, beginning with a small set of high-profile studies and searching for the research those works cited, as well as subsequent studies that cited my index cases. I identified the “founding paper” in the field (Phoenix et al. 1959), in which the theory of brain organization was first proposed, and systematically searched for research reports on humans that cited that paper. Using the ISI Web of Science, Medline, and PsychInfo databases, I combined keywords about hormonal “inputs” (such as prenatal, in utero, organizing effects, hormones,

testosterone, estrogen, progesterone) with psychosexual “outputs” (such as masculinity, femininity, eroticism, sexual behavior, psychosexuality, sexual orientation). Early on, I limited myself to analyzing studies that explored the connection between prenatal hormone exposures and human sexuality. I ultimately analyzed virtually every study on the ostensible prenatal hormone-sexuality connection published from 1967, when the theory was first applied to humans, up to the year 2000, when the increased flow of research in this area made it no longer possible to examine every published study in depth. I continued to examine all major studies (those published in the most important journals, those that garnered a lot of scientific attention, and those by well-established scientists) through 2008. Further, because brain organization research had always addressed broader questions of masculinity and femininity, I expanded my ongoing search strategy to identify studies that focus on those variables, too. I have now done a close analysis of over three hundred studies that span all of the many research designs used to explore the hypothetical connection between prenatal hormone exposures, on the one hand, and human sexuality or gender, on the other.

I also interviewed scientists. I did an influence analysis to identify the twenty-five most influential people doing brain organization research, based on how their studies were cited by other scientists. Twenty-one of them generously agreed to be interviewed; five spoke with me at least twice. They talked with me about the theory and about the nitty-gritty details of their studies: subject recruitment, questionnaires, statistical analysis, and so on. I enjoyed those conversations, and many of the scientists told me that they were happy for the chance to step back from their work and think about the field as a whole, or about conceptual issues that sometimes take a backseat to daily research practice. I am grateful for their time and their openness with me. The interviews were of immeasurable value to me in making sure I understood scientists’ varied research approaches, as well as their disagreements with one another. I use quotations from the interviews somewhat sparingly, though, because I am less interested, in this particular book, in what scientists *say* than in what they *do* in their research practice. Several scientists were wary about controversies that had erupted shortly before I began my interviews, so I follow standard ethnographic practice, and treat the interviews as confidential. In a few places where I have used material that bears directly on a particular scientist’s work, I have used their names with permission. Elsewhere, they are identified by simple pseudonyms (Dr. A, Dr. B, etc.).

The closer I looked at brain organization research, the less it made sense. My initial focus on methods gradually gave way to the realization

that the evidence simply does not support the theory. In this book, I ask readers to follow the same journey I have taken, looking closely at the measures and methods in order to understand gaps and fundamental contradictions in the data. Although I find much fault with research done in connection with brain organization theory, I hope that the researchers who generously shared their time with me, as well as others who read this book, will take it in the spirit of constructive criticism in which it is offered.

Brain organization research is important because it engages deep and enduring questions that practically everyone shares: how do we come to be the sort of people we are? How do our bodies matter to our personalities, skills, interests, and desires? These are great questions, but sometimes we treat them like rhetorical questions, and plug in answers that seem right just because they are familiar.

For instance, when I talk to people about the idea that people are born with a male brain or a female brain, I almost always end up hearing about their experiences with children. A few people relate tales of unexpected flexibility or gender-bending, but most relay their experiences with boys who practically radiate “boyness” from some deep space within (interestingly, I hear many fewer stories of girls who are sugar and spice and everything nice). Recently, my mother happened to be present for one of these conversations, and she very matter-of-factly presented what is considered these days to be a radical idea. Before relaying her idea, let me explain that my mother is a lovely southern lady who has raised an impressive number of children: four boys and four girls of her own, in addition to playing a major role in raising some half dozen of her more than two score grandchildren and great-grandchildren (I think she’s drawing the line at the great-great-grandchildren). When my friend talked about her girl being so different from her younger brother, who is “all boy,” my mother literally snorted. “That’s because you only have two,” she said. Mama went on to explain that with just a couple of children, gender looms large—it’s the most obvious explanation for every difference you see between them, and unless your children are really unusual, it’s going to be easiest to see their personalities as “boy” versus “girl.” But when you have a lot of children, you begin to notice that they all come with personalities of their own, and they are all quite different from one another. Gender recedes in importance.

Of course, that doesn’t mean that gender isn’t real. One particular question about the “realness” of gender is at the heart of this book: how is gender connected to bodies, specifically, to the brain? And how does sexuality figure in that connection?

Brain organization theory offers a pretty tidy answer: it's all down to "sex hormones" that shape the brain before birth. Get enough "male hormones" and you'll be masculine in desires, interests, and personality. Maybe it's the Missouri in me, but I am never happy with an answer if it's too easy. I really do demand evidence: "show me." And I expect you want the same, so that's what I do in this book.



## Sexual Brains and Body Politics

A LITTLE GIRL, given trucks to play with, calls them “daddy truck” and “baby truck.” A child, raised as a girl after a tragic accident that destroyed his penis, rejects a female identity and insists that he is a boy. A woman wisecracks that the difference between how females and males learn is that you can teach her how the dishwasher works, but you can’t make her interested in it.

Stories help us understand the world. These modern parables about sex differences underscore what it means to be male or female, and suggest answers to questions that intrigue us, such as how changeable or resistant to “tampering” sexual natures are. For many people today, the most authoritative stories come from science, and the dominant scientific tale about sex differences goes like this: Because of early exposure to different sex hormones, males and females have different brains. Moreover, the same process that makes men and women fundamentally different is responsible for within-sex differences in sexual orientation, so “gay brains” are also different from “straight brains,” courtesy of early hormone exposures.

Increasingly, scientists in the public eye have characterized skeptics of this idea as politically correct ideologues who are, in Steven Pinker’s words, “on a collision course with the findings of science and the spirit of free inquiry” (2005, 15). Writing for *Scientific American* a few years ago, neuroscientist Doreen Kimura (2002, 32) put it this way:

For the past few decades, it has been ideologically fashionable to insist that [male–female] behavioral differences are minimal and are the consequence of variations in experience during development before and after adolescence. Evidence accumulated more recently, however, suggests that the effects of sex hormones on brain organization occur so early in life that from the start the environment is acting on differently wired brains in boys and girls.

Of course, science itself isn't just one thing, and there isn't just one story that “science” tells about sex in the brain. But this particular story—brain organization theory—is especially prominent. Unlike tales about childhood play or a woman's level of interest in how her dishwasher works, it's hard for the average person to critically engage with a story that comes from the rarified domain of cognitive neuroscience. (It's not that easy for scientists to critically engage with the story, either: there's nothing more chilling to a scientist than being called “ideologically fashionable.”)

In daily life, people understand that it is important not only to listen to talk about how things work but also to directly observe how things work. Likewise, it could be very informative to go behind scientists' theories about sex in the brain and look instead at the research they do. But how? The volume of studies is enormous, and the technology involved seems ever more sophisticated, making it very difficult for laypeople—or even for scientists outside of particular subspecialties—to do anything but stand back and be impressed with the sheer quantity of this work. But the first premise of this book is that ordinary people can take a hard look at how research supporting brain organization theory is done. In the chapters that follow, I present a synthetic, critical analysis of the evidence that human brains are “hardwired” for sex-typed preferences and skills by early hormone exposures, and I build the analysis on simple rules of “symmetry” that are easily understood by nonspecialists.

Despite the high profile of this theory, this is the first synthetic analysis of organization theory research on humans, including more than three hundred studies conducted from the late 1960s through 2008. There have been other summaries of brain organization research (Berenbaum 1998; Hines 2004; Cohen-Bendahan, van de Beek, and Berenbaum 2005), and past critics have pointed out methodological weaknesses that call the results of particular studies into question (Longino and Doell 1983; Schmidt and Clement 1990; Byne and Parsons 1993; Fausto-Sterling 1985 and 2000). But these summaries haven't been systematic in the same sense as my analysis here. What has been missing is a rigorous and comprehensive *synthesis* of the studies, considering all the different findings from studies across the many designs that scientists use to test the theory. If the studies on brain organization were more similar, it would be ideal to do

a meta-analysis—a statistical approach in which data from many studies are pooled and analyzed together. Unfortunately, the network of research on brain organization theory is too diverse to do a conventional meta-analysis.

But there is something special about these studies that offers another way of doing synthetic analysis. Each study of how prenatal hormones sexually “organize” the human brain is a *quasi experiment*, rather than a true experiment. In true experiments, subjects would be randomly assigned to receive particular hormone exposures, and their development would be observed over the life span, keeping rearing experiences and environments constant across experimental and control groups. Of course, this isn’t possible with humans, so scientists must piece together evidence from animal studies, and from individual human quasi experiments that are by definition partial and uncontrolled. The interpretation of every quasi experiment depends on carefully placing that study within the overall body of evidence. So, a synthetic analysis of quasi experiments can actually be done by mapping the *structure* of studies, to see how well the studies fit together.

The second premise of this book is that it is not just possible, but urgently necessary, to reopen the questions that have been closed by accepting brain organization as a done deal. To broadly anticipate my findings, evidence that human brains are hormonally organized to be either masculine or feminine turns out to be surprisingly disjointed, and even contradictory—and the stakes involved in prematurely promoting this theory to a “fact” of human development are high, both for the advancement of science and for social debates that draw on science. In the last chapter, I begin to sketch some ways in which breaking free of brain organization theory as it is currently conceived may stimulate better science, specifically more dynamic research on human development. As for the importance of this theory to social debates, I don’t devote much space in the rest of this book to how such research matters in the broad world outside of science. So I want to point here to just a few of the reasons why a hard-nosed understanding of brain organization research is so important.

The idea that men and women “naturally” think and act differently because they have distinctly male or female brains is not new, but a number of high-profile controversies over the past few years have given the idea new prominence, especially as an explanation for why there aren’t more women in science, engineering, and mathematics. Shortly after a report from Massachusetts Institute of Technology cited widespread discrimination against women at MIT (1999, 15), the debate over women in math, science, and engineering moved from its usual simmer into one of its peri-

odic fierce eruptions. While the report drew widespread praise and attention (Loder 2000; Goldberg 1999), it also provoked skepticism and counterarguments suggesting that the dearth of senior women at MIT was less a reflection of discrimination than an inevitable reflection of the difference in male and female brains. In a presentation to the National Academy of Engineering that garnered a good deal of press, Patricia Hausman flatly asserted that barriers for women in science and engineering “don’t exist.”<sup>1</sup> Instead, she argued, occupational gender differences can be explained by “exposure to testosterone during a key phase of fetal development [that] appears to influence spatial ability and some aspects of personality” (P. Hausman 2000, 4).

A few years later, in *The Essential Difference: The Truth about the Male and Female Brain*, psychologist Simon Baron-Cohen (2003a) put some new clothes on the male brain/female brain idea in the way of updated terminology for these sex-differentiated organs: “systemizers” and “empathizers.” Baron-Cohen notes that “professions such as maths, physics, and engineering, which require high systemising, are also largely male-chosen disciplines,” and points to high fetal testosterone as explaining the connection (Baron-Cohen 2003b, 4). Although Baron-Cohen’s work has mostly been warmly reviewed, some such pronouncements have provoked fierce controversy. Most famously, Larry Summers, former president of Harvard University, suggested that innate “taste differences” between the sexes and a male predominance in high-end aptitude are probably the most important reasons women are underrepresented among tenured scientists and engineers at top research institutions (Summers 2005). Summers didn’t directly invoke brain organization theory to support this idea, but almost all the scientists and pundits who came to his defense in the ensuing firestorm did (for instance, Pinker 2005; Kliemfeld 2005).<sup>2</sup> The notion that there are male brains and female brains has continued to crop up in controversies over women in math and science, but it has also recently shown up in discussions of single-sex education, sex disparities in wages, “abstinence-only” sexuality education curricula, the protocol for medical treatment of children born with ambiguous genitalia, and yes, even the out-of-control, risky trading that recently brought the economy to its knees (see, for instance, Kliemfeld 2005; Byne 2006; Stein 2009; Weil 2008; Tyre 2005; Sax 2005; U.S. House of Representatives 2004; Kay and Jackson 2008; Ceci and Williams 2007; Wilson and Reiner 1998; National Academy of Sciences 2006).

The same theory connects sex differences in the brain to sexual orientation, and it’s common for scientists to explain the theory by connecting “sex-typical” patterns of interests, cognitive skills, and sexuality. For ex-

ample, in explaining evidence that many scientists find to be some of the strongest for demonstrating that the human brain is hormonally organized to be masculine or feminine, neuroscientist Sandra Witelson cites “girls who have a hormone disorder that causes them to have higher testosterone levels in utero. ‘In these girls, their play patterns, their spatial ability and even their sexual orientation are much closer to the male pattern’” (Weise 2006, quoting Witelson).

In an era where diversity is celebrated, the idea of “sex in the brain” no longer equals an endorsement of male superiority, and critics of the idea are increasingly cast as not only antisience, but antidiversity. The most straightforward claims of this sort are made in connection with the rights of sexual minorities. In a popular media campaign, the Gil Foundation uses a cartoon dog named Norman who moos instead of barks: Norman was “born different.” The gist of the ad is that gay men, lesbians, and transgender people of both sexes have brains that are “wired” differently from most people, and that accepting this is an important component of combating anti-gay prejudice. And while the notion of innately different preferences in men and women was once politically suspect, it is now often suggested that accepting these innate differences will encourage a more rational approach to equality. Should boys and girls be taught differently, because the sexes have innately different patterns of learning? Perhaps we should stop striving for parity in the professions, for example, or for an equal division of parenting labor, because women and men want different things out of life and are temperamentally suited both for different work and for a different balance between career and family (Hewlett 2002; Story 2005; Young 2006). Sociologist Linda Gottfredson warns, “If you insist on using parity as your measure of social justice, it means you will have to keep many men and women out of the work they like best and push them into work they don’t like” (quoted in Holden 2000, 380).

No area of social policy has been so charged by brain organization theory as the protocol for the medical treatment of intersex infants, children born with genitalia that either are ambiguous or don’t “agree” with genetic sex. Research on intersex conditions is a cornerstone of studies related to brain organization, and the various conditions are discussed throughout this book. It is also important to consider one high-profile case that concerns someone who was *not* born intersex, but on which ideas about both brain organization in general and intersex management in particular have disproportionately hinged. David Reimer, also known in the research literature as “John/Joan” (Money and Ehrhardt 1972) and in the popular media as “the boy who was raised as a girl” (Colapinto 2001), was born a normal male infant, and an identical twin. Reimer, whose life

tragically ended in suicide a few years ago, was raised as a girl after his penis was irreparably damaged in infancy. Reimer's doctors, especially the famous sexologist John Money, had believed that Reimer could not develop a normal male identity without a penis. Money's prior research suggested that gender (one's sense of self as male or female) was "flexible" until about age 2 and was largely determined by socialization rather than by biology. Money's advice was to rear the child as a girl, and Money cited "Joan's" successful adjustment as especially compelling evidence for the importance of rearing in gender development (for example, Money and Ehrhardt 1972).

In the late 1990s, however, news began to emerge that Reimer had rejected a female identity, insisting that he was really male. Another group of scientists, especially Money's archrival Milton Diamond, interpreted this to indicate that Reimer's male identity was "fixed" in his brain because of the testosterone he had produced as a normal male fetus. After a revision of the case in the medical literature, and a BBC documentary, Reimer's story was broken to a broader public with an article in *Rolling Stone* (Colapinto 1997). The article was followed by a popular book (Colapinto 2001), which in turn was followed by almost countless popular articles, television and radio stories, and even a special science documentary for the PBS science series *NOVA*. It is safe to say that nothing has done more to dramatize the theory of brain organization. So it is particularly fascinating that the psychologist John Money, who is presented in the popular version of that story as the absolute embodiment of "blank-slate thinking" about gender, was in fact the very first scientist to extend brain organization theory to humans. In an especially odd coincidence, Money first tentatively applied brain organization theory to humans in 1965—the year David Reimer was born (Money 1965a).

The riveting story of "the boy who was raised as a girl" is perhaps an allegory for the story of brain organization theory. Things aren't always what they seem. In spite of the much-heralded early reports, it is not "easy" to turn a child who is born a normal boy into a girl. And in spite of later reports that painted him as an extreme proponent of socialization theory, John Money turns out to have been one of the most important and prolific researchers producing studies of hormone effects on human behavior. There are more complications to tell and corrections to be made, but these must wait until later chapters. Suffice it to say that while we should honor David Reimer's fundamental sense of masculinity, and take sober account of the ways in which his treatment was mismanaged, we should be cautious about accepting the conclusion that so many have drawn from his

difficult life: that testosterone in the womb was responsible for his unshakable sense of masculinity.<sup>3</sup>

### Brain Organization Research and the Biosocial Model

Given the popular image of John Money as a radical proponent of social factors in sexual development, it may be particularly confusing that feminist biologists and philosophers of science usually describe Money and his longtime collaborator Anke Ehrhardt as primarily “biologically oriented” researchers (Bleier 1984; van den Wijngaard 1997). Money and Ehrhardt, as well as *her* collaborator of more than thirty years, Heino Meyer-Bahlburg, have thought of themselves all along as “interactionists” who believe in the importance of both biology and the rearing environment. When I interviewed Anke Ehrhardt in 1998, she indicated that the idea that hormones would create inborn, behavioral predispositions for masculinity or femininity was unexpected and went against the grain of the working hypotheses among Money’s team:

[It was] really in some ways, against what we all believed in Money’s unit. . . . as you know, at the same time, Money had broken the taboo about gender identity, that gender identity could develop in contrast to biological variables and in concordance with the sex of rearing. But Money was really an early interactionist and thought always that you can’t split the body from the mind. But he thought, well . . . here are these interesting animal experimental data . . . so I think we were truly open, because of these different kinds of mental sets. (Ehrhardt interview, December 4, 1998)

Among scientists conducting brain organization research, Money, Ehrhardt, and their respective collaborators have indeed devoted more ink than others to emphasizing social aspects of development. In the interviews I conducted, some scientists praised their approach as especially thoughtful (e.g., Drs. H and J), while others ridiculed them for using “disclaimers galore” about the effects of hormones (Drs. A and F). The range of opinions about Money, especially, mirrors a number of other differences among brain organization researchers and helps counter any simplistic notion that these scientists pursue biological research on sex differences out of some uniform agenda. This is not to say that scientists conducting this work do not have agendas, some more explicit than others. Rather, the point is that their ideas diverge in significant ways from one another, so the

story of how brain organization research unfolded should not be read as the story of a tight scientific club. It's especially important to keep that in mind when I describe the "network of brain organization research," because the term *network* is often employed to describe groups of connected people, and in science studies especially to describe how personal and professional connections among scientists shape the scientific knowledge they produce. Certainly there are quite deep connections among many scientists who study brain organization, and such connections undoubtedly influence their work, but that is simply not the subject of this book. Instead, I use the idea of a *network of studies* to signify that all such studies are connected by a single theory about how hormones affect the developing brain. This is not a sociological claim, but a scientific one: because these studies all relate to the same theory, they must also all relate to one another. That point is important for understanding how strong the theory itself is, as will become increasingly clear throughout this book.

The fact that scientists doing brain organization research disagree about the relative importance of social experiences versus hormone exposures can obscure a fundamental issue on which they do agree, namely, the *way* in which nature and nurture contribute to development. Money and Ehrhardt's analytic model, which set the stage for decades of research on hormones and sexuality, is not truly interactionist, because it does not attempt to account for how physical and social variables actually *work in tandem*. Biologist Ruth Doell and philosopher Helen Longino put their finger on this problem years ago when they dubbed Money and Ehrhardt's approach as a "linear model" of development that treats the brain as a "black box with prenatal hormonal input and later behavioral output" (Doell and Longino 1988, 59).

These days, opposing "interactionism" is about as popular as being against "freedom," so brain organization researchers all call themselves interactionists, though they actually continue to employ the same model. This additive "biosocial" model can allow for both biological and social variables, but each is seen making a separate contribution that can be added or subtracted without changing the *way* the other affects development. An interactionist model, on the other hand, suggests that the *character* (not just the amount) of biological influence is affected by specific aspects of the environment, and vice versa. I explore the idea of interaction in much more detail in Chapter 10. I also show, especially in Chapter 9, that most brain organization studies do not include social variables in their models at all, let alone explore potential interactions between these and the hormones that they see as the main actors.



## Some Notes on the Aims of This Book

Because the research I've described is so politically important, and so politicized, it's worth pausing briefly for some comments on what this book is, and is not. First, this book is *not* about the political commitments of scientists who do the research. Scientists who conduct brain organization research often participate in policy debates, sometimes quite directly. Richard Udry, for example, who has reported correlations between prenatal testosterone levels and women's "gendered" behavior, concludes that "males and females have different and biologically influenced behavioral predispositions." That sounds neutral enough, but the lessons he suggests for society are rather more ominous: "If [societies] depart too far from the underlying sex-dimorphism of biological predispositions, they will generate social malaise and social pressures to drift back toward closer alignment with biology" (Udry 2000, 454). Other scientists are usually somewhat more cautious in this regard. Psychologists Anke Ehrhardt and Susan Baker (1974, 50) suggested more than three decades ago that prenatal hormone effects "in human beings are subtle and can in no way be taken as a basis for prescribing social roles." More recently, Sheri Berenbaum and Melissa Hines have both cautioned against abandoning efforts "to increase the participation of girls and women in science and engineering, because it is likely that other factors are also involved in the underrepresentation of girls and women in these fields" (Berenbaum 1999, 108; Hines 2007).

Nonetheless, while comments like this indicate that scientists are interested in—and sometimes perhaps a bit worried about—the social implications of their work, I think it would be a mistake to view research related to brain organization theory as motivated primarily by the political commitments of scientists. In fact, after analyzing hundreds of studies, listening to talks that scientists have given on the topic, and interviewing nearly two dozen of the most important scientists in this research area, I've come to the conclusion that there is a huge diversity of opinion among scientists themselves about what the idea of brain organization means for social questions like programs to address gender disparities, or policies regarding the rights of sexual minorities. Thus, although scientists' political and social ideas are sometimes quite interesting, and no doubt important for the way they conduct their own research, I don't think it's the really big story for understanding the research overall. Ditto for who does the research: though the research is often deployed in debates about sexual difference

and equality, the important scientific actors in this field have always included many women, and increasingly include gay scientists. There are no obvious associations between the social identities of the researchers and the studies' focus and findings.

Second, this is also not a book about whether differences are “good” for women, sexual minorities, or anyone else. Like many other scientists and science watchers, I believe history shows that there is never a clear message to be drawn from scientific findings of “difference” versus “sameness” (Fausto-Sterling 2000; Terry 1997).<sup>4</sup> For the record, I'm a strong supporter of sexual equality—including full rights and justice for sexual minorities—but so are many of the scientists with whom I disagree when it comes to interpreting research on brain organization.

Finally, and most emphatically, this is not an “antiscience” book, and it's also not an “antidifference” book. Questioning brain organization theory is not the same as rejecting either science or biology. To the contrary, the aim of this book is to invite a deeper consideration of the science that underlies the theory—not to foreclose, but to reinvigorate, free inquiry on the subject of sex differences in the brain. I do not reject the idea of sex differences in the brain as either “dangerous” or implausible—to be honest, I'm somewhat amazed that there are not *more* sex differences in the brain, for reasons that I will explain in the final chapters. But the standard stories about male and female brains are both stale and unscientific, often incorporating unfounded assumptions (about how and when differences arise, and so on), and premature leaps (for instance, drawing firm conclusions from small and unreplicated studies). In more than a decade of looking closely into the very interesting world of research on hormonal organization of the brain, I have tried to reopen key questions that many other scientists assume have been closed, asking not just what we know, but how we know it.

Throughout this book, I examine scientists' methods in conducting brain organization research in a way that can be seen as a hybrid between the kind of critique that is the model for scientific peer review and the kind of critique that is more common in science and technology studies (STS). STS scholars have demonstrated that scientific research contributes to, rather than simply reveals, the meaning of phenomena that are studied. Rather than continuing to point this out as though good science can and should be purged of contaminating social factors, science and technology studies over the past few decades have focused on illuminating *how* social or practical factors are woven into the practice of science—all science, that is, excellent as well as bad or simply run-of-the-mill science. Dozens of empirical studies demonstrate how factors that the conventional account

would suggest are contaminating or irrelevant to the legitimate business of science, including but moving well beyond scientists' expectations or beliefs, shape science every step of the way. These factors range from the choice of what constitutes interesting problems or "puzzles" for research, to understandings of what information in the world gets recognized as relevant data for the problem at hand, to the physical layout of labs or the choice of one technique over another, to the interpretation of experimental results (Gieryn 1999; Haraway 1989; Latour and Woolgar 1986; Mol 2002; Star 1989).

This book is indebted to recent work in science and technology studies, but it is distinct from recent STS work in two key ways. First, there has been a highly productive emphasis on ethnography and "lab studies" over the past few decades, in which analysts take the role of a naïve, curious, and nonjudgmental outsider whose task is to document the "strange native practices" of scientists. By taking scientific practices as "strange" and refusing either to bracket what scientists *say* is irrelevant or to foreground the conventional descriptions and explanations for what scientists do, these studies have yielded refreshing and often startlingly new ways of understanding scientific knowledge, the objects that science purports to know, and scientific ways of working and knowing. This approach also avoids the circularity common in conventional accounts of science (science is objective, because scientists put aside their biases, because science is objective . . .).

While I did not physically observe scientists in their work, I did interview them—and some of the interviews turned into ongoing collegial conversations. Still, I did these interviews as another scientist—a sort of "critical insider," rather than taking a more anthropological approach. The main way my analysis fits into the ethnographic thread in STS is that I focus very concretely on aspects of scientific practice, especially definitions and measures. I looked much more carefully at how scientists do their studies than at how they describe the theory in question. The gap between theory and practice is interesting, and readers will see that it is often informative to notice when practices and the "overarching narrative" of brain organization theory don't match up.

Second, as I noted above, I do not pay much attention to how so-called social factors shape the work, nor do I examine the "culture" of this branch of science. I look, instead, at technical matters that are well within the conventional account of what science is supposed to be about. I look at things like study designs, measures, and statistical practices, and I evaluate them in terms of accepted scientific practices that prevail today. The good news about this is that even hardheaded "realists" who prefer the older

view of good science as objective, and who believe that following scientific methods will yield a value-free reflection of nature, should be able to follow my analysis and agree that I am mounting a fair and reasonable evaluation of brain organization studies. The bad news, for me, is that the present study is apt to be seen as rather old-fashioned among STS scholars, because it is, essentially, a critique. In an age where most academic science watchers think it is somewhat passé (or at least boring) to point out that science-in-practice doesn't actually live up to science-in-the-ideal, critique has gone out of fashion.

I chose this method for a simple reason. I am trained as a scientist, and I value scientific method. I believe it is worth holding scientific research—especially high-profile research on a topic that is of great social and political importance—to the highest standards.<sup>5</sup> Brain organization theory is undeniably powerful, and it derives this power from a widespread belief that it is supported by excellent, state-of-the-art science. There is value in showing in great detail how this work does, and does not, measure up to that billing.

### A Three-Ply Yarn: Sex, Gender, and Sexuality

The language of sex, gender, and sexuality is more than vexing—it is confused, confusing, and contentious. And I am in the unhappy position, in this book, of writing at the juncture of multiple disciplines who use these same terms in irreconcilable ways. Clarifying my own usage is necessary, but I am going to attempt this in a way that neither casts my usage as “correct” nor places my analysis in firm opposition to the works that I analyze in this book.

Begin with the distinction between sex and gender. Since the late 1970s, many scholars have used the terms in a general way to signal biological (*sex*) versus social and behavioral dimensions (*gender*). But there has been an important difference in usage among people who followed this distinction. John Money's elaboration of the notions “gender role” and “gender identity,” first articulated in 1955 (Money, Hampson, and Hampson 1955b) and expanded in his landmark 1972 book with Anke Ehrhardt, *Man and Woman, Boy and Girl*, is the one adopted by nearly all brain organization researchers:

*Gender identity*: The sameness, unity, and persistence of one's individuality as male, female, or ambivalent, in greater or lesser degree, especially as it is experienced in self-awareness and behavior; gender identity is the private expe-

rience of gender role, and gender role is the public expression of gender identity.

*Gender Role:* Everything that a person says and does, to indicate to others or to the self the degree that one is either male, or female, or ambivalent; it *includes but is not restricted to sexual arousal and response*; gender role is the public expression of gender identity, and gender identity is the private experience of gender role. (Money and Ehrhardt 1972, 4, emphasis added)

Notice two key things: eroticism, the realm of sexual attractions, fantasies, and behaviors, is entirely subsumed under gender in this usage. And note, too, that there is nothing in this definition about etiology. Without positing causation, Money's distinctions between gender and sex are at the level of location, or possibly measurement: that is, gender is located, perceived, and measurable at the level of behavior and language; sex is located, perceived, and measurable at the level of the physical body.

Money thought of gender as "his" term, and the record does seem to support his claim to at least the popular rise (if not the actual invention) of the term *gender* to describe people as feminine or masculine (Ehrhardt 1988; Gooren and Cohen-Kettenis 1991; Bullough 1994). It was a matter of great irritation to him that feminists, in particular, deployed the term with a critical difference (Money 1980). Perhaps the greatest credit in this regard is due to anthropologist Gayle Rubin's (1975, 159) classic identification of the "sex/gender" system, which she identified as a "set of arrangements by which a society transforms biological sexuality into products of human activity." Rubin's distinction placed the emphasis squarely on etiology. That is, *gender*—the attribution of aspects of human personality, relationships, behaviors, privileges, and prohibitions to the domain of either "masculine" or "feminine"—is a social effect, rather than the result of human biology. *Sex*, in this regard, is conceived as the remainder—the material body, and those bodily interactions that are necessary to reproduce it.

In addition to the disagreements between mainstream psychologists and medical doctors, who favored Money's usage, and feminists, social scientists, and scholars in the humanities, who generally favored Rubin's, several later developments have further complicated the terminological field. For one thing, there is the matter of sexual desires and pleasures and bodily interactions. Money subsumed this domain entirely under gender, and Rubin initially conceptually distributed it (unevenly) across gender and sex. That is, male and female bodies must obviously meet (at least historically!) in order to accomplish the biological necessity of human reproduction. But the elaborate and particular rules about the *ways* in which bodies meet, touch, and interact, and the designation of some forms of de-

sire and pleasure as “masculine” versus “feminine” as well as “natural” versus “unnatural,” belonged to the realm of the social rather than the “raw material” of biology. Anthropologists, philosophers, sociologists, and historians increasingly designated this latter domain as “sexuality” (Foucault 1978; Weeks 1986; Vance 1991). Rubin’s work was especially influential in articulating that sexuality and gender overlap and yet need to be distinguished as two “distinct arenas of social practice” (Rubin 1992). Central to Rubin’s argument was a growing body of research that undermined the usual assumption that sexuality is grounded in and emerges “naturally” from the biological drive to reproduce. The best-known work on social construction of sexuality is that of the late historical philosopher Michel Foucault, who argued that *sexuality as a domain is itself a social product*. Two key claims in his work are, first, that certain sensations, bodily parts, desires, and forms of interaction are not “naturally” sexual, but *become* sexual (or get designated as sexual) through specific social practices that vary across time and place. The second point, which flows from the first, is that the relationship between the social world and sexuality is not merely one of regulation or imposing limits (a “negative force”), but one of production and instigation (a “positive force”). These points, supported by an ever-growing body of anthropological, sociological, and historical studies, suggest that sexuality is fundamentally contextual, rather than comprising a universal set of desires, sensations, and acts (D’Emilio 1983; Mosse 1985; D’Emilio and Freedman 1988; McClintock 1995; Katz 1995; Stoler 2002).

A classroom exercise developed by Carole Vance, a medical anthropologist who has been extremely influential in feminist sexuality studies, may help bring this discussion down to earth. Suppose a Martian shows up at your home and asks you to explain what sexuality is. What do you say? Reactions to this question can be quite varied, and in my experience, students generally begin with a great deal of confidence that they can arrive at a good answer, but their certainty breaks down. Someone offers “Activities that are related to human reproduction,” but that is met with counter questions like “What about oral or anal sex?” Someone else might note that most heterosexuals go to a lot of trouble to make sure that most of their sexual behavior does not result in reproduction. (Of course, some people might answer that sexuality *should* be about reproduction, but as a matter of definition, it is no defense, because if one simply discounts all other activities from being sexual, then there would be no complaints about all the “wrong” or “sinful” expressions of sexuality! These things simply would not count.) Predictably, some other brave soul will offer the definition “Touching genitals” as sexual (hmm: gynecological exams?

touching breasts?) or “Becoming physically aroused by touching someone else” (masturbation? fantasy? a sexual encounter that doesn’t really arouse one of the partners?). I could go on, of course, but presumably you begin to see the scope of the difficulties. And this is without even entertaining differences across cultural groups or historical periods. Try this yourself, and I guarantee that if you come up with a really solid answer that can withstand all the counterexamples that any three or four smart adults can offer, you have an immediate career in sexuality studies waiting for you.

But back to my dilemma. For some time, I have thought of sex, gender, and sexuality as a three-ply yarn. I find the metaphor appealing because it suggests three strands that are simultaneously distinct, interrelated, and somewhat fuzzy around the boundaries. As a double entendre, “three-ply yarn” also suggests the narrative aspects of the domains and their relations with one another (Young 2000). In other words, the *perceived* relationships among bodies, desires, and a wide range of social norms governing roles and interactions are central to the stories that we tell ourselves about human nature and the meaning of maleness and femaleness. Following dominant usage in the humanities and social sciences as well as feminist biology (for example, Vance 1989; Butler 1990; Laqueur 1992; Oudshoorn 1994; Kessler 1998; Fausto-Sterling 2000), I have used the term *sex* to refer to characteristics of the physical body, *gender* to refer to psychological attributes and social behaviors that are associated with masculinity and femininity, and *sexuality* for the realm of erotic desires and practices.

The key way that these distinctions have been useful is to enable critical questioning of the relationships among phenomena that are usually taken to be inseparably fused or causally related. Without questioning these assumptions, we cannot probe the boundaries among them, nor perceive shifts in these boundaries. Nor can we investigate causal processes if the theory of causation is already built into our definition of the phenomena. But the slippery nature of the relationships among sex, gender, and sexuality has made it difficult to pin them down long enough to systematically study them: scholars have encountered ongoing difficulty with assigning phenomena definitively to one domain versus the other.<sup>6</sup> An example that is relevant to the story at hand, and the particular dilemmas of terminology that I face in telling it, is that empirical research has revealed the relation between sex and gender to be even more complex than originally thought.

Works by Nelly Oudshoorn and Suzanne Kessler have been especially important for my own understanding. In a text that deserves much wider audience than it has received, Oudshoorn (1994) advanced the somewhat astonishing claim that “sex hormones” were “invented” by twentieth-

century endocrinologists. Oudshoorn does not dispute that steroid hormones like estradiol, testosterone, or progesterone are actual material substances, nor does she claim that these substances were not already being produced and used by bodies long before scientists had any notions about them. Her argument specifically concerns the idea that certain steroid hormones are fundamentally *about* and *for* sex. She documents how preexisting ideas of masculinity and femininity caused scientists to look for, create tests for, classify, and perceive steroid hormones in a way that fit them into a dualistic system of sex. This might not be particularly compelling if she didn't also document how this commitment to a fundamentally sexual classification of hormones systematically blocked some kinds of information. For example, scientists had repeated difficulty assimilating the information that both males and females produce and use *both* androgens ("male sex hormones") *and* estrogens ("female sex hormones"). Likewise, biologist Marianne van den Wijngaard (1997) has documented the great difficulty mid-twentieth-century experimental psychologists had with the information that estrogen—the quintessential "female" hormone—was even more important than testosterone for the development of certain "masculine" characteristics. Van den Wijngaard and Anne Fausto-Sterling (2000) have both shown how a dualistic notion of "sex hormones" has been a repeated stumbling block in recognizing the great variety of functions that steroid hormones accomplish, apart from those related to development and function of reproductive capacities. In this vein, a colleague and I recently confirmed that attachment to the term and concept *sex hormones* is associated with systematic misinformation in widely used high school biology texts (Nehm and Young 2008).

Classifying some substances as "sex hormones," then, presumes that we already know the most relevant functions, divisions, and even ultimate "purposes" of these substances. That is not the strongest perspective from which to launch a rigorous scientific program to explore and elucidate the mechanisms of hormonal action. This insight has obvious relevance to my own evaluation of how early steroid hormone exposures may shape the developing brain. The works by Oudshoorn, van den Wijngaard, and Fausto-Sterling form an important backdrop against which I have worked. But I have aimed to take the studies I analyze here on their own terms as much as possible. Thus, the book is littered with the terms *sex hormones*, *androgens* (literally, "that which creates men"), and *estrogens* ("that which induces estrus," the fertile period in some female mammals). I often place the term *sex hormones* inside quotation marks as a reminder to readers that this is a philosophically invested way to conceptualize the substances I am discussing. I do not put quote marks around *androgens* or *estrogens*, mostly because I found that doing so made the text too clunky



and conveyed a less open, less neutral attitude about specific brain organization studies than I had during this research.

A second example of work that problematizes the now-conventional distinctions among sex, gender, and sexuality concerns studies on intersexuality, especially Suzanne Kessler's crucial *Lessons from the Intersexed* (1998). The term *intersex* typically describes a person who is born with "mixed" male and female bodily characteristics: for instance, the female (XX) chromosome pattern but a penis and no vagina, or the male (XY) chromosome pattern but a vulva (including labia and clitoris) and vagina. The most common intersex conditions involve steroid hormone atypicalities, such as a genetic male who produces but cannot respond to testosterone and so develops "female" structures, or a genetic female who produces unusually high levels of androgens, which masculinize her own developing genitalia. An enormous proportion of brain organization research has been conducted with intersex subjects, because many scientists believe intersex people offer an opportunity to study the effects of hormones that "disagree" with gender socialization.

Kessler's groundbreaking analysis showed how ideas about gender and sexuality are called upon by medical doctors and psychologists, both to determine the male or female status of bodily structures and to reshape those structures so that they can be seen as fitting with "normal" male versus female bodies. For example, Kessler explores the centrality of the idea that "boys urinate standing up" in determining whether a phallic structure can, or cannot, count as male. She and, more recently, Katrina Karkazis (2008) have also demonstrated how doctors who conduct "feminizing" surgeries on intersex infants deploy a definition of "normal" female sexual function that is built around vaginal penetrability rather than around the capacity for sexual pleasure and orgasm. Together with analyses of biology and endocrinology, these studies add up to a take-home message that gender causes us to perceive the natural world (the body) in a particular way, and thereby to impose upon it the dichotomous category "sex." Sex, then, is no longer the raw material from which culture produces gender. Instead, sex is in some important sense an *effect* of gender.

Here is another interesting twist. The sex/gender distinction has broken down on the other side of the disciplinary divide as well. While feminist scholars (to oversimplify a bit) increasingly see sex, gender, and sexuality as all proceeding from the same interrelated processes of materially based social relations, an increasing number of biologically oriented thinkers, especially psychologists, see the whole complex as proceeding from biological processes. In some sense, the entire foundation of brain organization theory takes this idea as its starting point, and researchers devise studies that are intended to demonstrate a causal trajectory from the sexed body

to gendered behavior, under which most of them subsume sexuality. This fundamentally biological process is in turn often taken to be the basis for broader social structures related to gender. There will be much more on this throughout the book, but for now the point is merely to note that the sex/gender/sexuality distinction has been questioned in the very realm that forms the basis for this study. What, then, is the best way to use these terms in an analysis of brain organization research?

I have settled on this. I am returning to my three-ply yarn of sex, gender, and sexuality, but with an additional twist. It is necessary to explicitly state that my usage, at least in this book, is agnostic as to etiology. In particular, I will not presume that gender is the result of social relations, because to do so would be to discount the studies I review at the outset. Nor, however, will I presume that (bodily) sex is simply given by nature and is not a result of socially inflected classifications and commitments. Finally, I will not presume that sexuality, the realm of pleasures and desires and practices as well as erotic self-concept, flows neatly from either the exigencies of reproduction or the structure of masculinity and femininity. There are two key phrases for which I make an exception and use “sex” where I might, in another work or for another purpose, use “gender.” *Sex differences* is a phrase used widely in both popular and scientific work, and I have chosen to use that term rather than *gender differences* much of the time, reserving the term *gender difference* for those situations that are clearly restricted to aspects of psychology and/or behavior. Although the popular and scientific usage of the term *sex differences* does often refer to psychology and behavior, there is often an implicit or potential physiological claim involved, as with the idea that behaviors are a direct reflection of brain structure or function. Likewise, I have chosen to retain the term *sex-typed interests* for a core domain of psychology that some scientists link to brain organization theory. I keep the term largely because I found it created too much confusion to jump back and forth between the term common in the studies and the term I preferred (*gendered interests*). There will probably be other times when I am describing the positions and theories of brain organization researchers where the usage I describe here may seem to falter. But this does reflect my fundamental intentions in usage for the terms throughout this book.

## Structure of the Book

The discussion in this book is organized in three parts. I begin with introductory material concerning emergence of the theory, and my own analytic

approach. Chapter 2 explains the theory itself in more detail, and provides some historical context to help readers get a better feel for where this theory came from and how the human research fits with animal studies. It also explains, crucially, why brain organization research in humans has focused so often on particular clinical syndromes and on homosexuality. Chapter 3 covers some basic information about research design, particularly elaborating the importance of understanding brain organization research as comprising a “quasi-experimental network.” In this chapter, I introduce several symmetry principles that guide the core analyses in the remainder of the book.

Next I present specific symmetry analyses. Chapters 4 and 5 examine, in turn, the two broadest sets of brain organization studies in humans, those that begin with people for whom scientists have some information about early hormone exposures (cohort studies), and those for whom scientists don’t have information about hormone exposures, but instead have information about sex-related characteristics (“case-control” studies). Within each of these designs, studies can be broadly grouped to see the overall pattern of findings for studies that have both similar “inputs” and “outputs” (the most basic sort of symmetry that is described in Chapter 3). The analysis in these chapters is extremely straightforward. I take the findings of particular studies almost completely at face value, and simply highlight how the various studies do and do not support one another, and in some cases show how they set up important internal contradictions. From these chapters, certain kinds of claims about brain organization can be put aside as unsupported, but others appear to merit closer investigation. Chapters 6, 7, and 8 take a closer look at three specific domains in which initial evidence suggests that human brains may be “organized” by early hormone exposures: feminine or masculine patterns of sexual response and behavior, sexual orientation, and sex-typed interests. Each of these chapters constitutes a detailed examination of the studies providing evidence for one specific domain, with special attention to how the “outcomes” of brain organization—that is, the traits that are purportedly influenced by hormones—are actually defined and measured. Chapter 5, for example, examines studies used to support claims that prenatal hormones create permanent “templates” for masculine or feminine sexuality. Although the researchers were mostly interested in what *causes* masculine or feminine sexuality, I am mostly concerned with what they think masculine or feminine sexuality *is*. (I am sure most readers will find the extent of scientific disagreement on this point as surprising as I did!)

In the third section, I begin to look forward to a new kind of sexual science, and indeed, a new science of human development. In Chapter 9 I ex-

tend the critique of previous chapters by offering some detailed consideration of how crucial elements of context, both biological and social, have been disregarded in nearly all brain organization studies. Chapter 10 offers some thoughts for moving toward a more sophisticated, scientifically accurate theory of embodiment, including but going beyond issues of gender and sexuality. Here I describe some critical experiments in genetics and developmental biology that suggest how context and processes matter, and how we might do a better job of studying this.

### Toward the Open Door

The body unquestionably matters in shaping who we are, what we want, and how we behave. The devil is in the details. One frustrating aspect of brain organization research is that its prominence has gotten in the way of serious and sophisticated research on how biology and the social world interact to shape behavior. I hope this book will point to some potentially more fruitful directions for research on the material and social bases of temperament, skills, desires, and self-concept. Many people have asked me, as I've discussed brain organization research over the years, to comment on how I think psychosexual development *really* works—how do we become the gendered and sexual beings that we are? I must confess ahead of time that I will not present the real and true process of human development. This will be a disappointment to some readers who are seeking a fully developed alternative theory. But for those of you who find open doors inviting rather than vertiginous, I have a number of ideas. Toward the end of the book, I begin to sketch some of the methodological approaches that I think will be more promising. But first I want to open doors that are closed by the “brain organization” story—and that requires painstaking work to move the giant pile of data that sits in front of the door, looking very solid indeed. In the chapters ahead, I show that the evidence for hormonal sex differentiation of the human brain better resembles a hodgepodge pile than a solid structure, and it is a pile that blocks from view the complex and fascinating processes of biology. Once we have cleared the rubble, we can begin to build newer, more scientific stories about human development, stories that narrate genuine exchanges between matter and experience, and that incorporate random events.

Bodies matter—genes matter, hormones matter, brains matter. But how? Even though I'm not entirely certain about the answer, I am certain that we have enough information to eliminate the simplistic story of brain organization as it's currently understood. Let me show you why.