CHAPTER SEVEN

Sexual Orienteering

Orienteering, n. 1. The action of determining one's position correctly. 2. A competitive sport in which runners have to find their way across rough country with the aid of a map and compass.

WHY ARE SOME PEOPLE gay and other people straight? This question is quite intriguing to some scientists, judging by the amount of time and money they put into looking for evidence that hormones influence sexual orientation. And the public seems very interested, too, because studies about the possible origins of homosexuality always get great news coverage. But sexual orientation is notoriously difficult to define. So when a headline proclaims, "Prenatal Environment May Dictate Sexual Orientation," just what is it, exactly, that is said to have been dictated?[1] Is it whom someone desires? Whom one has sex with? What a person calls him or herself? Because these various aspects of sexuality don't always point toward the same orientation, it matters very much which one a scientist picks. In this chapter, I'll show that brain organization research on sexual orientation is fraught with internal contradictions. In particular, scientists' extremely different ways of measuring "homosexuality" have contributed to a network of studies that look convincing and mutually supportive on the surface, but in fact are fundamentally at odds with one another.

In interviews, when I asked prominent scientists who study brain organization what they thought about various theories of sexual orientation, most of them were uninterested in these questions. Dr. A, whom I introduced in Chapter 6 by way of his comments on the "commonsense" difference between masculine and feminine sexuality, told me a story to illustrate his conviction that current theoretical work on sexuality is pointless

and distracting. Referring to the Klein Sexual Orientation Grid (Klein, Sepekoff, and Wolf 1985), a measure that treats sexual orientation as multidimensional and changeable over time, Dr. A's comments show how brain organization researchers filter out the complications that trouble scientists in other fields:

> Fritz Klein had some complicated grid for determining sexual orientation, and [Dr. N], who was doing the bulk of these interviews, said, "You know, these are all nice scholarly heuristics, but 99 times out of a hundred, listen to the subject and they'll tell you who they are." You don't run into too many straight men who lie and say they're gay. You may have some gay men who will tell you they're heterosexual, but not if they know that the confidentiality of the project is high. And under questioning and interactions and interchange, as well as filling out questionnaires, if they're consistent, we keep them as subjects. If they check off straight and then they start checking off gay and then they tell you straight and they tell you gay, we're not sure what's going on. We're not sure if the person is just having fun with us, we're not sure if the person is themselves not sure of who they are, and when that would happen, we'd exclude them from participating further in the study. (Dr. A. interview, January 21, 1999)

To Dr. A and Dr. N, who are among the most influential scientists in the world studying biological influences on human sexuality, subjects who give equivocal or contradictory answers to questions about sexual orientation are either obstructive or confused. Ironically, Dr. A's admonition to "listen to the subject" ends up being qualified by "*if* they're consistent." Dr. A doesn't consider the possibility that subjects' hedging and ambiguity reflect meaningful complexity—that the phenomenon of sexual orientation *is* complex and sometimes ambiguous. Instead, he thinks of sexual orientation as a simple categorical trait—the objections of modern intellectuals and old cranks like Alfred Kinsey notwithstanding, you *can* sort people into discrete types. In this view, subjects who don't fit the profile are simply lying, or perhaps more charitably, self-deluded.

But in more than two decades of conducting sexuality research, I've noticed that subjects sometimes object to questions when they feel forced into unsatisfying choices to describe their sexuality. More often, they struggle to provide the "right" answers, and this can lead to confusing and contradictory responses. In fact, that's one major reason to have multiple questions that are supposed to assess the same thing—not to screen out "bad" research subjects, but to screen out bad questions. Perhaps Dr. A's subjects hedge and equivocate because his questions or response categories are not adequately nuanced. How much same-sex attraction or even experience is required to invalidate someone's identity as straight, and vice

versa? Such questions are literally out of the question in brain organization research.

While other scientists vigorously debate what sexual orientation is and how best to measure it, brain organization researchers almost never address the fundamental questions involved. The majority of studies linking early hormone exposures with human sexuality have focused primarily or exclusively on sexual orientation—yet most brain organization studies that are "about" sexual orientation either have not defined sexual orientation at all, or have used vague and contradictory definitions that often do not agree with the measures scientists have used.[2] In the last decade or so, scientists who study prenatal hormones and sexual orientation have been somewhat more likely to include definitions and even detailed information on measures in their research reports, but most continue to simply declare that their own definition is best, leaving the impression that measures are mundane and predictable rather than plucked from a wide range of alternatives. However, as we shall see, the "quiet" disagreements among brain organization scientists about sexual orientation are profound.

The point is not that some scientists choose the "wrong" measures for sexual orientation. The point, instead, is that the measures they choose often put them at odds with one another. As a result, their studies just don't add up to the consistent findings about brain organization that are commonly claimed. In fact, a good many of the studies supposedly showing that prenatal hormones influence sexual orientation *contradict* other studies that supposedly show the same thing. Between slippery measures and other problems, such as illogical dose–response inferences about hormone effects, the overall evidence that early hormones affect sexual orientation is murky, at best.

This chapter focuses first on problems related to measurement and next on inconsistent patterns of evidence in the studies. I am particularly interested in how scientists define homosexuality in this body of research, and in the way measurement problems amplify some breaches in evidence that appear when comparing the major subsets of studies. That is, some studies start with hormones, and other studies start with endpoints (sexual orientation). Somehow the evidence from all these studies should meet up, but it does not. In part because researchers have very different notions of what counts as homosexual (usually related to what measures will give "better" results in the individual studies), it's impossible to get the findings to align in a way that supports brain organization theory.

I explore measurement of sexual orientation by focusing on three major issues. First, what part of sexuality *is* sexual orientation? Some of the most commonly identified elements include the sex of actual sexual partners;

self-description as heterosexual, homosexual, or bisexual; the sex of the people one falls in love with; and (the one that most scientists prefer) the degree of desire for same- or other-sex partners. Moreover, is there a single, definitive criterion, or do multiple aspects of sexuality constitute orientation? Second, do scientists think of sexual orientation as being toward men versus women, or toward one's own sex versus the other sex? This distinction might seem trivial, but it turns out to have major implications for study design, because it determines which people are considered to have similar versus contrasting sexual orientations. That is, are lesbians like straight men, because both are attracted to women? Or are they like gay men, because both are attracted to people of their own sex? The third question involves issues of quantity: what are the cutpoints, or boundaries, between heterosexuals, homosexuals, and bisexuals? How much interest in or sexual activity with people of the same sex disqualifies someone from a heterosexual orientation, and vice versa?

In the second part of this chapter, I again use symmetry principles to think about how the findings in studies of sexual orientation align with one another. My main strategy is to compare the two major kinds of brain organization studies, cohort studies and case-control studies. As I described in Chapter 3, quasi experiments like those used in human brain organization studies have to be judged together, not individually. With quasi experiments, it is especially important to see if there is triangulation of findings, meaning that evidence from studies with significantly different designs should point in the same direction. Do the cohort studies (where scientists begin with early hormone exposures, then investigate psychosexual characteristics) point to similar conclusions as the case-control studies (where scientists begin with "distinct" psychosexual groups, then look for evidence that these subjects had different prenatal hormone exposures)?[3] As you will see, the very different pattern of measurement in the cohort compared with the case-control studies is a key factor in judging how the findings stack up across the two sets of studies.

After considering the overall pattern of findings, I return briefly to the issue of generalizability. In research on sexual orientation and prenatal hormone exposure, no one is really interested in the prenatal hormone histories of specific individuals who are gay or straight: people want to know something about the mechanisms of sexual orientation development in general. So the question of generalizability is a critical issue, and the major question that must be answered is whether "homosexuality" in brain organization research means the same thing as "homosexuality" outside the lab.

Let us begin, then, with the basics: what *is* sexual orientation?

Measurement Issue #1: Constituents of Sexual Orientation

Sexual attraction? Behavior? Love? Identity? Sexual orientation is a great example of a "commonsense" concept that seems fairly transparent, but turns out to be complicated and slippery when you try to pin it down. And deciding *how* to pin it down generally requires knowing *why* you want to pin it down. In epidemiological work on HIV/AIDS, for instance, it is often more important to know what people *actually do* sexually than what they would *prefer* to do. But that is not always the case. Depending on one's objective, it might be most important to understand how people think of themselves (so that we can devise and target public health campaigns in which people can "recognize" themselves), or to understand what specific sexual practices they enjoy and value (so that we can help them negotiate and maintain safe behaviors) (Young and Meyer 2005). So just what is the best indicator of sexual orientation for brain organization research?

It seems wise to begin by considering the experimental foundation for brain organization studies, that is, to begin with the animal models for sexual orientation. When scientists draw parallels between animal behavior and human homosexuality, they nearly always make a series of assumptions that draw a line between male-typical sexual behaviors and orientation to females, on the one hand, and female-typical sexual behaviors and orientation to males, on the other. Their reasoning goes like this: in animals, *masculinizing* hormones lead to the development of "male-typical" sexual behaviors, such as mounting. An animal who mounts requires a partner who allows itself to be mounted (or, in the case of rodents, facilitates a partner's mounting by performing lordosis, raising the rump). Because allowing a partner's mount or performing lordosis is considered "female-typical" behavior, scientists reason that an animal who exhibits mounting is signaling a preference for female partners. An animal who performs lordosis or allows another animal to mount it is taken to be signaling a preference for male partners. (It's worth knowing the fact that normal animals of both sexes both mount and are mounted, and that mounting behavior can be about other things besides sex, like play or dominance. Certainly, mounting is much more common in males, and lordosis or receiving mounts is more common in females, but even these definitively masculine or feminine behaviors aren't the sole purview of either sex. This does not necessarily discredit the model, but it suggests that characterizing even rodent sexual behavior in dichotomous "masculine" or "feminine" terms is a simplification that might foreclose other ways of thinking about sexual variability.)

Researchers have long interpreted associations between *masculinizing* hormone exposures (which, as you will recall from earlier chapters, can be either androgen or estrogen exposures) and mounting behavior to mean that *masculinizing hormones lead to an orientation toward females, regardless of sex*. By the same reasoning, researchers suggest that *feminizing* hormones (or a lack of masculinizing ones) lead to an orientation toward males.

The chain of reasoning that equates male-typical sexual behaviors (especially mounting) to male-typical sexual orientation has been subject to devastating critique (Byne and Parsons 1993; Fausto-Sterling 1985). Briefly, as an animal model for human homosexuality, it is a logical error to designate just one individual in a same-sex pair as "the homosexual" (that is, a female who mounts, or a male who is mounted). Moreover, according to this model, even male–female pairs can be interpreted as "homosexual" if a female mounts a male (a not too rare occurrence, even among animals not experimentally treated with hormones). Of course, human homosexuality is not typically understood as the preference for particular behaviors or sexual positions, but is understood as a preference for partners of one's own sex. In response to such critiques, animal researchers increasingly conduct studies that assess actual preference for female versus male animals rather than testing for the display of certain "sex-typed" sexual behaviors (Watabe and Endo 1994; Adkins-Regan 2002; Roselli, Larkin, Resko, et al. 2004; Bodo and Rissman 2007).

A Closer Look at Animal Sexual Preferences: Rams on the Down Low?

Unfortunately, some of the contexts in which animals are tested for "sexual" preferences are still not necessarily good models for human sexuality—for example, scientists might measure whether an animal spends more time investigating bedding that has been used by male versus female animals (Bodo and Rissman 2007), or simply spends more time "visiting" male versus female animals when given the opportunity to approach tethered conspecifics of either sex (Watabe and Endo 1994). Enormous assumptions must be made to interpret these measures as sexual orientation. For example, is time spent with a tethered female an indicator of sexual interest in the female, or an indicator of some aversion to the male? These are not the same thing, because the latter might just be conflict avoidance.

The most stringent sexual orientation test that any team has used was devised by Charles Roselli and his colleagues at Oregon State University (Resko et al. 1996; Roselli, Resko, and Stormshak 2002; Roselli, Larkin, Schrunk, and Stormshak 2004). Their three-part test for "male orientation" among rams consists of the following:

> Only rams that would not mount estrous females in the preliminary tests satisfied the criterion for entrance into the Sexual Preference Paradigm. . . . Briefly, in November and December of the second year of life, rams were exposed simultaneously to two restrained estrous females and two males that were chosen at random for use. Rams that courted and mounted males in preference to females during a 30-min test that was repeated at least three times were classified as male-oriented. Male-oriented rams were given an additional preference test 5 days before they were killed. Using these procedures, we identified six males that would not mount females in the preliminary sexual tests, mounted males in a group setting, and mounted males in preference to females in the sexual preference tests. (Resko et al. 1996, 121)

It turns out, though, that most, if not all, of these "male-oriented" rams are having sex with ewes when the scientists aren't looking. In a study that was designed to test the relationship between successful reproduction (siring of lambs) and behavioral measures of "sexual performance," 5 male-oriented rams impregnated at least 330 ewes over 21 days, resulting in more than 480 offspring.[4] The number of ewes they impregnated and the number of lambs sired fell roughly midway between the groups of rams whose behavioral tests had classified them as "high performing female-oriented" and "low performing female-oriented" (Stellflug, Cockett, and Lewis 2006). In behavioral tests after breeding, only 3 of the 5 "male-oriented" rams exhibited *any* sexual activity, and 2 of the 3 continued to show a strong (though not exclusive) preference for mounting other rams. Offering the understated conclusion that "individual sexual partner preference tests did not absolutely reflect the breeding performance of the male-oriented rams," the scientists speculated that aspects of the testing environment differed sufficiently from the "competitive breeding environment" to make the preference test an imperfect predictor of rams' mating behavior (466).

There are two take-home messages from this brief discussion of animal research on sexual orientation. First, it is not an easy task to come up with good animal models for partner preferences, and much research focuses on animals' *sexual positions* rather than on preferred category of partners. Second, even when sexual preference tests in animals focus on identifying partner preferences, the preferences that look like a "stable, dispositional trait" in one context look unstable and fluid in other contexts. (Of course, it's not really clear which aspects of the context are relevant for inducing different sexual behaviors in these rams. It's tempting to speculate that these rams, like an awful lot of humans, act differently when their aim is procreation than when it is recreation. At any rate, given the number of gay people who sometimes have sex with other-sex partners, and straight people with same-sex partners, maybe the sheep is a better animal model for human sexual behavior than we thought!)

What about humans? Social scientists and public health researchers who study human sexuality spend a great deal of time discussing what counts as sexual orientation, and debating how best to measure it (Klein, Sepekoff, and Wolf 1985; Laumann et al. 1994; Sell 1997; Solarz 1999; Young and Meyer 2005). Some suggest it is best defined by the sexual attraction or desires one feels, while others point to patterns of falling in love or pair-bonding, the (physical) sex of sexual partners, the *gender* (masculinity or femininity) of sexual partners, or even whether one identifies as straight, gay, lesbian, or bisexual. At first blush, though, there seems to be much greater consensus among researchers conducting brain organization studies. For one thing, they all agree that sexual orientation is a "status," meaning that what people do, or what they call themselves, might change, but the basic character of their desires is fixed from a very early stage of development. John Money, always an imaginative writer, explained the difference between a status and sexual behavior via the elaborate example of a "crazed sex-terrorist":

> The Skyscraper Test exemplifies the difference between act and status. One of the versions of this test applies to a person with a homosexual status who is atop the Empire State Building or other high building and is pushed to the edge of the parapet by a gun-toting, crazed sex terrorist with a heterosexual status. Suppose the homosexual is a man and the terrorist a woman who demands that he perform oral sex with her or go over the edge. To save his life, he might do it. If so, he would have performed a heterosexual act, but he would not have changed to have a heterosexual status. (Money 1987)

Of course, the idea that sexual orientation is organized by early hormone exposures depends on such a definition, because the notion of organization invokes a point of irrevocable commitment of structure or function in the developing organism. Importantly, the reverse is not true—that is, thinking of sexual orientation as a status that is stable from very early in life does not depend on any particular theory about how sexual orientation develops. Thus, it is not the case that skeptics of brain organization theory necessarily reject the idea that sexual orientation is a status. In any case, let us turn away from this single point of agreement among brain

organization researchers to consider their many points of *disagreement* about what sexual orientation is.

If "Kinsey 5" Was the Answer, What Was the Question?

A common measurement tool that helps maintain the illusion of broad agreement among scientists is the familiar "Kinsey Scale." Kinsey rated men's "psychologic reactions and overt experience" as follows:

0 Exclusively heterosexual with no homosexual
1 Predominantly heterosexual, only incidentally homosexual
2 Predominantly heterosexual, but more than incidentally homosexual
3 Equally heterosexual and homosexual
4 Predominantly homosexual, but more than incidentally heterosexual
5 Predominantly homosexual, but incidentally heterosexual
6 Exclusively homosexual.(Kinsey, Pomeroy, and Martin 1948)

Investigators generally write as though "Kinsey scale" fully explains a method of assessing sexual orientation. Yet a Kinsey scale is a format for *answers;* it does not explain what *questions* were asked. There are in fact many different kinds of questions that one might ask about sexual orientation and answer with a Kinsey scale. Kinsey intended the scale to reflect the heterosexual–homosexual balance in people's sexual *activities and responses.* It was not meant to be a tool for rating people *per se.* In fact, Kinsey was adamantly opposed to the use of the terms *heterosexual, bisexual,* or *homosexual* to refer to people, in part because he thought it implied a biological basis for homosexuality, which he thought was an extremely silly theory. Historian Stephanie Kenen has wryly observed that "Kinsey's rating scale was not a resounding success" in this regard, because "the ratings themselves have become reified categories of identity, and references to 'Kinsey 4s' and 'Kinsey 6s' are not uncommon in popular parlance or scientific literature" (Kenen 1997, 208–209).

Alfred Kinsey has posthumously lost the battle over using his famous scale to rate people, rather than behaviors, as homosexual versus heterosexual, but there are still many questions to resolve among those who use the scale to rate people. Which aspects of sexuality are most interesting and important? Brain organization researchers strenuously disagree about this, and various scientists have chosen many different dimensions to measure with Kinsey scales, such as

Sexual behavior (have sexual contacts been with men, women, or both?) (e.g., Lindesay 1987)
Identification with a particular sexual orientation term such as *homosexual, bisexual,* or *heterosexual* (e.g., Holtzen 1994)
Sexual fantasies (are they about men, women, or both?) (e.g., Bailey, Willerman, and Parks 1991)
Composite assessments of night dreams, day dreams, erotica, masturbation fantasies, sexual attractions, and sexual partners (e.g., Meyer-Bahlburg et al. 2008)

All this variety is often obscured by the simple notation that a study "employed Kinsey scales to rate subjects as homosexual, heterosexual, or bisexual." Reading beyond the studies that employed Kinsey scales, the manifestations of sexuality that count as sexual orientation continue to expand. For example, John Money saw sexual orientation as comprising multiple dimensions, but believed that *romantic love* rather than sexual behaviors, fantasies, or even sexual attraction or arousal was "the definitive criterion of homosexual, heterosexual, and bisexual status" (Money 1987).

Even the most widely accepted definition of sexual orientation as "sexual attraction" to a certain category of partners does not resolve the issue, because opinions differ about the best indicator of sexual attraction. An excellent example of disagreement about measuring sexual attraction is found by contrasting Dr. A, mentioned above, with Michael Bailey, another prominent psychologist who does brain organization research. Recall that Dr. A suggested that people can "tell you who they are," so this would suggest you could also simply ask people if they are attracted to men, or women, or both. Bailey, though, believes that measures of physiological arousal are superior. In a study that made the front page of the *New York Times,* Bailey's team, led by graduate student Gerulf Rieger, compared subjective attraction to a measure of physiological response (Rieger, Chivers, and Bailey 2005).[5] The team had advertised for "'heterosexual,' 'bisexual,' and 'gay' men for a paid study of sexual arousal," and men who volunteered "were asked about their sexual attraction toward men and women" as well as "their sexual identity as straight, bisexual, or gay" (580). Each man was then tested with a penile plethysmograph, which involves showing the man explicit sexual images while measuring his penis to detect circumference changes associated with erection. The stimuli in this study were four short erotic films, two of which showed two men having sex with each other, and two of which showed two women having sex with each other. (To avoid confusion regarding which person in the film

was arousing to the research subjects, the scientists did not use films of men and women having sex together.) As expected, the men who said they were attracted to men were more aroused by pictures of men, and the men who said they were attracted to women were more aroused by pictures of women. But the men who said they were attracted to both men and women, instead of showing arousal patterns that were intermediate, mostly showed more erectile response to the male images (a few mostly showed response to the female images). Bailey concluded from the study that "in men there's no hint that true bisexual arousal exists, and that for men *arousal is orientation*" (quoted in Carey 2005).

This study got a great deal of attention, but very little of it was critical (for exceptions, see the letter by Barker, Iantaffi, and Gupta [2006] and the comments by Fritz Klein, Gilbert Herdt, and Randall Sell in Carey 2005). It provides a dramatic example of the variety of measures that are available even for apparently obvious aspects of sexual orientation like "erotic attraction." It also shows how subtle inconsistencies between abstract definitions and concrete measures slip into studies. Bailey's comment that "*arousal* is orientation" omits other aspects of orientation that he and his co-authors identified when they wrote: "Sexual orientation refers to the degree of sexual attraction, fantasy, and arousal that one experiences for members of the opposite sex, the same sex, or both" (Rieger, Chivers, and Bailey 2005). What is the justification for dropping out subjective attraction and fantasy? The report also begs the question of choosing between different measures for the same aspect of orientation—in this case, arousal. That is, even if we could agree that arousal is the most important or "core" aspect of orientation, Bailey's team doesn't offer any rationale for judging the *physiological* measure to be the best indicator of arousal. By what criterion is blood flow to the penis a better indicator of desire than someone's subjective sense of arousal? One reason to question the assertion that the physiological measure is superior is that more than a third of the men in the study did not have enough erectile response to the films to be classified as aroused at all (581). Does this mean that one-third of men who believe that they are either heterosexual, gay, or bisexual actually have *no* sexual orientation? That seems to be an absurd conclusion. A test that provides interpretable results only for two-thirds of healthy, normal subjects is problematic, to say the least.

And it's not necessarily the case that a "better" physiological measure is the answer to this problem. Biologically oriented researchers have tended to invest great faith in physiological measures as superior, but this requires justification. In particular, the subjective sense of arousal may be a better predictor of other aspects of sexual orientation (such as actual sexual pairings or love relationships). In fact, there was a second measure of arousal that was overlooked when the study was reported in the popular press. Men were directed to move a lever in response to their own sense of sexual arousal while watching the films. By this measure, bisexual men's arousal actually matched quite well with their self-reported patterns of attraction and identity. Other researchers have found that self-described orientation correlates well with "implicit" measures that, like the physiological measure here, "reflect immediate reactions that may not be available to introspection" (Snowden, Wichter, and Gray 2008).

Finally, think back to the sheep for a moment. It is possible that some aspect of the testing context (such as the specific images chosen, the types of sexual activity portrayed in images that included men versus women, and/or the fact of being watched by the scientists running the experiment) made attraction to men more salient for the bisexual subjects. Recall that the images of women were of *women having sex with each other.* Perhaps bisexual men generally have less interest in "lesbian" imagery than heterosexual men do. (I have no idea if that's true.) The point is that *context and subjectivity matter to sexuality,* and the attempt to get around subjectivity in order to "drill down" to a more valid substrate is misguided. This is a point I take up in the final chapters. For now, let's return to how sexual orientation is measured in brain organization studies.

Getting Real: Sexual Orientation as Multidimensional

Many brain organization researchers ask about more than one aspect of sexual orientation in their studies. Treating sexual orientation as multidimensional fits more closely with the data on sexuality from social science and public health research, in which the most commonly assessed aspects are the gender of actual sexual partners ("behavior"), the gender of ideal sexual partners ("fantasy"), and self-identification as homosexual, heterosexual, or bisexual ("identity") (Sell and Becker 2001; Laumann et al. 1994).[6]

The reason for assessing multiple dimensions, of course, is that many people's sexual orientation does not neatly line up across all the different possible aspects of this phenomenon. Take a look at the Venn diagrams in Figures 7.1 and 7.2. The data are from the National Health and Social Life Survey (Laumann et al. 1994), one of the largest and most comprehensive sexuality surveys conducted to date in the United States. Each of the diagrams shows how three aspects of sexual orientation (behavior, fantasy,

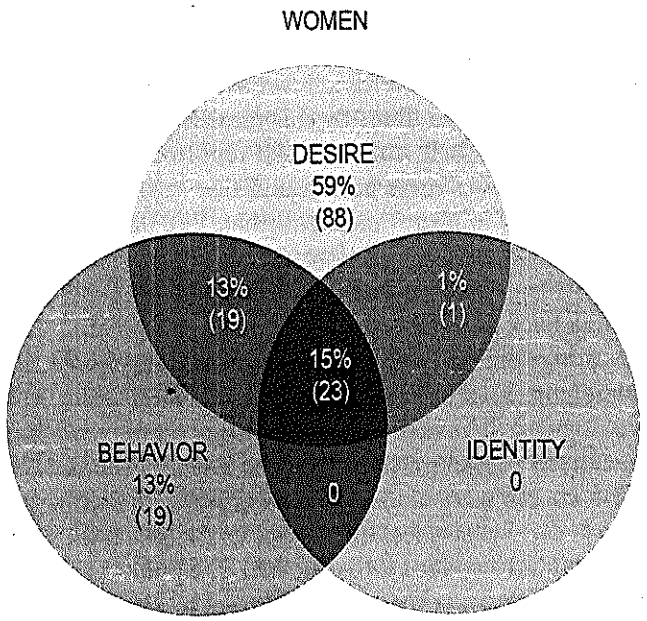

Figure 7.1. Among women, the relationships among same-sex desire, sexual behavior with same-sex partners, and lesbian or bisexual identity. (Laumann et al. 1994, page 298, used with permission of University of Chicago Press.)

and identity) are related in people who reported any kind of same-gender sexuality as adults. The most important thing to notice is the small shaded area at the center of each diagram. This area shows the proportion of people for whom all three aspects of sexual orientation align: just 15 percent of the women and 24 percent of the men indicated sexual desire for same-gender partners, *and* sexual activity with at least one same-gender partner in adulthood, *and* also consider themselves to be either homosexual (including equivalent terms such as gay or lesbian) or bisexual.[7]

These are also the most common dimensions of sexual orientation assessed in brain organization studies, both those that rely on just one dimension and those that ask about two or more dimensions. Note, though, how differently this team of sociologists perceives their finding of discrepancy among the various dimensions of sexual orientation, compared with the researchers (usually psychologists) who conduct brain organization studies. Laumann and colleagues approach the discrepancy itself as the phenomenon of interest, rather than as "noise" to be eliminated. In other words, they measure multiple dimensions in order to answer the question "How are these three aspects of homosexuality related?" (1994, 298).

Further, they use their analysis of continuity and divergence among the three aspects as an opportunity to reflect on various ways that "homosexuality is both organized as a set of behaviors and practices and experienced subjectively" (300). They are also attentive to the way that the subjective experience of sexuality is, at least in certain respects, responsive to the social environment, noting that "the group of people who report behavior and desire but not identity is quite small among the men but fairly sizable among the women, comparable to the women who had sex partners but nothing else and to those who exhibit all three characteristics. This may indicate a slightly lower threshold of homosexual and bisexual identity among men than among women. This would fit with the historically greater visibility of gay men as opposed to lesbians" (300). Finally, they sensibly suggest that at least some of the discrepancies they find are related to their own manner of questioning: "Not surprisingly, since these questions are asked in different places [in our survey] and in different ways (face-to-face vs. self-completion, directly vs. indirectly, etc.), there were some inconsistencies between responses" (298 n. 17). Accordingly, Laumann and colleagues firmly endorse multidimensional assessments:

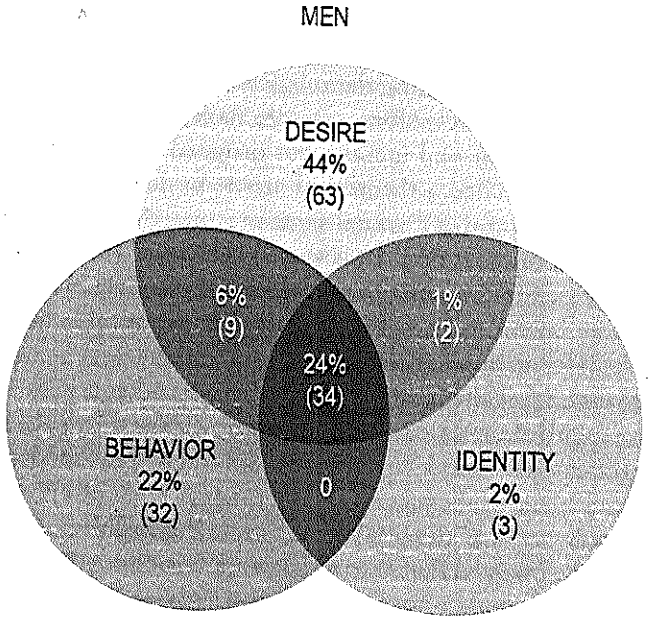

Figure 7.2. Among men, the relationships among same-sex desire, sexual behavior with same-sex partners, and gay or bisexual identity. (Laumann et al. 1994, page 298, used with permission of University of Chicago Press.)

> While the measurement of same-gender practices and attitudes is crude at best, with unknown levels of underreporting for each, this preliminary analysis provides unambiguous evidence that no single number can be used to provide an accurate and valid characterization of the incidence and prevalence of homosexuality in the population at large. In sum, homosexuality is fundamentally a multidimensional phenomenon that has manifold meanings and interpretations, depending on context and purpose. (301)

Some brain organization studies do use multidimensional measures (for instance, Ehrhardt et al. 1985; Dittmann, Kappes, and Kappes 1992; McCarty et al. 2006; Meyer-Bahlburg et al. 2008), but the rationale and execution behind such assessments is quite different. Rather than seeking to explore the patterns and meanings of same-sex orientation that multiple measures can reveal, brain organization researchers assess more than one dimension exclusively to increase their ability to detect any same-sex eroticism. I explore problems with this approach further along in this chapter. Meanwhile, it is also fairly common for scientists to agree *in principle* with the idea that sexual orientation is multidimensional, but to omit certain dimensions when they measure the construct, or when they conduct their analyses (as the study of arousal among bisexual men discussed above illustrates). In such cases, the working definition remains one-dimensional (for instance, Ellis et al. 1988; Reite et al. 1995; Bailey, Willerman, and Parks 1991). For a study testing the relation of hormones and sexual orientation via maternal stress, for example, Bailey and colleagues (1991) measured both behavior and fantasy, but dropped the behavior measure. As justification, they cited a theoretical article by John Money (1987) to support their assertion that "sexual fantasy is more closely linked to the concept of sexual orientation than is sexual behavior" (Bailey, Willerman, and Parks 1991). Money actually pointed not to fantasy but to falling in love as the key factor; in any case, why *ask* about sexual behavior if you don't *use* the information?

Just as some criteria are frequently dropped out, other criteria are sometimes slipped into the measure without acknowledgment, because of the ways subjects are recruited into studies. Several highly cited studies by Gunter Dörner (Dörner, Rohde, et al. 1975, 1976, and 1983; Dörner et al. 1980; Dörner, Schenk, et al. 1983) demonstrate this problem. In one study, Dörner *explicitly* asserted that sexual orientation is desire (being "sexually excited by another male") and sexual experience (a "homosexual" has "unambiguous homosexual behavior since puberty"), and *implicitly* suggested that it is role in sexual activity (a homosexual man is one who is "receptive" in intercourse, or exhibits some other human homologue of "female-like" sexual behavior as it is experimentally manipulated in rats)

(Dörner, Rohde, et al. 1975). These components of orientation are not equivalent, and are independent: men can be sexually excited by other men and be sexual "tops" or be sexually versatile (preferring an insertive role in intercourse or having no role preference); and men can be sexually excited by other men but have sexual experiences only with women. In practice, Dörner has never assessed specific position or activity preferences. Moreover, his recruitment procedure relied heavily on referrals from clinicians who deal with various "sexual problems" (therapists, sexologists, and venerologists, and so on). He thereby included "having sexual problems" as one of the unstated criteria for being classified as a "homosexual" in his studies.[8]

To be fair, it is difficult to address measurement issues adequately within the page limits of a typical journal article, and in recent years brain organization researchers have been much more likely to publish monographs that do address the definition and appropriate measures for sexual orientation (Wilson and Rahman 2005; Bailey 2003; LeVay 1996). Unfortunately, though, even in these lengthier treatments, there is a tendency for scientists doing brain organization research to make their arguments in a manner that is more like advocacy than like science (see the discussions below about choosing boundaries between homosexual and heterosexual subjects, and generalizability of definitions).[9] Meanwhile, in the brain organization studies themselves, investigators tend to sweep measurement issues aside as either unimportant or already settled.

Measurement Issue #2: Framing

There are two basic ways to frame sexual orientation for a study. The first possibility is to conceive of someone's sexual attraction being directed toward women (gynephilic), men (androphilic), or both. The second possibility to is conceive of attraction as being toward one's own sex (homosexual), the other sex (heterosexual), or both (bisexual). For many purposes, it might not make a big difference whether you use a gynephilic/androphilic frame or a homosexual/heterosexual/bisexual frame, but in brain organization research the choice is critical. This is because the frame determines how people are grouped to reflect similar versus different sexual orientations in studies. Those groupings, in turn, matter when you are looking at multiple studies to see whether the findings are consistent.

In the gynephile/androphile frame, heterosexual men and lesbians have similar orientations, and gay men and heterosexual women have similar orientations. In the homosexual/heterosexual/bisexual frame, gay men and

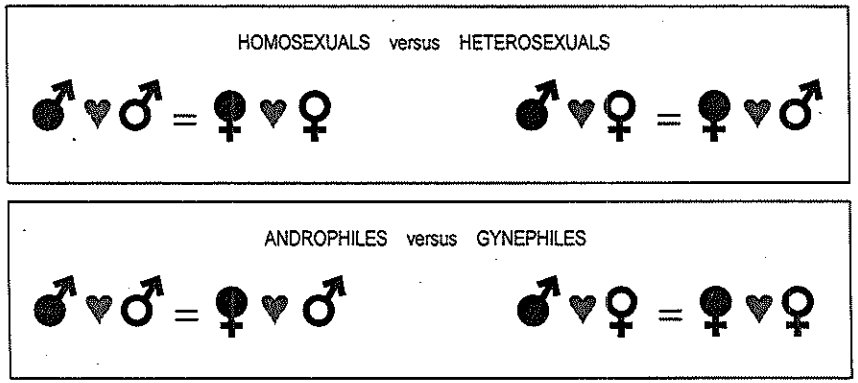

Figure 7.3. Homosexual/heterosexual frame versus androphile/gynephile frame.

lesbians have similar orientations (both are attracted to people of the same sex as themselves), as do heterosexuals of both sexes (attracted to people of the other sex), and bisexuals of both sexes (attracted to both same-sex and other-sex partners). This simple difference is illustrated in Figure 7.3.

The first thing to recognize is that the androphile/gynephile frame is the one that is implicit in the theory of brain organization. It is easiest to understand this by considering how some scientists think of the "representation" of sexual orientation in the brain, and how this line of thought is connected to research in other animals. Not all traits that are thought to be organized by early hormone exposures are credited with corresponding to distinct or "dedicated" brain areas, but sexual orientation often is. Michael Bailey (2003) is among the prominent researchers who have speculated that there is a specific "sexual orientation center of the brain" that differentiates as masculine or feminine under the influence of early hormones (but compare LeVay 1996; see also Morris et al. 2004; Rahman 2005).[10] As Bailey explains it, "If [the sexual orientation center] is masculine, then attraction to women results. If it is feminine, then attraction to men results. Thus, both straight men and lesbians would be expected to have masculine sexual orientation areas, and gay men and straight women to have feminine areas" (Bailey 2003).

As support for this idea, Bailey and others appeal to research in nonhuman animals. Manipulation of early hormone exposures affects various mating and courting behaviors in a range of species. Furthermore, manipulating hormones during development, as well as removing or damaging certain brain areas, indicates that specific neural areas, especially parts of the hypothalamus, are necessary for various components of sexual activity and interest in infrahuman animals. And in sheep, one of the few animals

in which natural variations in partner preference have been studied, the size of a cell group in the hypothalamus seems to vary according to both sex and partner preference (in general, the area is larger in rams than in ewes; but larger in "female-oriented" rams than in "male-oriented" rams) (Roselli, Larkin, Resko, et al. 2004). Given the complexity and context-dependence of ram "sexual orientation" that I discussed earlier, it is not entirely clear what this cell group signifies. Moreover, interspecies differences are important, making easy extrapolation from animal experiments to human sexual orientation impossible. Simon LeVay has pointed out an especially useful contrast for understanding the difficulty:

> In rats, for example, lesions in the medial preoptic area interfere with mounting but leave the male rat with some interest in estrous females. They behave as if they still want to do something with the female but have forgotten what it is. In primates, on the other hand, even quite small lesions within the medial preoptic area can cause males to lose all interest in estrous females; they still have a sex drive (as evidenced by their continuing to masturbate), but they seem to lose any notion that females offer a means to satisfy it. (LeVay 1996, 134)

Whether based on behavioral displays or on partner preferences, the animal research on the "organizing" effects of hormones always employs a gynephile/androphile frame for sexual orientation. But psychologists and other scientists often frame human sexual orientation differently. Consider the definition offered by Martin Lalumiere and colleagues (2000) in a review and reanalysis of data on handedness and sexual orientation: "a person's erotic preference for opposite-sex individuals (heterosexuality), same-sex individuals (homosexuality), or both (bisexuality)" (575). Figure 7.3 showed how this "homosexual" versus "heterosexual" framework is different from the androphile/gynephile framework. Figures 7.4 and 7.5 show how each of these frameworks requires a different developmental model for relating early hormones to the development of sexual orientation. Figure 7.4 shows the original model using the gynephile/androphile frame, which does not depend on the sex of the developing individual—the same model applies to males and females. In Figure 7.5, the sexual orientation variable is framed as "*homosexuality* versus *heterosexuality*." In this case, to fit with brain organization theory, there must be two different causal models: one for females, and one for males. Exposure to masculinizing hormones supposedly leads to homosexuality for women, but a *lack of exposure* to masculinizing hormones is purported to cause homosexuality in men. Only bisexuality (among those investigators who allow for this category of orientation) has a similar model for males and females:

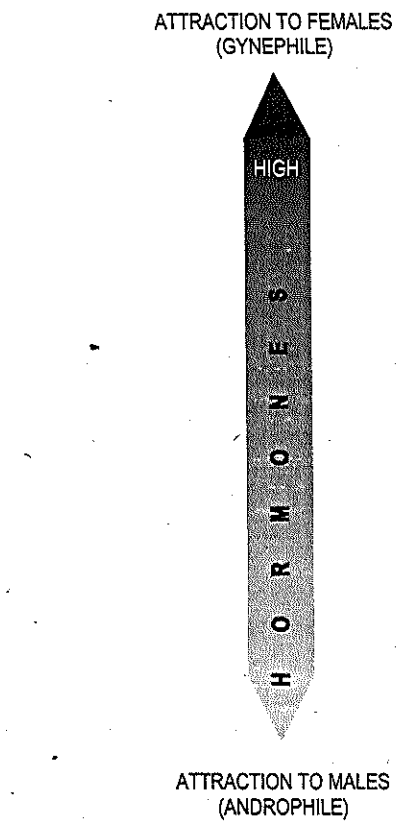

Figure 7.4. Original model of brain organization, which uses the gynephile/androphile frame.

hormone exposures that are partially, but not completely, masculinizing are thought to underlie bisexuality in both males and females.

And here's where the trouble begins. Remarkable as it seems, some scientists studying brain organization have completely failed to appreciate the implications of alternative ways of framing sexual orientation. Beginning with some of the studies on prenatal stress in the 1980s (Rosenstein and Bigler 1987; Ellis et al. 1988) and continuing with studies of handedness and other markers of brain lateralization (for example, McCormick, Witelson, and Kingstone 1990; Holtzen 1994; McFadden and Champlin 2000), a significant subset of studies have employed the terms *homosexual*

and *heterosexual* as if they represented the same phenomenon in women and in men—that is, they switch to the homosexual/heterosexual frame. Yet they do not seem to grasp that this changes how one would model the influence of hormones on sexual orientation, and this leads them to either misinterpret earlier research or design studies that are at odds with the main thread of brain organization. Ellis and colleagues (1988), for example, interpreted animal data on stress to suggest that maternal stress during pregnancy "interfere[s] with fetal production of various sex hormones, especially testosterone. . . . Should this disruption occur when crucial brain parts controlling sexual orientation are being sexually differentiated, per-

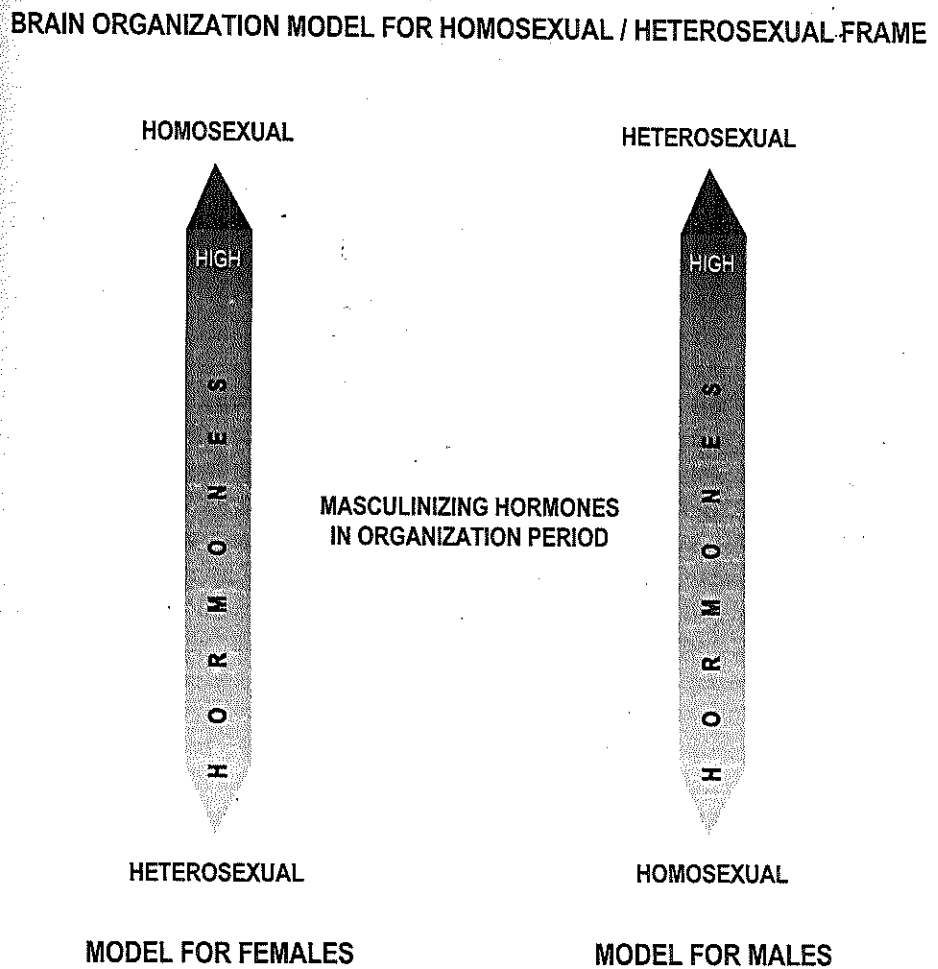

Figure 7.5. Brain organization using the homosexual/heterosexual frame. Note that this frame requires a different model for males versus females.

manent inversions could occur" (153). In the remainder of their discussion of animal experiments, they explicitly noted that the studies concerned *male* rodents, but they structured their own study "to determine if maternal stress during pregnancy was predictive of sexual inversions [i.e., homosexuality and bisexuality]"—*without regard to sex*. They proceeded to construct identical hypotheses regarding an association between prenatal stress and sexual orientation for both men and women.

McCormick, Witelson, and Kingstone (1990) ran into the same problem, but from a different angle. In their literature review, they noted that higher androgens in CAH women are related to "higher ratings of homosexual behavior" and *contrasted* this with CAH men, in whom androgens are also increased but "homosexual behavior was extremely rare" (70). Yet brain organization theory predicts that high androgen exposures in *both* CAH women and CAH men will lead to an orientation toward females (gynephile orientation). Using the "homosexual versus heterosexual" frame, McCormick and colleagues conclude that the hormonal mechanism for development of sexual orientation is *different* in men and women. Of course, if they had used the androphile/gynephile way of framing sexual orientation, they would have interpreted the data to indicate that the mechanism is *identical* in men and women.

Many researchers seem to unwittingly shift theoretical frames as a result of simply linking "atypicality" on two variables—like sexual orientation and handedness, or sexual orientation and prenatal stress. Reasoning that homosexuality is an atypical orientation, they advance the generalized prediction that homosexuals are also likely to be atypical on the second variable. But when researchers fail to differentiate *male* homosexuality from *female* homosexuality, they end up (implicitly or explicitly) hypothesizing that both male and female homosexuality are influenced by the same type of atypical hormone exposures (Rosenstein and Bigler 1987; Ellis et al. 1988; Gladue and Bailey 1995b).[11] Of course, this does not fit either with the animal research on hormones and sex-typed behavior or with the usual predictions and interpretations of the human "cohort" studies of brain organization, all of which argue that exactly the *opposite* type of hormone exposures would lead to male versus female homosexuality. Yet these studies are folded into the brain organization literature as if they concur with studies that use the exact opposite model.

Studies that link sexual orientation with prenatal stress and/or handedness are particularly problematic in this regard. The main tension between these studies and other brain organization research is disagreement about whether homosexuality in men would be related to *higher* or *lower* testosterone during development. The cohort studies (as well as virtually all animal studies on sexual behavior and partner preferences) proceed from the assumption that lower than typical testosterone would lead to male homosexuality. But because left-handedness is slightly more common in men, it is generally thought that *higher* than typical levels of testosterone lead to left-handedness. Thus, studies on various aspects of lateralization introduced a murkier set of predictions—so murky, in fact, that quite a few scientists doing this work have been unable to sort them out.[12]

Linking male homosexuality and left-handedness through prenatal hormone exposures requires a model that involves *lower* than typical testosterone during the critical period when sexual orientation is thought to develop with *higher* than typical testosterone during the period when handedness develops. Norman Geschwind and colleagues (Geschwind and Behan 1984; Geschwind and Galaburda 1985a, 1985b, 1985c) proposed that rodent studies of neuroendocrine response to prenatal stress could resolve this difficulty. As you may recall from earlier chapters, intense stress has been shown to create a brief surge in testosterone, followed by a long-term drop to levels much lower than normal; consequently, male rats subjected to prenatal stress exhibit more-feminine sexual behaviors (Ward 1972, 1984). Geschwind and colleagues suggested that intense stress during the early prenatal period could provide a mechanism whereby gay men (whose sexuality these scientists considered to be "feminized") would nonetheless exhibit the "hypermasculine" characteristic of increased left-handedness. (This solution, of course, requires exquisite timing, and relies upon a developmental sequence in terms of specific critical periods that was purely hypothetical when proposed, and remains so.)

The elaborate wedding of theories proved to be popular, but confusing. Of the first ten studies of lateralization and homosexuality, five included basic errors in their descriptions of the main theories they drew on (Lindesay 1987; McCormick, Witelson, and Kingstone 1990; Marchant-Haycox, McManus, and Wilson 1991; Becker et al. 1992; Holtzen 1994).[13] Three others evaded the issue by predicting a "link" between testosterone, left-handedness, and homosexuality, but without specifying whether higher or lower levels of testosterone would be implicated in this link (Rosenstein and Bigler 1987; Gladue and Bailey 1995b; Reite et al. 1995). In only two of these ten studies did the scientists accurately characterize the various research and theories they built upon (Satz et al. 1991; Götestam, Coates, and Ekstrand 1992).

It is surprising how many articles containing fundamental framing errors have gotten through peer review in well-respected journals, and are repeatedly cited as supporting brain organization theory. In a study that purported to find a link between prenatal stress and homosexuality, Ellis

and colleagues (1988) compared homosexual with heterosexual men, and homosexual with heterosexual women, in both cases hypothesizing that homosexuality in offspring is linked to higher maternal stress during the prenatal period. But of course, there is no theoretical reason to link higher maternal stress to homosexuality in females, other than a vague folkloric prejudice that links lesbianism as a "problematic" outcome with various problematic developmental inputs. Still, in spite of methodological problems that go well beyond this issue of framing, this study, as well as a closely related review article (Ellis, Burke, and Ames 1987), are particularly highly cited (44 and 153 citations, respectively, as of July 2008, according to the ISI Web of Science).[14] Further compounding the error, many other scientists cite Ellis and colleagues' (1988) stress research as if it simply supported the theory of brain organization instead of considerably *complicating* it (for example, Holtzen 1994; Hall 2000; Yasuhara, Kempinas, and Pereira 2005; Meek, Schulz, and Keith 2006; Swaab 2007). Similarly, Rosenstein and Bigler (1987) compared male and female heterosexuals (grouped together) to male and female homosexuals (also grouped together) in order to look for an association between homosexuality and handedness. As with Ellis and colleagues, Rosenstein and Bigler assembled their groups in a way that hypothesized a *common* hormonal history for all homosexuals (males and females).

When I began analyzing brain organization research more than ten years ago, I noticed these framing problems and was surprised that not one study in the literature addressed the issue.[15] Recently some scientists have recognized the contradictions in framing and have suggested that their findings should be understood in light of other neurohormonal theories of development rather than classic brain organization theory (Lindesay 1987; McFadden and Champlin 2000; Lippa 2003). For example, in their meta-analysis of sexual orientation and handedness in men and women, Lalumiere, Blanchard, and Zucker (2000) acknowledge:

> The findings that handedness and sexual orientation are associated in both men and women are difficult to explain with current etiological theories of homosexuality, especially the well-known hormonal theory of sexual orientation, which we here call the prenatal androgen exposure theory. The prenatal androgen exposure theory postulates that homosexuality in men is due to undermasculinization and in women to overmasculinization of the brain during a critical period of development. (586)

Nonetheless, even those scientists who have tried to use a broader range of evidence to decide among the various theories that are currently at play

for linking sexual orientation with handedness or other "sex-linked" traits have not gone as far as they could. In particular, these discussions still fail to account for the overall pattern of findings among human studies of prenatal stress, lateralization, and the expected versus demonstrated constellation of sex-typed traits among lesbians and gay men under various theories. For example, prenatal stress is often invoked to explain higher rates of left-handedness among gay men, but studies of prenatal stress have themselves repeatedly come up empty-handed: gay men have not experienced higher prenatal stress. Further, as the discussion below will detail, the overall constellation of sex-typed traits might be expected to be more mixed in gay men, but more consistently "masculinized" in lesbians, but this does not fit the data, either.

Measurement Issue #3: Quantifying Orientation

How gay is gay? How straight is straight? In deciding "who counts when you're counting homosexuals" (to quote Stephanie Kenen [1997] again), scientists must consider not only what elements matter, but what are the *cutpoints* for labeling someone as heterosexual, bisexual, or homosexual. Does same-sex orientation count only if it meets certain time-based rules (lifelong; after a certain age; consistent for the past five years)? Do *all* of one's partners have to be same-sex, or a majority, or some, or perhaps only partners with whom there is a certain emotional attachment? Do very small incremental differences in terms of how much attraction is toward persons of the same or the other sex matter? Brain organization researchers have very different answers for these questions, meaning that they select different scores as the cutpoints to divide their subject groups for analysis. This, too, is unacknowledged in the literature.

To illustrate how variable the cutpoints for specific sexual orientation categories are in brain organization research, it's again helpful to refer to Kinsey scales. Each of the studies in Figure 7.6 classified subjects according to scores on some Kinsey-type scale. The variations in shading on the bars show where the scientist set the cutpoint for considering a subject as homosexual, bisexual, or heterosexual. Note that the figure *understates* the amount of variability in the meaning of Kinsey scores, because the aspect(s) of sexuality used to assign a Kinsey score vary from study to study. In some cases the aspects measured were not reported; rarely were actual questions reported. Nonetheless, it is illustrative to see the great variation in how scientists assign Kinsey scores to particular groups (homosexual,

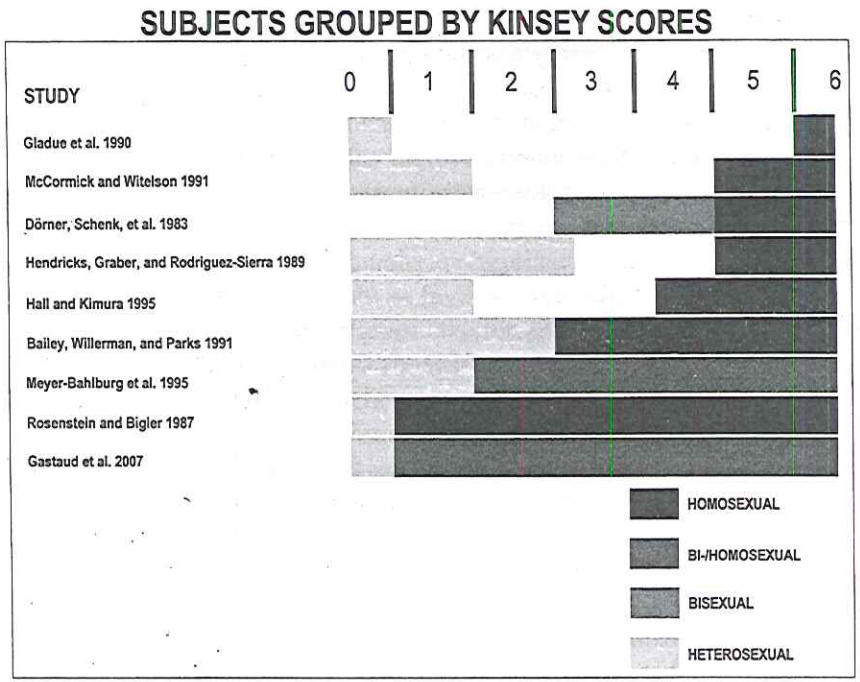

Figure 7.6. Variety in scientists' use of Kinsey scores. Note that in Meyer-Bahlburg et al. 1995 and Gastaud et al. 2007, the labels are implicit rather than explicit (for example, "subjects with homosexual inclinations" rather than "homosexuals").

bisexual, heterosexual). Subject groups were split into homosexuals versus heterosexuals by different scientists at literally every point along the scale between 0 and 6.

As Figure 7.6 shows, one scientist's heterosexuals are another scientist's homosexuals. Compare, for example, Gladue et al. (1990) with Meyer-Bahlburg et al. (1995). Gladue's team excludes from their homosexual category anyone with a lifetime Kinsey score less than 6. In contrast, Meyer-Bahlburg and colleagues' article on "the development of homosexual orientation" among DES-exposed women is based on a sample that includes not a single woman with a Kinsey 6 rating for lifetime "sexual responsiveness" and only one woman with a Kinsey 5 (Meyer-Bahlburg et al. 1995). (Meyer-Bahlburg and colleagues may argue that they do not technically set cutpoints for labeling women as heterosexual, bisexual, and homosexual, but they *functionally* assign the cutpoint for homosexual at a Kinsey 2, because they describe the study in the title and elsewhere in the article as an exploration of the development of "homosexual orientation.")

Decisions about boundaries are related to whether scientists think sexual orientation is categorical or continuous. Those who think of sexual orientation as continuous regard any cutpoints between homosexuals, bisexuals, and heterosexuals as matters of convention, because they imagine that "true" sexual orientations come in many degrees of difference between the poles of homosexuality and heterosexuality. Those who think of sexual orientation as categorical think of cutpoints as reflecting the boundaries between objectively different groups. While both views are widely held among scientists, there is an interesting and somewhat troubling pattern for where these two views show up in the brain organization studies. Briefly, those researchers who group their subjects by prenatal hormone exposure status tend to adopt the continuum approach (Ehrhardt et al. 1985; Dittmann et al. 1990, "Congenital Adrenal Hyperplasia," parts I and II; Meyer-Bahlburg et al. 1995; Zucker et al. 1996; Stikkelbroeck, Beerendonk, et al. 2003; Titus-Ernstoff et al. 2003; Meyer-Bahlburg et al. 2008),[16] while researchers who group people by sexual orientation overwhelmingly adopt the category approach (Dörner, Rohde, et al. 1975, 1983; Swaab and Hofman 1990; Bailey, Willerman, and Parks 1991; Dörner et al. 1991; LeVay 1991; Allen and Gorski 1992; Gladue and Bailey 1995a, 1995b; Ellis and Cole-Harding 2001; Rahman and Wilson 2003; Anders and Hampson 2005; Rahman 2005; Kraemer et al. 2006; Blanchard and Lippa 2007; Witelson et al. 2007).[17] To put this another way, researchers who study people with unusual prenatal hormone exposures (intersex individuals and the offspring of hormone-treated pregnancies) measure adult sexual orientation in very finely graded ways that can pick up the slightest difference between hormone-exposed and unexposed groups. But studies that look for evidence that gay men or lesbians have had different prenatal hormone exposures than their straight counterparts seek to create comparison groups that are as starkly different as possible in terms of sexual orientation, so they choose "extreme" homosexuals and "extreme" heterosexuals.[18] As I will explain below, it is easier in the former group (the cohort studies) to find hormone effects with a very loose criterion for homosexuality, while the latter (case-control) approach will benefit from a very restrictive criterion. It is impossible not to see this as scientific gerrymandering—that is, moving the boundaries of sexual orientation categories around in a way that favors the scientist's own hypothesis.

In an interview, one scientist explained the benefit of looking at "the extremes" in terms of sexual orientation for her studies of handedness and sexual orientation: "I was thinking that, well, if the differences are going to be subtle and small, we need to look at the two most different groups" (Dr. L interview, January 1999). Likewise, Gladue, Green, and Hellman

(1984) explained that their strategy of screening out subjects who did not give absolutely "consistent" responses for each dimension of sexual orientation "was especially critical for the integrity of the homosexual sample," further claiming that "it was essential to distinguish volunteers having exclusively homosexual desires, interests, and experiences lifelong from men having such characteristics intermittently, periodically, or only recently" (1499 n. 10). And indeed, this rationale does make sense on the assumption that subjects with the most "extreme" orientations will be those who had the most extreme cross-sex hormone exposures, *and* that "extreme" homosexuals and heterosexuals will be most likely to differ on other characteristics that are presumably influenced by hormone exposures, such as laterality or cognitive traits. This is the "dose–response" logic I described in Chapter 3. I highlight the expectation here because the evidence runs counter to this expectation, in spite of some scientists' attempts to assemble "pure" sexual orientation groups. I will have more to say about this below, when I review the findings of sexual orientation studies. In any case, it turns out that while these very strict measures make sense if you're considering just one corner of the evidence linking hormones to homosexuality, they create problems when you look at the bigger impact of this research.

THE CHOICE of high-contrast sexual orientation groups for the case-control studies is initially driven by the desire to maximize the chance of finding group differences on hormone-related traits. Yet for some scientists these rigid groupings have literally redefined what it means to be homosexual or heterosexual. Defying social science evidence that there is significant independence among the various aspects of sexual orientation, brain organization scientists treat consistency across behavior, attraction, and identity as "true" or "correct," rather than simply an operational necessity for their particular studies. Ellis and Cole-Harding (2001), for example, go so far as to describe inconsistency across all dimensions of sexual orientation as "erroneous responses."[19]

While this approach is at odds with survey and in-depth qualitative research on how people's desires, behaviors, and identities intersect and diverge (Laumann et al. 1994; Diamond and Savin-Williams 2000; Young 2004), insisting on erotic consistency fits well with some recent theoretical developments in psychology. As Dr. L put it, regarding the late 1980s, "a lot of the literature was questioning whether there was really such a thing as bisexuality" (Dr. L interview, January 1999; see also Bailey 2003). Further, while they differ on the prevalence and stability of bisexuality as a category, many well-known researchers have strongly argued that sexual

orientation is basically a categorical phenomenon—at least in men (Hamer and Copeland 1994; LeVay 1996; Wilson and Rahman 2005). In this view, the categories of homosexual, heterosexual, and possibly bisexual are adequate and exhaustive for describing the meaningful patterns of human eroticism. If this is the case, then "weeding out" people who give inconsistent responses on measures of multiple dimensions of sexual orientation is a perfectly good idea. Moreover, some scientists imply—and others state outright—that the "discrete categories" idea of sexual orientation fits better with brain organization theory (LeVay 1996; Bailey 2003; Wilson and Rahman 2005; Kraemer et al. 2006).[20]

There are difficulties, though. It turns out that many researchers have to jettison a high proportion of their research subjects because they don't meet the criterion of "consistency" on sexual orientation. Investigators often allude to, but rarely offer details about, how this affects the composition of the "homosexual" and "heterosexual" groups they compare. For example, Ellis and Cole-Harding's (2001) study of prenatal exposures to stress and alcohol used a method that excluded a "substantial minority" of subjects (with no additional details on the number or proportion involved), because some people's responses weren't totally consistent across the three dimensions of sexual orientation (217). In one study that offered more detail on subjects who were excluded, 20 of 90 (over 22 percent) otherwise eligible men did not meet the criteria for being either *homosexual* or *heterosexual* (Gladue, Green, and Hellman 1984). This is particularly striking given the recruitment strategy for the study, which consisted of advertisements for "homosexual and heterosexual" individuals for a study of endocrine function and sexual orientation. Thus, volunteers likely were already a self-selected group in terms of sexual identity, and more likely than a random group of individuals to identify as either gay or straight, rather than bisexual. Still, over one-fifth of volunteers were not adequately consistent on each of three criteria (sexual fantasies, sexual behaviors, and stability of each since puberty) to be counted as homosexual or heterosexual. Likewise, in one of several recent studies that link brain organization theory to the observation that gay men tend to have more older brothers than heterosexual men have (the "fraternal birth order" effect), Blanchard and Lippa (2007) dropped nearly one-third (3,375 out of 11,654) of all subjects who gave some indication of same-sex orientation but whose answers were deemed "inconsistent" (165).

To place these decisions in context, consider some additional data from the large, population-based study of American adults that I mentioned earlier. In addition to showing how the various aspects of behavior, fantasy, and identity were related, the investigators report the proportion of men

and women who have had partners of the same sex, the other sex, and both sexes during various time periods, as well as the proportions who are attracted to persons of the same, the other sex, or both sexes. Interestingly, of all the people who have any same-sex experiences as adults, less than 20 percent of men and just 10 percent of women have been exclusively homosexual in behavior since age 18 (Laumann et al. 1994, 311). Among those who report same-sex attractions and/or behavior, 85 percent of women and 72 percent of men identify as heterosexual (301). Brain organization researchers are not unaware of this issue, at least in terms of their female samples. In the literature on biology and sexual orientation, as well as in the interviews I conducted with prominent scientists, there was general agreement that women are even less likely than men to be totally consistent among the various dimensions of orientation.[21]

One result of filtering out "inconsistent" responses is that many brain organization studies exclude the *majority* of self-described lesbians and many gay men from studies on sexual orientation. This most certainly hampers the utility of brain organization research for elucidating the processes that underlie sexual orientation development. But it is equally important to note that the process of "cleansing" samples to remove apparent sexual inconsistency results in artificially homogeneous "heterosexual" groups, as well. This creates a serious problem of bias. In an artificially homogeneous group, other traits besides sexual orientation will be present to a degree that one would not see in a truly random group of "heterosexuals" or "gay men" or "lesbians." When we analyze the data, there will be irrelevant variables present that can make it seem like there are group differences related to sexual orientation, when in fact there is another hidden variable at play. A historical example will help clarify the problem. Prior to the 1980s, it was common to recruit samples of gay men and lesbians in gay bars, because there were very few other public meeting places for sexual minorities in most communities. Unsurprisingly, these samples showed higher rates of problem drinking than heterosexual people who were recruited from a wide range of community settings.

What about the cohort studies? In one regard these studies look superior, because they assess multiple aspects of sexuality and they do not demand consistency across aspects of sexual orientation. Yet the "homosexuality" found in the studies of hormone-exposed individuals is possibly even further away from homosexuality as understood in the real world. There are two problems with the way even the most careful researchers use multidimensional measures in these studies. The first, content validity, is a technical issue having to do with the way specific dimensions are chosen to

be included in these measures, and the second has to do with the way results are eventually labeled in summary statements and titles.

Content validity concerns the key question of meaning in measurement: Does the measure accurately capture the phenomenon of interest? In a classic text on measurement, Carmines and Zeller (1979, 43) define content validity as "the extent to which a specific set of items reflects the construct [i.e., the phenomenon of interest]." Even without settling on a "gold standard" definition of sexual orientation, it is possible to discern content validity problems in some of these comprehensive sexuality assessments. The Sexual Behavior Assessment Schedule–Adult (SEBAS-A) is a good instrument to consider in this regard, both because it was developed by the well-regarded team of Heino Meyer-Bahlburg and Anke Ehrhardt (1983) and because it has been used in several brain organization studies. In studies of DES-exposed women, for example, Meyer-Bahlburg and Ehrhardt combined multiple aspects of sexual orientation assessed via the SEBAS-A to create a global score for sexual orientation (Ehrhardt et al. 1985; Meyer-Bahlburg et al. 1995). When a scale is used for this purpose, the scale items should be a *randomly chosen subset* of the universe of potential items that could reflect sexual orientation, but most of the SEBAS-A items are nonbehavioral aspects of imagery and attraction. In other words, the scale oversamples the nonbehavioral aspects of sexual orientation, thereby exaggerating the importance of imagery items, such as dreams. Further, these questions (especially masturbation fantasies, daydreams, and type of erotica used) may be so closely related that it doesn't make sense to assess them separately, because combining them via simple addition may exaggerate tiny shifts toward same-sex orientation in one's overall profile.

The second issue is how subjects are described in studies that rely on complex assessments of this sort. Sensibly, in the precise passages where scientists discuss subtle differences in sexual orientation, they tend to avoid attaching labels such as *heterosexual* or *homosexual* to their subjects. Yet this careful approach drops away quickly—sometimes even in the article abstracts and titles that scientists themselves choose.

Thus, in the sections where most readers look for the "bottom line," it seems that the exhaustive assessments work on a sort of sexual "one-drop rule": any hint, in any one of the elements, of same-sex attraction or response is glossed as evidence of homosexuality (see, for example, Ehrhardt et al. 1985; Dittmann, Kappes, and Kappes 1992; Meyer-Bahlburg et al. 1995; Dessens et al. 1999). It is little wonder, then, that by the time studies are reported in the popular press, terms like *homosexual* and *lesbian* are used freely.

An interesting example is found in Meyer-Bahlburg and colleagues' (1995) report on women exposed to DES: the sections on methods and results carefully refer to the women in this study by their dichotomized Kinsey scores of 0–1 and 2–6, while the title and introductory section address the development of "homosexual orientation." In a striking understatement, the authors note that "the extent to which bisexuality and homosexuality were increased in DES women was rather modest" (17). In fact, only 1 of 90 women was rated as being "largely homosexual but incidentally heterosexual" and *none* of the 90 was rated as "entirely homosexual" (17). Does it make sense to talk about the "development of homosexual orientation" based on a sample that many people—including most brain organization researchers—would agree *does not include any homosexuals?* If we count the one woman who was rated as "largely homosexual," that's still a rate of lesbianism that's just 1.1 percent, which is *lower* than most estimates for the general population (Laumann et al. 1994; Sell, Wells, and Wypij 1995).

Even if we were to agree that researchers can use whatever definitions they like for "homosexual" or "heterosexual," as long as they are clear and consistent in their own research, the yawning gap between the approaches in the two main sets of studies creates a problem. Although scientists' choices of different measures make sense in terms of studies' internal logic (it gives each kind of study a better chance of finding that there is some kind of hormone effect on sexuality), it is very difficult to reconcile findings across the studies. Bluntly, you can't have "homosexual" groups that are mutually exclusive in two sets of studies, but then claim that the studies complement or build upon each other. Still, as serious as the *measurement* discrepancies are between cohort and case-control studies of brain organization, the *findings* from research on prenatal hormones and human sexual orientation are just as problematic. It's not just that they don't add up "yet" because of incomplete data. Rather, the pattern of findings in the hundreds of studies done to date, and especially between the cohort and the case-control studies, suggests that brain organization theory is not a good explanation for human sexual orientation.

Sexual Disorientation: Lost in a Jungle of Findings

How do these measurement issues figure into the overall assessment of brain organization theory? Recall that all the data from all the studies of sexual orientation combined still form a mere fragment of the evidence that is needed to come all the way from hormones to homosexuality. Because the studies are all quasi experiments, they must work together with each other, as well as with the experimental evidence from nonhuman animals, to form a coherent picture of how early hormones affect human sexual differentiation. The overall network of associations that is predicted by brain organization theory links early hormones with sex-differentiated cognitive skills; gender-related aspects of personality and interests; signs of brain lateralization, including handedness; anthropometry (measurement of any bodily traits that are thought to be influenced by early hormones); and sexual orientation. Yet most studies consist of a search for associations between just two factors at a time, such as sexual orientation and cognitive traits (in the case-control studies), or early hormones and sexual orientation (in the cohort studies). Thus, in addition to the fact that all of the evidence is limited to correlations (which do not entail causation), the expected correlations are tested in pairs, which then are presumed to fit together to complete the overall picture. Figure 7.7 shows the main two-way associations that are predicted by brain organization research on sexual orientation.

Each association is represented either by a solid arrow that runs in a single direction (from early hormones to the domains of personality and behavior that hormones are presumed to influence) or a dotted line that connects two domains, signaling noncausal associations between two traits that are expected to vary together if both are influenced by early hormone exposures. One way to judge whether quasi-experimental evidence is robust is to do something called path analysis. In this case, that means that if brain organization theory is correct, we should be able to trace at least one complete loop on the diagram. For example, some studies might show that early masculinizing hormones are correlated with more "masculine" sexual orientation, and other studies might show that masculinizing hormones are correlated with more "masculine" cognition. If there are additional studies that show masculine cognition to correlate with masculine sexual orientation, then the loop is complete. Conversely, the data are less convincing if some of the loops cannot be closed, and the theory begins to fall apart if the data at hand demonstrate a lack of association where the diagram shows there should be a connection, or negative associations where there should be positive ones (for example, if "masculine" hormone exposures are associated with "feminine" traits).

In part because researchers approach sexual orientation so differently (especially when they switch between the gynephile/androphile and the homosexual/heterosexual frame), it's difficult to lay out the studies together in a way that makes sense. With that limitation in mind, we can still use the evidence at hand to get a preliminary sense of how well the brain orga-

nization picture would come into focus if the measurement of sexual orientation weren't an issue. That is, we can plug in the cohort study data to see whether individuals who have had "cross-sex" hormone exposures also show "cross-sex" cognition, personality and interests, bodily traits (other than genital morphology), handedness, and sexual orientation. We can also plug in case-control data to see if "sex atypical" sexual orientation is correlated with sex atypicality on any of these other traits. Note, though, that *only* the cohort studies can give us the information we need to test the hypothetical associations on the left side of the diagram in Figure 7.7 (those that connect early hormones with any individual outcome). And note, too, that *only* the case-control studies give us any information on associations between the various "outcomes" (for instance, between sexual orientation and cognition).[22] That's because the cohort studies generally have small sample sizes, and even fewer subjects with any same-sex orientation, so they do not have sufficient statistical power to look for associations among multiple hypothetical outcomes of brain organization. The solid arrows on Figure 7.7 indicate the associations that cohort studies explore, and the dotted lines indicate the associations explored in case-control studies.

Later I will address how the difference in measurement between researchers conducting cohort studies and those who conduct case-control studies affects the flow of associations. For now, though, let's try to complete some of the loops, first by looking at the evidence that early hormones affect sexual orientation, and next by looking at the evidence connecting each of four other domains either to hormones or to sexual orientation. We can also address the important question of whether the associations are shown for both genetic males and females, only one sex, or neither. This is important because, like other possible breaks in the flow of associations, the evidence does not add up to a convincing picture if some connections can be made only for men, while others can be made only for women.

Let's briefly recap the data reviewed in Chapter 4 to see how well the link between early hormones and sexual orientation is borne out when we have direct information about hormone exposures. The studies that are most relevant concern people who have had higher than typical fetal exposure to "masculinizing" hormones. As I detailed in Chapter 4, these include studies of males and females with congenital adrenal hyperplasia (CAH) and prenatal exposure to the synthetic estrogen DES. (I exclude studies on genetic males with androgen insensitivity from the following discussion because femininity may relate to female rearing rather than a lack of effective androgen exposure. I exclude males with CAH, too, because there is no consensus on whether they do in fact have much higher than typical prenatal androgens, or on how androgens above the typical male range would affect them.)

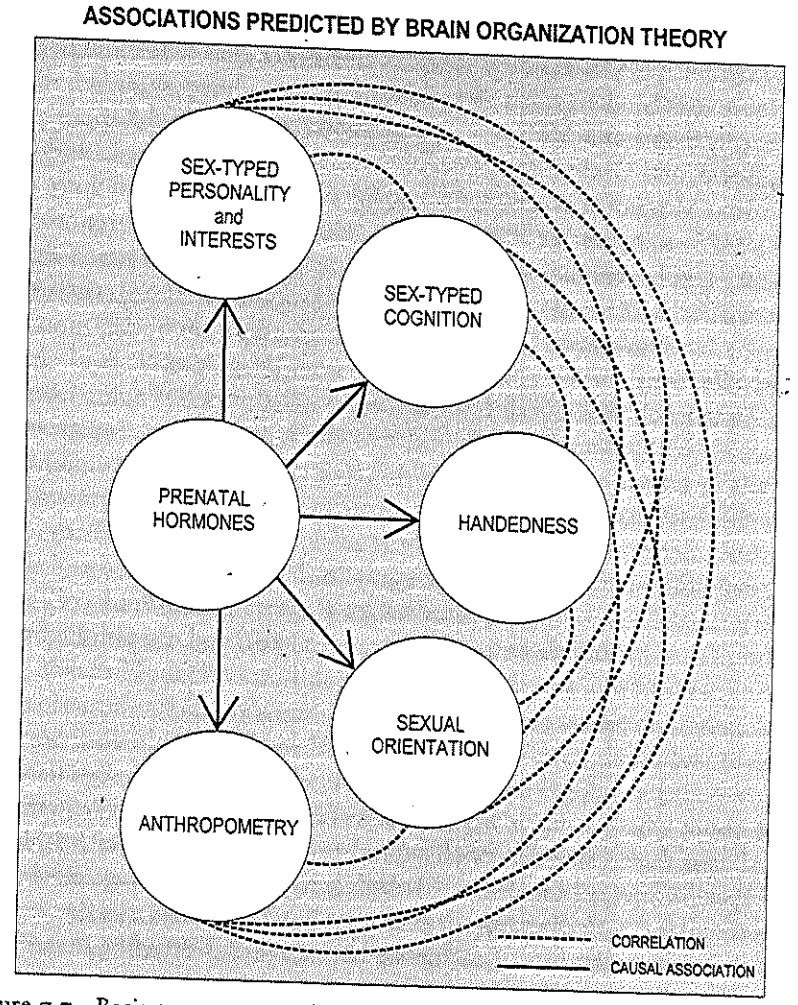

Figure 7.7. Basic two-way associations predicted by brain organization research on sexual orientation. Solid arrows represent causal associations (early hormones are theorized to cause sex differentiation of each trait on the right); dashed lines represent correlations that are predicted if both traits undergo sex differentiation by hormones. Note that the domains are broad, and early hormones hypothetically could affect either a whole domain or limited elements within a given domain.

Brain organization theory suggests that women with CAH or prenatal DES exposure should have higher rates of lesbian or bisexual orientation, and that men with CAH or with prenatal DES exposure may have lower

rates of gay or bisexual orientation compared with the general population. Let's start with CAH. There is consistent evidence that women with CAH are more likely than their female relatives to have same-sex desire (Ehrhardt, Evers, and Money 1968; Dittmann, Kappes, and Kappes 1992; May, Boyle, and Grant 1996; Zucker et al. 1996; Hines, Brook, and Conway 2004; Morgan et al. 2005; Gastaud et al. 2007; Meyer-Bahlburg et al. 2008). DES exposure is a different story. Contrary to some early evidence that DES exposure may increase lesbian or bisexual orientation among women (Ehrhardt et al. 1985; Meyer-Bahlburg et al. 1995), the largest study by far shows that DES-exposed women are, if anything, *more heterosexual* than their unexposed counterparts (Titus-Ernstoff et al. 2003). (The Titus-Ernstoff study did not assess sexual attractions apart from behavior, but with a sample that includes nearly 5,700 women, any meaningful shift in sexual orientation should certainly have been detected.) Likewise, there is no evidence that sexual orientation is affected among men with DES exposure, though several studies have investigated the question, including the same very large study by Titus-Ernstoff and colleagues (2003), as well as two early studies by Richard Green and colleagues (Yalom, Green, and Fisk 1973; Kester et al. 1980), and unpublished work by Anke Ehrhardt and Heino Meyer-Bahlburg (Anke Ehrhardt, personal communication, 1998). In genetic males there are two situations for which it is difficult to draw any clear conclusion about the link between early hormones and any sex-typed traits in adulthood. First, scientists disagree about whether males with 5-alpha reductase deficiency are generally reared as girls, or whether a substantial proportion are reared within locally specific intersex categories. Second, genetic males who are reassigned and reared as female, because of either congenital anomalies affecting the genitals or traumatic loss of the penis, have a rearing situation that is dramatically unlike any other, because parents, clinicians, and possibly others are aware that the child has been gender-reassigned. From these latter two groups, it is not possible to draw a clear message regarding the effect of early hormones.

Thus, for only one out of the four most relevant groups studied do the data support the prediction that masculinizing prenatal hormones shift sexual orientation toward greater interest in females. Notably, of the four groups, the members of this group, women with CAH, are the only ones with identifiably unusual social experiences related to gender, such as high rates of sex reassignment and genital surgery due to intersex birth, and widespread expectation that they will be gender-atypical. Also, as noted above, when women with CAH are compared with the general population, or when behavioral measures rather than fantasies are the criterion, women with CAH do not seem more likely to be sexually oriented toward women. Now let's examine the specific loops that would hypothetically link sexual orientation with early hormones via another domain, beginning with cognition.

Cognition. Comparing lesbians to heterosexual women, most studies find no differences in terms of cognitive ability (Tuttle and Pillard 1991; Gladue and Bailey 1995b; Wegesin 1998; Neave, Menaged, and Weightman 1999; Rahman, Wilson, and Abrahams 2003; Rahman 2005), and those studies that do find differences have been about as likely to find that lesbians are more feminine (Gladue et al. 1990) as they are to find that lesbians are more masculine (Rahman, Abrahams, and Wilson 2003; Collaer, Reimers, and Manning 2007).

Many studies suggest that gay men are somewhat more "feminine" on selected cognitive traits compared to straight men (for example, Willmott and Brierley 1984; Sanders and Ross-Field 1986; Gladue et al. 1990; McCormick and Witelson 1991; Sanders and Wright 1997; Wegesin 1998; Neave, Menaged, and Weightman 1999; Rahman and Wilson 2003; Rahman, Wilson, and Abrahams 2004; Collaer, Reimers, and Manning 2007), but most studies find differences in only one or two out of multiple aspects of cognition tested, and a substantial minority of studies find no difference between gay and straight men (Tuttle and Pillard 1991; Gladue and Bailey 1995b; Cohen 2002; Esgate and Flynn 2005).[23]

It could be that more consistent cognitive differences between heterosexual and gay men would be found if studies had larger samples.[24] But small sample sizes cannot fully explain the inconsistent results, because even nonsignificant differences are too often against the predicted direction. Tuttle and Pillard (1991), for example, found that gay men scored in a more masculine direction on three of six cognitive tasks. Similarly, Cohen (2002) found that spatial ability was not broadly different among gay versus straight men, and he also found that among gay men, *higher* scores on a masculinity scale were correlated with *worse* spatial relations. Other studies find a few limited differences, but these are complex. For example, McCormick and Witelson (1991) found cognitive differences between gay and straight men, but only among men who were not consistent right-handers. Likewise, Rahman, Andersson, and Govier (2005) found that gay men were no less likely than straight men to use "male-typical" navigational strategies (using cardinal directions, and identifying distances on a map), though they were more likely than straight men to use the "femi-

nine" strategy of referring to landmarks, which resulted in gay men using more navigational strategies overall—a difference, but not one that neatly supports the idea that gay men are "gender-atypical" or "feminine."

What about evidence of more male-typical cognitive traits in people with higher-than-typical prenatal androgens? Here, the evidence is clear: cognitive traits are not consistently masculine among either males or females with high "masculinizing" fetal hormones, including those with CAH and those with DES exposures. While some studies have shown cognitive differences between CAH-affected and unaffected groups, the differences do not support the theory that androgens specifically enhance masculine, or inhibit feminine, cognitive traits (see Chapter 4 for more details). Instead, there may be subtle, generalized cognitive deficits among people with CAH, especially those with severe forms of the disease (Helleday, Bartfai, et al. 1994; Helleday, Siwers, et al. 1994; Plante et al. 1996; Johannsen et al. 2006). Likewise, there are no consistent cognitive differences among DES-exposed males or females compared with unexposed groups. (See Hines 2004, especially chapter 9, for a good review of cognition among subjects with unusual early hormone exposures.)

Handedness. Lateralized brain function refers to how certain tasks (both cognitive processing and motor tasks like right- versus left-hand dominance) are accomplished by one hemisphere versus the other, or both hemispheres. As Mustanksi, Chivers, and Bailey (2002) have noted, "Despite inconsistent results across studies, narrative reviewers of the literature have suggested that men show a modest increase in functional asymmetry compared to women, meaning that the hemispheres of men are more specialized" (99). Many brain organization studies have been built on this tentative foundation, based on the further supposition that functional asymmetry is related to testosterone exposure. But meta-analyses suggest that the only aspect of functional asymmetry that is clearly sex-differentiated is handedness (Sommer et al. 2008; Vogel, Bowers, and Vogel 2003; see also my discussion in Chapter 3). Because brain organization theory is meant only to explain the development of traits that are sex-differentiated, the following discussion does not include every study that has reported some evidence relating sexual orientation to various indicators of functional asymmetry. Instead, I confine myself to studies of handedness.

In the scenario predicted by classic brain organization theory, gay men would be less left-handed, and lesbians more so, than their same-sex heterosexual counterparts. But as I noted when discussing how sexual orientation is framed, studies of handedness do not consistently build upon the

usual brain organization hypothesis that homosexuals of either sex will be sex-atypical. Instead, a great many studies build on the Geschwind-Behan-Galaburda (GBG) hypothesis (Geschwind and Behan 1982; Geschwind and Galaburda 1985b), which suggests a complex web of associations among dyslexia and other learning disabilities, migraine, immune dysfunction, left-handedness, and male homosexuality. Geschwind and his colleagues linked their proposal with classic brain organization theory by way of research showing that under intense stress, pregnant rats first have a large increase in testosterone production, followed by testosterone dropping to atypically low levels. If this same process applied to humans, prenatal stress could account for a link between some traits that are thought to be caused by high levels of testosterone (such as dyslexia and left-handedness) and traits thought to be caused by low levels (as is usual, they slot male homosexuality in here).

But the finer details of the GBG hypothesis often get lost along the way, and the idea has morphed into a fuzzy notion that homosexuality should be correlated with left-handedness and other signs of atypical lateralization. Thus, contrary to one of the central tenets of brain organization theory, some authors have argued that male homosexuality is a sign of "hypermasculine" prenatal hormone exposures (for instance, Lindesay 1987; McFadden and Champlin 2000). Conversely, McCormick, Witelson, and Kingstone (1990) tried to reconcile greater left-handedness among both gay men and lesbians with brain organization theory by suggesting that, contrary to the GBG hypothesis that *higher* prenatal testosterone leads to left-handedness, left-handedness in *men* results from *lower* prenatal testosterone. (They suggested that the development of lateralization, cognitive functions, and sexuality might follow different paths in women and men, but did not suggest how to reconcile their idea with the greater population prevalence of left-handedness in men.)

Given the complexity of the hypotheses involved, and the fact that so many of the associations are hypothetical, it should not be surprising that there is some mutual contradiction among the studies of handedness and sexual orientation. Reviewers differ as to whether the current evidence does or does not point to differences between heterosexuals and homosexuals, especially men (Lalumiere, Blanchard, and Zucker 2000; Mustanski, Chivers, and Bailey 2002). And specific patterns of associations do not hold up. First, and perhaps most importantly, the association between left-handedness and male homosexuality is grafted to brain organization theory by way of a hypothetical association between male homosexuality and prenatal stress—but the studies of prenatal stress on balance don't support the theory (Ellis et al. 1988; Schmidt and Clement 1990; Bailey, Willer-

man, and Parks 1991; Ellis and Cole-Harding 2001). Second, scientists tend to agree that there should be a stronger pattern of left-handedness among lesbians than among gay men (because male homosexuality would be associated with left-handedness only in cases of prenatal stress early in gestation, whereas female homosexuality would be associated with left-handedness whenever androgen levels were sufficiently high throughout gestation to affect both traits). On its own, the evidence on handedness would seem to support this (Lalumiere, Blanchard, and Zucker 2000). However, this same general constraint on the model should suggest that virtually all aspects of cognition, emotion, or morphology that are affected by early hormones should be linked more consistently in lesbians than in gay men, and this is decidedly not the case. Lesbians do not show consistently "masculinized" traits, nor are there stronger associations in general among sexual orientation and cognition, emotion, or morphology among lesbians than among gay men (quite the converse, in fact). Keep this in mind as we review the other kinds of traits that have been studied to explore a link between sexual orientation and hormones.

The data from studies of handedness among gay men and lesbians are lackluster, at best. But the major difficulty in reconciling these studies with other brain organization research is that the cohort studies (research on individuals with documented prenatal hormone anomalies) do not support the GBG hypothesis: that is, there is no good human evidence that the many traits investigated as indicators of lateralization are actually "perturbed" by early hormone exposures. Just as men and women with CAH are no more likely to have masculine cognitive profiles, they are no more likely to be left-handed. There is suggestive evidence, however, regarding DES exposure and left-handedness. A few small studies have found more left-handedness among DES-exposed women (Schachter 1994; Scheirs and Vingerhoets 1995; Smith and Hines 2000), but the largest study by far, which included 5,686 DES-exposed women, found no evidence of increased left-handedness (Titus-Ernstoff et al. 2003). That same study, though, did find a slight increase in left-handedness among 1,432 DES-exposed men. However, given that DES has a wide range of documented teratogenic effects, subtle shifts in handedness might be seen in DES without specifically implicating the brain organization hypothesis. And again, the full set of associations that might be expected to show up, especially in very large studies with great statistical power, are absolutely contradicted: there is no link between DES and sexual orientation in men or in women (Titus-Ernstoff et al. 2003).

Sheri Berenbaum, a prolific and well-respected researcher of CAH, has offered this sharp rebuttal of the GBG hypothesis:

> Anomalies in cerebral dominance, immune functioning, abilities, and neural crest development are hypothesized to correlate with each other because all result from high levels of prenatal testosterone. Studies directly evaluating the effect of testosterone on these traits do not validate the model: sex ratios and animal studies suggest that testosterone has a protective, rather than facilitory, effect on autoimmune diseases; individuals with high levels of early testosterone do not have elevated rates of left-handedness or learning disabilities. (Berenbaum and Denburg 1995, 79)

Gay men and lesbians may be more likely than other people to be left-handed, but the evidence suggests that this cannot be explained by "cross-sex" hormone exposures. Findings of increased left-handedness among gay men and lesbians may be a simple result of noncomparable sampling in the studies done to date (that is, the association is not real, but instead is the result of biased sampling). In particular, heterosexual samples are disproportionately recruited from university students and staff, then compared to lesbian and gay samples recruited from various community sources. Alternatively, even a real association does not necessarily point to a common single factor influencing both handedness and sexual orientation. Instead, various alternative explanations are possible, including the chance that multiple factors operating very early in development make some individuals less "conforming," which in turn could affect a broad range of traits with diverse developmental trajectories.[25]

Anthropometry. As should by now be clear, there is a seemingly endless array of possible traits on which scientists might focus to explore the possibility that early hormone exposures shape sexual orientation. Any trait that is thought to differ, on average, between males and females, or any trait that is also possibly influenced by testosterone early in development, is a potential candidate for investigation. Many studies use an approach that can be broadly described as anthropometry. To date, studies have examined such body parts and proportions as penis size, height and weight and/or body mass index (Perkins 1981; Bogaert and Blanchard 1996; Bogaert 1998; Bogaert, Friesen, and Klentrou 2002; Bogaert 2003; Martin and Nguyen 2004), and the ratio of the second to the fourth digit, or 2D:4D (for instance, Robinson and Manning 2000; Williams et al. 2000; Lippa 2003; Rahman and Wilson 2003; Putz et al. 2004; McFadden et al. 2005; Rahman 2005; Rahman, Korhonen, and Aslam 2005; Kraemer et al. 2006; Collaer, Reimers, and Manning 2007).

A few things should be noted about this line of inquiry. First, the notion that lesbians and gay men have cross-sex body types predates the theory of brain organization (Terry 1999); the notion of homosexuals as "sexually

intermediate" was an implicit rationale for applying brain organization theory to the question of human sexuality in the first place (van den Wijngaard 1997). Thus, some studies in this vein are not included in the current review (which is not necessarily a shortcoming, given pervasive biases in subject recruitment for sexual orientation studies well into the 1970s). Second, other than the 2D:4D studies, nearly all such studies in recent years have been conducted by Ray Blanchard and/or Anthony Bogaert; generally data are more convincing when they come from independent research teams. Third, although there are isolated findings of gay versus straight differences on one bodily trait or another, overall these studies do not suggest any consistent pattern regarding cross-sex body types among gay men and lesbians (see the summary in Bogaert, Friesen, and Klentrou 2002). Finally, the strongest evidence for brain organization theory would consist of a triangulation among studies that find some trait to differ between gay men and/or lesbians and their heterosexual counterparts (the case-control studies), on the one hand, and between hormone-exposed and unexposed subjects (the cohort studies, such as CAH versus controls), on the other hand. There are no such patterns.

Prior to the last decade, the default line of inquiry with the anthropometric studies was the old "intermediacy" hypothesis, which fits best with brain organization theory's predictions: gay and bisexual men were expected to be less "masculine" (or more similar to heterosexual women) on any given physical trait measured, while lesbian and bisexual women were expected to be less "feminine" (or more similar to heterosexual men). With the rise of the GBG hypothesis, though, which raised the possibility that gay and bisexual men might have *higher* than typical androgen exposures during development (at least during some periods), a few studies appeared that suggested hypermasculinization of physical traits among gay men. One highly cited (but unreplicated) report, for example, suggested that gay men have larger penises than straight men (Bogaert and Hershberger 1999). Conversely, another report—also unreplicated—suggested that gay men and lesbians both have a cross-sex pattern of long bone development (Martin and Nguyen 2004).[26]

The 2D:4D studies, which constitute the largest subset of anthropometric studies and are currently quite popular, have produced contradictory results that suggest no real effect. The "2D:4D" ratio refers to the relative length of the second (index) finger to the fourth (ring) finger. This ratio is considered sexually dimorphic, because men's 2D:4D ratio tends to be lower, reflecting a relatively longer index finger in women. Of eight studies comparing 2D:4D in gay and straight men, three studies reported higher (feminized) ratios in gay men (Lippa 2003; Collaer, Reimers, and

Manning 2007; McFadden and Shubel 2003); two studies found gay men to have *lower* (hypermasculinized) ratios (Robinson and Manning 2000; Rahman and Wilson 2003); and three found no difference in men's digit ratios by sexual orientation (Williams et al. 2000; Voracek, Manning, and Ponocny 2005; Kraemer et al. 2006).[27] The findings are similarly mixed for women. Three studies report "masculinized" digit ratios to be associated with same-sex orientation in women (Williams et al. 2000; Rahman and Wilson 2003; Kraemer et al. 2006), while one small study (Anders and Hampson 2005) and the two largest studies (Lippa 2003; Collaer, Reimers, and Manning 2007) found no differences between lesbians and heterosexual women.[28]

Thus, the anthropometry studies, even the popular comparison of 2D:4D ratios, do not on balance suggest that gay men or lesbians differ from same-sex comparison groups. Likewise, there are only weak and inconsistent indications that 2D:4D is affected as predicted in people with known unusual prenatal hormone exposures. The strongest evidence to date that 2D:4D ratios are affected by prenatal hormones comes from a recent study by Sheri Berenbaum and colleagues (Berenbaum et al. 2009), who found a statistically significant, but very slight, shift toward the "feminine" pattern of a high 2D:4D ratio among women with complete androgen insensitivity syndrome. Berenbaum and colleagues concluded that "digit ratio is related to effective androgen exposure, but the relation is too small to use digit ratio as a marker for individual differences in androgen exposure" (5123).

Personality and interests. The domain of sex-typed personality and interests is harder to summarize, for a variety of reasons. The specific variables involved range over a wide terrain, including things like aggression, career and hobby interests, nurturing, toy and game preferences, preference for friendships with same-sex or other-sex peers, risk seeking, and interest in objects versus in people, as well as broad composites of "gender role behavior" and masculinity and femininity inventories. Although there are increasing trends toward using more standardized measures of these variables, standardization in this domain lags behind the specificity that has been achieved in tests for aspects of cognitive function. Among the cohort studies, reports seldom focus on those aspects of personality that do not differ, but highlight only those items that are correlated with hormone exposure status. So it is especially difficult to track the precise results in these studies. I defer detailed consideration of these issues to Chapter 8, which is devoted to sex-typed interests, especially as they are assessed in the cohort studies.

I have devoted a section of Chapter 9 to thinking about the relationship between sex-typed personality and interests, on the one hand, and sexual orientation, on the other hand, as a deeply neglected issue of context in brain organization research. Here I just briefly sketch some of the difficulties involved. Unlike the other studies I examine, data on gay men's and lesbians' personality and interests are available from many kinds of research, most of which have nothing to do with brain organization theory. For one thing, sex-atypical personalities and interests might develop much later than most traits that are hypothetically affected by prenatal hormones, so investigators posit a wide range of causal theories for possible differences between lesbians and gay men and their heterosexual counterparts, including entirely social theories. For another thing, quite a bit of data on personality and interests among gay men and lesbians is not generated or interpreted in terms of *any* causal theory of sexual orientation; rather, a great many studies explore how experience as a sexual minority may lead to personality differences, because of such factors as stigma, discrimination, and specific subcultural influences.

With those important caveats in mind, a great many studies suggest that gay men are somewhat atypical (gender-nonconforming) as a group, in terms of interests and other aspects of personality that are considered sex-differentiated; there is less association between gender and sexual orientation in women, but many studies do find more masculine traits among lesbians compared with heterosexual women (for instance, Whitam and Mathy 1991; Lippa 2002; Udry and Chantala 2006; Lippa 2008). The specific variable in this domain that may be of greatest interest is childhood play, especially toy preferences. At least in theory, this is one aspect of personality and interests that is unlikely to be affected by the experience of being gay or lesbian (because play behaviors are usually assessed for early childhood). Retrospective recall bias probably inflates the extent to which lesbians and gay men recall their childhood play behaviors as atypical, but one recent study used home videos from subjects' childhoods to demonstrate that gender nonconformity was in fact evident in children who would later become gay and lesbian adults (Rieger et al. 2008). Below and in the following chapters, I offer some cautions against too readily attributing these associations to the influence of early hormones.

Sex-typed interests, especially childhood play, is the domain in which girls and women with CAH are more consistently masculine than their CAH-unaffected counterparts. However, data do not on balance suggest sex atypicality in terms of personality and interests (often called "gender role behavior") among genetic females with atypical hormone exposures other than CAH, nor for genetic males with sex-atypical early hormone exposures from any cause. Among nonclinical samples where some information on hormones is available (for instance, hormones in amniotic fluid or in maternal blood during pregnancy), there are mostly negative findings. Neither study that examined hormones in amniotic fluid found any relationship between testosterone and sex-typed play (Grimshaw, Sitarenios, and Finegan 1995; Knickmeyer, Baron-Cohen, et al. 2005), in either girls or boys. (Several papers from a longitudinal study by Simon Baron-Cohen's team have reported associations between testosterone in amniotic fluid and some indicators of sex-typed interests, but the findings are both inconsistent across the various reports and unreplicated, as I detail in Chapter 8.) Two studies have tested associations between testosterone and sex-hormone-binding globulin in maternal blood, on the one hand, and gendered behavior, on the other (Udry, Morris, and Kovenock 1995; Udry 2000; Hines, Golombok, et al. 2002). Both studies found associations, but they are both small and contradictory between the two studies. (See Chapter 8 for detailed discussion.)

This is a good time to return to the diagrams showing the network of associations that brain organization theory predicts. If the enormous variation in measurement and study design were not at issue, formal meta-analysis would be ideal. Because that is not possible, the diagrams should be understood as heuristic devices rather than precise representations of the evidence. Just as with formal meta-analysis, the kind of synthesis I do here involves a tension between aiming to include as much of the data as possible and aiming to maintain precision in terms of which components of domains show association with other variables, how variables are measured, and the precise nature of the relationships that have been found. For example, an association between sexual orientation and a cognitive trait might be found only in right-handed men, which raises the question of whether and how to include that with associations that are found without regard to handedness. I have resolved the tension in this way: the narrative summaries above draw attention to gaps, discontinuities, and inconsistencies, but the figures that follow err on the side of "lumping" the data to generate a critical-yet-generous summary.

The evidence for virtually every trait studied is different for males versus females, so it is important to map these associations separately by sex. Figure 7.8 shows the pattern of associations for genetic females, and Figure 7.9 shows the pattern for genetic males. Filling in the left side of Figure 7.8 from the cohort studies, early masculinizing hormone exposure is associated with more sexual orientation toward women and more "masculine" toy preferences. But both of these associations are found only in women

with CAH, and the sexual orientation difference is found only when CAH women are compared with small and apparently unusual control groups, not when compared with rates of same-sex desire or behavior among women in the general population. The exception might be the subset of women with the most severe form of CAH, among whom a high proportion were initially "announced" and reared as boys, but later sex-reassigned.

Filling in the right side of Figure 7.8 from the case-control studies, lesbians, as a group, tend to be more left-handed, or less consistently right-handed, than heterosexual women, and also seem to have had more "masculine" behavior as children, including toy preferences. Nonetheless, there are two big problems with concluding that these two pairs of associations mutually support brain organization theory. First, you can't trace a causal pathway that allows you to conclude that the association between left-handedness and lesbianism is due to early hormones, because the only association between early hormones and left-handedness is a weak and inconsistent finding among women who experienced prenatal DES exposure—but DES is clearly not associated with shifts in sexual orientation. Second, as demonstrated in the discussion of how sexual orientation is measured, the phenomenon of same-sex orientation is quite different between these two pairs of associations. The "lesbianism" that is linked with prenatal hormone exposures is not at all the same as the "lesbianism" that is linked with left-handedness or childhood toy preferences. In fact, most of the case-control studies use a definition of "lesbianism" that would exclude nearly all the subjects in the cohort studies—even though the latter, as a group, have had the highest exposure to "masculinizing" hormones of any women. In other words, the pattern of data strongly contradicts the dose–response relationship that the theory would predict, a point I elaborate below. Finally, given the broad variability among lesbians on the huge array of variables that have been assessed, the general trend of similarity among lesbians and other women, and the long history of important psychological and somatic "differences" that have been found between lesbians and other women only to be discarded later (Terry 1999), one might do well to recall the advice of Dr. A, and "not get bogged down in dogma . . . thinking of lesbians as sort of like guys" (Dr. A interview, February 5, 1999).

For men, the pattern looks different, but equally problematic. First, consider the left side of Figure 7.9. From cohort studies, there is no support for the idea that hormonal variation among genetic males who are reared as male is associated with sexual orientation. Likewise, cohort studies

don't indicate any association between male-typical cognition and masculinizing hormone exposures. There does seem to be an association between DES exposure (a "masculinizing" condition) and increased left-handedness (also masculine, so in accord with the theory). And finally, there seems to be a very small association between androgen insensitivity syndrome (a "feminizing" condition) and more female-typical digit ratios among genetic males.

Moving to the right side of Figure 7.9, the case-control studies show associations between sexual orientation and both cognition and personality. They also show associations between handedness and sexual orientation—but this association is in the opposite direction to the previous two findings. That is, though data are notably mixed, quite a few studies suggest that gay men as a group have one or more aspects of cognition or personality that are more feminine than heterosexual men, but gay men have a slightly more masculine profile in terms of handedness. (Recall, too, that the hypothesis that is supposed to reconcile this discrepancy—the notion that gay men have experienced greater prenatal stress—is also not supported by evidence.) In the end, even with the generous interpretation that lumps together scattered findings for various aspects of cognition and personality in order to support a link between these domains and sexual orientation, there is no way to trace a loop between sexual orientation and prenatal hormones for genetic males.

What if we loosened the criteria for evidence and included males with 5-alpha reductase deficiency and other males who have anomalous or damaged genitals, and who were therefore sex-reassigned as female and reared more or less as girls? Keeping in mind all the caveats implied by that "more or less," there is a much higher rate of sexual orientation toward women among this group than among genetic females who are reared as girls. And among those who assign themselves back to the male sex in adolescence or adulthood, there is a predictably masculine pattern of personality and interests. If these cases were included, we could close the loops that link sex-typed cognition and personality and interests (but not handedness) to early hormone exposures. In the end, the decision to include or exclude these cases is a subjective matter. Given that there is not even a hint from any of the other cohort study designs that early hormone exposures influence sexual orientation, I believe we should not ask these extremely idiosyncratic cases to bear much weight in the overall network of evidence.

Considered together, data from the studies on cognitive traits, handedness, anthropometry, and personality and interests suggest that, as the particular traits examined in case-control studies of brain organization have

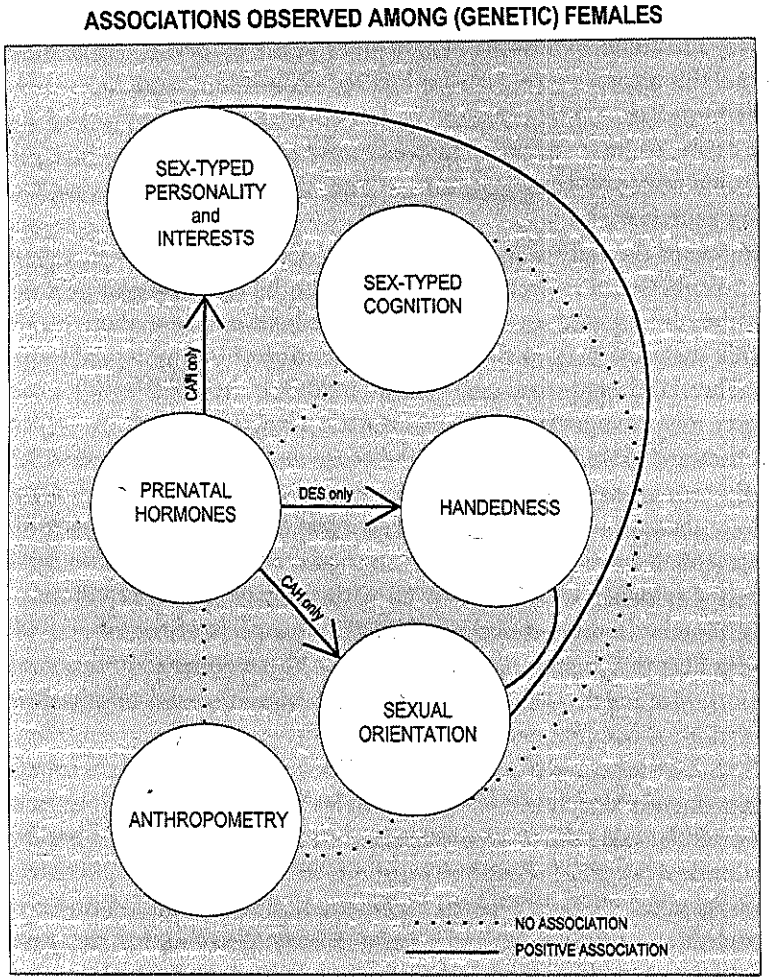

Figure 7.8. Two-way associations in genetic females. Support for the theory requires at a minimum that we could trace a complete loop from prenatal hormones to sexual orientation, from sexual orientation to another domain, and from that second domain back to prenatal hormones. There is a complete loop if we focus only on the case of genetic females with CAH, and on the (weak, but generally positive) association between sexual orientation and sex-typed personality and interests. This chain of associations is better explained by variables of biological and social context than by prenatal hormone exposures (see Chapter 9).

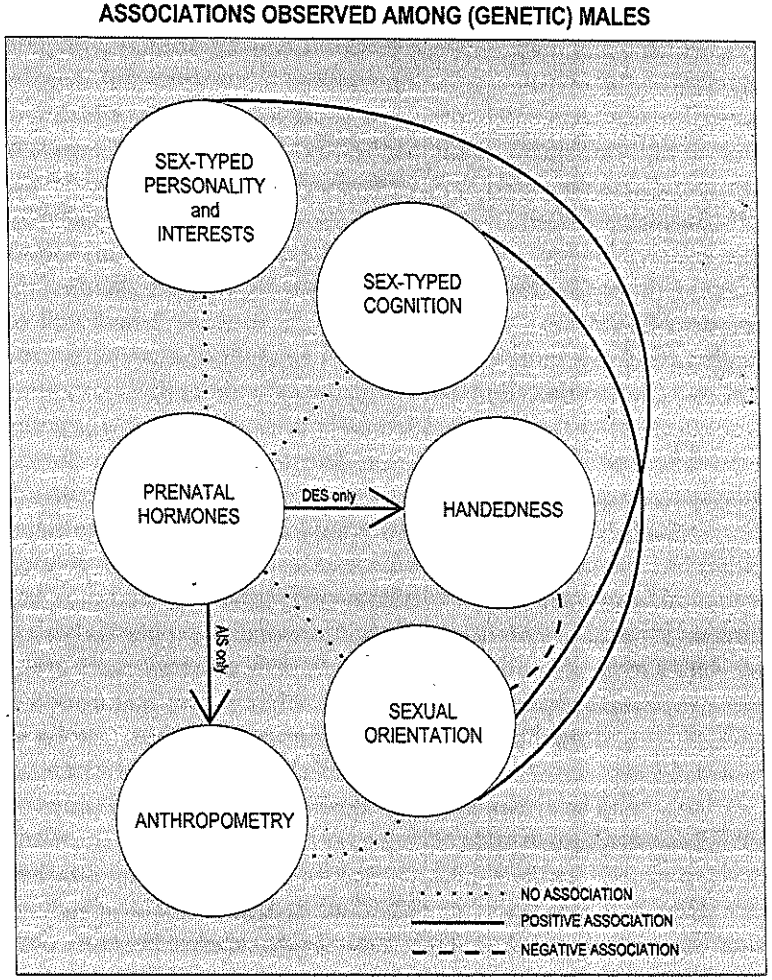

Figure 7.9. Two-way associations in genetic males. Support for the theory requires at a minimum that we could trace a complete loop from prenatal hormones to sexual orientation, from sexual orientation to another domain, and from that second domain back to prenatal hormones. Note that the associations between sexual orientation and both sex-typed cognition and sex-typed personality and interests are not entirely consistent. Because there is no complete loop, the theory is not supported for genetic males. Note that these associations do not include genetic males who are reassigned and reared as females, nor males with 5-alpha reductase deficiency; there are too many confounders in rearing conditions and adult gender role for these data to yield a clear message.

proliferated, the evidence gets farther and farther from the theory—yet many scientists, and many laypeople who follow the studies with great interest, cling to the notion that better evidence is just around the corner. It's common for researchers to argue that small sample sizes, which make it difficult to find statistically significant differences between groups, can explain major discrepancies in brain organization research on sexual orientation (Mustanski, Chivers, and Bailey 2002; Lalumiere, Blanchard, and Zucker 2000). However, it is not simply the case that the many failures to support the theory are due to small sample sizes, because the *direction* of differences often defies the predictions of the theory. Virtually all of the studies that find gay men to be more feminine on various traits are at odds with those studies that link left-handedness with male homosexuality. Conversely, the slight increase in left-handedness among lesbians is at odds with the failure to find masculinization on other traits, such as the 2D:4D ratio or spatial relations. Lesbians are not, on balance, more consistently gender-atypical than gay men are, as would be required by the elaborate story that has been spun around homosexuality and left-handedness.

If prenatal hormones really do create a sex-differentiated brain, including directing sexual orientation, then homosexuality should be both more frequent and more extreme among groups with more extreme cross-sex hormone exposures. But that is not, in fact, what brain organization studies show. In fact, the cohort studies show that *extreme cross-sex hormone exposures are associated with only very small shifts in sexual orientation.* How can this be? The evidence defies "dose–response" relationships of the sort that have made such a strong case for the causal role of hormones in the development of genitals, and in certain behaviors among infrahuman animals. Here is where the great difference in measurement of sexual orientation between the cohort and the case-control studies becomes particularly problematic.

Women with CAH have the "highest dose" of prenatal androgens, so it is useful to ask what exactly "same-sex orientation" looks like among women with CAH. Among studies that find more homosexual or bisexual orientation, several find higher rates of *same-sex fantasy or attraction* but not same-sex *behavior* (Dittmann, Kappes, and Kappes 1992; Zucker et al. 1996; Gastaud et al. 2007). An article by Ralf Dittmann and colleagues (1990, "Congenital Adrenal Hyperplasia I") provides a helpful example. In spite of extremely liberal statistics and a vast number of comparisons, no sexual behavior item reached either "significant" or "trend" level, for any age group; the only single item that reached significance—and it does seem to be an interesting item—is that more women with CAH express the

wish for a steady or long-term relationship with a female partner (6 of 34 women, or almost 18 percent, versus 0 for the sisters).[29]

As I've noted, while many studies do find increased rates of same-sex behavior among women with CAH compared with their female relatives and other controls, most studies do *not* show same-sex orientation (fantasy *or* behavior) among women with CAH to be any higher than among women in the general population. Several large, population-based studies in the 1990s provided estimates of same-sex orientation and behavior in the general population for the United States, as well as France and Great Britain (Laumann et al. 1994; Sell, Wells, and Wypij 1995; Michael et al. 2001). These studies have estimated that the proportion of women who have same-sex partners sometime in their adult lives is somewhere between 1.8 percent and 3.6 percent, and the proportion who report being attracted to other women runs from 8.6 percent to 18.6 percent. Thus, the differences between women with CAH and control women are not necessarily because women with CAH have unusually high levels of same-sex orientation, *but may be the result of unusually low levels of same-sex orientation among comparison groups.* This might be due to very small sample sizes, especially among comparison groups. Even though CAH itself is rare, the comparison groups are kept artificially small because of a belief that female relatives of girls and women with CAH provide the best control for rearing experiences and social environment variables. In practice, investigators include cousins as well as sisters—and even female relatives of males with CAH, in some cases—so the strategy is of questionable value, particularly because the ideal strategy for studying a rare condition is to have a large unaffected-to-affected ratio.

It's worth pointing out that the largest and most comprehensive study of sexual orientation and CAH done to date confirms the general failure of evidence from CAH to show that increasing levels of prenatal androgens lead to increasing levels of female homosexuality, contrary to the authors' interpretation. Heino Meyer-Bahlburg and colleagues (2008) have conducted the only study of sexual orientation that includes women with the mildest, nonclassical form of CAH. This subset of patients is especially interesting because while a high proportion of women with the classical form of CAH are born with masculinized genitalia, women with the nonclassical form are not. Yet women with nonclassical CAH still have some increase in same-sex orientation compared with their same-sex relatives, leading Meyer-Bahlburg and colleagues to consider their findings as a positive indication of a dose–response relationship between early androgens and male-typical sexual orientation. There are four big problems with this

interpretation. First, a dose–response relationship would be best indicated by a progression of increasing same-sex orientation from the lowest level of early androgens, seen in women with nonclassical CAH, through the midrange of early androgens, seen in women with the simple virilizing form of CAH, to the highest range, found in women with the most severe, salt-wasting form. Instead, women with the simple virilizing form showed *less* same-sex eroticism on all aspects of orientation than did the women with nonclassical form (Meyer-Bahlburg et al. 2008, 91). Second, increased same-sex orientation among women with the nonclassical form does not fit well with brain organization theory, because according to most evidence at this point, androgens are not elevated in these women until after the prenatal period when hormones are purported to organize the brain for human psychosexuality (Meyer-Bahlburg et al. 2008, 96). Thus, the increase in same-sex orientation in this group may speak more to other variables—possibly later hormone effects, including indirectly through self-image in response to bodily masculinization, and just as possibly to the social effects of being told during late childhood, adolescence, or even adulthood that androgens are high and that personality and sexuality may be affected (see Chapter 9 for elaboration). Third, only women with the most severe, salt-wasting form had a rate of same-sex eroticism that is clearly higher than general population levels, which for women are estimated to be around 3 percent for same-sex behavior, and up to 19 percent when same-sex attraction is included (Sell, Wells, and Wypij 1995). Fourth, note that the control group in this study is again very small (just 24 unaffected women, compared with a combined CAH sample of 141 women). Given the relatively low prevalence of same-sex orientation in the general population, it is not very surprising to find little indication of same-sex eroticism in this group.

The way that Zucker et al. (1996) characterized their own sample of CAH-affected women applies equally well to the pattern of findings across the multiple studies of sexual orientation in CAH: *"the majority of the CAH women were exclusively heterosexual in their sexual orientation, despite the clear evidence that such women were exposed to prenatal levels of androgen that are in the range of normal males"* (314, emphasis added). Even strong proponents of brain organization theory occasionally notice that this is unexpected, given the predictions of the theory. Wilson and Rahman (2005), for instance, find it "surprising that larger shifts in sexual orientation were not reported" in studies of women with CAH (75).

There is something badly wrong, then, when other researchers summarize the evidence as indicating that in women with CAH, sexual orientation is "much closer to the male pattern" (Witelson, quoted in Weise

2006), or that "lesbianism in these girls is off the charts" (Dr. I interview, August 1998). The popular summaries of data on sexual orientation among women with CAH can be particularly far-fetched. In her pop-science book *Why Men Don't Iron,* Anne Moir asserts that "48 percent of them confess to having homosexual fantasies, and 44 percent are actively lesbian" (Moir 2003, 46). Moir's summary should raise eyebrows, even aside from the gratuitous insinuation that same-sex fantasies are something to "confess" rather than merely "report." This points back to the last question about defining and measuring sexual orientation, to which I promised to return. Given that these scientists don't agree with each other about who counts as a homosexual, how well do they agree with the rest of us? Are the people designated "homosexual" in these studies the same people that are generally thought of as homosexual in the real world?

THROUGHOUT this book, I argue that technical decisions associated with measuring human sexuality are important at a number of levels. It is important to look at what scientists measure when they study sexuality, and it is especially important to see how consistent their definitions are within and between studies. But these technical decisions gain more meaning when they are viewed in larger contexts. Each set of hypotheses and data does not stand on its own, but is delivered by way of a scientific interpretation whereby the researchers guide readers toward the conclusions that they believe are appropriate. As research findings are generalized beyond particular studies or projects, the finer points of operational definitions fade and general terms like *homosexuality* or *masculinization* are left to carry the burden of meaning. Earlier I described measures as "assumption containers," through which certain ideas get carried along through and beyond scientific work, gaining the implicit authority of being "scientific" without ever being tested.

In the dominant schema of brain organization research, sexual orientation is a fairly solid and well-defined variable. People generally are attracted to people of the same sex or of the other sex; many fewer individuals are genuinely attracted to both. While social pressures may restrict the amount of same-sex activity that some people have, and even push some people into opposite-sex activity in order to "conform," people's fantasies and behaviors would generally pretty much go together if their behavior weren't constrained by homophobic convention. According to these researchers, it doesn't really matter that much if you pick one aspect versus another (such as fantasy, behavior, or identity) in order to "capture" sexual orientation in a particular study population. If it is most convenient to recruit subjects by self-identity, then confirm by sexual fantasies, you

won't get a different group than if you began by screening a much larger group for behaviors, fantasies, and identity.

These important assertions can be checked against a large body of research on sexual behavior, desires, and identity. From the studies in the public health literature that separately report data on two or more of these dimensions, one can see that it actually makes a great deal of difference which is used as a gauge of orientation (see, for example, Laumann et al. 1994; Sell 1997; Diamond and Savin-Williams 2000; Young et al. 2000). It is also important to note that most brain organization studies do not give enough information to see how their definitions and exclusion criteria affect the composition of their samples, though several investigators indicate in published work and in interviews that they retain only subjects whose sexual orientation perfectly lines up across all aspects of behavior, attractions, and identity. Technically, their selection methods add "consistency" to the operational definition of sexual orientation. When scientists who do this turn around and cite one another to support their definition of sexual orientation as "stable" and "consistent" and a "dichotomous trait" (as Simon LeVay, Dean Hamer, and Michael Bailey all do in recent books), this is misleading. That is because their methods have forced a tight association among fantasies, behavior, and self-declared sexual orientation.

These are teleological methods. Such methods do more than filter out complexity and variation in terms of the subjects of any particular study. And they do more than make brain organization theory seem stronger than it actually is. They add to the overall literature on sexual orientation, creating supposed evidence that sexual orientations come in neat categories, and making disagreement among specific dimensions of orientation look unusual and respondents who "fail" to be consistent look like liars or candidates for therapy.

Moreover, even if one adopts the most generous possible attitude toward the measures used in brain organization studies, the findings across the two main sets of studies cannot be reconciled with one etiological theory. The people with the most extreme cross-sex hormone exposures show little, if any, shift in sexual orientation. They also show no shift from typical populations in most other traits that supposedly signal early hormone effects, and are therefore used to infer different hormone histories between homosexuals and heterosexuals. And among the various studies in which homosexual versus heterosexual subjects are compared for differences that might implicate early hormone exposures, it is not just the case that there are many negative findings (that is, findings of no difference, which might be the result of sample sizes that are too small to find the effect). More importantly, the *direction* of evidence is often broadly inconsistent across the

studies. The data on left-handedness, which seems to be increased in both gay men and lesbians, cannot be reconciled with one developmental theory for sexual orientation for both sexes. Yet if sexual orientation develops differently in genetic males and females, then brain organization theory can't explain the association of sexual orientation with cognitive or personality-related traits in men. That's because there is no evidence linking early hormone exposures with sexual orientation in genetic males with typical rearing. In short, the data aren't just weak, they are broadly contradictory.

Some psychologists have expressed awareness that the pattern of evidence doesn't fit the current theory, and suggest that a revision in thinking about the relationship between homosexuality and gender is in order. Even Dr. A, who is quite convinced that sexual orientation is a fairly straightforward variable, thinks it is time to abandon the age-old "theory of intermediacy"—the idea that gay men and lesbians are psychologically sandwiched between the dichotomous poles of masculinity (represented by straight men) and femininity (represented by straight women). He suggests it is better to "not get bogged down in dogma . . . thinking of lesbians as sort of like guys. Think of them as killer women." Further, "maybe heterosexual men are less masculine than gay men in areas that count, like number of sex partners per lifetime" (Dr. A interview, February 5, 1999). Several other researchers, including Michael Bailey (2003), James Lindesay (1987), and Dennis McFadden and Craig Champlin (2000), have suggested that gay men are "hypermasculine" in some important respects.

In my view, this is an interesting development, and promising insofar as it suggests a willingness to leave behind brain organization theory (and the "psychological hermaphroditism" idea in which it is anchored). But as long as researchers try to shoehorn the data on the complexity of sexual desires and the complicated and variable expression of "masculine" and "feminine" aspects of personality and behavior into a tidy and linear model that injects "cross-sex" hormones on one end and spits out homosexuals at the other, such promising insights will go nowhere. What the messy tug-and-clash of research on early hormones and sexual orientation shows most clearly is the inadequacy of the current research paradigm.