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Editor's Comments

The National Association for Research and Therapy of Homosexuality (NARTH) is a professional and scientific organization founded in 1992. Its mission is to promote and ensure a fair reading and responsible reporting of scientific research about the factors that contribute to and/or co-occur with homosexuality (same-sex attraction, or SSA) and that allow psychological care to be effective for those with unwanted homosexuality. NARTH upholds the rights of individuals with unwanted SSA to receive competent professional mental health care and the rights of professionals to offer that care. In 2009, NARTH launched the *Journal of Human Sexuality (JHS)* to serve its mission and as a way of presenting, encouraging, and producing quality clinical and scientific scholarship on these topics. After its inaugural issue, *JHS* also has included articles on othersexual minority issues and onhuman sexuality in general.

Volume 4 of the *JHS* offers a lineup of papers, book reviews, and NARTH official statements. Several of the papers and all of the official statements express NARTH's commitment to the responsible conduct, dissemination, and use of science by professionals, public policymakers, legislators, and other non-mental health professionals involved in promoting medical and mental health on both a personal and public level. In particular, these documents express directly (in the official statements) and indirectly (in some of the papers written by members) NARTH's unabashed *advocacy* in support of the rights of licensed mental health professionals and their clients to give and receive competent care.

Advocacy is not to be confused with *activism*. While *advocacy* includes espousing, supporting, recommending, or explaining a view, cause, person, or group, *activism* involves and commonly emphasizes taking direct, vigorous, and sometimes militant action in support of or in opposition to an often controversial political or social goal (see www.merriam-webster.com/dictionary/; http://dictionary.reference.com/). While scientists and professionals may engage in public activism as private citizens, in their role as researchers, educators, and therapists they may only *propose,* not *impose* the truths that they perceive are relevant to or for a given person, group, situation, or issue. As such,

scientific and professional activism serves not only to undermine the credibility of scientific institutions and the mental health professions but also to erode public confidence in individual scientists and professionals.

On February 8, 2007, NARTH's Board of Directors adopted the Leona Tyler Principle that was adopted by the American Psychological Association (APA) in 1973. The Leona Tyler Principle essentially mandates that "when psychologists are speaking as members of their profession, any advocacy in which they engage should be based on scientific data and demonstrable professional experience." This means that NARTH professionals, like APA psychologists, may speak publicly "as concerned citizens, either individually or as members of groups. However, official positions taken by NARTH as a scientific organization must meet this standard" (Nicolosi, 2007).

Unfortunately, as recently deceased NARTH cofounder and past president Dean Byrd observed, "the national mental health associations (including APA) seem to have been taken over by ideologues whose activist agendas show little concern for science or professional experience. In fact, [the Leona Tyler Principle] seems to have been repeatedly violated by APA itself" (Nicolosi, 2007). Nicolas Cummings, APA past president, has offered a similar critique of the organization he once led (Byrd & Cummings, 2010).

Authors of *JHS* articles, reviews, and official statements are held to the same criteria; what is written needs to be based on a fair reading and the responsible reporting of scientific data and demonstrable professional experience. Readers of *JHS* are invited to review this, as well as past and future volumes, and to decide how well—or how poorly—we have achieved this goal. We invite your feedback and criticism. Authors interested in submitting papers for future volumes should contact the editor at 1-888-364-4744 or via e-mail at info@narth.com.

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Editor, *Journal of Human Sexuality*

National Association for Research and Therapy of Homosexuality (NARTH)

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At Risk: Single Young Women Having Nonmarital Sex

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Abstract

The purpose of this paper is to provide a rationale for restoring the primary public-health principle of risk avoidance versus risk reduction when advocating for the reproductive health of young women. Many sexually active youth hold mistaken beliefs about the potential benefits of nonmarital sex; misconceptions are identified and explained. Due to the incomplete development of the adolescent brain, youth also have cognitive limitations that make many of them poor sexual decision makers, a phenomenon that is also explained. Finally, the physical, psychological, relational, social, and potential spiritual risks that young women disproportionately face when they have nonmarital sex are described. We conclude that these risks cannot adequately be addressed by a continued primary reliance on the secondary public health principle of risk reduction (such as the promotion of condoms and contraception); rather, risk avoidance needs to be emphasized.

At Risk: Single Young Women Having Nonmarital Sex

Sexual activity is defined as bodily contact meant to give or derive sexual gratification; a chief form of sexual activity, of course, is sexual intercourse. Oral sex, anal sex, and vaginal sex are all forms of sexual intercourse that can spread many sexually transmitted infections (STIs). But intercourse isn't the only sexual activity that puts its participants at risk: Mutual masturbation also carries risk of transmitting some STIs.

The risks inherent in such activity are affecting those at progressively younger ages. In the United States, 20% of American youth initiate some form of sexual activity prior to 14 years of age (Sexual Health Statistics, 2006; U.S. Teenager, 2010), and 75% of graduating high school seniors have had vaginal sexual intercourse (Fast Facts, 2010).

There is significant medical and social science data to suggest that, in addition to the risk of unwed pregnancy, early onset of sexual activity disproportionately places young women at risk for STIs, mental illness, and dating violence (Grossman, 2007; Steenhuysen, 2008).To be sure, young men may also suffer negative consequences relative to unwed pregnancy, STIs, mental illness, and dating violence, but the heaviest burden resulting from these is borne by young women (Zavodny, 2001). Thus it is imperative that young people, especially young women, fully understand these risks before making decisions about nonmarital sex.

Sexual abstinence is the act of refraining from intentional sexual gratification, whether through fantasy, self or mutual masturbation, oral or anal orgasm, and vaginal intercourse. Sexual abstinence—also known as *risk avoidance*—is the only absolutely certain way to avoid the risks of unwed pregnancy, STIs, and the emotional, social, and relational harm associated with nonmarital sex. A survey of American parents and adolescents released in August 2010 by the Department of Health and Human Services revealed that a majority of Americans consider premarital sex unacceptable. This study, entitled the "National Survey of Adolescents and Their Parents: Attitudes and Opinions about Sex and Abstinence," found that approximately 70% of parents and just more than 60% of

adolescents believe that sex should occur only between those who are married (Olsho et al., 2009).

The surprising pervasiveness of this conservative cultural norm should encourage health-care providers and educators to find ways to effectively promote the primary public health principle of risk avoidance in the area of sexual risk-taking. Risk avoidance—in contrast to the secondary public health principle of risk reduction—guarantees every individual's right to optimal health. Consequently, promotion of risk avoidance in the area of sexual health should be vigorously pursued by all individuals regardless of personal worldview.

Seventeen published studies demonstrate a positive impact from school-based abstinence programs (Abstinence Works, 2010; c.f., Ericksen, Weed, Birch, White, & Evans, 2009; Jemmott, Jemmott, & Fong, 2010; Tortolero et al., 2010). The majority of these programs successfully delay onset of sexual debut by two years, and some have also been associated with decreased rates of teen pregnancy (Cabezon et al., 2005; Denny, Young, & Spear, 1999; Weed, Ericksen, Lewis, Grant, & Wibberly, 2008; Sather & Zinn, 2002). Clearly, delay of sexual debut among adolescents is possible and is beneficial not only to adolescents but to society at large (Manlove et al., 2002). It is our hope that this paper will aid parents and professionals alike in promoting optimal sexual health while eliciting a greater respect for women.

Why Do Youth Have Nonmarital Sex?

One way to approach youth about the risk of premarital sex is to discuss what they believe are the benefits of sex before marriage (Abbott & Dalla, 2008). Recognizing these beliefs is not an endorsement of them, *but rather a prerequisite to effectively expose their limitations and liabilities*. The following findings come from two studies (Abbott & Dalla, 2008; Abbott & Stortvedt, 2012). Quantitative and qualitative data was collected from a group of 68 sexually active teens, and 60 sexually abstinent adolescents and 42 abstinent young adults (ages 17–26).

Benefits of Sexual Activity as Perceived by Sexually Active Teens and Young Adults

The sexually active teens surveyed by Abbott and Dalla (2008) reported five perceived rewards of engaging in nonmarital sex: (1) physical pleasure from foreplay and orgasm, (2) increased bonding and closeness to partner, (3) the ability to test sexual compatibility, (4) a demonstration of love as a natural part of a growing relationship, and (5) a way to avoid being teased or humiliated for being a virgin. Those dedicated to influencing the sexual behavior of youth cannot dismiss these perceived rewards, especially since there are significant limitations to each of them. In explaining this, the word *outcome* will be used instead of the word *reward*—while a reward also implies something desirable, an outcome may be positive or negative, healthy or unhealthy.

Perceived Outcome #1 is that premarital sex provides physical pleasure (Kunz, 2011; Steinberg, 2005). Sexually active teens made statements such as "It's fun," "Sex gives pleasure to both of us," and "It feels good, especially when you're hopped up on hormones." It's difficult to dispute claims of "pleasure," but physical gratification in a young, uncommitted relationship is fleeting. Juvenile sex has not been shown to enhance growth for self or partner, because at its heart, nonmarital sex is selfish. It is the momentary use of another's body to "relieve the pressure and anxiety built up by abstinence," as one teen admitted (Bryner, 2011).

Perceived Outcome #2 is the belief that sharing physical intimacy brings increased closeness and bonding with the partner (Gross, 2009; Steinberg, 2005). Teens commented that "It will bring you and your partner closer" or "It deepens the relationship." Increased closeness is more possible for young women than young men, who can more easily have sex without emotional attachment or deep caring for the well-being of the partner (Sprecher, 1988). The cognitive and biochemical basis for this gender difference will be explained in a later section of this paper. Even if sex is perceived as a positive bonding experience by both partners, however, statistics show that the rela-

tionship is unlikely to last past high school—so deep, physical intimacy is wasted on a temporary affair and temporary feelings (McIlhaney & Bush, 2008; Lavoie, Robitaille, & Herbert, 2000).

Perceived Outcome #3 is the common belief that premarital sex is a way to test sexual compatibility. Sexually active teens reported, "It sounds bad, but who buys a car without test-driving it?" Another argument is that "You won't be shocked, surprised or disappointed when you marry." It is a common belief among youth that one must try out a sexual relationship prior to marriage in order to guarantee sexual compatibility within marriage. Intuitively, this seems reasonable. People explore, research, and try out many things—from potential purchases to colleges and even careers—before committing to a final or long-term decision. Experience generally broadens a person's perspective, increases knowledge, and improves judgment.

There are, however, some exceptions to this general rule. One example is taking illegal drugs. Trying out methamphetamine will not improve one's judgment or enhance one's decision-making capacity with regard to drug use. The same is true for premarital sex. Research has demonstrated that premarital sexual experience is not predictive of later marital sexual satisfaction (Day, 2010). In fact, having many—or, in fact, any—sexual partners before marriage, including one's future spouse, may be harmful to achieving marital sexual satisfaction. Nonvirgins who eventually marry or cohabit do not report higher levels of sexual satisfaction than do virgins who marry (Crooks & Baur, 2011). And persons who have had sex before marriage not only are more likely to be unfaithful and or divorce (Hsiu-Chen, Lorenz, Wickrama, Conger, & Elder, 2006), but also are more likely to have difficulty adjusting to marriage and are less likely to experience marital happiness, satisfaction, and love (Finger et al., 2004). Some of this difficulty appears to result from one spouse comparing the other with past sexual partners.

Research shows that those who have had sex before marriage are less likely to experience marital happiness, satisfaction, and love (Finger et al., 2004). And, on aver-

age, marriages preceded by cohabitation are 46% more likely to end in divorce (DeMaris & Rao, 1992; cited in Popenoe and Whitehead, 2002).

Achieving good sex in marriage is not dependent on prior love-making experience but on the love-making qualities possessed by both partners. These include kindness, unselfishness, humor, playfulness, and the ability to openly communicate needs and desires. Sexual satisfaction in marriage is one of those rare situations where prior experience is not needed (Abbott, 2011; Byers, 2005; Haavio-Mannila & Kontula, 1997; Litzinger & Gordon, 2005; Young, Luguis, Denny, & Young, 1998).

Perceived Outcome #4 is the belief that premarital sex is a way to show love in a growing relationship and that it is a natural progression of serious dating that may lead to a more stable marriage. Sexually active teens commented that "Sex is a sign of love." "It is a natural part of the relationship." "Sex is OK if a person is in a steady, loving relationship" (Abbott, 2011, p. 18). Some sexually active teens seem to believe that marital happiness is primarily related to sexual satisfaction. Sharing sexual intimacy certainly taps into one aspect of love, but it is not in and of itself a guarantee of marital success or satisfaction.

Perceived Outcome #5 is the belief that one will avoid being teased or embarrassed for being a virgin—a benefit of being sexually active that does not have lasting value. All teens face teasing—if not for one thing, then for another. It's just part of growing up. In many circumstances, abstinent youth are in the minority among peers and friends and are occasionally teased or ostracized by peers (Abbott & Dalla, 2008). However, Abbott and Dalla (2008) could find no empirical evidence to support the notion that being sexually active made one more popular and/or well regarded by friends or peers. There is certainly the possibility that a young woman who won't have sex will be rejected by some young males, but this does not seem to be a common occurrence among those who report being abstinent (Abbott & Dalla, 2008).

Benefits of Abstinence as Seen by Abstinent Youth

It is interesting to compare the perceived benefits reported by sexually active youth with those reported by the sexually abstinent. Nearly 80% of the abstinent teens reported that the main benefit of abstinence was *no worry about pregnancy or STIs*.

Second in importance, stated by almost half the youth, was that *abstinence until marriage would strengthen and preserve their future marriages*. One teen said, "My future husband will know that I love him because I waited for him." Another said, "Choosing abstinence before marriage makes one's relationship with one's wife or husband more special than if one had already had sex with earlier partners." Another said that sex was "a special gift to the future spouse" and would make sex in marriage more enjoyable (Abbott, 2011, p. 21).

Third, nearly half the youth reported that *abstinence makes them feel emotionally healthy*. Abstinent youth reported feeling good about themselves and positive about the future (Abbott, 2011).

The fourth important benefit reported by 25% of the youth was *more self-respect and self-esteem*. As one respondent said, "Self-esteem increases from resisting temptation." Another believed that "many people respect those who are abstinent and some wish they could be that way too" (Abbott, 2011, p. 22). Nearly one in five youth believed that having sex before marriage would bring shame and guilt over disappointing parents, friends, or God.

Finally, another fifth of the sample stated that a benefit of abstinence was the *avoidance of emotional pain if and when the relationships failed*, as most do in adolescence and early adulthood. The ache and hurt of having shared such intimacy and then having the relationship end can be devastating for some youth and can lead to depression and even thoughts of suicide (Teen Suicide Statistics, 2012; Portner, 2001; Teen Suicide Statistics, 2012).

Empirical data suggests that each of these perceptions about the benefits of sexual abstinence is correct, as will become evident in the second half of this paper.

The Adolescent Brain: A Work in Progress

In addition to the previously discussed misperceptions about nonmarital sex, youth who engage in sex before marriage often make poor decisions due to their stage of brain development. The prefrontal cortex of the brain—the master center for executive functioning, judgment and restraint—does not fully mature until the mid-twenties. For this reason, the authors have defined both teens and young adults under age 26 as *youth*. As a result of this physiological reality, many young adults are still in the adolescent phase of brain development (Giedd, Blumenthal, Jeffries et al., 1999; McIlhaney & Bush, 2008).

Barbara Strauch, a medical science editor for *The New York Times*, spent nearly a year interviewing the top researchers in the field of adolescent brain development—including Jay Giedd, Chuck Nelson, Marian Diamond, Francine Benes, and Larry Steinberg—and concluded, "The teenage brain may be briefly insane. . . . The teenage brain is in flux, maddening and muddled" (Strauch, 2003, p. 8).

The MRI (Structural and Functional Magnetic Resonance Imaging) has improved the ability of neuroscientists to view brain development *in vivo* and to follow changes over time. It has also allowed experts in the field to see that adolescent brains are not yet structurally mature—a reason why youth do not possess the same capacity as adults for consistent intellectual judgment and mature impulse control (Nelson et al., 2002; Silveri et al., 2006; Yurgelun-Todd, 2007).

Major structural growth of the preadolescent brain begins three to four years before puberty with an overproduction of gray matter, consisting of neurons, dendrites (treelike branches from each neuron), and synapses (junctions across which impulses pass via neurotransmitters from one neuron to another). This increase in gray matter expands the potential to think and learn in novel and creative ways. However, during mid and late adolescence, dramatic *neural pruning* occurs. Unused or infrequently used neurons and their connections atrophy, while the remaining neural pathways are strengthened. This

process can be likened to pruning fruit trees of unwanted growth so that the remaining branches will grow and produce more fruit (Giedd et al., 1999; Steinberg, 2005).

In addition to pruning, *nerve myelination* occurs. Myelin is a fatty substance that coats and insulates nerves, increasing the speed and efficiency of electrochemical transmissions in the brain. This process is similar to the way that insulating electrical wires improves their conductivity. Pruning and myelination cause "the brain [to become] leaner and more efficient" and adolescent reasoning capacity to increase (Weinberger, Elvevag & Giedd, 2005, p. 1).

Those processes don't change the fact that the structure of the brain responsible for reasoning and critical thinking is not fully mature before the midtwenties. The prefrontal cortex of the brain, located behind the forehead, enables a person to (a) manage impulses, (b) regulate emotions, (c) forgo immediate pleasure for long-term gains, (d) reason hypothetically, (e) weigh positive and negative consequences, and (f) plan for the future (Casey, Galvan & Hare, 2005; Fuster, 2002; Giedd, 2004). The immaturity of this portion of the brain explains much of the inability of adolescents to properly interpret experience and to make healthful decisions.

Psychiatrist and neuroscientist Jay Giedd summarized the significance of this fact, stating, "Adolescents have the passion but no brakes [in the midst of emotionally charged situations] until they are *twenty-five*" (Strauch, 2003, p. 33 author's emphasis). Thus, many adolescents and young adults lack the full adult capacity to reason, judge, and control emotional responses (Yurgelun-Todd, 2007). Dr. Miriam Grossman postulates that this is one reason why adolescents fail to use condoms and contraception correctly and consistently despite repeated instruction and demonstration of use. When students in comprehensive sex education classes are taught and allowed to practice putting condoms on bananas or dildos, for example, they are in an emotionally neutral setting. The ability to correctly and consistently "use condoms" in this classroom setting is no guarantee that the same correct and consistent use will occur in the throes of passion (Grossman, 2009).

Common Cognitive Limitations during Adolescence

The fact that the adolescent brain is in a dramatic state of structural flux accounts for at least 11 common cognitive limitations that render adolescents and young adults poor candidates for engaging in nonmarital sexual activity (Berk, 2007; Feldman, 2008; Santrock, 2008; Steinberg, 2005; Walsh, 2005). These limitations vary from one person to another, are more likely to occur in younger teens than in older young adults, and may not all occur simultaneously.

1. *Long-term consequences of behavior are often unanticipated or ignored*. Youth tend to focus on the here and now. They rarely look ahead and think to themselves, "If I do this now, it could lead to that in the future. Maybe I should reconsider my actions in light of my long-term goals."

2. *Youth have limited impulse control*. As a result, passion and pleasure can drive behavior—resulting in irrational and illogical choices. They may not want to participate in a particular activity, but the emotion of the moment may propel them into reckless behavior. For many teens, getting pleasure now is better than enjoying satisfaction later—even if later satisfaction would bring greater rewards. In other words, they may not be capable of conducting an unbiased cost/benefit analysis regarding potential behaviors and outcomes.

3. The adverse consequences of risky behaviors are frequently underestimated. Youth believe they are impervious to the negative costs and penalties of dangerous behavior that others may suffer. This adolescent perception of invulnerability creates in them a belief that they can get away with heavy drinking, fast driving, or unprotected sex without suffering any of the associated consequences. They simply believe the negative outcomes will not happen to them.

4. *Personal values that are not fully formed can be overwhelmed by peer pressure, media propaganda, and situational factors*. Because adolescents may be susceptible to influences that push them away from their values and goals, a youth may cave in to the whims and wishes of others.

5. *Youth egocentrism limits a teen's ability to empathize with peers and family members,* though they can show great enthusiasm and sympathy for just causes (such as the homeless or mistreated animals). Their egocentrism may "blind" them from perceiving the potential harm their actions may have on others. In this state of self-absorption, they may fail to ask, "How will my behavior affect this person for good or ill? Who else could be adversely affected by my actions?"

6. *Strong emotions can overwhelm rational thinking*. Teens tend to do things based on gut feelings or expectations of the peer group instead of on well-reasoned thought. They may also act contrary to what they know they should do.

7. *Moral reasoning is precarious*. Issues or actions that had previously been clearly black or white now become gray. Teens are skeptical of parental, societal, cultural, or religious values that proscribe behavior, even if they intellectually believe the prohibitions make sense. As they see faults and hypocrisy in parents and other adults, they may begin to question almost everything. They may also justify breaking rules because others are doing it—and may lie, cheat, or steal with ease if a good excuse is handy.

8. *Youth are often overly self-conscious*. They imagine that their appearance and/or behavior are the constant focus of peers and adults. This imaginary audience makes them more vulnerable to self-criticism and subject to the opinions of the

peer group. They may be more worried about how they look to peers than about doing what they know is right.

9. *Convergent thinking dominates youth problem-solving*. Youth rely on their unique experience and current knowledge to solve present problems. A single-solution answer is the usual outcome. Divergent thinking—the capacity to derive novel, multiple solutions to various predicaments—is an emerging skill for adolescents.

10. *Youth may misinterpret other people's emotional reactions*. They often misread the verbal or behavioral cues in interpersonal communications, and as a result are often easily hurt and offended. For example, a teen who is explaining a bad day at school sees a parent frown and look away. The teen quickly becomes angry because she misinterprets the parent's scowl as disapproval or criticism when, in fact, the parent wasn't thinking about the teen's story at all.

11. *Alcohol and other drugs affect the teen brain more dramatically than the adult brain*. The young brain is more sensitive to any type of chemical imbalance. Even small amounts of alcohol or marijuana, for example, can significantly impair the adolescent's ability to reason and make prudent choices (Brown, Tappert, Granholm, & Delis, 2000).

Taken as a group, these deficiencies present difficulties in adolescent and young adult thought and behavior—hormones are flowing, but the restraining power of the brain has yet to fully engage and put the brakes on foolish or risky behavior (Giedd, 2004). Thus, youth lack strong emotional control, often fail to reason logically, rarely plan ahead or envision adverse consequences, tend to make impulsive choices, and often overreact emotionally to real or imagined counsel or correction. Youth lack the capacity to foresee the

possible ramifications that sex can have not only upon them, but also upon their partners (Abbott, White, & Felix, 2010; Carr, 2007). Daniel Weinberger and colleagues (2005) summarize the cognitive limitations of the adolescent brain this way:

> Teens are not the same as adults in a variety of key areas such as the ability to make sound judgments when confronted by complex situations, the capacity to control impulses, and the ability to plan effectively. Such limitations reflect, in part, the fact the key areas of the adolescent brain, especially the prefrontal cortex that controls many higher order skills, are not fully mature until the third decade of life. . . are full of promise . . . but neurologically they are not adults. (p. 3)

Consequently, youth require parents, mentors, and others in authority to function as a surrogate prefrontal cortex for them (McIlhaney & Bush, 2008).

Risks to Young Women from Nonmarital Sex

Thus far we have reviewed common misperceptions about premarital sex among youth and the inherent cognitive limitations of adolescents. We now turn attention to describing the biological reasons why young women are at a proportionately greater risk for experiencing more challenging or harmful physical, psychological, and relational consequences from premarital sex than the young men with whom they have sex (McIlhaney & Bush, 2008; Waller, Hallfors, Halpern, Iritani, Ford, & Guo, 2006).

Physical Consequences for Women

Unwed Pregnancy

The prevailing approach to preventing unwed pregnancy—particularly among teens—is to promote the use of condoms and hormonal contraceptives. This "pregnancy

as a disease" model, however, fails on three counts. First, teens do not appear to have the cognitive ability to use artificial contraception as efficiently as do adult women. Second, the chemicals and hormones in contraceptives can have adverse side effects for some women, resulting in nausea, weight gain, blood clots, increased blood pressure, increased risk of gallbladder disease, and increased risk of liver tumors (CDC, 2010c). And third, many unwed pregnancies occur beyond the teen years because more women are actively choosing to have children without marriage (Sheffield, 2011).

In 2010, nearly 1.5 million children were born outside of marriage. The majority of these infants (825,000) were born to women between the ages of 20 and 29; another 15% were born to mothers between the ages of 30 and 39; and 400,000 were born to teenagers aged 15 to 19 (CDC, 2010b). Clearly, fertility is not a disease—but the adverse medical, social, emotional, educational, and vocational consequences of unwed pregnancy in general, and of teen pregnancy in particular, are significant (Kearney, 2009; Terry-Humen, Manlove, & Moore, 2001). The medical, psychological, and financial support for these women, and for their often fatherless children, costs the United States government more than $11 billion a year (Sheffield, 2011).

The most recent statistics from the Guttmacher Institute indicate that in 2008, an estimated 750,000 teen girls became pregnant (Kost & Henshaw, 2012). Adolescent pregnancy results in decreased educational and vocational opportunities for the mothers, an increased likelihood of the family living in poverty, and significant risk for negative long-term outcomes for the children. For example, children of adolescent mothers are more likely to be born prematurely and at a low birth weight; suffer from poor health; perform poorly in school; run away from home; be abused or neglected; and grow up without a father (Guttmacher, 2006; quoted in The Institute for Research and Evaluation, 2007).

Regarding the use of contraceptives by teens, roughly 60% of sexually active American teens report using a condom or birth control pills, but few do so correctly and consistently (Guttmacher, 2010a). It has been reported that 20% of teen women between the ages of 12

and 18 will become pregnant within the first six months of being on a birth control pill (Dinerman, Wilson, & Duggan, 1995). Moreover, nearly 50% of cohabiting teens become pregnant within a year of starting oral contraceptives as compared to only 8% of married females over age 30 (Fu, Darroch, Haas, & Ranjit, 1999). The disparity in these rates between women under age 30 and those over age 30 has remained largely unchanged over the last decade (Kost, Singh, Vaughan, Trussell, & Bankole, 2008). Clearly, reliance on the "pregnancy as disease / risk reduction" model for preventing unwed pregnancy is insufficient.

Sexually Transmitted Infections (STIs)

Young women are significantly more likely to contract an STI than are young men. For example, if an adolescent female has chlamydia and engages in a single act of intercourse with a male who is *not* infected, his risk of acquiring the infection is 30%. However, if a young man has chlamydia and engages in one act of intercourse with a female who is *not* infected, her risk of acquiring infection is 90% (Sultan, 2004). Similarly, nearly 75% of HPV infections occur in females between the ages of 15 and 25 (Indman, 2010).

There are physiological reasons for the difference: The vagina, cervix, and uterus are warm, moist, dark environments conducive to the growth of bacteria and viruses. Natural cleaning occurs only during menstruation, about once a month. In addition, women under the age of 21 produce thinner cervical mucus and have a more physiologically immature cervix. The cervix is composed of two different cell types: rectangular columnar cells and flat squamous cells. Columnar cells are less resistant to infection than are squamous cells. Women in their early twenties and younger have columnar cells that are continually transforming into squamous cells. The area of the cervix where this occurs is called *the transformation zone*. Due to the high cellular turnover, this area is susceptible to both infection and carcinogenic transformation. In addition, hormonal contraception enlarges the transformation zone in young women, placing them at even greater risk.

The penis, in contrast, is external, readily cleaned, and dry, and the male urethra is regularly flushed out, making men less prone to acquiring STIs. Additionally, while the majority of STIs are asymptomatic for both genders, when symptoms are present it is easier for men to notice those symptoms—such as genital lesions, ulcers, sores, warts, or a purulent penile discharge—that would alert them to seek treatment *(*CDC, 2010b).

Pelvic Inflammatory Disease (PID)

If a woman becomes infected with either chlamydia or gonorrhea, she has a one in five chance of developing pelvic inflammatory disease (PID) (Guttmacher, 2010b). PID is most commonly a complication of chlamydia or gonorrhea that involves the uterus, fallopian tubes, and ovaries. Symptoms may include pain in the abdomen, pain during intercourse and/or urination, vaginal discharge, and/or irregular menstrual bleeding. PID can lead to chronic pelvic pain as well as the scarring of the fallopian tubes, which places women at risk for both ectopic pregnancy and infertility. Although PID is curable with antibiotics, scarring significant enough to cause infertility may have already occurred by the time the disease is diagnosed and treated. Consequently, women who develop PID have a one in five chance of becoming infertile—in other words, an estimated 150,000 to 200,000 American women lose their ability to have children each year because of this complication (Guttmacher, 2010b).

Even if chlamydia is successfully treated before scar tissue forms, women may still be at an elevated risk for infertility. When the chlamydia bacterium dies, it releases a protein called hsp, similar to a protein produced by early human embryos. A woman's immune system may produce antibodies to the hsp protein, and a pregnant woman's immune system may not distinguish between the two similar proteins. This causes an autoimmune reaction that results in recurrent miscarriages for some women. (Grossman, 2007).

Emotional Risks to Women

Surveys of college students have shown that 25 to 30% of women who have casual sex suffer some psychological and emotional consequences ranging from mild guilt to worry about negative consequences (STIs and pregnancy) and even acute anxiety and depression (Grossman, 2007). Moreover, McIlhaney and Bush (2008) reported that after controlling for confounding factors, sexually active young women were three times more likely to experience depression and three times more likely to have attempted suicide as compared to their sexually abstinent peers. In addition, young unmarried women who become pregnant and choose abortion may also suffer emotional distress over the abortion for many years (Grossman, 2007). During the last decade, emerging research suggests that innate gender differences may underlie these negative emotional risks.

The first brain-imaging study comparing brain areas activated in women and men during sexual arousal was published in 2002 (Sax, 2005). Men had significant activity in the base of the brain, especially the hypothalamus. Women, on the other hand, showed proportionately greater activity in the cerebral cortex. A 2004 study at Emory University replicated these results (Sax, 2005). This finding partly explains what many, including UCLA psychologist Anne Peplau, have observed: "[W]omen's sexuality tends to be strongly linked to a close relationship. For women, an important goal of sex is intimacy; the best context for pleasurable sex is a committed relationship. This is less true for men" (Sax, 2005).

If a young woman decides to have nonmarital sex, she is probably hoping for three things: (1) emotional closeness, (2) increased commitment, and (3) physical pleasure. However, when a young man has premarital sex, his reasons are similar but in a different order: (1) physical pleasure, (2) emotional closeness, and (3) increased commitment—something that's not absolutely required. In other words, women have sex primarily for relationship reasons while young men are primarily seeking physical pleasure without commitment (Sax, 2005).

However, structural arousal of the brain is not the only thing that differs between the genders. There's also a difference in hormonal responses to sexual arousal that affects the degree and significance of emotional bonding during physical touch. During intimate touching, the hormone oxytocin is released in women and vasopressin is released in men. These hormones are the biochemical basis for the emotional bond that forms between the couple even in the context of a single sexual encounter (Sax, 2005; McIlhaney & Bush, 2008).

Oxytocin—colloquially known as "the bonding hormone"—is released not only during labor and breast feeding to promote bonding between mother and child but is also released during sexual intercourse. It can even be released in women with a lesser degree of physical touch, such as a hug. McIlhaney and Bush (2008) described the bonding effect of oxytocin to be "almost like the adhesive effect of glue—a powerful connection that cannot be undone without great emotional pain" (pp. 36, 37).

When men and women become physically intimate with each other, oxytocin exerts still another effect on women: it impairs judgment, making it more difficult for women to assess the character of their partners. In Dr. Miriam Grossman's words, this is why hooking up "turns attachment 'on' and critical thinking 'off'" (Grossman, 2009, p.48).

Although some oxytocin is released in men, there are far more extensive oxytocin circuits in the brains of women, and the bonding effect of oxytocin is generally stronger for women than the effect of vasopressin for men. Consequently, women seem to suffer more emotional heartache when the relationship fails than do young men (Regan, 2008). This does not mean that men never suffer emotional consequences from the breakup of sexual relationships (Lydon, Menzies-Toman, Burton, & Bell, 2008). For example, McIlhaney and Bush (2008) report that Rector, Johnson, and Noyes (2003) found that, after controlling for confounding factors, sexually active adolescent girls were three times as likely to report being depressed and to have attempted suicide than girls who are not sexually active. Similarly, sexually active teen boys are more than twice as likely to report being depressed and seven times more likely to have attempted suicide compared

with their sexually abstinent male peers (McIlhaney & Bush, 2008, p. 20, 78). Overall, however, young women are more likely to be depressed and hurt by the frequent dissolution of temporary sexual relationships than are their male partners (Sax, 2005).

Relational Harm

Another growing concern is the correlation between dating violence and sexual activity among youth (Banyard & Cross, 2008; Lavoie, Robitaille, & Herbert, 2000). *Dating violence* is defined as a pattern of abusive behaviors used to exert power and control over a dating partner. Physical, emotional, verbal, and sexual abuse, as well as stalking (which includes digital harassment), are all forms of dating violence. Females aged 16 to 24 are more vulnerable to intimate partner violence than any other age group—at a rate almost triple the national average (U.S. Department of Justice, 2001). A woman's risk of experiencing dating violence seems to increase as the woman's age of sexual debut decreases. An online survey conducted by Dr. Elizabeth Miller and colleagues (2007) of the University of California-Davis, for example, found that among youths who reported sex by age 14, 33% had been hit, choked, or punched, and 58% had been verbally abused.

Young women who engage in nonmarital sex risk more than dating violence, depression, and suicidal ideation. Repeatedly disrupting the emotional bonds forged by oxytocin reduces the ability to attach to subsequent sexual partners (Brizendine, 2006; Fisher, 2004). In other words, casual sex damages a woman's ability to ultimately bond in a long-term, committed relationship like marriage (McIlhaney & Bush, 2008, p. 43; Heaton, 2002; Kahn & London, 1991). When sexual contact creates a bond that is then broken and replaced by another sexual relationship, and that cycle repeats itself over time, the brain's natural bonding mechanisms are damaged (McIlhaney & Bush, 2008, p. 103). For women with multiple sexual partners, oxytocin gradually loses much of its bonding effect, "almost like tape that loses its stickiness after being applied and removed multiple times" (McIlhaney & Bush, 2008, p. 43).

Social Risks to Women

If a woman has sex with multiple partners over many years, she is disadvantaged in the marriage market for three reasons. First, the double standard of sexual behavior exists in almost all modern cultures; a sexually experienced woman can be seen as a slut, a whore, or a "loose woman." A promiscuous male, on the other hand, is seen as virile, sexy, and "a stud." Most men looking for a mate want a woman with little prior sexual experience (Austin, 2011; Lyons, Giordano, Manning, & Longmore, 2011).

If a woman spends many years cohabitating with one or more males and the relationship ends, she is at a disadvantage for a second reason: She will find fewer available men to date who are her own age, and she must consider older men. On the other hand, a man who sleeps around and/or cohabits for several years retains a greater field of eligible women because it is socially acceptable for him to date much younger women, women his own age, and older women (Burleson, Trevathan, & Todd, 2007; Veevers, 2003).

A third disadvantage women with multiple sex partners face in the marriage market is that age affects fertility more for women than men**.** A woman's ability to ovulate and carry a fetus drops significantly after age 35. If she waits until her mid to late thirties or early forties to marry, she may have difficulty conceiving a child. In addition, the chances of having a special-needs child increase significantly with age (Dunson, Colombo, & Baird, 2002; Pawlik-Kienlen, 2009; van Noord-Zaadstra et al., 1991).

According to the American Society of Reproductive Medicine, 7% of women between the ages of 20 and 24 are infertile. Between the ages of 25 and 29, that number increases to 9%; between ages 30 and 34, infertility among married women increases to 15%; and between the ages of 35 and 39, female infertility rates rise to about 22% (American Society of Reproductive Medicine, 2010; Morris, 2010).

Even though fertility among men is less affected by age than among women, a man's ability to impregnate a female also decreases over time, especially after age 50. In other words, a middle-aged man is more likely to be able to sire a child than a middle-

aged woman is likely to be able to become pregnant (Girsh et al., 2008; Kidd, Eskenazi, & Wyrobek, 2001).

Psychological Risks to Women Who Violate Their Religious or Spiritual Beliefs

Across all cultures, a majority of people view human beings not as mere animals but as spiritual beings created by God or some other higher power (Denys, 2004; Overman, 2009). This is true for Christians, Muslims, and Jews. Consequently, these religions state unequivocally that nonmarital sex is "sinful"—in other words, contrary to the fact that God created men and women to give and receive love as sexually complementary beings. These faiths view nonmarital sex as a behavior that is inherently harmful for the individual spiritually and also teach that this can lead to many problems for children born outside of a healthy and stable marriage (Amato, 2005; Booth, Scott, & King, 2010).

Some women believe that those who engage in premarital sex violate God's will. The Hebrew Bible declares that "he who commits adultery has no sense; he who does it destroys himself" (Proverbs 6:32). Muhammad, the prophet of Islam, declared that sex outside of marriage is a great sin, and Allah (God) has forbidden *zina*, which means adultery and fornication (The Noble Qur'an Sura 4, aya 16 and 23). Christ, the Christian messiah, said that adultery and fornication are serious offenses against God (Matthew 15:19, 19:9; Mark 7: 21). Paul, the disciple of Christ, declared:

> Shun fornication! Every sin that a person commits is outside the body; but the fornicator sins against the body itself. . . . Do you not know that wrongdoers will not inherit the kingdom of God? Do not be deceived! Fornicators, . . . adulterers . . . none of these will inherit the kingdom of God.And this is what some of you used to be. (1 Corinthians 6:18, 9a, 10b–11a)

It is clear from the Hebrew, Muslim, and Christian scriptures that sex outside of marriage is a violation of religious standards. If a woman adheres to this belief deep in her heart and mind, she may suffer psychological and/or spiritual consequences if she engages in premarital sex (Harris, 2006). Following is a list of possible concerns for women of faith who have sex outside of marriage (Abbott, 2011; Grossman, 2007; Nicholi, 2003; Overman 2009; Polkinghorne, 2003):

1. Reduction or loss of the presence of God's Spirit or grace, which guides and comforts
2. Emotional desensitization, resulting in greater likelihood of condoning or overlooking other immoral behaviors
3. Less concern for others, more self-centeredness, and less sensitivity to the suffering of others
4. Decrease in self-esteem and self-worth and possible corrosive guilt and regret
5. Depression and despair
6. Less confidence, hope, and optimism in the future

Even women who have either partly or wholly rejected the religion of their youth may feel pangs of guilt if they have sex outside of marriage. Core principles, ideals, and values that are instilled in youth but later violated in adulthood can result in depression, anxiety, worry, and shame (Bogart, Collins, Ellickson, & Klein, 2007; Rector, Johnson, & Noyes, 2003; Waller et al., 2006).

Yet another situation occurs in women who perceive sexual self-control as being right or good, independent of religion or faith; this view of sexual self-control may lead to a psychological, even spiritual, path of self-fulfillment. As a result, such women may abstain from or cease nonmarital sexual activity—and may suffer significant distress if they engage in such behavior.

Spiritual consequences may occur to a greater or lesser degree, depending on the religious belief of the woman. Such outcomes may also result as a natural consequence of violating God's will independent of anyone's belief or disbelief in God's existence (Buckely, 2004). Other women, however, reject the notion of spiritual consequences for engaging in nonmarital sex and believe that any psychological consequences arise merely from a morally repressive view of sexuality. In their view, the only concerns related to sexual intercourse are the prevention of unwanted pregnancy and the risk of STIs (Steinberg, 2005; c.f., Dawkins, 2008; Dennett, 2007; Hitchens, 2009).

Conclusion

The purpose of this paper is to advocate for restoring the primary public-health principle of risk avoidance to its proper place in achieving improved reproductive health for young women. Approximately half of American teens and three-fourths of young adults are or have been sexually active. Three in ten will become pregnant and one in four will acquire one or more STIs (CDC, 2012). Most of this sexual activity, both vaginal and oral, is done without much thought or effort to reduce the risk of pregnancy and STIs (Holcombe, Carrier, Manlove, & Ryan, 2008).

Science does not indicate that there is no role for the secondary public-health principle of risk reduction. However, even if a significant portion of youth are able to improve their use of condoms and contraception, nonmarital sex carries with it significant psychological, relational, and social risks—as well as potential spiritual risks—that cannot be mitigated by condoms and contraception. Those risks include, among others, depression, suicidal ideation, and dating violence.

Many youth subscribe to false beliefs about perceived benefits of premarital sex. Part of the problem arises from brain development: Youth have several cognitive and affective limitations stemming from the fact that the prefrontal cortex is not fully developed until the mid to late twenties. As a result, youth lack the adult capacity for intellectual

judgment and are at high risk for making unhealthy decisions, particularly in emotionally charged situations.

Several other factors that contribute to negative outcomes in sexually active youth, especially young women, have been presented. For biological reasons, young women are especially vulnerable to the negative effects of nonmarital sex in temporary relationships. Women suffer greater physical, psychological, social, and relational harm than do the young men with whom they have sex. Unfortunately, many young women are unaware of their increased risk for these adverse outcomes. Consequently, many young women adhere to the popular culture's male model of promiscuity without giving thought to their higher propensity for experiencing harm—something that profoundly impacts their future welfare (Grossman, 2009; Kern, 2008; McIlhaney & Bush, 2008; Stepp, 2007).

Adolescent sex may not be injurious for all youth, but it certainly carries grave risks for those involved. Youth who are sexually abstinent avoid these risks with 100% certainty and can devote more time and energy to academics, extracurricular activities, friendships that are not focused on sexual activity, and pursuit of their dreams (Carnegie Council on Adolescent Development, 1995). Considering the risks as opposed to potential benefits of nonmarital sex among youth, we conclude that abstinence is the best course of action for most adolescents.

It is our hope that this paper will help parents and professionals provide age-appropriate sexuality and relationship education that promotes the knowledge and skills necessary to delay sexual involvement, with the aim of preparing for sex exclusively within the context of marriage. This is not an impractical or unattainable goal (Weed, Ericksen, Lewis, Grant, Wibberly, 2008). Both parents and professionals must raise the primary public-health principle of risk avoidance to its proper place in the promotion of optimal sexual health (Oman, Vesely, Kegler, & McLeroy, 2003).

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A Critical Evaluation of the *Report of the Task Force on Appropriate Therapeutic Responses to Sexual Orientation*, Resolutions, and Press Release

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Abstract

The American Psychological Association (APA) Task Force on Appropriate Therapeutic Responses to Sexual Orientation reviewed the research literature pertaining to sexual orientation change efforts (SOCE) and concluded that the studies were either poorly designed or contained serious methodological flaws and lacked empirical rigor. Based on the task force report, the APA issued resolutions for appropriate affirmative responses to sexual orientation change efforts, and the resolutions were then followed by a press release. In this critical evaluation, we discuss the APA task force report, resolutions, and press release in the context of a methodological, clinical, and ethical framework.

Introduction

The American Psychological Association (APA), an influential organization comprised of approximately 150,000 members, asked a six-member committee—the APA on Appropriate Therapeutic Responses to Sexual Orientation—to review selected research articles dealing with sexual orientation change efforts (SOCE) that were published in English between 1960 and 2007.

The task force report—titled report *of the American Psychological Association on Appropriate Therapeutic Responses to Sexual Orientation*—along with proposed resolutions, was released during the APA's 2009 annual convention in Toronto, Canada. It was adopted by the APA's governing Council of Representatives by a vote of 125–4 (Crary, 2009). A press release was subsequently disseminated worldwide via the Associated Press (APA Press Release, 2009).

This paper presents our critical evaluation of the findings of the task force report (APA, 2009), resolutions (APA, 2009, Appendix A), and press release/media coverage (APA Press Release, 2009; Crary, 2009; and Maugh, 2009) in the context of a methodological, clinical, and ethical framework.

Methodological Contexts

In reviewing and evaluating the task force report, some methodological concerns became apparent. One of the task force's principal rationales for the creation of the report was that "some APA members" (p. 12) believed a previous resolution (APA, 1998) needed to be reevaluated, mainly because it did not address questions regarding SOCE efficacy or safety. Unfortunately, the report never mentioned who these members were, how many there were, and in what format they addressed such concerns. Most importantly, if questions of efficacy and safety were to be answered in the present tense, then it would seem more appropriate if they had conducted a controlled research study testing efficacy and safety rather than a review of literature.

The task force interpreted SOCE studies as relying "almost exclusively of individuals who [had] strong religious beliefs" (p. 25), "a highly select[ed] group of people" (p. 28), and "composed almost exclusively of Caucasian males" (p. 33). However, in examining published studies from 1954 to 2004, 17 of which the task force reviewed, 82% did not report the religion of participants and 79% did not report race (Serovich et al., 2008). Further, a systematic review of SOCE concluded that the numerous omissions of demographics in SOCE studies threatened the validity of interpreting the data (Serovich et al., 2008). It appears that the failure to point out the findings of Serovich and colleagues (2008) is a shortcoming of the task force report.

In a footnote to the overview of their review, the authors commented that they excluded a study by Byrd, Nicolosi, and Potts (2008), alleging it was published after the time their review was completed *and* that it appeared to be simply a reworking of an earlier study by the same authors. However, the latter statement assumes the task force reviewed the study, at least in part. It may be plausible to exclude the study solely on the factor of its publication date—but to subjectively describe it militates against the task force's stated rationale. Interestingly, the task force authors managed to include other citations as late as 2009 in the writing of their report, so their statement that a 2008 report was too late raises doubts.

Another methodological concern is in the report*'s* allegation that SOCE studies showed that "enduring changes to the individual's sexual orientation [was] uncommon" (p. 2) and "unlikely" (p. 63). However, by recognizing in the report that the majority of those studies were not longitudinal, it would appear premature to make a conclusion about "enduring changes." A more appropriate conclusion would state that based on the studies the task force cited, no conclusion about enduring changes could be made.

The authors cite two pieces of literature, American Psychiatric Association (1973) and Gonsiorek (1991), as evidence that "same-sex sexual attractions, behavior, and orientations *per se* are normal and positive variants of human sexuality and are not

indicators of either mental or developmental disorders" (APA, 2009, p. 14). However, the APA (1973) document was not a scientific study (Bayer, 1987), and the Gonsiorek (1991) citation came from a chapter in a book he coedited. Although Gonsiorek wrote an interesting article, his purpose was to point out earlier studies that he and his fellow authors judged as having faulty samples and poor design; he did not try to present new empirical research on that subject.

Furthermore, we note methodologically that Serovich and colleagues (2008) excluded *all* studies that were not published in a peer-reviewed scientific journal when she and her coauthors conducted a systematic review of research on SOCE. The task force was inconsistent on this point. Regrettably, it appears as though it picked and chose among the literature, which in and of itself represents a methodological flaw. Thus, it is fair to conclude that the task force did not uphold the same methodological standards it ascribed to its critique of SOCE.

Failure to Review and Report All Evidence

The authors stated they reviewed "83 studies" (six experimental, three quasi-experimental, 46 nonexperimental); however, what they listed adds up to only 55 (APA, 2009, pp. 125–130). Additionally, some assertions made by the authors lack substantial support. For example, they claim that "people will report change under circumstances in which they have been led to expect that change will occur" (APA, 2009, p. 29). However, no evidence is presented to validate this statement. The report claims that "external validity (generalization) of earlier [SOCE] studies [was] unclear" (p. 34), but then asserts that these same studies indicate that sexual orientation was not likely to change. If the validity of such studies was "unclear," it appears invalid to make an affirmative claim about actual therapeutic outcomes.

The APA task force was not able to say whether or not sexual orientation "can or cannot change" (p. 3) due to limited research and methodological flaws. It also said

that any conclusion was "tentative" (p. 44), that no studies could enable them to "make a definitive statement about whether recent SOCE is safe or harmful" (p. 83), and that more research is recommended to "improve our knowledge" (p. 90) about sexual orientation. If the conclusion is tentative and more research is needed, then it begs the question as to why the task force concludes that "sexual orientation is unlikely to change" (p. 84) or that fostering hope that sexual orientation could change was "inappropriate" (p. 66).

Inconsistent Application of Standards

The task force claimed that there is "*no* [emphasis added] . . . peer-reviewed research that supports theories attributing sexual orientation to family dysfunction" (APA 2009, p. 54). It cited one study by McCord, McCord, and Thurber (1962) to repudiate the theory that sexual orientation was associated with family dysfunction. However, it disregarded several published reports that specifically correlate sexual orientation to family dysfunction, such as Bieber et al. (1962); Lung and Shu (2007); Seutter and Rovers (2004); Silverman, Kwawer, Wolitzky, and Coron (1973); Wadler (1998); and Wilson and Widom (2009). Rosik (2012) investigated whether the task force consistently applied the same standards to SOCE studies as it did for the majority of studies the report referenced regarding developmental theories of sexual orientation. Rosik concluded that the report's standards were inconsistent and indeed contained many of the same methodological flaws that led the task force to dismiss SOCE research. In all fairness, the study by McCord, McCord, and Thurber (1962), which the task force used to repudiate the theory that sexual orientation was associated with family dysfunction, is no better methodologically than the studies they criticized as supportive to theories attributing sexual orientation to family dysfunction.

The task force also criticized SOCE studies on the grounds that the studies had high dropout rates. However, many treatment cohorts have high dropout rates; take, for example, a drug and alcohol treatment program (Polich, Armor, & Braiker, 1981). De-

spite the fact that other treatment programs also have high dropout rates, the APA does not caution against their efforts. As such, this inconsistency forces the reader to assume that the task force holds SOCE studies to higher standards than others.

Another example where the task force did not apply its research methodology standards consistently is their citation of Kurdek (2004) to support the essential similarity between gay, lesbian, and heterosexual couples. This study, which they used to justify their conclusion, committed eight—or 50%—of the methodological problems ascribed to SOCE studies (Rosik, 2012). Additionally, the authors stated that "research on the impact of heterosexism and traditional gender roles indicates that an individual's adoption of traditional masculine norms increases sexual self-stigma and . . . negatively affect[s] mental health" (p. 62). To support this claim, they provided only one citation, and that study was based on a convenience sample. Again, this is not the same rigorous research standard they called for in their review of SOCE.

The task force informs the readers that the greatest level of ethical concern was that SOCE were based on the presupposed notion that same-sex sexual orientation is a disorder, a symptom of a disorder, or evidences greater underlying pathologies. Their claim that homosexuality was not a disorder and that those who were identified as homosexual did not evidence any greater pathologies than heterosexuals was based, they claimed, on consensus in research and by professionals. However, this conclusion was not supported by the same type of review of literature to which they subjected SOCE studies. In fact, research has shown that homosexuals, in comparison to heterosexuals, do show greater pathologies (Hughes, 2006; Sandfort, de Graaf, Bijl, & Schnabel, 2001; Zietsch,Verweij, Bailey, Wright, & Martin, 2009).

The authors claim that sexual orientation distress in adolescents is likely found "in families for whom a religion that views homosexuality as sinful and undesirable is important" (APA 2009, p. 73), without providing any valid substantiation for this proposition. Once again, the lack of rigorous research is evident and contradicts the

standards they seek for SOCE studies. In supporting the claim that adolescents with a lesbian, gay, or bisexual identity faced exclusion and rejection, they provided case studies as proof (e.g., Cates, 2007), which is something they specifically rejected when reviewing SOCE efficacy.

Different Standards for *Gay Affirmative* Approaches than for SOCE

In a section titled "Affirmative Approaches" (APA, 2009, p. 22), the task force authors asserted that the underlying theories driving SOCE were "ill-founded" (p. 22). In the attempt to prove its point, the task force cited three studies that were not methodologically sound. The first study was Kinsey, Pomeroy, and Martin (1948), a controversial research report that claimed homosexuality was more common and usual than originally thought at the time. However, this study contained some of the very flaws of which SOCE studies were accused. The samples in the study were not random, and the study also had limits due to the fact that some samples consisted of pedophiles.

The second study cited was Ford and Beach (1951), which suggested that because homosexual behavior was observed in the animal kingdom, it must be natural. Although homosexual behavior can indeed be found in the animal kingdom, it is not the rule, and when observed, is usually circumstantial (e.g., the result of domestication, misinterpretation, interrupted environments, etc.) (Phelan, 1998). The study by Ford and Beach is also limited because their definition of sexuality included only stimulation and excitation of the sexual organs. Additionally, the authors (who were not psychologists) admitted that they were not qualified to assign application of their findings to the field of human psychology. They indicated that the study was meant to discuss the relationship between ethology and anthropology, specific to that sample, and not meant to be generalized (Lyons & Lyons, 2004).

The third study cited—Hooker (1957)—is also flawed. Using only a small convenience sample, a limited amount of psychometrics, and no longitudinal follow up, Hooker

concluded that homosexuals were no more pathological than heterosexuals. However, Gonsiorek (1991), whom the APA cites in their report as reliant, found Hooker's study to be "seminal …[and that] this research was so consistent in its lack of findings suggesting inherent psychopathology in homosexuality that researchers began moving on to other projects by the 1980s. Recent research has dropped off because the inherent pathology of homosexuality has been answered from a scientific point of view and has not been seen as requiring more research" (p. 132).

It is interesting that Gonsiorek, like the APA, sees Hooker's study as "scientific" and therefore dismisses further needs for research, and yet it has similar methodological flaws to what the APA assigned to SOCE. By contrast, in a reanalysis of the Hooker results, Schumm (2012) has discovered that Hooker actually found significant differences in test results and lifestyle choices between heterosexual and homosexual men. These results challenge the interpretations and uses of Hooker's study to attempt to justify the "no differences" hypothesis.

In sum, by citing these three studies, the task force authors use a double standard: They fault the research on SOCE based on perceived methodological flaws, yet cite studies with similar methodological flaws to support their own conclusions. The authors claimed to have presented a framework for *affirmative therapeutic interventions* [emphasis added] that were based on a "comprehensive review of the research and *clinical literature* [emphasis added]" (APA 2009). However, they chose to exclude clinical reports of sexual orientation change when considering their review of SOCE. In fact, they dismissed as inadequate at least 34 psychoanalytic reports, involving more than 500 patients who had undergone SOCE, even though they admitted that psychoanalysis (along with behavior therapy) was "the dominant psychiatric paradigm" (p. 21) of the first half of the twentieth century. They excluded reports of both clients and clinicians who noted complete reversals in sexual orientation (e.g., Bieber, et al., 1962; Caprio, 1954; Ellis, 1959; Gordon, 1930; Hadfield, 1966; Hatterer, 1970; MacIntosh, 1994; Ovesey, 1969; and Siegel, 1988).

An apparent decision to promote *gay affirmative psychotherapy*, a specific form of psychotherapy that encourages same-sex attracted persons to accept and embrace homosexuality and that is opposed to SOCE, was evident throughout the report. Although the phrase *affirmative therapeutic interventions* (p. 1) was introduced early in the task force's report without a specific definition, it is not until page 11 that the authors state, "This approach to psychotherapy is generally termed affirmative, gay-affirmative, or lesbian, gay, and bisexual (LGB) affirmative" (p. 11). Therefore, the phrase *gay affirmative psychotherapy* should be inserted wherever the phrases *affirmative therapeutic interventions*, *affirmative approaches*, or *gay affirmative therapeutic interventions* are seen throughout the report.

While the task force contends that *gay affirmative therapy* [emphasis added] is supported "on the basis of *growing* scientific evidence" (p. 11) and believes it is the best form of treatment for those who present with same-sex sexual orientation conflicts, advocates of SOCE state very similar arguments as to why they favor SOCE. While the task force states that few forms of SOCE have been subjected to "rigorous examination of efficacy and safety" (p. 83), they do not demand a comparable standard for other widely used types of psychotherapy, specifically for the *gay affirmative psychotherapy* advocated by their report.

Their bias toward *gay affirmative psychotherapy* is transparent. The APA endorsed *gay affirmative psychotherapy* over that of SOCE a dozen years earlier (APA, 1998). In fact, the APA's prior guidelines for affirmative models were used as a reference in the formation of *The Handbook of Affirmative Psychotherapy with Lesbians and Gay Men* (Ritter & Terndrup, 2002). Thus, it appears there is a preference for *gay affirmative psychotherapy* rather than SOCE, which calls the task force's objectivity into question.

Clinical Contexts

Clients' Autonomy and Right to Self-determination

The task force authors first claimed that a factor leading people to seek SOCE was *internalized stigma* but then said that "clients' motivation to seek out and participate in SOCE seems to be *complex* [emphasis added]" (p. 45). Even conceding the complexity of clients' motivation, the task force gives little or no credence to clients' desire to change sexual orientation, let alone sets the same standard to measure *internalized stigma* as it did to measure the efficacy and safety of SOCE. The task force goes so far as to suggest that interpretation of traditional religious doctrines even guides some SOCE. We noted that while the task force included external factors of client's motivations, it neglected to consider possible internal motivators. An overt focus on external motivations without considerations to internal motivations is a slippery slope toward negating clients' right to self-determination and autonomy.

The authors also admitted "participants reported benefits from mutual support groups, both sexual-minority affirming and ex-gay groups" (p. 59). If, as they state, benefit was reported in both types of support groups, and if indeed the task force was supportive of a client's right to choose, logically the footnote on page 59 of the report would refer readers to both types of groups. However, in the footnote, the authors provided resources for only gay affirmative communities' web links; they exclude web links for ex-gays' sites.

While the authors state, "We encourage LMHP [licensed mental health professionals] to *support* [emphasis added] clients in determining their own . . . *behavioral expression* [emphasis added] of sexual orientation" (p. 62), they neglect to discuss what might be appropriate components of caution for clients whose *behavioral expression* may be potentially unsafe. Curiously, while encouraging a client's behavioral expression of sexuality, the authors discourage clients from seeking SOCE. This is a disconnect—on

one hand, they support a client's choice to express his/her sexuality unconditionally, yet on the other hand they apparently seem to deny him/her support if he/she chooses SOCE.

The authors compound problems when discussing the safety and autonomy of adolescent clients. While they say that "adolescents are in the midst of developmental processes in which the ultimate outcome is unknown" (p. 77), they recommend that "LMHP support adolescents' exploration of identity by accepting homosexuality and bisexuality as *normal and positive* [emphasis added] variants of human sexual orientation" (p. 76). At the same time, however, the task force dismisses affirmative SOCE research by suggesting those studies were not scientific enough and accuses SOCE proponents of engaging in philosophical conclusions. However, the authors are engaging in the same offense of which they accuse SOCE proponents. For example, they use terms such as *normal* and *positive*, which are philosophical instead of scientifically operationalized.

In the section on appropriate application of affirmative intervention with children and adolescents, the authors recommend that LMHP provide "information and education" (APA 2009, p. 80) to LGB children to support them and that their parents "be provided accurate information about sexual orientation" (p. 87). Absent, however, is any mention that LMHP discuss, and parents be taught, the known high-risk dangers associated with many aspects of LGB sexual practices. Most glaring is the omission of the empirical fact that since the inception of AIDS, gay men are at high risk for acquiring this disease. For example, the Centers for Disease Control and Prevention (CDC) has consistently published evidence that gay men and other men who have sex with men (MSM) have HIV/AIDS at a rate much greater than nongay/nonbi men (Lansky, 2009). The task force says on one hand that it is concerned about safety and welfare, yet on the other hand it omits essential educational recommendations vital to youth entering a high-risk subgroup.

SOCE Efficacy

In an effort to dismiss the efficacy of SOCE, the authors claimed information that stressed sexual orientation can be changed was based on "very limited empirical evidence" (p. 74). Their choice of language actually admits the existence of evidence, albeit what they perceived as limited. They had no substantive grounds on which to say "no evidence." Interestingly and coincidentally, to the contrary of their conclusion—and at the same 2009 APA convention where the task force released its report (APA, 2009)—an extended longitudinal study by Jones and Yarhouse (2009) was also released. Jones and Yarhouse noted that they used the "most rigorous longitudinal methodology ever applied to [the] question of sexual orientation change and possible resulting harm" (p. 4) and concluded that "the findings of this study would appear to contradict the commonly expressed view of the mental health establishment that sexual orientation is not changeable and that the attempt to change is highly likely to produce harm for those who make such an attempt" (p. 12). Neither the task force resolution nor press release took note of this; at a minimum, this data should be included as an addendum to the task force report and to the media in like manner.

Definitional Problems

Sexual orientation identity was defined in the report as what or how people label themselves, based on factors such as "individual or group affiliation" (p. 2), sexual values, and behaviors. In the report, the authors dichotomized *sexual orientation identity* and *sexual orientation* and concluded that it was unlikely that one could change *orientation*, and that changes occur only in *identity*. To support such a contention, the task force suggested that the SOCE research it studied did not adequately distinguish between *sexual orientation* and *sexual orientation identity* (even though SOCE research exists that distinguishes between three separate aspects of sexual orientation—attraction, conduct, and self-identification). The authors conclude that SOCE research "obscured what actu-

ally can or cannot change in human sexuality" (APA, 2009, p. 3). At a minimum, they concede that "sexual orientation identity—not sexual orientation—appears to change via psychotherapy, support groups, and life events" (p. 63). However, in spite of its own finding that the research is *obscured* (perhaps not properly distinguishing whether all indicators changed or only some), the task force issued a press release telling mental health workers to *avoid* telling clients that they can change their sexual orientation through therapy or other treatments (APA Press Release, 2009). Since the task force concludes that the research it reviewed made it difficult to find out what can or cannot change, it would seem more appropriate to *avoid* telling clients that they can *or* cannot change their sexual orientation through therapy or other treatments.

Sexual Minority Stress

The task force described *sexual minorities* as "the entire group of individuals who experience significant erotic and romantic attractions to *adult members* [emphasis added] of their own sex" (p. 1). Although it uses the term *adult* in its definition, it describes youth and adolescents as *sexual minorities* in other areas of the report.

The report*'s* authors also claim "internalized homophobia"—in other words, *minority stress* and *sexual stigma* (p. 1)—as evidence for the psychiatric vulnerability to a wide variety of mental health issues seen among nonheterosexuals. The authors claim that there is a "growing body of evidence concluding that sexual stigma" (p. 1) directed at nonheterosexuals is primarily responsible for such harm (see also p. 54). However, this "evidence" does not adhere to the same research standards requested of SOCE, and what the authors do use to support their case is from *gay affirmative* resources, again displaying inconsistent application of standards.

In an effort to find out what mechanisms—*minority stress*, environmental factors, and/or genetic factors—may elevate psychiatric vulnerabilities among nonheterosexuals, Zietsch and colleagues (2009) also attempted to find support for a *minority stress* hypoth-

esis; however, the sexual stigma hypothesis was weakened by evidence of mental health issues even in liberal, gay-affirming countries, such as the Netherlands (Sandfort et al., 2001; Zietsch et al., 2009). In fact, some studies reveal that nonheterosexuals have higher rates of psychopathology when compared to heterosexuals, regardless of *minority stress* (Sandfort et al., 2001; Zietsch et al., 2009). There is also no conclusive evidence to support that society or other environmental factors are causal of *minority stress*.

Ethical Contexts

According to the APA Ethical Principles, psychologists should refrain from taking on interests that impair their objectivity (APA, 2002). One of the task force's principle rationales for the creation of its report was that "Advocates [those who opposed SOCE (e.g., Drescher, 2003) and those who promoted SOCE (e.g., Nicolosi, 2003)] asked" for such a report (p. 12). However, when it came to assembling the task force, advocates who were preopposed to SOCE (i.e., Drescher, 2003; Glassgold, 2007) were actually chosen to be members of the task force, while no proponents of SOCE were chosen (Nicolosi, n.d.).

Although the authors said that "guidelines and standards for practice are created through a specific process that is *outside the purview* [emphasis added] of the Task Force" (APA, 2009, footnote, p. 65), they made recommendations for public policy. Despite their own principle to not overtly influence public affairs (Tyler, 1969), this has been a recent trend for the APA. In several recent cases the APA has directly advocated for legal and policy changes (APA, 1998, 2003, 2005, 2008a). The task force undoubtedly was well aware that its report would be used as such and would be voted on by the APA's governing Council of Representatives at its annual convention. In fact, the report*'s* authors asked for such a resolution. The policy aspect was passed without much scrutiny. Likewise, it did not accomplish a survey of its own membership, the mental health profession, or the general population for approval/disapproval, nor was there an established review period for feedback, despite the fact that the voice of the APA's members is generally solicited

when the APA governance wishes to issue a major statement or resolution on behalf of the association (APA, n.d.).

False Pretensions about Sexual Orientation and Biology

In the task force report, the authors make the claim that "sexual orientation is tied to physiological drives and biological systems that are beyond conscious choice" (APA, 2009, p. 84); however, research that has tested biological origins of homosexuality is not definitive (Osmundson, 2011). The fact remains that any conclusive genetic causality for homosexuality has not been found (APA, 2008a). Such a pronouncement saying that sexual orientation is tied to physiological drives and biological systems that are beyond conscious choice can be misinterpreted by the public and could potentially influence public policy, creating an ethical concern. Such a pronouncement also contradicts the APA's own public-disseminated information regarding sexual orientation and etiology, which says:

> There is no consensus among scientists about the exact reasons that an individual develops a heterosexual, bisexual, gay or lesbian orientation. Although much research has examined the possible genetic, hormonal, developmental, social, and cultural influences on sexual orientation, no findings have emerged that permit scientists to conclude that sexual orientation is determined by any particular factor or factors. Many think that nature and nurture both play complex roles. (APA, 2008b, p. 2)

The APA task force authors *assume* that those who seek SOCE will inherently be harmed (see section below) because their desire to change sexual orientation will "not fit the individual's predispositions" (APA 2009, p. 58). They further contend that a client's desire and actual ability to change is "irreconcilab[le]" (p. 58) and argue that it creates the need

for emotion-focused strategies to *affirm* sexual orientation identity. The task force's solution is that therapeutic outcomes should include helping clients "com[e] to terms with the disappointments, losses, and dissonance between psychological and emotional needs and possible and impossible selves" (p. 58). Such a position appears to take an *a priori* assumption that homosexuality is inborn and therefore immutable—in truth, however, such a contention is unsupported and contradicts the task force's own statements. It is also not fully supported by other APA members (Cummings, 2010; Jones, Rosik, Williams, & Byrd, 2010)—and, according to the APA's own Ethical Principles, the APA should not make deceptive statements regarding research findings (APA, 2002).

Conclusions about Harm

In their section on outcomes of "improving mental health" (APA 2009, p. 41), the authors fail to discuss those studies that demonstrate positive outcomes of SOCE. After discussing three studies from earlier research (1970–1972), the authors shift their attention to alleged *harm* from SOCE. However, by excluding numerous studies that evidence benefit rather than harm, they continue to demonstrate inconsistent standards.

The authors claim that SOCE should be avoided because "reports of harm suggest that such treatments can reinforce restricting stereotypes, increase internalized stigma, and limit a client's development" (APA 2009, p. 87). Such an opinion is based on limited research and interestingly employs the very same arguments for which they criticized SOCE studies: nonlongitudinal and flawed methodology, use of opinion pieces, inconclusive outcomes, and so forth.

The authors state that they found no study that systematically evaluated potential harm. Yet, they claim that SOCE "can produce *harm* [emphasis added]" (APA 2009, p. 83). Such claim is based on anecdotes. Conversely, as previously emphasized, they dismiss any anecdotal evidence for positive SOCE outcomes. The authors' own language sets forth an inconsistency, both in their conclusions as to evidence of perceived harm or benefit and in

their manner of presenting findings on this point. An example of a lack of consistency is seen when the report states that "some [former participants in SOCE] perceived that they had benefited from SOCE …" (APA, 2009, p. 3), while also stating that "some [former participants in SOCE] perceived that they had been harmed [from SOCE]" (p. 3). Although the evidence cited by the task force includes a random variety of symptoms taken from individual clients' reports, the report categorically rejects SOCE studies that rely on individual self-reports of change. To present material in such a manner shows an assumptive bias, particularly when other findings of the same studies were dismissed under the notion that the results of the studies were not obtained through the rigor of true *experiment*.

The authors continually contradict themselves in this respect. For example, they say, "[studies] provide no clear indication of the prevalence of harmful outcomes among people who have undergone [SOCE]" (p. 42) due to inadequate designs, but then complete the thought by a statement that SOCE "may cause or exacerbate distress and poor mental health in some individuals, including depression and suicidal thoughts" (p. 42). If no "clear indication" about the prevalence of harmful outcomes was found and the studies were flawed, the conclusion of the authors that attempts may cause or exacerbate distress is flawed and presents ethical concerns. The use of language to precondition the reader to a desired conclusion of the task force is evident. Another example is where the task force authors discuss twelve studies in which anecdotal cases of harm were reported. They claim that "we found that there was *some* [emphasis added] evidence to indicate that individuals experience harm from SOCE" (APA, 2009, p. 43), but then they cite at least fifty-five studies where the evidence reportedly related to patients who reduced homosexuality. To buttress their conclusions, the authors describe these latter outcomes as "rare" and stated that "few studies provided strong evidence" (p. 43) of the efficacy of SOCE. Note the use of the word *some* to evidence harm from a lesser number of cases and the words *few* and *rare* to describe the greater number of cases that suggested benefit (e.g., "*some* evidence…harm" vs. "*few* studies . . . evidence . . . changes" [p. 43, emphasis added]).

The Role of Media

Even though participants in some recent studies reported beneficial effects of SOCE, such as a perceived change in their sexual orientation, the APA stated in its press release that "mental health professionals should avoid telling clients that they can change their sexual orientation through therapy or other treatments" and that sexual orientation "was unlikely to change" (APA Press Release, 2009, p. 1). However, the APA claim is contraindicated by other reports that document sexual orientation change (Cummings, 2010; Hughes, 2006; Jones & Yarhouse, 2007, 2011; Phelan, Whitehead, & Sutton, 2009; and Throckmorton, 1998). The APA's press release clearly leads to media fabrications. For example, after receiving the press release, the *Los Angeles Times* headlined: "Psychologists say sexual orientation *can't* [emphasis added] be changed through therapy" (Maugh, 2009, n.p.). Note the use of the word *can't*. While the APA cannot completely control how the media interpreted its press release, it does have an obligation to correct such errors. It states in its own Ethics Code that when its research is misinterpreted or misquoted, it should take reasonable steps to correct the misinterpretation (APA, 2002).

The task force states that research on SOCE *can go forward*, as long as it is done with "high-quality measures" (APA, 2009, p. 6). At the same time, the authors recommend that practitioners refrain from attempting to alter sexual orientation because they do not believe it is appropriate to foster expectations that SOCE works. However, this begs the question of how SOCE can meet research standards if the advice by the task force is designed to dissuade its practice.

The resolutions recommended by the task force and subsequently approved by the APA's governing Council of Representatives—as well as subsequent reports in the news media—appear to contradict the APA's own Ethics Code (APA, 2002). They also appear to contradict and the APA's adopted Leona Tyler Principle (Tyler, 1969), which obligates the APA to support client self-determination and to not mislead the public with data that

supports bias agendas meant to persuade policy. In the context of the APA's Leona Tyler Principle, it is not only important to determine what science can or cannot say but that ethicality and diversity be abided (A. D. Byrd, as cited in Cummings, 2010). It is misleading when SOCE is painted as harmful in the absence of conclusive, randomized, comparison studies that prove otherwise. It is misleading to say sexual minority status is tied to biological systems that are beyond conscious choice, when in fact this has not been conclusively supported in the research.

Finally, the contexts of ethics need not be taken lightly when there are implications for influencing public policy and applied therapeutic changes.

Concluding Discussion

The APA task force's sensitivity to put forth efforts to understand the studies relevant to SOCE are commendable. However, many concerns surfaced when we evaluated the report within the context of a methodological, clinical, and ethical framework. In sum, the task force did not consider all the relevant literature; they admit the population who sought SOCE is largely *unknown*; they utilize inconsistent standards; and the evidence they chose to use is no better than the evidence they use to discredit SOCE. They do not ascribe the same standards for SOCE—the need for strong empirical rigor—as they do for gay affirmative therapy, family dysfunction of sexual minorities, psychopathology of sexual minorities, and sexual minority stress.

Moreover, the task force admits that its report is not substantive enough to make any conclusive and definitive recommendations about the efficacy and safety of SOCE. Yet, the task force states that it would be inappropriate to recommend that LMHPs use SOCE, despite the APA's *Ethical Principles of Psychologists and Code of Conduct*, which states that psychologists should respect the rights of client self-determination (APA, 2002). We concur with a prior critique of the task force report that found the report problematic both in its overly scrupulous application of methodological rigor to the SOCE and

its failure to apply enough rigors to a number of other issues on which it touches (Jones et al., 2010; Rosik, 2012). It is not unusual that an APA task force report, albeit on a different subject matter, has been critiqued and fallen short when independently reviewed (Coleman, 2008).

The task force accuses the authors of literature dealing with SOCE to have made "inappropriate conclusions drawn from data" (p. 90), and it goes into a discussion about how studies with social implications need to be held to high standards due to their potential influence on policymakers and the public. It also says that misleading information can have serious costs. Yet this criticism mirrors the errors of the task force report. The task force issued a press release telling mental health workers they should *avoid* telling clients that they can change their sexual orientation through therapy or other treatments (APA Press Release, 2009). Since research has made it difficult to find out what can or cannot change, it would seem ethically appropriate to *avoid* telling clients whether they can *or* cannot change their sexual orientation through therapy or other treatments. Likewise, it would be appropriate to correct the media they have influenced to purport headlines that sexual orientation "can't" change (Maugh, 2009). This approach would be more compatible to the APA's Ethics Code (APA, 2002) and the APA's adopted Leona Tyler Principle (Tyler, 1969), which obligate the APA to support client self-determination and to not mislead the public or persuade public policy.

While the task force suggested that SOCE is unlikely to produce change in sexual orientation and can even be harmful, its own review of the research revealed insufficient evidence to say whether or not harm resulted from SOCE—or even whether sexual orientation can or cannot be changed. In fact, it contended that "the research on SOCE . . . has not answered basic questions of whether or not it is safe or effective and for whom" (p. 90) and "there are no studies of adequate rigor to conclude whether or not recent SOCE do or do not work to change a person's sexual orientation" (p. 120). Thus, for the authors to make positional recommendations with policy implications based on evidence that they admit is

not definitive presents potential ethical problems for both the public and the mental health profession. Likewise, while they say studies that support SOCE lack adequate rigor, their support for gay affirmative approaches have not been tested with equal empirical rigor.

After a review of the research literature pertaining to SOCE, the task force concludes that those studies were either poorly designed or contained serious methodological flaws and lacked empirical rigor. As such, it recommends against SOCE in its published report. This seems to suggest a straw man argument: Since SOCE lacks adequate scientific rigor, it is therefore inadequate.

Further, because the resolutions come from an authoritative organization, they hold the risk of being perceived by lawmakers and state licensing boards as *policy* and therefore by proxy prescriptive to LMHP practice. A case in point is where the task force report is currently being used as a reference for proposed California Senate Bill 1172 that would ban psychotherapists from offering SOCE to clients under the age of eighteen, regardless of clients' and their parents' wishes. This bill states that "sexual orientation change efforts pose critical health risks," claiming—falsely—that the task force report supports this assertion (Senate Bill 1172, 2012, p. 1).1

Lawmakers and state licensing boards who use the task force findings as *policy*—and therefore by proxy prescriptive to LMHP practice—could potentially create legal entanglements for LMHP, particularly those who follow a conservative religious framework (DeBoer, 2009). Any *policy* based on the task force report that favors *gay affirming psychotherapy* while abandoning one that is more compatible with a conservative religious worldview will burden the prior religious practices of some and presents any potential *policy* as unconstitutional (DeBoer, 2009). What DeBoers (2009) means is that the true practice of free exercise will have to include "the incorporation of conscience"

1 Each time SB 1172 has come up for vote, both in committee and before the California Senate and Assembly, elements of the bill have changed. The key provision banning the practice of SOCE for minors has consistently remained. After having passed both houses of the California legislature, SB 1172 was enrolled on September 5, 2012, and was sent to the governor for authorization.

(p. 430), and therefore clients will have "freedom to pursue [a] full range of counseling approaches, including change therapy or sexual identity therapy" (p. 430).

Finally, it is recommended that the APA task force report, with its voted resolutions and press release, be cautiously reviewed in light of our evaluation so as not to mislead the media, the public, and the mental health profession, and by such actions impede certain clients from receiving treatment respectful to their personal values and preferences.

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Did the American Psychological Association's *Report on Appropriate Therapeutic Responses to Sexual Orientation* Apply Its Research Standards Consistently?

A Preliminary Examination[1]

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Abstract

In August 2009, the American Psychological Association Task Force released its report, *Appropriate Therapeutic Responses to Sexual Orientation* (APA, 2009). The report discouraged attempts at changing sexual orientation, asserting that such efforts are unlikely to succeed and involve some risk of harm. The task force further recommended affirmative therapeutic interventions based in part on the conclusion that research has not found developmental influences to be involved in the origin of sexual orientation. In this critical analysis, I identify several methodological limitations cited by the task force in critique of sexual orientation change efforts (SOCE) and apply them to a review of the majority of studies to which the report makes reference regarding developmental theories of sexual orientation. Based on this examination, it appears most of the studies the task force cited in support of its conclusion had similar methodological flaws that led to its dismissal of SOCE research. Thus, it appears the task force applied its methodological critique inconsistently, raising questions about what might give rise to such variation in reviewing standards.

Did the American Psychological Association's *Report on Appropriate Therapeutic Responses to Sexual Orientation* Apply Its Research Standards Consistently?

A Preliminary Examination

The American Psychological Association's recent task force report (APA, 2009), *Appropriate Therapeutic Responses to Sexual Orientation*, concluded that "efforts to change sexual orientation are unlikely to be successful and involve some risk of harm, contrary to the claims of SOCE practitioners and advocates" (p. v). The task force further recommended affirmative therapeutic interventions based in part on the conclusion that research has not found developmental influences to be involved in the origin of sexual orientation.

The task force report contains a major section dedicated to identifying the methodological problems in research on sexual orientation change efforts (SOCE). This section (pp. 26–34) is meticulous in its efforts to identify any and all limitations within SOCE research in order to discredit this literature. At the same time, the report also highlights literature pertinent to developmental theories of sexual orientation. While no body of research is free from limitations, one measure of the degree of thoroughness and objectivity behind scientific critiques of this nature is the extent to which the criticisms are uniformly applied to research affirmed by the reviewers. The current examination seeks to determine if the APA task force scrutinized the limitations of the research supporting its conclusions to the same degree it did the SOCE research.

Method

In order to obtain at least some preliminary assessment of this issue, one claim made by the task force was assessed: "Studies failed to support theories that regarded family dynamics, gender identity, or trauma as factors in the development of sexual orientation" (APA, 2009, p. 23). In support of this claim, the task force cited ten different references (Bell, Weinberg, & Hammersmith, 1981; Bene, 1965; Freund & Blanchard, 1983; Freund

& Pinkava, 1961; Hooker, 1969; McCord, McCord, & Thurber, 1962; Peters & Cantrell, 1991; Siegelman, 1974, 1981; Townes, Ferguson, & Gillem, 1976).

I was able to obtain the source materials for seven of these articles through the EBSCO database and local libraries. Two of these articles (Freund & Pinkava, 1961; Bene, 1965) appeared in relatively obscure or defunct journals, while the other (Siegelman, 1974) was not locally obtainable. Another of these articles, the Hooker (1969) reference, was in fact a review piece and thus not suitable for the present analysis of research methodology. Moreover, a review article does not fit the task force's billing as being a study that "failed to support" the theories in question, since a review article is an interpretation, not an empirical study. The remaining six research studies cited by the task force thus comprise the focus of my analysis.

Table 1 presents the major methodological limitations ascribed by the task force to the SOCE literature along with the frequency of those limitations in the six studies cited by the task force in support of its etiological conclusion. In order for the task force to conclude so unequivocally that the studies cited failed to support developmental theories of sexual orientation, the research it noted should be free from most, if not all, of these limitations. As a check on my objectivity, another psychologist blind to the purposes of this project randomly reviewed three of the six research articles using the same list of limitations. The tabulations indicated agreement in 72% of the ratings, an acceptable degree of reliability.

Results

Before examining the findings, it should be noted that all of these studies are cross-sectional in nature. While one study (McCord, McCord, & Thurber, 1962) did utilize some longitudinal data, it was not analyzed in a manner that took advantage of the cross-sectional character. This lack of prospective data would appear to be an important limitation in considering the task force's utilization of such research to support its position.

Specifically, it would seem to negate the validity of using such studies for making claims for or against any developmental factor in the etiology of same-sex attractions, yet this is not mentioned by the task force. Overall, as Table 1 indicates, all six studies had important methodological weaknesses germane to cross-sectional research designs that the task force used to disqualify SOCE. Below I provide subsections for each limitation to highlight how consistently the SOCE methodological problems were applied to the etiological literature.

Lacks a clear definition of terms. Most of the studies I reviewed generally attempted to provide some clarification in the definition and operationalization of their variables, at least as far as parental relationship or childhood abuse constructs are concerned. However, there were confusing descriptions. For example, terms such as *affectional interaction* or *dependency* in the McCord et al. (1962) study seemed to lack clarity. The authors identified the latter condition as present if the boy "showed an unusually strong desire for adult approval" (p. 363). Boys evidencing high dependency were classified as showing feminine identification, although it was not immediately clear what constituted "strong desire," a qualifier that appears to lend itself to significant subjectivity in interpretation, which is one reason used by the task force to disqualify some SOCE studies.

Reliance on self-report measures. Of the six studies reviewed, all six involved self-report instruments; for five of the studies, self-report measures were the only ones utilized. The McCord et al. (1962) study was partially based on direct observations, but these chart records were reviewed more than a decade later by the researchers, and at that time the observations were categorized into variables of interest to the study. Thus, the task force conclusions regarding the etiological significance of developmental factors for sexual orientation are based almost entirely on respondents' retrospective memory as opposed to observational assessment, placing a serious limitation on definitive conclusions in this regard.

Reliance on measures of unknown validity/reliability. The six studies reviewed were highly inconsistent in reporting the psychometric properties of the instruments they em-

ployed. Most appeared to employ some instruments that presumably had been developed with an eye toward validity and reliability issues; however, statistics such as alphas were rarely reported, so one is left with little or no evidence that psychometrics were considered.

Siegleman (1981; reported in Siegleman, 1978) was the most forthcoming with information about reliability. Freund and Blanchard (1983) reported alphas for two of their scales (p. 14), but it appeared that these alphas were for prior research using the scales with different samples and not for the current study and sample, constituting a major problem (Thompson & Vache-Haase, 2000). Peters and Cantrell (1991) modeled their questionnaire after a preexisting measure but provided no psychometric information for either, even with items described as attitudinal. This is not in line with common practice of ascertaining the reliability and validity anytime a scale is changed or adapted substantially (Thompson & Vache-Haase, 2000). This was also the case for the Townes et al. (1976) questionnaires and the scale variables derived from them. McCord et al. (1962) alluded in a footnote where reliability information can be located (p. 362), but that doesn't address the validity concerns that seem to arise with how some of their variables were operationalized. Bell et al. (1981) reported their composite measures to be reliable but did not provide the specific alphas. These omissions and uncertainties appear to constitute serious psychometric inadequacies when considered in light of the task force standards.

Study participants not blind to study purposes. The frequency of this particular methodological shortcoming is difficult to ascertain from the information provided in the studies. In keeping with the task force sentiment that studies are generally at risk of this problem if they do not explicitly endeavor to address it, I estimated that four of the studies had potentially introduced bias of this nature. In all but the McCord et al. (1962) study, some or all of the participants were recruited by the researchers or their assistants and very little is stated regarding the wording used to encourage involvement in each study. We can infer that participants were often known by the researchers as patients (Freund & Blanchard, 1983), students (Peters & Cantrell, 1991), or social acquaintances

(Bell et al., 1981; Townes et al., 1976). Bell et al. (1981) noted that some field staff who conducted the interviews "attended private parties held for our benefit by individuals supporting the aims of the study" (p. 11) as part of their recruitment strategy.

If the task force applied the same scrutiny to these studies as they did to the SOCE studies, then these contacts could quite conceivably have introduced a bias that left participants far from "blind" to the general aims of the research. Only the Siegleman (1981) research included a measure of social desirability that could provide some check for response bias. The task force observed, "Knowing that one is being studied and what the experimenter hopes to find can heighten people's tendency to self-report in socially desirable ways and in ways that please the experimenter" (APA, 2009, p. 32). This is a most germane concern for four of the six studies examined here and is also applicable to the next problem explored.

Recruiter/selection bias and/or demand characteristics. The present examination suggests that such bias and/or demand characteristics were likely to be present in each of the six studies. McCord et al. (1962) employed data collected by social workers who "would appear unannounced, with a frequency which made it possible to observe families at meals, during their leisure, in the midst of crisis, and during their ordinary daily routines" (p. 362). It is hard to imagine that the sudden presence of an observer in the room would not impact the behavior of parents and children who knew they were in an experimental program aimed at the prevention of delinquency. In another study (Townes et al., 1976), participants were recruited "following 6 months of observation and involvement by the second author in homosexual institutions" (p. 263). These institutions included "homosexual bars" and a "homosexual counseling center." This advanced familiarity with the recruiter could potentially influence responding in an unknown manner.

Of particular interest is the decision of Bell et al. (1981) to remove from their sample all participants who were reportedly influenced by psychoanalytic theory regarding the etiology of homosexuality when these individuals differed from the heterosexual

subgroup in a manner dissimilar to the difference found for homosexual participants not exposed to such theories. The clear assumption is that such exposure would bias these participants into responding in a way consistent with the psychoanalytic perspective. However, it is just as plausible that participants whose background was consistent with aspects of psychoanalytic theory sought such information due to their sense of its applicability to their developmental narrative. Whatever one wants to believe about this matter, the decision of Bell et al. most certainly reduced the likelihood that their findings would support the psychoanalytic school of thought. In spite of this, the authors did report support for the nonuniversal application of some aspects of psychoanalytic theory in comprehending pathways to homosexuality, such as a modest role of identification with the same-sex parent and poor relations with father in the development of sexual orientation (pp. 189–191).

It appears probable that the researchers in these studies had little sympathy for the psychoanalytic view of homosexuality; thus, to quote the task force, "It cannot be assumed that the recruiters sought to encourage the participation of those individuals whose experiences ran counter to their own view of these approaches" (APA, 2009, p. 34). This could plausibly introduce "unknown selection biases into the recruitment process" (p. 34).

Small sample size. While there is no strict definition for what constitutes a small sample, most of the studies cited by the task force report total samples with well under 200 subjects, with comparison groups sizes for five of the studies varying from five (McCord et al., 1962) to 147 (Freud & Blanchard, 1983). The vast majority of comparison groups in these studies had sizes in the 30 to 100 range. Small samples limit the reliability and generalizability of subsequent findings. The Bell et al., (1981) study was a welcome exception to this problem with a total sample of nearly 1,500 and comparison groups of nearly 1,000 (homosexual) and 500 (heterosexual). By contrast, only four of the 54 SOCE studies examined by the task force (APA, 2009, pp. 126–130) reported a sample size of 200 or more, with most samples below 50.

An empirical analysis of these sample sizes also brings into question consistency of the task force. The mean sample size of the 54 SOCE-related studies included in Appendix B of the report ($M = 46.9$, $SD = 128.4$) was not significantly different than the mean sample size of the seven studies ($M = 227.7$, $SD = 338.7$) cited as discounting developmental influences on sexual orientation when equal variances could not be assumed, $t(59) = -1.40$, $p = .21$. When simple case studies and one extreme outlier from both groups (Nicolosi, Byrd, & Potts, 200 [$N = 882$] and Bell, et. al, 1981 [$N = 1456$]) were removed from the analysis to allow a more accurate comparison, the SOCE studies ($M = 65.0$, $SD = 70.1$) continued to have sample sizes statistically similar to the those found in the developmental studies cited by the task force ($M = 102.5$, $SD = 77.4$, $t[29] = -1.25$, $p = .22$).

Thus, despite roughly comparable sample sizes in the respective literatures, the task force chose to level this critique at the SOCE literature but not at its own cited etiological research. The limited sample sizes of these studies clearly make population generalizations an endeavor fraught with uncertainty for both of these literatures, to say the least. Quoting the task force again, "Small samples, sample heterogeneity, weak measures, and violations to the assumptions of statistical tests (e.g., non-normally distributed data) are central threats to drawing valid conclusions" (APA, 2009, p. 32).

In addition, significant sample attrition occurred in the McCord et al. study, with the full sample decreasing from 325 to 255 over the five years of observation. This degree of attrition (22%) was less than the task force reported for many of the early SOCE studies, but in line with the attrition rate (26%) reported in the more recent Jones and Yarhouse (2007) longitudinal research, which the task force summarily dismissed. As the task force noted, "Put simply, dropout may undermine the comparability of groups in ways that can bias study outcomes" (APA, 2009, p. 29). Why such cautions by the task force apply only to the SOCE literature is not readily apparent.

Violations of statistical assumptions. None of the six studies examined provided any statistical information that would allow the reader to assess whether or not applicable

univariate and/or multivariate data assumptions (such as linearity and normality) were met. In fact, none of the researchers mentioned that these assumptions had been checked and confirmed. While it is possible the data assumptions were investigated, absence of comment to this effect creates a degree of doubt as to how confident one can be about the results.

Narrow sample compositions. This was a problem with all six of the studies in question. The studies included samples that were recruited from "lower-class" boys (McCord et al., 1962); highly educated, younger residents of the San Francisco Bay Area (Bell et al., 1981); university students (Peters & Cantrell, 1991); and psychiatric patients (Freud & Blanchard, 1983). Homophile organizations were sampled in four of the studies. For example, recruitment of participants in the Siegelman (1981) study involved "The Albany Trust," a group "made up of members who support tolerance and freedom of psychosexual expression" (p. 3), and the "Cosmo Group," a college organization that "attempts to reduce censorship on television and radio, and fosters informed and tolerant opinion" (p. 3). For the majority and strongest of these studies, if the findings can be said to be representative of any group, they seem applicable to white persons who are younger, liberal, well educated, and reside in urban settings. Thus, the same criticism of narrowness in sample composition applies to these studies.

Convenience sample. This problem was also present in all six studies. None of the studies utilized a population-based sample, which is another serious obstacle to generalizing these research findings. As Bell et al. (1981) acknowledged, "In our case, we do not claim to have a representative sample of American homosexuals or heterosexuals, or even of those residing in the San Francisco Bay Area" (p. 19). The task force criticized the lack of "population-based probability sampling strategies" (p. 34) found in the SOCE literature, but this was not an obstacle for them when it came to referencing research that purportedly dispelled developmental theories of sexual orientation.

Failure to differentiate between sexual behaviors, attractions, and orientation identity. None of the six studies made all three of these distinctions in its operationalization of

homosexuality. Townes et al. (1976) did not indicate how they defined the homosexual group. McCord et al. (1962) considered boys to have strong homosexual tendencies "if they played with dolls, sometimes wore dresses, frequently expressed the wish to be a girl, or were overtly homosexual" (p. 363), a term that was not defined. Peters & Cantrell (1991) utilized a single item regarding self-reported same-sex versus opposite-sex preference. Of course, the APA task force noted that this distinction has arisen in the past twenty years, after the publication date of all but one of the studies investigated here. This did not prevent the task force from applying the standard to all the SOCE literature dating back to the 1960s.

Failure to differentiate sexual orientation from sexual orientation identity. According to the task force, sexual orientation refers to a person's pattern of sexual, romantic, and affectional arousal and desire, whereas sexual orientation identity refers to one's acknowledgement and internalization of sexual orientation as an identity. Again, none of the studies examined here made this distinction in its methodologies. The APA task force (2009) warning is thus applicable: "Recent research has found that distinguishing the constructs of sexual orientation and sexual orientation identity adds clarity to an understanding of the variability inherent in reports of these two variables" (p. 30). If one were applying the criteria evenhandedly, might this clarity also apply to the study of etiological factors?

Failure to assess for bisexuality. Not surprisingly, none of the six studies assessed participants for bisexuality. Bell et al. (1981) specifically dichotomized their use of the six-point Kinsey scale, where respondents with an average score from two through six "were classified as homosexual" (p. 32).

Discussion

I came to this examination as a psychologist and researcher prepared to agree with the task force and grant that we know very little conclusively about the efficacy of SOCE. I also approached this review not wedded to a one-size-fits-all etiological explanation of same-

sex attractions; rather, I wanted to see how consistently the task force treated the literature it cited to dismiss potential developmental factors such as family dynamics, trauma, and gender identity. Based on the analysis, which surveyed two-thirds of the research studies cited by the task force, including all of the more recent studies, it appears that these studies were not assessed by the task force with the same level of scrutiny or the same standards it applied to the SOCE literature. In fact, the task force cited studies in support of its positions that actually had the same problems as the SOCE studies it criticized.

Regarding SOCE, the task force concluded, "Due to these limitations, the recent empirical literature provided little basis for concluding whether SOCE has any effect on sexual orientation" (APA, 2009, p. 34). Given that many of these same limitations exist in the etiological literature cited by the task force, questions have to be raised as to why it chose to definitively dismiss this literature as "failing to support" developmental theories. It appears, based on the same criteria the task force used to dismiss SOCE, that its own conclusions have little basis in the literature.

A fairer rendering of the etiological literature the task force references would appear to be that this research is so methodologically flawed that we cannot make any conclusive statements concerning the applicability of developmental factors in the origin of homosexuality. Thus, by the task force's own methodological standards, the literature it cites fails to support *or rule out* a role for these potential developmental influences in the genesis of sexual orientation. If such ambiguity exists in the SOCE literature on methodological grounds, then by the task force's own criteria, this ambiguity also is present in the referenced etiological research. It appears that the task force has been inconsistent in the application of its methodological critique to the broader literature on homosexuality, and it may have been willing to offer more definitive conclusions about theories it wishes to dismiss than is warranted by its own standards. In a word, there is the appearance of substantial bias.

The extent to which such a tendency may permeate the APA report is not ascertainable from this examination, but the findings are enough to raise legitimate questions about

the task force's attention to detail and/or its impartiality. Thus, while the report may in some respects be a step forward in the conversation currently occurring over SOCE, it should not be considered as definitive as many who oppose such psychological care may proclaim it to be. Hopefully, the task force's efforts will be a stimulus to much more and sophisticated research on SOCE that includes the active recruitment and participation of diverse perspectives. Such inclusiveness represents the true spirit of our discipline, is essential to understanding human sexual behavior, and may well be the best means to ensure that scientific knowledge is furthered rather than stymied as it pertains to SOCE.

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Table 1

Frequency of the APA Task Force's SOCE Research Methodology Problems among Studies Cited as Disproving Traditional Developmental Theories of Sexual Orientation

Methodological Problem	Number of Studies Containing the Problem[a]
1. Lack clear definition of terms	1 of 6
2. Relies on self-report measures	5 of 6
3. Relies on measures with unknown validity/reliability	5 of 6
4. Participants not blind to study purposes	4 of 6
5. Small sample sizes	5 of 6
6. Violations of statistical assumptions	? of 6[b]
7. Narrow sample compositions	6 of 6
8. Convenience (vs. population-based) sample	6 of 6
9. Potential recruiter/selection bias and demand characteristics	6 of 6
10. Fails to differentiate sexual behavior, attraction, and orientation	6 of 6
11. Fails to differentiate sexual orientation from sexual identity	6 of 6
12. Fails to assess for bisexuality	6 of 6

NOTE. [a]Studies assessed are Bell, Weinberg, & Hammersmith, 1981; Freud & Blanchard, 1983; McCord, McCord, & Thurber, 1962; Peters & Cantrell, 1991; Siegelman, 1981; and Townes, Ferguson, & Gillam, 1976. [b]None of these studies presented data that would enable the reader to evaluate whether these assumptions were met, so the prevalence of this problem cannot be ascertained.

Psychological Practice with Lesbian, Gay, and Bisexual Clients

A Review of the American Psychological Association's 2012 Guidelines

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Abstract

The American Psychological Association recently published an update of its *Guidelines for Psychological Practice with Lesbian, Gay, and Bisexual Clients* (APA, 2012). In this critical review, I find much to commend about this document but also express concern with what appears to be the influence of ideology guideline content and presentation. Five examples of this ideological shaping are addressed in this review: (1) the treatment of religion, (2) sexual orientation change, (3) nontraditional relationships, (4) gay parenting, and (5) use of research.

Psychological Practice with Lesbian, Gay, and Bisexual Clients: A Review of the American Psychological Association's 2012 Guidelines

The American Psychological Association (APA) recently released its latest update of *Guidelines for Psychological Practice with Lesbian, Gay, and Bisexual Clients* (APA, 2012; hereafter referred to as *Guidelines*). It is an important document because it represents the latest recommendations of the APA (of which I am a member) to psychologists for professional practice with lesbian, gay, and bisexual (LGB) clients.

It is critical to understand that *guidelines* are different from *standards*—guidelines are aspirational, whereas standards are mandatory and can be accompanied by an enforcement mechanism (including disciplinary measures for noncompliance). This is an important distinction for NARTH member psychologists who are also APA members, because were these guidelines offered as standards, some aspects of those standards might place NARTH psychologists at risk of ethical censure, as will be noted below. That said, there is much that NARTH clinicians can learn from the *Guidelines*, and every mental health professional who works with LGB clients should be familiar with them.

The *Guidelines* begin with a preamble that provides a helpful definition of terms. This is followed by twenty-one specific guidelines, each of which includes a rationale and recommendations for application. On the positive side, the *Guidelines* provide useful reviews of some of the literature that can assist clinicians in being helpful when working with LGB clients, including clients with unwanted same-sex attractions. First, the *Guidelines* bring attention to the many stressors impacting LGB clients and the importance of assessing for these (see Guidelines 1, 5, 10, and 11). The *Guidelines* urge psychologists to create a safe environment for these clients, which is a common factor in any beneficial psychotherapy (Guideline 1).

Second, the *Guidelines* remind psychologists that they should fully assess the motives of clients requesting to change their sexual orientation and that they should

guard against any use of coercion in their treatment, particularly with youth (Guideline 3).

Third, the *Guidelines* encourage psychologists to be aware of their own values, beliefs, and limitations (Guideline 4).

Fourth, the *Guidelines* recommend psychologists to be aware of how family of origin, culture/ethnicity, age, socioeconomic status, and disability status might impact the presentation and treatment of LGB clients (Guidelines 10, 11, 13, 15, and 17, respectively).

Finally, psychologists are encouraged to be aware and respectful of diverse religious and spiritual practices (Guideline 12).

While NARTH members might quibble some about certain aspects of how these recommendations were derived and applied by the APA, the general issues addressed above should nevertheless be considered components of good practice for anyone who works with LGB clients.

Although there is valuable information in the *Guidelines*, NARTH members and others who practice sexual orientation change efforts (SOCE) will need to be discerning as they review this document. There is ample evidence that the authors approached their task from an almost exclusively gay identity-affirming position, which shaped their presentation of the science. I personally do not have a problem with this, since all of us have values and worldviews that impact how we approach the literature; I only wish that the APA would have been honest about its own worldview in the context of such an important document. Five examples of this ideological shaping will have to suffice for the present review: (1) the treatment of religion, (2) sexual orientation change, (3) nontraditional relationships, (4) gay parenting, and (5) use of research.

Treatment of religion. The *Guidelines*, following in the footsteps of the APA task force's report on sexual orientation change efforts (SOCE; APA, 2009), does make an effort to deal more substantively with the religious values of LGB clients, particularly in the context of the pursuit of SOCE (Guidelines 3 and 12). On the positive side, APA

concedes that for some clients, religious affiliation and identity will be prioritized above sexual orientation, and an affirmative approach will therefore be incompatible with the goals of these clients.

Unfortunately, the *Guidelines* provide little if any help for assisting such clients if they wish to pursue their heterosexual potential or even a chaste lifestyle. At several points in the *Guidelines* these issues are addressed, and the general thrust of this guidance is to provide these clients with every reason and opportunity to revise their religious beliefs so as to embrace an LGB identity. Wherever religious resources are recommended to these clients (including an entire appendix), the only recommended organizations are those committed to assisting individuals in affirming an LGB identity—for example, Soul Force, DignityUSA, and the Metropolitan Community Church.

Moreover, attributions provided to explain these nonaffirming religious beliefs are seen only through the lens of stigma, such as the internalization of heterosexist norms. This raises the question as to whether any nonaffirming, non-stigma-based identity can exist for the APA among clients with unwanted same-sex attractions. It also brings into question whether respectful and sensitively conducted values-based referrals can be made by therapists when client goals conflict with therapist values, a topic the *Guidelines* fails to discuss.

Sexual orientation change. Probably the largest inaccuracies in the *Guidelines* appear in the APA's treatment of SOCE. In Guideline 3, the APA asserts that the SOCE literature is too methodologically flawed to seriously consider yet, then it proceeds to conclude on this basis that SOCE is ineffective. How a therapeutic approach lacks credible studies to evaluate its efficacy can then be definitively said to be ineffective defies explanation. This highlights the fine line the APA appears to be trying to walk—without success, in my view—wherein it dismisses the credibility and relevance of existing SOCE literature while at the same time preserves the notion that this literature gives us some unambiguous reason for discouraging the practice of SOCE. Not surprisingly, a similar

tension appears in the task force report (APA, 2009), which makes equivalent contradictory statements, dismissing SOCE for lacking efficacy while maintaining that the relevant literature is so methodologically flawed that we cannot determine if SOCE is effective. In addition, Guideline 3 states that the APA cannot recommend SOCE but then recommends an exclusively gay affirmative therapeutic approach—which, as the 2009 task force report stated, also has no empirical support of the kind it demands for SOCE.

Nontraditional relationships. Another feature of the *Guidelines* worth mentioning is its treatment of what are termed *nontraditional relationship structures*. Guidelines 5 and 7 specifically address such relationships when dealing with, respectively, bisexuality and LGB relationships. Guideline 5 observes, "Bisexual individuals may be more likely than lesbian or gay persons to be in a nonmonogamous relationship and to view polyamory as an ideal, although there are many bisexual people who desire and sustain monogamous relationships" (p. 7). Guideline 7 includes the statement that "Nonmonogamous or polyamorous relationships may be more common and more acceptable among gay men and bisexual individuals than is typical for lesbians or heterosexuals" (p. 8). The *Guidelines* then make a critical analysis of nonmonogamy and polyamory exceedingly difficult by implying that this would constitute discriminatory practice: "It is useful for psychologists to be aware of the diversity of these relationships and refrain from applying a heterosexist model when working with lesbian, gay, and bisexual couples" (p. 9). It appears that such nontraditional relationships among gay men and bisexuals are insulated from moral or other evaluative critique through labeling such critiques as prejudicial and discriminatory practices.

Same-sex parenting. A brief guideline (Guideline 8) addresses the issue of LGB couples who are raising children. While understanding the experiences and challenges of LGB parents is a sound recommendation, the *Guidelines* base this on the conclusion that LGB parents are as capable as heterosexual parents. In fact, according to Guideline 8, lesbian parents are superior to heterosexual parents in several areas. Although lesbians

can be good mothers and gay men can be good fathers, it is far from clear to this reviewer whether lesbians can make good "fathers" or gay men can function as good "mothers." This guideline relies heavily on the methodologically limited gay parenting literature, a fact recently detailed in an important critique by Marks (2012) concerning the APA's brief (2005) on lesbian and gay parenting.

Thus, while the *Guidelines* contend that there are no major differences in well-being between children raised by lesbian parents and those raised by heterosexual parents, Marks observes that the same-sex parenting literature used in the APA brief relies on small, nonrepresentative, homogeneous samples of privileged lesbian mothers. In light of this, he concludes, "This pattern across three decades of research raises questions regarding lack of representativeness and diversity in the same-sex parenting studies" (p. 739). This concern was voiced at the time by Meezan and Rauch (2005), two gay men who favor same-sex marriage yet acknowledged, "What the research does not yet show is whether the children studied are typical of the general population of children raised by gay and lesbian couples" (p. 97). It appears the APA's treatment of this topic has lost all appearances of a circumspect science and has crossed over into the realm of an advocacy agenda in search of data.

Use of research. The final guideline (Guideline 21) encourages psychologists to eliminate bias in interpreting and disseminating research findings and to take into account the limitations and complexities of the LGB research literature. While representations of this literature should strive to be fair to the data, multiple interpretations should be expected when the subject matter has significant sociopolitical implications that are being debated within the culture. The *Guidelines* appear to violate these cautions regarding potential misuse and misrepresentation of research findings.

To cite only two examples, the *Guidelines* reference the Hooker (1957) study as support for the hypotheses of no differences between gay and heterosexual men and cite the Shidlo and Schroeder (2002) research as confirming the potential harm of SOCE.

Both studies possess the types of methodological problems that the APA task force (2009) found sufficient to dismiss the entire body of SOCE literature and that render generalizing beyond the study sample inappropriate. Nevertheless, these two studies are broadly cited in the *Guidelines* without qualification or context, suggesting a partisan application, whether purposeful or not. Also of note is the fact that of the 239 references cited in NARTH's recently published practice guidelines (2010), only 23 are included among the 518 references listed in the APA's *Guidelines.* This highlights how important it is for those wanting to be educated on the subject to be familiar with multiple perspectives, providing a sufficiently wide grasp of the relevant professional literature.

In summary, the APA *Guidelines* are an important resource with which mental health professionals who work in this area should be familiar. It does not, however, give an account of the relevant issues and literature that is unaffected by latent ideological bias, as evidenced in how topics such as religion and SOCE and are addressed and what literature is selected or omitted from the discussion. Clinicians wishing to be broadly educated on practice issues with LGB clients may begin with these *Guidelines* but certainly cannot afford to end there. These clinicians therefore need to become familiar with additional resources such as NARTH's (2010) practice guidelines.

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Book Review of Janelle M. Hallman's

The Heart of Female Same-Sex Attraction: A Comprehensive Counseling Resource

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Review of Janelle M. Hallman's *The Heart of Female Same Sex Attraction: A Comprehensive Counseling Resource*

Janelle Hallman's *The Heart of Female Same Sex Attraction* (2008) is a bold work in a time when the very notion of the right to therapeutic self-determination for individuals dissatisfied with their same-sex attraction and behavior (SSA) is under attack. Hallman philosophically and practically demonstrates the importance of privileging the client's story and agenda. Between the lines of the text is her consistent claim to clients' rights to make their own choices and to walk their chosen therapeutic path. Further, she offers an alternative to practices that limit the client's agenda based on the therapist's personal values.

Hallman states that the book "is primarily intended for mental health professionals, educators, and pastoral-care counselors who are interested in clinical perspective on the issue" (p.12). Additionally, she recommends the book—especially chapters 3 through 5—for friends, family members, and women experiencing conflict with SSA.

Hallman is the first to critique her own book as she identifies possible limitations of her clinical perspectives and insights, specifically as they relate to the demographics and motivations of the women addressed in her book. Her clients are 25 to 55 years old, which eliminates adolescents and traditional college-aged women. Her clients are also dissatisfied with their same-sex feelings and behaviors and want to bring their experiences in line with their values. But because Hallman undergirds her work with mainstream philosophical principles and practices of psychotherapy, we believe other clinicians can generalize her insights and practices to SSA women from various backgrounds.

The text is organized in two main parts that are preceded by a prologue. Traditionally, readers often skip over the prologue in their rush to get to the "meat" of a book. In this case, however, the prologue is essential for understanding Hallman's foundational respect and valuing of her clients and her passion for helping this unique group of women. Hallman

pursued her education and credentialing specifically so she could help women with unwanted SSA. Further adding to her distinct perspective, Hallman holds an orthodox Judeo-Christian view of human sexuality. She explains that her view of the connection of gender and sexual identity is illustrative of humankind's reflection of God's image.

Part one (chapters 1–5) provides a background for Hallman's approach to helping women in conflict with their SSA by introducing ethical and philosophical considerations. A review of Hallman's approach counters the claims of those who characterize therapists willing to work with individuals with unwanted SSA as unethical or unorthodox. Her approach to psychotherapy is philosophically and strategically reflective of mainstream psychodynamic and humanistic therapies.

Chapter 1 reveals the importance of a respectful and nurturing relationship between therapist and client. It includes a detailed description of the need for safety and trust, which involves a woman's right to choose how she defines herself sexually. This can be difficult for some pastoral counselors, but Hallman identifies as essential the construction of a safe place where there is unconditional freedom to grow and heal. This is a crucial point: Unless a woman can freely choose (or not choose) the lesbian lifestyle without judgment by her therapist, her choice is not freely made. Many of the women that we as reviewers and that Hallman have worked with come from family backgrounds of rigidity and constraint. A therapist's predetermined treatment goal of renouncing or promoting acquiescence to SSA only replicates what many have experienced in their families of origin, and such treatment may cause additional injury. We agree wholeheartedly with Hallman: Practitioners must hold the outcome of therapy lightly, fully respecting the client's right to self-determination.

This first chapter also includes a discussion of the controversy over change versus immutability, including a description of sexual fluidity as not being synonymous with changeability. The importance of grasping the difference between fluidity and changeability is essential for women who may feel victimized and blamed for something over which

they have little control. Hallman's discussion underscores the idea that the factors causing a woman to change her attraction are complex.

Chapter 1 continues with a discussion of the ethics of treatment by addressing the issue of whether an attempt to change can actually cause damage. Hallman carefully highlights research on the topic indicating the positive impact of sexual orientation change-effort (SOCE) therapy; she also offers a bulleted list reminding the reader of standards of ethical practice (p. 31). It should be noted that this list of ethical practice standards applies to the practice of most therapists and their therapeutic goals with most clients, regardless of a client's presenting concerns. The research Hallman presents clearly indicates that with standard ethical practice and sufficient sensitivity to the needs of the woman she is likely to experience benefits, regardless of the degree of change in attraction.

Chapter 2 builds on the first chapter's discussion of ethical practice by emphasizing the importance of a therapist's professional competency. This chapter highlights Hallman's knowledge and insights related to working with this unique population that can come only through extensive experience. Hallman lists the empathic concerns that a therapist must fully grasp when working with these women, who commonly experience a depth of loneliness and shame, live in fear of themselves, sense more love and acceptance from gay or nonreligious friends than from religious friends and family, seem to lack the freedom to talk, and perceive an expectation to change "overnight."

Hallman cautions therapists against using a stance of "professional" detachment, arguing that it may be detrimental to the therapeutic relationship. She contends that the client, but not the therapist, may detach. This application of a traditional object relations approach continues throughout the book. Hallman includes several developmental trajectories of SSA as viewed from an object relations perspective. The use of orthodox psychotherapeutic approaches further communicates her concern for ethical practice, reminding the reader that ethical SOCE therapy is simply the professional application of standard therapeutic strategies.

Chapters 3 to 5 provide a good introduction to the developmental issues surrounding SSA, so we suggest that our clients focus on these three chapters. Chapter 3 examines the nature versus nurture issue and states that "who we are directly affects how we perceive and process our worlds" (p. 51). The author discusses the significance of biology in determining SSA, describing these components as hormonal, neurological, genetic, or inborn personality characteristics or traits. Using existing research, Hallman comes to the same conclusion as the American Psychological Association (2008, p. 2), explaining that biology and environment each influence sexual orientation but that the exact role of biology is inconclusive.

Addressing biological influences, Hallman lists the common personality characteristics she has observed in women with SSA (pp. 54–55):

- Above-average intelligence
- Profound sensitivity and attunement to other people and relational dynamics
- Curiosity and sharp observation
- Gender-nonconforming abilities and interests
- Innate sense of justice
- Talent and far-reaching creativity
- High level of energy
- Adventurousness
- Athleticism

Concerning the influence of nurture, chapter 3 describes four developmental processes that are often compromised in women with SSA: attachment, formation of the self, gender identity, and socialization. Hallman states, "Typical in the history of women with SSA are interferences, stressors or failures in their most primal attachments, often arising at birth and continuing throughout childhood" (p. 57). Examples of the events that cause

a daughter's defensive detachment include birth and postnatal complications, maternal deficits, abuse, or abandonment. Hallman has significantly broadened the classic "reasons why" and taken much of the blame off mothers for the development of SSA, which we consider a major contribution of her work. The author's insight and compassion allow her to see beyond typical explanations and help us to see with new eyes the complex and delicate dance of attunement between mother and daughter. The chapter's discussion of the interplay between biology and environment is accessible to clients, and reading this chapter often leads to further self-acceptance and reduction of shame.

Chapter 4 continues to address the developmental losses of women struggling with SSA. Hallman clearly but respectfully describes a girl lost in confusion who has settled for a re-created self rather than her true self. Hallman lists a variety of losses and disappointments that may be added to the young woman's gender nonconformity, creating even greater confusion. These losses may come from an over-identification with her father, trauma, sexual abuse, too few childhood girlfriends, same-sex admiration, the shock of puberty, a disparaging self and body image, and disappointing or negative encounters with young men. Such experiences may leave the young woman vulnerable, and she may eventually attempt to compensate by developing dependent relationships with other women.

While chapter 4 sets the stage, chapter 5 continues the story as the young woman begins "looking for home" (p. 98). Hallman's simple way of describing emotional dependency in women within same-sex relationships is to say that the woman is "depending on you for me" (p. 98). Hallman presents the complications of emotional dependency using the voices of the women she has worked with, and her respectful insight into this dilemma clarifies what she and we believe to be the core of female SSA.

Part two (chapters 6–12) continues to emphasize the author's collaborative approach and the priority she gives to the client's agenda. Hallman successfully balances the tension between psychodynamic developmental themes, profiles, and techniques with

the call to see each woman as unique. The therapeutic practices of empathizing, valuing, and being genuine with the client also give evidence of a traditional humanistic approach.

Hallman's consistent use of orthodox practice is important. Some popular and academic media propagate misinformation about SOCE therapy, presenting it as a unique set of unorthodox practices. Part one clearly refutes this view as Hallman lays a foundation of ethical, mainstream therapeutic philosophies; Part two builds on this foundation, describing a therapeutic approach grounded in the traditions of classical psychotherapies.

Chapter 6 identifies four stages of therapy: formation, transformation, integration, and consolidation/maturity. Hallman contends that therapeutic attachment, which involves building trust and empathy, is not a one-time event but happens repeatedly throughout the therapeutic process. Unconditional acceptance is a theme of this chapter, particularly as it relates to the client's same-sex feelings and behaviors, her same-sex partner, and her goals for therapy. This modeling of acceptance is essential for helping the client become able to practice "radical self-acceptance" (p. 124).

Chapter 7 describes the process of attunement and attachment with clarity and grace. The detailed explanations present the intellectual as well as emotional process for both therapist and client. Hallman expresses the clear view that acceptance of the client includes acceptance of her survival strategies. This expression of the head-heart connection resonates with us as the ideal example of what it is like to do this work.

Chapter 8 describes a variety of personality profiles of women struggling with SSA that provide a starting place for the practitioner without pigeonholing the client into a single "type" of personality. Without exception, our clients have identified with a combination of profiles. If the book had been longer, it may have been helpful to increase the number of profiles presented.

Chapter 9 underscores Hallman's practice of concurrently providing nurture and challenge to her clients. She presents the importance of the additive structure of validating techniques and posture while simultaneously providing insight into identifying and dis-

mantling the negative core beliefs. This chapter clearly paints the picture of a child devoid of feeling, blaming herself and moving into self-hatred because basic needs went unmet. Hallman describes the need to help clients face the false self and give up defenses—such as self-defeating and self-protective behaviors—allowing them to receive from others and experience the fulfillment of their unmet needs. She includes a detailed description of the progression of defensive attachment commonly found in women with SSA.

In chapter 10, Hallman highlights some of the challenges clients face, such as identifying and facing infatuation while staying present in relationships. She describes teaching clients how to make friends with women while understanding and dismantling the motivations behind the sexualization of these friendships. She walks the reader through the client's process of ending enmeshment, beginning differentiation, and putting to rest the need to caretake. Hallman states that this is a time for the client to

> openly discuss her struggle with God and general ambivalence and confusion with respect to her future and future choices. . . . She will need the freedom to openly and frankly discuss her same-sex longings and her specific same-sex attraction and behaviors. . . . At this stage of therapy my client may need the freedom to integrate and openly proclaim a lesbian identity, even as she makes decisions about ending a present relationship or foregoing a new relationship. This does not dismay me. (p. 210)

We are glad that she states this so strongly. Allowing the client the right to self-determination is crucial to the woman's individuation process.

Chapter 11 explores transference and counter-transference and focuses the therapist's experiences. Hallman lists common feeling states of therapists who are experiencing counter-transference, including severe anxiety, helplessness, inadequacy, defensiveness, anger, feeling guarded or violated, a fear of engulfment, and exhaustion. We suspect that

therapists who are successful with this population may be more sensitively wired and especially attuned to their clients. If so, it may be necessary to restrict the number of SSA clients that a therapist sees in order to avoid "burnout."

Chapter 11 continues by addressing the client's experience of transference, projection, and regression. If a client expresses romantic feelings or attraction to the therapist, Hallman coaches the therapist to show no shock or surprise, to validate the client's feelings, to reassure the client that the relationship will not terminate, and to assure the client that the therapist will maintain appropriate boundaries. Hallman follows this with a warning to avoid sexual involvement with the client. While this should be assumed, we have had more than one client present with a history of betrayal through seduction by a female therapist. Overall, the client's experience of therapeutic attachment typically leaves her with new vulnerabilities for which she needs both protection and nurturing.

In chapter 12, Hallman suggests a myriad of ways to affirm the feminine and challenge the misogyny within. With her usual theme of patience and respect, she discusses the splitting or burying of the feminine identity and the hatred of the feminine that many of her clients experience. She cautions that this may be a significantly threatening process for many clients and encourages therapists to understand that some clients will never accept the challenge of embracing the feminine. At the same time, Hallman does not soft sell the harm experienced by women who maintain and even nurture this self-hatred. She challenges misogynist presuppositions that her clients bring to the conversation and in her final argument ultimately returns to her view of woman as God's image-bearer.

Chapter 13 wraps up the book with a discussion of closure by first asking the question, "What about men?" Echoing the consistent theme of respect for the client's process and the acknowledgment of each woman's unique history and agenda, the author warns that not all women's therapy will result in heterosexual attraction or even the desire to pursue relationships with men. The core of the chapter identifies specific challenges

her clients often have in relating to men. Hallman finishes the chapter by offering strategies for successful termination.

As stated earlier, Hallman includes in the book's intended audience women in conflict with their SSA, their friends, and their family members. However, many of our clients who have read the whole book describe feeling overwhelmed by it and indicate their belief that therapists are the targeted audience. As a result of that fairly universal reaction, we now generally recommend that they read only the more client-friendly chapters 3 through 5.

Hallman's inclusion of the Theophostic prayer ministry approach (p. 255) is out of place in this otherwise theoretically well-grounded book. She mentions the approach in passing and without explanation as a method for dealing with trauma. Though this approach has not been researched adequately, it is included in the same sentence with the well-researched EMDR (Eye Movement Desensitization and Reprocessing) method, suggesting that they share similar credibility.

This book was well named—*The Heart of Female Same-Sex Attraction*. While providing much information, explanation, and direction, the most important aspect—the heart of these women—is captured with authenticity and clarity. The book is a beautiful treatise on how to work with any client who is reluctant, passive, defensive, or avoidant. Hallman shows clinicians how to navigate the difficulties of the initial relationship, build trust, and move forward with dignity and purpose.

A significant element that sets this book apart from others is the use of the women's own voices and the knowledge Hallman brings as a therapist who has in-depth experience working with this population. The dialogues, explanations, and understanding that come from Hallman's insight and discussion of her clients are invaluable. Hallman's respect for the dignity of each woman and her profound understanding of the human condition makes this book a work of art.

While there are more resources available now for guiding therapists who work with conflicted SSA clients than there were just ten years ago, many of these resources

are primarily, and often exclusively, focused on helping men. Others address only limited aspects of the therapeutic process. Janelle Hallman's *The Heart of Female Same Sex Attraction: A Comprehensive Counseling Resource* is unique as a single-authored, comprehensive resource for therapists who want to understand developmental issues and therapeutic strategies for helping women who experience SSA.

References

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Book Review of Simon LeVay's

Gay, Straight and the Reason Why

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Summary: This book reports on a very good survey of the possible biological/prenatal/neurohormonal origins of SSA (same-sex attraction) and a small but inadequate survey on OSA (opposite-sex attraction). While it contains a large number of new insights and useful interpretations, some of the points need significant modification in view of recent literature. The book presents a rather confused picture of whether social/psychological factors have any importance, and its discussion of them is far too superficial. It opts for most SSA being caused by prenatal hormones, a conclusion not consistent with the much replicated results of twin studies, which show all prenatal factors are only a minor influence. That twin studies conclusion is a maximum estimate not likely to change. Prenatal origin for sexual orientation is now not the majority view of researchers, who assign a prime role to postnatal factors. Contrary to the position suggested in the book, the brain is not strongly gendered at birth. One section suggests that limited change in sexual orientation is possible. The book discusses the increasing doubts about the "elder brother hypothesis" (i.e., the more elder brothers the greater the possibility of SSA), but overvalues the biological explanations. Overall the book is complex and the confusing results from many experimental programs trying to explain SSA by biological factors suggest there are too many layers of hypothesis.

Book Review of Simon LeVay's *Gay, Straight and the Reason Why*

Gay, Straight and the Reason Why by Simon LeVay (O.U.P. 2011) is about sexual orientation, both gay and straight. It asserts the origin of both is the same: prenatal and hormonal. While both sexual orientations are discussed, the book concentrates on same-sex attraction (homosexuality); the explicit discussion of opposite-sex attraction (heterosexuality) is quite thin.

Author and neuroanatomist Simon LeVay is best known for his 1991 claim that the INAH3 nucleus size in the hypothalamus of brains of homosexual men is significantly more like that of women (LeVay, 1991). He has also written several books dealing with the science about SSA.

Published by Oxford University Press, the book appears to be scientifically careful. It cannot discuss papers dated after its 2010 publication, however, so please bear that in mind for the other literature cited in this review.

Gay, Straight and the Reason Why demonstrates clearly what a prereviewer on the back cover calls LeVay's "mastery of the literature." It provides an excellent discussion of the biological literature. While I do not agree with many of the biological arguments because of results described in later papers, I have never come across a text with so many stimulating new ideas—so despite my disagreement, I felt the book was well worth the purchase price. Contrary to a back cover reviewer's comment about its "balance," however, I found it surprisingly unbalanced; it is weak on the social side and misses many explanations that would occur instantly not only to clinicians but to general social scientists.

There are 295 pages of text, but a glossary, chapter references, and bibliography extend the book to 412 pages; like many academic books, the actual text ends well before expected. Of the actual text, only 16 pages are a superficial survey of social factors. Since LeVay concentrates on the science, so will I.

The Prenatal Neurohormonal Theory

LeVay's basic goal seems to be to present evidence for the neurohormonal, prenatal theory of homosexuality (and, of course, heterosexuality).

I should immediately say that because the theme of the book is sexual attraction, the overall framework of sex and reproduction is assumed but neglected. Yet that framework is ultimately needed. Most novels present life as mainly about sex, with little about the bringing up of children. Real life is the opposite!

The neurohormonal theory dates back at least fifty years (Phoenix, Goy, Gerall, & Young, 1959), but LeVay's book can be regarded as a good update. LeVay strongly endorses:

- "the idea that the origins of sexual orientation are to be sought in the interaction between sex hormones and the developing brain" (p. xi).
- "a chain of causal events that leads from genes to sexual differentiated mental or behavioural traits" (p. 50).
- "we should not expect to identify a specific cause behind every individual's sexual orientation beyond a general attribution to the prenatal hormonal mechanisms that I've been discussing" (p. 283).
- "I'm inclined to place most of the developmental control in the hands of prenatal hormones" (p. 279).

(Incidentally, how tentative that last comment is!)

The basic theory has been that the brain is sexually organized prenatally and intensely—mainly by hormones, particularly testosterone. The idea is that the created neuronal structure is similar to computer hardware, and that nothing can change its organization until puberty. The theory states that a child is born with a highly gendered brain but that the preferred gender is mainly invisible. The theory further states that sexual orientation is not

fully developed until puberty, when the sexual orientation of the brain is activated by high hormone levels, somewhat like throwing a switch. Proponents of the theory maintain that homosexuality is a prenatal disturbance in this process of sexual orientation development.

In the past, proponents have not been clear about the status of newborns because they should show no gender differentiation at all; it should be latent until activated at puberty. However, claimed gender differences at birth have been published quite often and have tended to be taken as some evidence that strongly gendered brain structure exists from earliest days. Some postnatal influence is seldom completely rejected in the book, which throws up the possibility that some sexual orientation is learned. LeVay reflects this ambivalence. On the one hand, he assigns a majority of influence to prenatal hormones. On the other, he allows for other (presumably minor) influences, unlike many other authors.

"The Reason Why" part of the title really refers to a single general unified theory of biological origins for sexual orientation, one that he thinks predominates over all others. However:

- "Is this preference 'organized' by sex hormones during development? The answer is yes—to a degree" (p. 56). (This quote is rather remarkable for its tentative air.)
- A diagram (p. 64) is labelled as a "prenatal hormonal theory," but it includes the possibility of (presumably minor) idiosyncratic influences and environmental influences that affect sexual orientation.

The belief that the prenatal hormonal theory is predominant implies at least a semiquantitative assessment of the strength of its influence, but no such assessment is found in the book. Perhaps LeVay might ultimately be reluctantly open to the idea that various psychological and social influences combined could be even stronger than genetic influences.

Innately Gendered Brain?

LeVay's original 1991 statement that homosexual male brains were more feminine than usual was not confirmed in a subsequent study by Byne (Byne et al., 2001); knowing that readers may be familiar with that issue, LeVay devotes a small section to it. He comments that although Byne did not find statistical significance to confirm that notion, he did find a trend, so Byne's study is not really a refutation. LeVay points out that parallel findings were found in sheep; however, such studies are notoriously irreproducible, as illustrated in the work he describes by Swaab and others on a hypothalamus region called INAH1, very near the INAH3 region he himself studied. A claimed sex dimorphism in size could not be confirmed by three laboratories, but it seems Swaab still believes it is sexually dimorphic. Because of known plasticity of the brain, I believe it is almost certain that any reproducible results in altered brain structure or neural networks will turn out to be the result of sexual/mental activity connected with sexual behaviour.

Levay is aware of and endorses the fifteen-year-old argument that "brain organisation cannot be genetically specified in precise detail" (p. 60) because 22,000 genes cannot specify 100 billion neural connections—there are far, far too many. LeVay does not explore the concept that most brain connections are formed in reaction to the environmental experiences undergone by the young child, so social influences should predominate.

LeVay believes that the adult brain is highly gendered. I agree that there are numerous male–female differences for adult subjects, but the perennial question is whether these are inborn or developed under the strong influence of maternal/paternal interaction, sexual experiences, and other factors. The idea that the new brain is highly gendered has almost no support; it certainly is not strongly anatomically dimorphic, but LeVay cites one of the rare established differences: Girls pay more attention to faces, and boys pay more attention to things. Even in this statistical difference there is a lot of overlap between the genders. The newborn brain is far less sexually dimorphic than the genitals by any criterion.

A salutary tale about gender differences is the historic idea that female newborns are much more sensitive to touch than are newborn males. Some authors cited studies from the last fifty years that showed complete sexual dimorphism, with results from the two genders not even overlapping. The most careful study—because it was blind—showed there was no difference at all (Jacklin, Snow, & Maccoby, 1981), and touch is now rarely cited as an example of gender differences. But this shows the strong human urge to demonstrate sexual dimorphism and the degree of self-deception that can result.

The following paragraphs show that sexual dimorphism in the brain, even at the biochemical level, is far less than commonly believed.

Recent Brain Research

LeVay and other authors state that most influences on sexual development are only prenatal and only hormonally driven. It's important to understand that such ideas are not currently accepted: "Our current knowledge of sex-based neurobiology has outgrown this simplistic model. Multiple lines of research have contributed to this conclusion" (Reinius, 2011, p. 15). LeVay either does not mention this literature or could not comment because it was more recent than his manuscript. But an extraordinary amount of work has been published, much drawing on genome studies of various animal tissues.

The perspective that is emerging is extremely interesting but its detail is beyond the scope of this book review; a review paper is in preparation. Following is a very condensed summary of experimental work done on various animals (mice and rats, unless otherwise noted):

1. Perhaps 50% of fetal brain biochemical sexual dimorphism is independent of sex hormones (Dewing, Shi, Horvath, & Vilain, 2003; Lee et al., 2009 [chickens]; Sreenivasan et al., 2008 [zebrafish]; Reinius, 2001 [humans]). The authors describe the idea of obligatory association of sexual dimorphism with prenatal hormones as a "dogma."

2. Sexual dimorphism in the brain and subsequent sexual behavior just after birth is sensitive to prenatal stress (Mychasiuk, Gibb, & Kolb, 2012)—called here an "epigenetic" (environmental) effect.

3. Extraordinarily, the brain, hypothalamus, and gonads are far less sexualized tissue when compared with liver and adipose tissue (Yang et al., 2006; Gregg, Zhang, Butler, Haig, & Dulac, 2010; Hadziselimovic, Hadziselimovic, Demougin, & Oakeley, 2011 [humans]; van Nas et al., 2009).

4. Most development of the brain is postnatal. There are growth and pruning cycles that are strongly influenced by the environment, including hormonal cycles that cause sexually dimorphic pruning (Kauffman, 2009; Martin et al., 2010; Hisasue, Seney, Immerman, & Forger, 2010; Semaan & Kauffman, 2010; Hines, 2011 [humans]). Some of the authors say that the *majority* (my emphasis) of known sex differences are induced by the sex steroid milieu during early postnatal development (as contrasted with prenatal influence only). This applies to laboratory animals, with social conditions held very constant and, I assert, presumably to humans.

5. The human brain changes biochemically in a sexually dimorphic way at the moment of birth and again at age twenty (in other words, at adulthood) but surprisingly not much at puberty (Colantuoni et al., 2011; Kang et al., 2011). One interpretation of this may be that we have overestimated the changes associated with sexual maturation.

6. In my view, recent observation of some transgender individuals who rapidly alternate their perceived gender (Case & Ramachandran, 2012) makes it unlikely that this perception is innate or tied strongly to neuronal pathways .

While the literature is complicated and will demand several changes in our concepts, it is shifting more and more in the direction that prenatal influences are less important than postnatal influences. This means that the view LeVay prefers—that sexual orientation is rather rigidly fixed before birth—is becoming less and less mainstream. He does not seem closed to fresh evidence, however, and some of the statements in his book seem couched to cover some of the recent possibilities.

Possible Social/Psychological Theories

LeVay spends a chapter explaining why social influences are inadequate and a biological theory ought to be investigated instead. In that chapter he mentions only three influences: psychoanalytical ideas (such as those of Freud), learning (the first sex act is very important), and gender learning.

Contemporaries Socarides and Nicolosi are quoted in this chapter, but their views are examined rather superficially. Nicolosi's recent work is not mentioned, nor is there much exploration of other views, including the considerable literature on effects of social factors on gender development. While he admits that the factor exists, LeVay attacks the idea of the father being important in the development of SSA among men. Again he seems to imagine that prenatal factors are strong and other factors are weak. I believe that *all* the factors are weak, but some postnatal factors are very important to some individuals.

One of the postnatal factors that can be important for some is sexual abuse. LeVay doesn't think that sexual abuse has much of a link to adult SSA, and there are indeed a few studies that have failed to show a link. Unfortunately, LeVay's citations are quite inadequate in this area, which could be due mainly to the time frame of publishing the book. Important studies showing such links include Arreola, Neilands, and Diaz (2009); Austin et al. (2008); Cutajar et al. (2010); Rosario, Schrimshaw, and Hunter (2009); Rothman, Exner, and Baughman (2011); Tomeo, Templer, Anderson, and Kotler (2001);

Wilson and Widom (2010); and Zietsch et al. (2012). I doubt any sex researcher today would seriously query the link, although its strength is open to debate.

In support of LeVay's idea that sexual orientation is not learned, he quotes the famous case of David Reimer, who was born XY but who was brought up as a girl because a medical error resulted in him losing his penis when he was very young. Contrary to upbringing, he ultimately settled for a male identity. Unlike most authors, LeVay goes on to cite the less-known case where a boy who had lost his penis was brought up as a girl and developed a stable feminine gender identity but is bisexual and currently in a relationship with a woman. From these two cases—along with cloacal exstrophy, or intersex, cases—that "nature" usually trumps "nurture" (p. 40). This is a poor summary of the literature.

In one far larger study in which penises were lost very early, 69% elected to remain female; in another study, 75% elected female identity (Meyer-Bahlburg, 2005). But telling patients they were fundamentally XY did make a difference, and some preferred to shift to male—a culturally favored gender. If anything, the bulk evidence here is that nurture trumps nature. Though the Reimer case received massive media attention, it is simply not typical.

To support the idea that learning has a negligible effect on sexual orientation, LeVay writes: "Children raised by gay parents don't differ in sexual orientation . . . from those raised by straight parents" (p. 40). But I find the critiques of that argument convincing (Schumm, 2008; Schumm, 2010; Schumm, 2011). Those children *do* differ, and there is some effect on their sexual orientation. Parental influence is not overwhelming but neither are prenatal influences.

Twin Studies

LeVay considers twin studies as evidence that there is at least some genetic influence on SSA, but even the sources he cites show that prenatal influences are not predominant.

The results of the eight major twin studies (many unreferenced in the book) varied. Even those that used twin registers or good, large random samples and that are the

least biased so far had different results. Some showed no genetic or prenatal influence on SSA, while some showed significant genetic/prenatal influence—but a mean of the first seven studies (Whitehead, 2011) gave a weak to modest 23% for males and 37% for females. (These should not be confused with the pairwise concordances mentioned below.) LeVay quotes a 30 to 50% figure relying on fewer studies, but genetic influence is still not predominant in that estimate—and for reasons given elsewhere in extensive detail (Whitehead, 2011), all the twin studies' genetic estimates are almost certainly much too high for adults and are zero for adolescents. LeVay fails to include or discuss any of the numerous papers in the literature that suggest this. A weak to modest influence is much more likely, and a majority influence of prenatal factors is probably impossible.

These studies sum up all the influences known and yet to be found, so there is no way we can say that prenatal common factors are predominant, or will ever be found to be. The error on the prenatal influence estimate is still relatively high. In contrast, the nonshared environmental factors gave 63% for both males and females and the error was relatively small. This leaves room for little else. These idiosyncratic factors predominate. (Heterosexual origin cannot be studied this way because of mathematical difficulties.)

LeVay does mention that in twin studies of SSA, the nonshared environment factor is significant (see p. 167). While this statement is correct, it is a little misleading. As shown above, not only is it consistently significant (and in many studies the genetic component is not), but it is consistently dominant. However, because there is so much interest in the genetic influence, this fact tends to get generally ignored in the wider literature.

On page 282, LeVay cites roughly even odds that if one member of a monozygotic twin pair is gay, the monozygotic cotwin will also be gay. At best this is misleading. LeVay may be citing the probandwise concordance for monzygotic twins, and this is the type of technically defined nonintuitive concordance essential in further calculations of genetic influence and other factors. But as an illustration for nonspecialists, it is much more helpful to give the pairwise figures. In the present case, taking Bailey et al. (2000)

as an example, if one male identical twin has SSA, only 11% of his cotwins will have the same attraction. For a female, the figure is 14%.

The landmark paper using traditional classic twin study methodology (Bailey, Dunne, & Martin, 2000)—and the one usually cited—did not find any genetic influence on SSA. Instead of that paper, LeVay cited another paper (Kirk, Bailey, Dunne, & Martin, 2000) that used an untried, unreplicated, and rather arbitrary measure of sexual orientation and was about trying different measurement models. It gave a result as high as 60% genetic influence for one of the models. It is worrisome that results are so model-dependent. At best, this selective literature evaluation demanded explanation by LeVay in a footnote. (For further, more detailed discussion, I propose consulting Whitehead, 2011.)

Diversity

LeVay thinks the diversity of gay people has not been studied enough. I agree, but one very recent result on diversity in sexual orientation is a little thought-provoking. That study (Savin-Williams, Joyner, & Rieger, 2012) used five categories—exclusively heterosexual, mostly heterosexual, bisexual, mostly homosexual, and exclusively homosexual—to classify the large sample of young adults from the ongoing ADD Health Study in the United States. As shown in Figure 1, the authors found the percentage of males who classified themselves as "mostly heterosexual" to be about 3% of those who classified themselves as "exclusively heterosexual." However, those who classified themselves as "mostly homosexual" were 55% of those who classified themselves "exclusively homosexual," which is much larger than the 3% for the heterosexual comparison. Although the authors do not examine the issue in detail, the diversity of orientation is many times stronger for the homosexuals compared with the heterosexuals. The same general results are found for the women. One could argue that the same factor is creating greater diversity in the homosexual groups and less in the heterosexual groups—that is, a drive to heterosexuality!

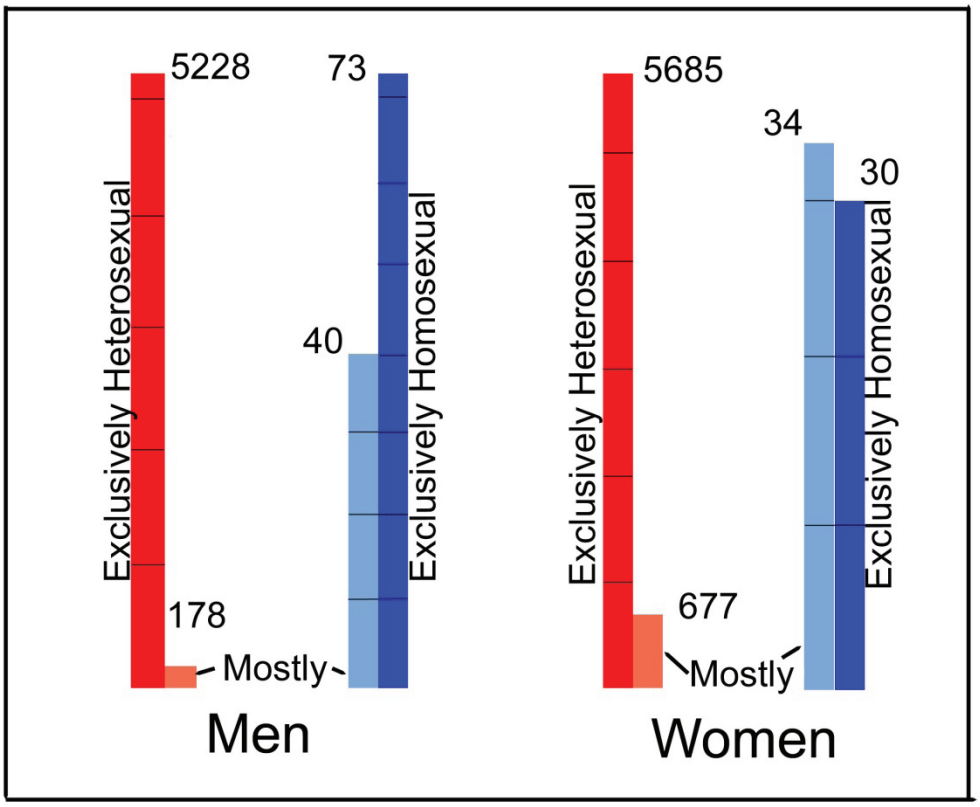

Figure 1. Relative concentration of subjects in the "Exclusive Heterosexual" class. Bisexual class not graphed. Numbers of subjects in a category are shown, and note that each group of two bars has its own independent numerical scale.

LeVay mentions that Bailey's research team could not verify the existence of bisexuals using phallometric techniques. However, a more recent study by Bailey using the same technique came to a different conclusion (Rosenthal, Sylva, Safron, & Bailey, 2011). People with responses to two different erotic stimuli—heterosexual and homosexual—did seem to exist. This was further confirmed recently by a different technique that involved measuring the length of time subjects paid attention to pictures of each gender (Ebsworth & Lalumiere, 2012). If bisexual people exist, there could be slow movement through the bisexual category to exclusive heterosexuality. If bisexual people do not exist, then movement toward heterosexuality would be impossible.

Mental Rotation Tests

Mental rotation tests—a rather pure test of brain function—are often used as an indicator of relative masculinity/femininity. LeVay argues that male-type mental rotation, in which boys score higher than girls, is seen as early as three months, which he thinks is too early for socialization effects. He therefore thinks mental rotation must be innate and related to prenatal testosterone exposure. I assert that such an effect is possibly influenced by the known early *post*natal testosterone surge.

More recently, this has been directly tested by analyzing amniotic fluid for testosterone, then checking mental rotation ability well after birth (Auyeung et al., 2011). There was no correlation. On the other hand, results of an embedded figures test (which measures attention to detail) in both boys and girls well into childhood *was* predicted by fetal testosterone. One might have predicted the girls would do better, but the opposite was true. This is a complicated issue and does not support the simple hypothesis of a prenatal hormone influence predictably organizing the gender of the brain.

LeVay also discusses the way that girls with congenital adrenal hyperplasia (CAH) who experience extreme levels of prenatal androgens are shifted in many of their behaviours toward the masculine side (p. 79). Considering the extreme levels of androgen involved, the real surprise for me is how small the effects actually are. He is disconcerted that these girls only show typical feminine values for the mental rotation test rather than more masculine values. However, they are better at the targeting visual test, which would be more typical of boys. He also mentions that the mental rotation test on CAH boys actually showed worse performance than on unexposed boys, an unexpected result. He introduces yet another level of hypothesis to explain this, saying that perhaps they are not exposed to androgens to the degree supposed. However, it is known that the adrenals are active in fetal development from weeks 10 to 13, before the development of the male genitalia and testosterone surge at weeks 14 to 20, so there should be adequate androgen exposure.

Do Elder Brothers Make You Gay?

The elder brother hypothesis derives from a long series of papers—particularly by Blanchard and Bogaert in Canada—showing that the greater number of elder brothers in a family, the more likely it is that a later-born male will be SSA. LeVay is not completely uncritical of these and cites a number of studies by other groups that are now failing to find elder brother influence. Another more recent study was done by Zietsch and colleagues (Zietsch et al., 2012). One of the best studies mentioned by LeVay (Frisch & Hviid, 2006) involving a very large Danish sample (2 million people!) did not find an elder brother influence but, contrary to the hypothesis, found an elder sister effect, which was also noted in a few other studies.

LeVay correctly points out that elder brother data depend on family size. The most important source for this emphasis is a paper by one research group that found an elder brother effect, only to have it vanish when family size was factored in (Langevin, Langevin, & Curnoe, 2007; see also the subsequent discussion with Blanchard in the same journal). LeVay does not cite this debate, and it is far more important than most people think. It may mean much of the literature on the elder brother hypothesis needs to be reevaluated, and this is now impossible because maximum family sizes are usually not available from historic census data.

However, LeVay concludes that if there is an elder brother effect, it "does not work by broadly feminizing the brain development of late born sons" (p. 269). I suspect further work will support that.

I personally think a simple psychological explanation of the supposed elder brother effect is rejection or rebelliousness toward the overbearing behavior of a set of elder brothers. Either one joins them, imitates them in the best way possible, and heads for the opposite-sex attraction (OSA) world, or the younger brother rejects their masculine modelling and starts on a path toward SSA.

The elder brother hypothesis is usually explained (including by LeVay) in terms

of the maternal immune hypothesis, in which a male fetus creates an immunological reaction by the mother, who does not have Y-chromosome-specified proteins. The hypothesis is that the mother's immune system could attack the brains of subsequent male fetuses, impairing the masculinization of their brains and causing them to develop SSA. This condition is also supposed to cause lower birth weight.

I have pointed out elsewhere (Whitehead, 2007), as have others (Zietsch et al., 2012), that the discordance of identical twins for SSA makes the scenario of immunological attack very unlikely. However, LeVay mentions an important Danish paper (Nielsen, Mortenson, Schor, Christiansen, & Andersen, 2008) that at least shows that the maternal immunological attack on male fetuses is real, but confirms doubts in my mind about whether this is relevant to sexuality. The study, based on about 350,000 births from the Danish medical records, found lesser birth weight for later males, consistent with the hypothesis.

There were some conflicts with a similar previous Norwegian study of 180,000 births (Magnus, Berg, & Bjerkedal, 1985) that concluded flatly that the maternal immune hypothesis was untenable (Whitehead, 2007). However, the much more recent Danish study is larger, more thorough, and better controlled, and the effect on birth weight is likely to be real. While it may be real, the birth weight effect is quite small and the conflict between the studies shows the classic difficulty of looking for a very small effect in the presence of many other factors that may easily overwhelm it.

Both studies found that birth weights of *girls* as well as boys were affected by the previous birth of a brother. This is contrary to the hypothesis, which argues that only subsequent *boys* should be affected; the Danish researchers think this is due to a lessening of specificity of immunology with time (another layer of hypothesis). It must be a large decrease of immunological specificity indeed if the mother's immune system can now attack either sex, but such a decrease calls into serious question whether the effect is still strong and specific enough to attack male brains and cause homosexuality.

The Danish group (Nielsen et al., 2010) posited further evidence of an "anti-male" antibody: when recurrent miscarriages occurred after a first successful birth of a boy, the sex of a successfully born later child was 83% likely to be female. The Danish researchers have followed this group of mothers for a long time—some of them since 1986—and an obvious test should determine whether there is increased prevalence of SSA in any second-born males in this group.

From a Danish study (Frisch & Hviid, 2006), LeVay fails to point out the many social correlates with SSA or "homosexual marriage"—older mothers, divorced parents, absent same-sex parents, and being the only or youngest child. Each younger sibling decreased the chance of homosexual marriage in the older sibling by 9.2% for men and 13% for women, which cannot be a prenatal effect. Frisch and Hviid concluded that whatever other factors might be involved, parental interactions were important. The same study also presents various effects on heterosexual orientation as measured by heterosexual marriage. Similar to genetic effect influence, most social effect influences were modest in size.

Blanchard/Bogaert introduced an extra layer of hypothesis in which left/right-handedness is a modifying factor as to whether a subsequent son is SSA. LeVay is dubious; so am I. Blanchard has very recently published yet another independent layer of hypothesis (this one also including lesbians) to try to explain SSA in firstborns (Blanchard, 2012). I'm even more dubious about this hypothesis and await some replication.

Bodily Differences

LeVay's book has a very good section reviewing the physical body differences in straight/SSA people, and it is now established that there are some statistical differences. Male gays are slightly shorter and lighter than other males, and lesbians have longer arm-to-body ratios than the average for women. Again this is statistical only, but could in principle be some evidence for a biological origin of sexual orientation. However, for me it raises the question of how far self-image actually arises from the bodily properties. Does a male smaller than

average find himself not well suited to sports, think himself less masculine, suffer ridicule, and tend to SSA? Does a woman with more masculine finger ratios and longer arms think of herself as less feminine? Such questions need to be answered. And how much of a factor is a conventional gender-appropriate bodily shape in pushing us toward heterosexuality?

Genes

LeVay mentions the studies trying to find genetic linkages and scans of the whole genome to find SSA-related genes. I believe these are unlikely to succeed. The perspective of the last ten years is that linkage studies to genes for any trait are notoriously, indeed embarrassingly, difficult, with numerous incorrect publications that are discredited when a whole genome scan is done—at which point entirely new and unsuspected candidate genes are revealed. The rule of thumb is now that many or very many genes are involved, each with very small contributions. There is a genetic contribution for many things, and I expect that this will ultimately be shown for SSA, but the task of establishing it is logistically immense, possibly demanding analyses on 100,000 people as it did for genetic studies on schizophrenia. It may show that some specific genes are involved, but to put a numerical figure on the extent of the combined influence, we will still need twin studies—and we already know from existing twin studies that the influence is minor.

I suspect many genes each have a small influence, but this makes it hard to explain why family studies give such erratic results for the appearance of SSA. A trait dependent on many genes will vary slowly with the generations, as *g* ("IQ") does. Many genes, each with small effects, should produce overall a bell-shaped distribution of sexual orientation, which is the opposite of what we find—sexual orientation has a J-shaped distribution. Aware of this difficulty, LeVay inserts yet another layer of hypothesis to explain why sexual orientation is channelled into two streams. According to LeVay, this demands prenatal organization of the brain into a kind of masculine and feminine channel, or at least awareness of the two genders. I find this proposition that something like Jungian archetypes are in the brain interesting and

novel but speculative; I also find it contradicted by the quite slow development of awareness of gender in young children rather than full gender recognition from birth.

Culture

LeVay hypothesizes there is a basic stratum of homosexuality in culture having a biological origin. But he agrees homosexuality is expressed in rather wildly varying ways in different cultures and eras. He does not mention that such diversity should usually mean that genetic influence is rather weak. Genetically caused homosexuality would be tightly circumscribed in expression and change little over centuries. He does acknowledge that modern patterns of SSA relationships between adults were historically rare. But if the predominant historic pattern has been pederastic, would this mean any worldwide genetic pattern causing SSA was also most "naturally" pederastic?

LeVay states, "My conclusion . . . [is] that sexual orientation is indeed a fairly stable aspect of human nature, and that straight, gay, and bisexual people have existed across many, perhaps all cultures" (p. xii). This is misleading because it implies SSA is stable. One can indeed say the statement about stability is true of heterosexuality, but it is far less true of homosexuality. This is shown to greatest extent in a study (Savin-Williams & Ream, 2007) in which sixteen-year-olds who were OSA were almost 100% still the same way a year later, but those initially SSA had overwhelmingly changed to OSA. From these data, and confining the time period to adolescence, OSA is at least 25 times as stable as SSA. A lifelong figure would be not so extreme, and a rule of thumb would be about 15 times the stability. This does not argue for SSA stability.

The cultural history also shows large change of homosexual customs, sometimes within one generation. This does not demonstrate SSA stability, nor does it argue for likely genetic influence.

LeVay points out the huge shift in family patterns over the last few centuries, and gives an argument why possible homosexual genes could have become more prevalent.

But selective breeding experiments with animals require about a dozen generations to change behavior profoundly, and under LeVay's hypothesis, an extraordinarily selective human breeding pattern would be needed to produce a change in sexual orientation. It is highly doubtful that his mechanism can be correct for the timescale quoted. We must also remember that with today's smaller family sizes, there would be far fewer elder brothers than in the past!

Animal Homosexuality

LeVay also surveys animal homosexuality, finding that "homosexuality in the sense of a durable preference for same-sex partners, has not been widely described among non-human animals" (p. 69). Rather, there is what he describes as "broad bisexual potential." Homosexuality to him seems like a "second-best choice" for the animal, saying that "male-male partnerships often break up if females become available" (p. 67). He correctly points out that the parallels with human homosexuality are often exaggerated but he thinks longer-term preference is seen in domestic sheep and reports on brain changes observed in them that apparently correlate with SSA.

From my point of view, however, it is similarly a question of how far these brain changes are due to the psychosexually artificial conditions of farming and whether the brains have changed in the short term in response to these environments. I would also note the relative lack of mention of reproduction as a theoretical framework; this, rather than sexual orientation itself, is overwhelmingly important for animals.

How far can human and animal sexuality be compared in any case? The richness of human sexuality and family life makes comparing it with sex in animals like comparing a love sonnet with the grunt of a pig.

Is Change Possible?

Given his adherence to a strong prenatal theory of sexual orientation origins, what does LeVay think about the possibility of change in humans? Only homosexuality is dis-

cussed: "The majority viewpoint among mental-health professionals is that this so-called *conversion therapy* has little chance of success and can cause significant harm by reinforcing the gay person's negative self-image" (p. 12). No in-depth references are given for this, and such in-depth support does not exist.

LeVay quotes the Spitzer study and agrees that "at least a few highly motivated gay people can be helped to engage in and derive some degree of pleasure from heterosexual relationships and to pay less attention to their homosexual feelings. This should hardly come as a surprise, since we know that many gay people were heterosexually married and had children before coming out as gay" (p. 13). This simple and reasonable conclusion from the Spitzer study seems to at last be getting some unbiased attention! It is pretty refreshing these days to find an evaluation that is not totally blinkered. But he thinks therapy to stop gay self-hatred is more useful.

He cites literature in which some rare medical conditions seem to have caused a change in sexual orientation and would not rule out others being found. This is a much better approach than the ideological one that change is *a priori* impossible. I suppose it makes sense even from a strongly essentialist point of view that if the essence—the biological hardware—changes, very possibly the sexual orientation might also. He agrees that "sexual partner preference can change" (p. 286), at least under some conditions.

Although LeVay's book is about the influence of prenatal hormones, this section does not directly comment on whether such a link makes any change more difficult. The fact that the section is present at all suggests there is such a link in LeVay's mind and probably in the reader's mind as well. But the outcome seems to be a qualified agreement that the influence of the prenatal hormones is not absolute, and perhaps some forms of therapy might even have some effect under some conditions.

As to whether choice could be a factor in homosexuality, LeVay quotes a survey from the *Advocate* in the mid 90s that only 4% of gay men and 15% of lesbians said that choice had anything to do with why they were gay (p. 41). Such a survey sample is very

biased (as is any sample from clinical experience) but seems numerically to be in the right region for an estimate of "choice." This is because unbiased Gallup polls show that 90% of GLB people currently believe they were born that way, which leaves little room for a large choice element. The latter, rather than the *Advocate* survey, is probably the best available simple evidence that choice is a minor factor in the origin of SSA.

Trying Too Hard?

I think the reader of this book or any book dealing with this subject will be rather overwhelmed by the complexity of the subject. There are multiple aspects, multiple theories, and lack of consistency in experimental results and surveys. I think the general welter of inconsistent results is a mark of a discipline that is not yet mature. I believe there is a lesson to be learned from the gene-linkage surveys (not just for SSA) that appeared to be so exciting but were often totally wrong. I see also the piling up of multiple hypotheses, particularly in the field of the elder brother effect. There is some truth there, but I think the whole subject will ultimately have to be reconstructed more cautiously. Multiple hypotheses are fun, but more than two layers of them is stepping out onto the abyss. We need to wait until some of the findings are explained without (please) yet more layers of hypothesis.

LeVay's conclusion—"We [homosexual people] should be valued, celebrated, and welcomed into society rather than merely being tolerated" (p. 298)—is one of the few such "activist" comments, and the tone of the book is overwhelmingly academic.

Conclusions

The subtitle of the book seems to imply basically the same principle for development of homosexuality and heterosexuality—prenatal exposure to greater or lesser amounts of hormones. Although these have some effect, so do a multitude of other factors, and it is time to abandon a single-origin hypothesis. Rather there are multiple reasons for SSA that

include biological randomness, body shape, prenatal stress, early maternal interaction, early postnatal hormone effects, sibling interaction, sexual abuse, media influences, mental/fantasy life, early sexual experiences, paternal influence, and nonstandard masculine or feminine interests. Since some of these impact a few individuals to an unusually strong degree, there is no substitute for individualized clinical analysis. This may far better reveal "the reason why" one is gay or straight. Hopefully the mix of causes means that surveys of large populations can be summarized profitably by the sociologists.

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IFTC Intervention at 2011 OSCE Human Dimension Implementation Meeting

To: The Organization for Security and Co-operation in Europe (OSCE): 2011 Review Conference Warsaw

From: Dr. Philip M. Sutton, PhD, International Federation for Therapeutic Choice (IFTC), USA

Date: October 4, 2011: *Working Session 13*

Regarding: *Tolerance and Nondiscrimination II*

Intolerance and Discrimination Against Medical and Mental Health

Professionals, Researchers, and Educators Threaten the Freedoms of the Professionals and Those Whom They Serve

This intervention is being given on behalf of the International Federation for Therapeutic Choice (IFTC). The IFTC supports the rights of sexual minorities who have unwanted attractions, orientation, behavioral tendencies, behaviors, and/or identity to receive competent professional guidance and therapeutic care. The IFTC also supports the rights of medical and mental health professionals to offer that care (www.therapeutic-choice.org).

Central Recommendation to Participating States of the OSCE:

To draft legislation to safeguard the freedom of medical and mental health practitioners, educators, and researchers:

1. To study, publish, and educate other professionals and the public about the possible causes, consequences, and amelioration of sexual minority attractions, behaviors, orientations, and identities; and

2. To offer professional guidance and therapeutic expertise to persons whose sexual minority behaviors, orientations, and/or identities are unwanted and who freely choose help in order to overcome or diminish their unwanted sexual attractions and behaviors.

Some sexual minorities find their attractions, behavioral tendencies, behaviors, and/or identity *unwanted*. Some of these people *freely choose* or have *freely chosen* to seek professional guidance and therapeutic assistance to avoid basing their relational and sexual lives on their sexual minority attractions, behaviors, orientations, and/or identifications. More than one hundred years of clinical reports and other research literature document that *some* people have been successful in achieving this goal. I refer to the first volume of the *Journal of Human Sexuality*, which reviews the clinical and scientific literature on this issue (http://www.narth.com/docs/journalsummary.html).

Medical and mental health professionals who research, educate, and offer guidance and therapeutic services to people with unwanted sexual minority concerns are experiencing increasing intolerance and discrimination. When they attempt to train for and conduct their work, such professionals are commonly labeled as "homophobic" and are even accused of hate crimes. This intolerance and discrimination likewise hinders the freedom of people who want to receive the information and other services of these professionals.
I offer several examples:

- In 2010, United Kingdom psychiatrist Paul Miller was accused of unethical behavior for offering to help someone change same-sex feelings and behaviors. The person seeking help proved to be an undercover, self-identified gay journalist who lied about his true intentions.
- In 2011, this same UK journalist again posed as someone who wanted help to resolve unwanted same-sex attractions and behaviors, this time from Christian counselor Lesley Pilkington. As in the first case, after a couple of sessions, the fraudulent client/undercover journalist accused Ms. Pilkington of unethical practice. This led to her being dismissed from the British Association for Counseling and Psychotherapy (BACP), although her case is under appeal. It is worth noting that during her hearing, a key witness in her support was threatened by homosexual activists, causing the hearing to be postponed.
- In 2011, activists in Poznan, Poland, interfered with and tried to prevent a conference that had been publicized as offering training to help professionals understand how to better serve people with unwanted same-sex attractions and behaviors. Public media reported absurd accusations by gay activists that led the Poznan Medical School to cancel the written contract for the use of the conference facilities. The presenter—Dr. Joseph Nicolosi, PhD—was falsely accused of many things, including teaching fake pseudoscience, forcing people to undergo therapy,

using electric shock therapy, and forcing homosexuals to have sex with female prostitutes. Unfortunately, the negative publicity generated by these false accusations led a second conference facility to break its verbal agreement to allow the conference to be held at its site. The conference was finally held in a sports facility under tight security. The Internet provider of the organization conducting the conference received two letters demanding that the provider close down the organization's conference website due to "homophobic content." The Internet provider was subsequently hacked and the entire server crashed, impacting not just the conference organizer but all of the provider's clients.

These examples illustrate just a few of many recent instances of harassment, intolerance, and discrimination toward medical and mental health professionals, researchers, and educators who attempt to serve people with unwanted sexual minority attractions, behavioral tendencies, behaviors, and/or identities.

Such intolerant behavior by people who themselve*s* claim to be victims of intolerance violates a number of rights upheld by the Universal Declaration of Human Rights (UDHR, http://www.un.org/en/documents/udhr/index.shtml#a11), including the rights to:

- freedom for the full development of one's human personality (UDHR, Article 26)
- medical care and necessary social services (UDHR, Article 25)
- freedom of thought, conscience, and religion (UDHR, Article 18)
- freedom of opinion and expression, which includes the freedom to hold opinions without interference and to seek, receive, and impart information and ideas through any media (UDHR, Article 19)
- freedom of peaceful assembly and association (UDHR, Article 20)
- the protection of the law against arbitrary interference with one's privacy, family, or correspondence and attacks on one's honor and reputation (UDHR, Article 12)

We therefore recommend to OSCE Participating States:

In light the aforementioned fundamental rights upheld by the Universal Declaration of Human Rights:

To recognize and condemn intolerance and discrimination against sexual minorities who freely choose to receive help in order to overcome or diminish their unwanted sexual attractions, orientation, behaviors, and/or identity.

To draft legislation to safeguard the freedom of medical and mental health practitioners, educators, and researchers: 1) to study, publish, and educate other professionals and the public about the possible causes, consequences, and amelioration of sexual minority attractions, orientations, behaviors, and/or identities; and 2) to offer their professional guidance and therapeutic expertise to people whose sexual minority concerns are *unwanted* and who *freely* choose help in order to overcome or diminish their unwanted sexual attractions, orientation, behaviors, and/or identity.

We recommend to OSCE/ODIHR and OSCE Missions:

To be aware of and condemn intolerance and discrimination against sexual minorities who freely choose help in order to overcome or diminish their unwanted sexual attractions, orientation, behaviors, and/or identity.

To assist OSCE Participating States in monitoring and drafting legislation, with special attention to safeguarding the above-mentioned rights upheld by the UDHR.

NARTH Statement on Sexual Orientation Change

Approved by the NARTH Board of Directors on January 25, 2012

Current discussions of sexual orientation change among homosexuals are unavoidably occurring within a sociopolitical climate that makes nonpartisan scientific inquiry of this subject very difficult. In light of this reality, a few considerations are crucial for accurately understanding the sometimes contradictory opinions regarding the possibility of sexual orientation change.

First and foremost, it is important to recognize that how change is conceptualized has vast implications for our thinking about change. Some of the more ardent proponents and opponents of sexual orientation change among homosexuals may view change in strictly categorical terms, where change is an all-or-nothing experience. Proponents and opponents of this view differ only in the direction of their desired outcome. Proponents of change understood in categorical terms may view homosexual sexual orientation as a lifestyle choice that merely needs to be renounced. Opponents who take this viewpoint, on the other hand, may conceive of sexual orientation as something that is hard-wired and simply not modifiable. NARTH does not support either of these perspectives.

NARTH believes that much of the expressed pessimism regarding sexual orientation change is a consequence of individuals intentionally or inadvertently adopting a categorical conceptualization of change. When change is viewed in absolute terms, any future experience of same-sex attraction (or any other challenge)—however fleeting or diminished—is considered a refutation of change. Such assertions likely reflect an underlying categorical view of change, probably grounded in an essentialist view of homosexual sexual orientation that assumes same-sex attractions are the natural and immutable essence of a person.

The delegitimizing of change solely on the basis of a categorical view of change is virtually unparalleled for any challenge in the psychiatric literature. For example, applying a categorical standard for change would mean that any subsequent reappearance of

depressive mood following treatment for depression should be viewed as an invalidation of significant and genuine change, no matter how infrequently those depressive symptoms occur or how diminished their intensity. Similar arguments could be made for any number of conditions, including grief, alcoholism, or marital distress. The point is not to equate these conditions with homosexuality but rather to highlight the inconsistency of applying the categorical standard only to reported changes in unwanted same-sex attractions.

Rather than pigeonholing sexual orientation change among homosexuals into categorical terms, NARTH believes that it is far more helpful and accurate to conceptualize such change as occurring on a continuum. This is in fact how sexual orientation is defined in most modern research, starting with the well-known Kinsey scales, even as subsequent findings pertinent to change are often described in categorical terms. NARTH affirms that some individuals who seek care for unwanted same-sex attractions do report categorical change of sexual orientation. Moreover, NARTH acknowledges that others have reported no change. However, the experience of NARTH clinicians suggests that the majority of individuals who report unwanted same-sex attractions and pursue psychological care will be best served by conceptualizing change as occurring on a continuum, with many being able to achieve sustained, satisfying, and meaningful shifts in the direction and intensity of their sexual attractions, fantasy, and arousal. NARTH believes that a profound disservice is done to those with unwanted same-sex attractions by characterizing such shifts in sexual attractions as a denial of their authentic (and gay) personhood or a change in identity labeling alone. Attempts to invalidate all reports of such shifts by presuming they are not grounded in actual experience insults the integrity of these individuals and posits wishful thinking on an untenably massive scale.

Finally, it also needs to be observed that reports on the potential for sexual orientation change may be unduly pessimistic based on the confounding factor of type of intervention. Most of the recent research on sexual orientation change among homosexuals has focused on religiously mediated outcomes, which may differ significantly

from outcomes derived through professional psychological care. It is not unreasonable to anticipate that the probability of change would be greater with informed psychotherapeutic care, although definitive answers to this question await further research. NARTH remains highly interested in conducting such research, pursuant only to the acquisition of sufficient funding.

To summarize, those who are highly pessimistic regarding change in sexual orientation appear to have assumed a categorical view of change, which is neither in keeping with how sexual orientation has been defined in the literature nor with how change is conceptualized for nearly all other psychological challenges. NARTH believes that viewing change as occurring on a continuum is a preferable therapeutic approach and more likely to create realistic expectancies among consumers of change-oriented intervention. With this in mind, NARTH remains committed to protecting the rights of clients with unwanted same-sex attractions to pursue change as well as the rights of clinicians to provide such psychological care.

(Retrieve at http://narth.com/2012/01/narth-statement-on-sexual-orientation-change/)

TO: California State Senate, Standing Committee on Business, Professions and Economic Development

April 23, 2012

The National Association for Research and Therapy of Homosexuality (NARTH) wishes to be on record as objecting to SB 1172 and strongly recommending that this bill not be passed out of committee. NARTH is a professional, scientific organization whose members include fully qualified academics and therapists who are fully licensed professionals and who abide by high standards of ethical care. NARTH supports the freedom of individuals to claim a gay identity or to explore their unwanted attractions and make changes in their lives. NARTH objects to this bill for the following reasons:

1. SB 1172 inaccurately represents the science on sexual orientation change efforts (SOCE).

SB 1172 makes serious errors in its representation of both the issue of change in sexual orientation and in the likelihood of harm from efforts to change. SB 1172 references the report by the American Psychological Association's task force on Appropriate Therapeutic Responses to Sexual Orientation (2009). When the task force committee was being formed, NARTH and others submitted the names of highly esteemed professionals who either practice or were sympathetic to the informed and professional provision of SOCE. However, none of these individuals was appointed to this committee, which ended up being comprised of professionals who essentially were in ideological lock-step with one another in their preconceived notions regarding SOCE. In NARTH's view, this limits the scientific authority of the task force document. However, even with this highly restricted range of viewpoints, the task force's statements related to change of sexual orientation and the resulting harm seem to be ignored by the crafters of SB 1172 in several important ways.

First, SB 1172 presents the issues of change and harm in a partisan manner. In Section 1(c) (and again in Section 865.1[b]), the bill states that "there is no evidence that any type of psychotherapy can change a person's sexual orientation." The task force report, however, actually "concluded that there is little in the way of credible evidence that could clarify whether SOCE does or does not work in changing same-sex attractions" (p. 28). Absence of conclusive evidence of effectiveness is not logically equivalent to positive evidence of ineffectiveness. A more accurate statement regarding SOCE's effectiveness based on the task force report would include a statement that there is also not sufficient scientific evidence to conclude that SOCE is not effective and that in the end, the current research allows the sole conclusion that "we simply do not know." We would submit that this omission seriously misrepresents the science on SOCE as presented in SB 1172.

Second, regarding the issue of harm, SB 1172 states that SOCE "may cause serious and lasting harms" (as amended in April 16, 2012 version, section 1 c).[1] While we have no doubt that harm can occur in SOCE—as it can occur in any form of psychotherapy—the task force report's statements about harm rely heavily on a study by Shidlo and Schroeder (2002). The authors of this study make clear what the task force report failed to mention and what SB 1172 therefore neglected: "The data presented in this study do not provide information on the incidence and prevalence of failure, success, harm, help, or ethical violations in conversion therapy" (p. 250). Again, we can say with confidence that some SOCE clients report harm and others report benefit, and we do not know from the scientific literature how often either outcome occurs. To present the issue of harm as done in SB 1172 constitutes a clear failure to provide necessary context and therefore creates an unfair characterization of SOCE.

NARTH believes that the task force employed unrealistically stringent methodological standards in dismissing the research on SOCE in order to make the blanket

1 Section 1 (n) of the 09/05/12 and final version of the law mentions California "protecting its minors against exposure to serious harms caused by sexual orientation change efforts."

conclusion that it is not effective. By these standards, it is quite conceivable that other approaches to psychotherapy currently in practice could be considered ineffective and potentially harmful. Does the committee really wish to become an arbitrator of psychotherapeutic approaches? We would further note that, to its credit, the task force also acknowledged that the gay-affirmative therapeutic approach "has not been evaluated for safety and efficacy" (p. 91) and that research meeting its methodological standards is still needed to establish this. Based on such considerations, we believe it is inappropriate for SB 1172 to single out SOCE for questioning on the grounds of efficacy.

2. SB 1172 would restrict the rights of parents to determine the appropriate psychological care for their minor child and would hinder the ability of adult clients to make informed choices regarding their preferred therapeutic approach.

SB 1172 frequently mentions the necessity for informed consent in clients' pursuit of SOCE. NARTH fully affirms the need for informed consent that provides accurate scientific information leading to autonomous choices by clients regarding the nature of their psychological care. Unfortunately, the informed consent mandated by SB 1172 in Section 865.1(b) of the bill repeats the inaccuracies we noted above concerning what science can currently tell us about SOCE. This incomplete and therefore inaccurate portrayal of the science seems likely to bias consumers against SOCE in a manner not warranted by the relevant literature and may therefore hinder the exercise of free trade within the profession.

NARTH finds particularly egregious the complete ban SB 1172 would place on the availability of SOCE to minors and the accompanying restrictions on parental rights. We affirm that no minor should be subject to a form of psychological care that the minor or his/her legal guardians do not wish to pursue and that great care must be undertaken by mental health professionals providing SOCE to assure that client freedom and autonomy is respected with minors. However, we need to point out that the great majority of co-

ercive experiences of minors purported to have occurred in SOCE—which are almost exclusively anecdotal in nature—took place in religiously based programs with pastoral providers who do not fall under the jurisdiction of this bill. In addition, mechanisms already exist within licensing boards and professional mental health organizations to address unethical behavior or malpractice by licensed clinicians.It is curious to us that the impediments and prohibitions SB 1172 places on consumers of SOCE far exceed the cautions already put into place by the relevant professional associations, which again brings into question the objectivity of those who are lobbying for this bill.

3. SB 1172 represents a usurping of the role of mental health organizations and licensing boards to provide oversight in psychological care.

As alluded to above, NARTH is concerned that SB 1172 transfers the oversight of proper psychological care from mental health professionals and licensing boards into the hands of politicians. In so doing, this bill would unfairly and unethically subvert the purposes of mental health associations and licensing boards and place in the hands of politicians the regulation of professional mental health practices. We believe that such oversight should be the sole purview of professional mental health associations and licensing boards. Such regulation should not be given to legislators who cannot be familiar with the breadth of the science on SOCE and, therefore, are at risk of making laws based on inaccurate or incomplete representations of the science provided by highly partisan activist groups.

The fact that this legislation is solely directed at SOCE should be a red flag suggesting that ideological and political motivations may motivate backers of this legislation as much or more than any concern for consumers derived from the relevant science. It appears that those opposed to the ethical and professional provision of SOCE, having been unable to impose their will on professional organizations and licensing boards, are now attempting an end-around power grab through the legislative process. NARTH believes

this effort, if successful, would set a dangerous precedent for the mental health professions, unjustly restrict client rights, and almost certainly invite legal action.

In summary, NARTH respects each client's dignity, autonomy, and free agency in choosing his or her preferred form of psychological care to address same-sex attractions. We believe that SB 1172 would make for bad law based on its misrepresentation of the science pertaining to SOCE, its potential to unnecessarily restrict client and parental choices, and its assumption of the regulatory functions of mental health associations and licensing boards. We urge committee members who are open to broadening their information base regarding SOCE to visit our website at www.narth.org and to review both our recent statement about SOCE and our Practice Guidelines for the Treatment of Unwanted Same-Sex Behavior and Attractions.

We deeply appreciate your willingness to consider our concerns.

Sincerely on behalf of the NARTH Board of Directors,

Christopher H. Rosik, PhD
NARTH President
Licensed Psychologist PSY10532

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NARTH BULLETIN (May 31, 2012): Spitzer's "Retraction": What Does It Really Mean?

No new scientific finding has discredited the study.

The same arguments originally made for or against it still stand.

Christopher H. Rosik, PhD

A great deal of attention is currently being given to the recent "retraction" by Robert Spitzer, MD, of his important study of sexual orientation change (Spitzer, 2003a). The quotation marks around "retraction" are purposeful, for what has happened should not be characterized as a retraction. While this turn of events has now become a favorite talking point for those opposed to sexual orientation change efforts (SOCE), the language of retraction reflects politically motivated speech rather than scientific analysis. What follows is intended to help those confused by Spitzer's actions and the subsequent media feeding frenzy to understand what has really occurred. I have outlined below some key points that seem to have been lost in the partisan utilization of this turn of events.

1. Spitzer has not retracted his study. The proper term for what Spitzer has done is provided in the title to his recent letter of apology: He has *reassessed* his interpretation (Spitzer, 2012). It appears that he may have originally wished to retract the 2003 study, but the editor of the journal in which the study was published—Kenneth Zucker, PhD—denied that request. Zucker has been quoted regarding his exchange with Spitzer as observing:

> You can retract data incorrectly analyzed; to do that, you publish an erratum. You can retract an article if the data were falsified—or the journal retracts it if the editor knows of it. As I understand it, he's [Spitzer] just saying ten years later that he wants to retract his interpretation of the data. Well, we'd probably have to retract hundreds of scientific papers with regard to interpretation, and we don't do that. (Dreger, 2012)

What Zucker is essentially saying is that there is nothing in the science of the study that warrants retraction, so all that is left for one to change is his interpretation of the findings, which is what Spitzer appears to have done.

2. Spitzer's change of interpretation hinges on his new belief that reports of change in his research were not credible. Instead, he now asserts that participants' accounts of change were "self-deception or outright lying" (Spitzer, 2012). In taking this position, Spitzer has aligned himself with original critics of the study. When the original study was published, peer commentaries about the study had been solicited and were published in the same issue. Among those who questioned the reliability of the self-reports of change were many familiar opponents of SOCE: A. Lee Beckstead, Helena Carlson, Kenneth Cohen, Ritch Savin-Williams, Gregory Herek, Bruce Rind, and Roger Worthington.

3. The case for the credibility of participants' account of change still remains. Remember that nothing about the science of Spitzer's research was flawed. Like all research pursuits, the methodology had limitations, but a reasonable case for accepting the validity of these accounts was made at the time and still stands today. At the time his study was published, Spitzer (2003a) reported that "there was a marked reduction on all change measures. This was not only on the three measures of overt behavior and sexual orientation self-identity . . . but also on the seven variables assessing sexual orientation itself" (p. 410). In addition, 119 of his sample of 200 participants reported achieving "Good Heterosexual Functioning," which was defined in terms of increasing satisfaction in opposite-sex sexual behaviors and decreased same-sex fantasy.

Among the peer commentaries that agreed with Spitzer's original interpretation, Wakefield (2003) noted that "to assume without evidence that reports of changes must be deceptions begs the question of whether change sometimes occurs" (p. 457). Spitzer (2003b) himself responded to the critics by noting:

> Therefore, the critics are correct in claiming that significant response bias could have been present but they certainly have not proved that it was present. They also did not point to anything in the study results that suggests response bias. I acknowledge that some response bias could certainly have occurred, but I find it hard to believe that it can explain all of the reported changes. . . . Surely if bias were present, one would expect that subjects (as well as their spouses) would be motivated to give particularly glowing accounts of marital functioning. They did not. (p. 471)

It is curious that Spitzer's (2012) apology seems to imply that he earlier claimed his research proved the efficacy of SOCE. As was understood at the time, the design of Spizter's study ensured his research would not definitively prove that SOCE can be effective. Certainly it did not prove that all gays and lesbians can change their sexual orientation or that sexual orientation is simply a choice. The fact that some people inappropriately drew such conclusions appears to be a factor in Spitzer's reassessment. Yet the fundamental interpretive question did and still does boil down to one of plausibility: Given the study limitations, is it plausible that some participants in SOCE reported actual change?

In spite of all the recent media hoopla, nothing has really changed regarding the interpretive choice one faces regarding the limitations of self-report in this study. Either all of the accounts across all of the measures of change across participant and spousal reports are self-deceptions and/or deliberate fabrications, or they suggest it is possible that some individuals actually do experience change in the dimensions of sexual orientation. Good people can disagree about which of these interpretive conclusions they favor, but assuredly it is not unscientific or unreasonable to continue to believe that the study supports the plausibility of change.

4. There is an unspoken double standard in the reports of Spitzer's reassessment. The probable influence of political and other nonscientific factors in how Spitzer's

reassessment is being portrayed can be seen in which interpretations of self-report data receive favored notoriety and which are relegated to unfavored exile. Yarhouse (2003) observed this lack of consistency at the time of the study:

> Memory recall of this sort can be unreliable. To be fair, however, much of what we know about LGB experiences, including theories for the etiology of sexual orientation and studies of sexual identity development and synthesis, is based upon retrospective studies utilizing memory recall. Any time proponents of the biological hypothesis for the etiology of homosexuality cite the Bell et al. (1981) study they are referencing a study that utilized retrospective memory recall. The Shidlo and Schroeder (2002) study also relied upon memory recall and is subject to the same criticism. (p. 462)

Spitzer (2003b) had similar observations in defending his findings, implying that demand characteristics could have influenced the self-reports of participants in other related research:

> This study had essentially the same design and a similar recruitment strategy of ex-gay subjects as in the Beckstead (2001) and Shidlo and Schroeder (2002) studies. This raises the question of why so very few of their subjects gave answers consistent with a change in sexual orientation whereas the majority of my subjects did. The possibility of researcher bias must be considered. (p. 471)

A triumphal embrace often accompanies self-report data that suggests harm from SOCE, the equivalence of gay and heterosexual parenting, and other foci that fit with the preferred narrative of gay activists. It is unfortunate but not surprising that reports of sexual-orientation change are subject to unrelenting skepticism while other self-report data, such

as that of Shidlo and Schroeder (2002), seem to be reified as universal fact even though they suffer from similar limitations. If Spitzer's study is to be rejected for its use of self-report data, should not methodologically equivalent research against SOCE receive a similarly skeptical reception? While scientific fairness would seem to demand this, political interests clearly do not.

5. Personal and sociopolitical contexts may provide insights into Spitzer's reassessment. I once spoke briefly with Dr. Spitzer by phone years ago following the publication of his research. He seemed to be a kind and compassionate man who exemplified the spirit of genuine scientific curiosity. No doubt he was grieved that some used his work to make unsupportable claims of SOCE efficacy, and this may have resulted in unfulfilled expectations by some gay and lesbian consumers. Yet it is certainly possible that other needs beyond his concern for human welfare were at play in his apology.

It is hard to imagine the fall from professional grace that Spitzer took due to this study. In a very short period of time, his status within his profession changed from that of a heroic pioneer of gay rights to that of an unwitting mouthpiece for practitioners of SOCE, whom many of his colleagues deem morally reprehensible. Before and after the study was published, Spitzer confirmed that he was receiving a high volume of hate mail and that significant anger was being directed at him (Spitzer, 2003b; Vonholdt, 2000). A decade of being hammered by your friends, your colleagues, and a gay community that once revered you would surely take a toll on any of us.

Spitzer currently suffers from Parkinson's disease and is in the twilight of his life; under these circumstances, he would understandably reflect on what sort of legacy he wants to leave. Hero or villain, icon or pariah—which legacy would anyone prefer to have? I cannot say for sure that these nonscientific considerations influenced Spitzer's decision to "retract" his study, but I can say that it is hard for me to conceive how they would not. Spitzer likely knows infinitely more gay and lesbian persons than he does individuals who report change in sexual orientation. This may have made it difficult for him

to see that in trying to atone for the harm gay men and lesbians in his professional network claimed resulted from the study, he simultaneously caused harm to participants in his study who experienced change and now are told they were deceived or are lying. All of this serves to underscore how personal and subjective the practice of social scientific discourse can be when the subject matter is entangled in a major sociopolitical debate.

Conclusion

A purely scientific approach to the limitations of Spitzer's research would be to conduct more rigorous outcome research, something that he, along with others, has been calling for all along (Jones, Rosik, Williams, & Byrd, 2010; Spitzer, 2003a, 2003b). Even the APA Task Force report on Appropriate Therapeutic Responses to Sexual Orientation (American Psychological Association, 2009) issued a call for such studies to be undertaken. Unfortunately, the reality appears to be that the APA and other institutions in a position to fund and conduct outcome research on SOCE in conjunction with NARTH and other SOCE practitioners have no real interest in doing so. They have nothing to gain by such research, as outcomes unfavorable to SOCE would not meaningfully change their current skepticism, while outcomes favorable to SOCE would be a public relations and public policy disaster for them.

I doubt that Spitzer would "retract" his assessment of the likelihood that needed follow-up studies would be conducted (Spitzer, 2003b):

> Given the cost and complexity of such a study and the current view in the mental health professions of the benefits and risks of reorientation therapy, such a study is not going to happen in the near future. This is unfortunate because of the real questions raised, albeit admittedly not resolved, by this study. (p. 472)

So instead of more and better research on SOCE, we find activists and their supporters in the media pouncing on a change of interpretation in an effort to preempt legitimate scientific debate. Nuance, context, and balanced analysis all be damned: What seems to be foremost is the use of Spitzer's reassessment to bludgeon SOCE supporters into submission and silence. Is it really far-fetched to suspect science is being held hostage to political agendas here?

I sincerely hope that this brief analysis helps clarify what did and did not happen when Spitzer "retracted" his earlier study. No new scientific finding was discovered that discredited SOCE. No egregious methodological flaw was identified. The same arguments forwarded in favor of or against the study a decade ago still stand. Legitimate debate about the study's significance can and should still take place. Nothing has changed except that Spitzer has revised his earlier interpretation for what are likely to be a host of understandable but inherently non-scientific reasons. This is his right, but let no one tell you that in doing so he has discredited his research or alternative interpretations more favorable to those who report change in their same-sex attractions and behavior.

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