

Reply: Study Results Should Not Be Dismissed and Justify Further Research on the Efficacy of Sexual Reorientation Therapy

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The reader of the commentaries will surely be impressed by the wide range of views expressed by the 26 different commentators as to the value of my study. On the one hand, *Yarhouse* says the study “has given a voice to the disenfranchised within a minority group,” *Wakefield* says the study “usefully moves questions about orientation change from the political to the scientific domain and opens them to fresh critical scrutiny, hopefully inaugurating overdue scientific examination of issues currently highly politicized,” and *Klein* says “Spitzer presents face valid evidence that changes in homosexual behavior and feelings of desire and satisfaction can be achieved by some, to varying degrees, via ‘reparative therapy.’” On the other hand, *Cohen* and *Savin-Williams* state that “As scientists, we must disbelieve Spitzer’s data because they are so compromised by subject selection bias as to raise serious objections to any claims Spitzer might make about their meaning and generalizability.” *Wainberg et al.* not only argue that the study was unscientific and harmful but in the title of their commentary (“Science and the Nuremberg Code”) imply that in conducting the study I violated the Nuremberg Code of medical ethics.

I enjoyed—and pretty much agree with—*Strassberg*’s assessment: “Spitzer is to be congratulated on trying to ‘light a candle’ rather than continuing to ‘curse the darkness’ when it comes to trying to understand what happens as a result of reparative therapy. . . . He asked more, and often better, questions of more people who have ‘successfully’ undergone reparative therapy than anyone else. . . . However, the amount of illumination provided by this particular candle does not strike me as quite as bright

as Spitzer seems to believe.” This verdict—that the study has value despite its limitations—is shared as well by several other commentators, such as *Klein*, *Wakefield*, and *Yarhouse*, who have made, at least to me, very cogent arguments in defense of the study. I now attempt to show that despite the study limitations, and my having to now qualify claims made in the paper, the study results are important and should not be dismissed.

TERMINOLOGY

Several commentators criticized me for using the term “reparative therapy” because of its derogatory assumptions. I used the term because the American Psychiatric Association (2000) position statement used it. I now will simply use the term “reorientation therapy.”

I will use the term “ex-gay” to refer to any individual who reports that their sexual orientation was predominantly homosexual but who now considers himself or herself to be heterosexual and attributes this change to having had some kind of reorientation therapy. By definition, such an individual now has a heterosexual identity, but sexual orientation may or may not continue to be predominantly homosexual.

THE STUDY’S RESEARCH QUESTION

The impetus for the study was the American Psychiatric Association’s (2000) “Position Statement on Therapies Focused on Attempts to Change Sexual Orientation,” which stated “APA encourages and supports research . . . to further determine ‘reparative’ therapy’s risks versus its benefits.” The study attempted to contribute to that research by focusing on the possible benefits to some gays of reorientation therapy. The study did not attempt to study possible harm of reorientation therapy (hardly

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controversial) but I certainly did not “dismiss” the issue of harm (*Drescher*). In the introduction to the paper, I referenced seven articles that argued that reorientation therapy is frequently harmful.

In assessing the value of the study, it is essential that one understand that when it was initiated the conventional wisdom in the mental health professions was clear: reorientation therapy can get some gays to identify themselves as “heterosexual” and therefore “ex-gay,” but few if any experience true changes in sexual orientation. This view has been supported by two important studies both authored by researchers critical of reorientation therapy (Beckstead, 2001; Shidlo & Schroeder, 2002). In both studies, ex-gays who claimed to have become “heterosexual” from reorientation therapy were interviewed about their sexual behavior and feelings before the therapy and at the time of the interview. Beckstead (2001) studied 20 ex-gays (18 men and 2 women). He concluded

Participants reported that their sense of peace and contentment did not indicate a change in sexual orientation but a change in self-acceptance, self-identity, focus, and behavioral patterns. No substantial or generalized heterosexual arousal was reported, and participants were not able to modify their tendency to be attracted to their same sex. (p. 103)

In other words, Beckstead claimed that these ex-gays reported that reorientation therapy helped them feel better identifying themselves as “heterosexual,” but not a single one reported changes in sexual orientation. Shidlo and Schroeder (2002) reported that only 8 of their sample of 26 ex-gays gave answers consistent with a change in sexual orientation.

CLARIFYING THE RESEARCH QUESTION

When I started the study and told colleagues about it, I was greeted with anger and disbelief that I would be so foolish as to believe what ex-gays said about themselves. I therefore should have realized that, despite any methodological improvements in assessment of change that I could incorporate into the study (such as the use of a fully structured interview schedule with detailed questions assessing multiple components of sexual orientation), many critics would never accept the subject’s self-reports as credible. In the Discussion section, I said “There is no doubt about what the subjects in the study reported. The key question is judging the credibility of their self-reports.” By my focusing on the issue of subject credibility, I made it easy for the critics to dismiss the study findings by their merely asserting—without proof—that the self-reports *could* be self-deception. I should have,

like Beckstead (2001) and Shidlo and Schroeder (2002), focused on what the subjects *reported*. Instead of the research question “Can some gays change their sexual orientation?” the primary question should have been “Contrary to conventional wisdom, do some ex-gays describe changes in attraction, fantasy, and desire that are consistent with true changes in sexual orientation?” The credibility of the subjects’ self-report, as it is in all treatment efficacy studies that use self-report, becomes an additional issue to be considered.

LISTENING TO WHAT EX-GAYS SAY

The basic assumption of the study is that research on the possible benefit of reorientation therapy must *begin* with listening to what ex-gays say about how they believe they have changed. Listening to what ex-gays say about how they have (or have not) changed is exactly what Beckstead (2001) and Shidlo and Schroeder (2002) did in their studies—the only difference being that, according to them, few if any of their subjects gave answers to their interview questions that indicated a change in sexual orientation, whereas the great majority of the subjects in my study did report major changes in multiple components of sexual orientation.

Critics of my study who question the credibility of my subjects’ self-reports of change should be equally critical of Beckstead and of Shidlo and Schroeder for accepting their subject’s self-reports of no change. It certainly makes no sense for ex-gays to be believed only when they acknowledge that they are still gay. Neither their studies nor mine are the last word, but they both represent reasonable first attempts to assess the possible benefit to some gays of reorientation therapy.

WAS SUBJECT RECRUITMENT BIASED TOWARDS FINDING BENEFIT?

Most of the critics argued that the recruitment strategy and study design were biased towards positive findings: According to *Herek*, “It is difficult to imagine how his recruitment strategy would have yielded anything other than reports of substantial shifts to a heterosexual orientation.” According to *Beckstead*, “It should not be surprising that individuals who identify as ‘ex-gay’ would report positive results when invited to participate in a study designed to support their position.” What *Beckstead* apparently fails to realize is that my study design is the same as his: listening to ex-gays who believe that reorientation therapy works as they answer questions about their sexual feelings,

both at the time of the interview and retrospectively. I fail to see how the study was designed to support the view that reorientation therapy actually changes sexual orientation. Nothing prevented large numbers of the ex-gays who wanted to be in my study from reporting no or only minimal change in various components of sexual orientation, as did all of Beckstead's (2001) 22 subjects and as did 18 of 26 subjects in Shidlo and Schroeder's (2002) study. With my design, I could have had equally negative results.

The astute reader might object to a comparison of my study results with theirs. The entry criteria for my study required at least some minimal report of change in sexual attraction whereas in their studies the only requirement was that the subject claimed to be an ex-gay. However, my actual referrals were the same as theirs: it asked for individuals who claimed to be ex-gays. As noted in the paper, only 18 such referrals were disqualified from the study because they did not meet the additional entry criterion of at least minimal change in sexual attraction. Even if the entry criteria for my study are made equivalent to their studies by *including* these 18 individuals, the majority of the total sample still reported changes in sexual attraction, fantasy, and desire consistent with a change in sexual orientation from predominantly homosexual to predominantly heterosexual.

Several commentators criticized the study design, but significantly, none offered an alternative design. According to *Herek*, "Recognizing that even subtle and unintentional biases can affect the data, researchers routinely adopt elaborate safeguards to prevent their own expectations and those of their research subjects from affecting a study's outcomes. Spitzer's study lacked such safeguards." However, as *Klein* and *Yarhouse* noted, study designs for treatment efficacy studies that have these safeguards, such as masked assessment of subjects randomly assigned to a treatment and to a control group, are clearly not possible when evaluating reorientation therapy (e.g., placing a subject on a waiting list or in a "sham" treatment condition group for several years' duration).

CREDIBILITY OF THE SUBJECTS' SELF-REPORT

Perhaps the major critique of the study is that the critics found my arguments for subject credibility unconvincing. I must (painfully) admit that they are correct in noting that the absence of extreme responses does not insure the absence of self-report bias. Consider a man whose same-sex masturbation fantasies have actually not changed in frequency. He may be able to deceive himself that they

are far less frequent now than before having reorientation therapy but be unable to deceive himself into believing that they are no longer present at all. 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.

None of the critics commented on the interesting finding that the mean score on the self-report measure of marital adjustment for the study subjects was not higher than the mean for the instrument's normative community reference group. Surely if bias were present, one would expect that the subjects (as well as their spouses) would be motivated to give particularly glowing accounts of marital functioning. They did not. There is another finding (not reported in the article) that is not what would be expected if, as suggested by several critics, religious fervor was an important variable predisposing to response bias. The 186 subjects who claimed that religion was "very" or "extremely" important did not report more change in sexual attraction than did the 14 subjects who reported that religion was "not very" or only "somewhat" important.

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. A major advantage of my study is that the reader can know, by looking at the appendix in the paper, exactly what 10 questions were asked to assess sexual orientation. In contrast, in the reports by Beckstead and by Shidlo and Schroeder, the reader has no way of knowing the specific questions that were asked of the subjects that led to the conclusions.

HISTORICAL PARALLELS

Yarhouse noted a historical parallel between my study and Hooker's famous studies conducted in the 1950s. Hooker (1957) studied the question of whether, contrary to conventional wisdom at that time, any homosexuals are without significant psychopathology. Her study did not attempt to find out how often homosexuals are without psychopathology, just that some homosexuals are (Jones & Yarhouse, 2000). My study asked a similar question: Are there some ex-gays that report changes in attraction, fantasy, and desire consistent with a change from a

predominantly homosexual orientation to a predominantly heterosexual orientation following reorientation therapy?

Another historical parallel is that just as many of the study critics assert that my subjects have deceived themselves into believing that their sexual orientation has changed, so, too, in the 1970s the conventional wisdom was that when homosexuals said they were comfortable with their sexual orientation they were also deceiving themselves. An additional personal historical parallel—the anger that has been directed at me for doing this study reminds me of a similar reaction to me during my involvement in the removal of the diagnosis of homosexuality from DSM-II in 1973.

CONCLUSION

In the paper, I wrote “The study provides evidence that change in sexual orientation following some form of reparative therapy does occur in some gay men and lesbians.” With the benefit of time and the many thoughtful commentaries on my study, a more accurate assessment is the following: The conventional wisdom in the mental health profession is that reorientation therapy can get some gays to identify themselves as “heterosexual” and therefore “ex-gay,” but few, if any, will report changes in sexual attraction, fantasy, and desire consistent with true changes in sexual orientation. The study findings call this view into question. In a sample of 200 ex-gays, the majority reported changes in sexual attraction, fantasy, and desire that are consistent with what would be expected if true changes from predominantly homosexual to predom-

inantly heterosexual orientation had occurred. Although some response bias could have occurred, it is unlikely that it can explain all of the reported changes in sexual orientation.

The study provides the level of evidence appropriate to the initial stages of therapeutic evaluation with regard to an important and controversial issue. What is needed is a prospective outcome study in which a consecutive series of volunteer subjects are evaluated before starting reorientation therapy and after several years. 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.

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