The Reincarnation of Shidlo and Shroeder (2002): New Studies Introduce Anti-SOCE Advocacy Research to the Next Generation

Christopher H. Rosik, PhD

Link Care Center and Fresno Pacific University

Abstract

This review examines three recent research studies that are being utilized professionally and politically to support broad claims of the ineffectiveness and harm of SOCE. These conclusions are deemed unjustifiable given a host of methodological problems. Paramount among these concerns are highly nonrepresentative samples, compromised outcome measures, and the confounding of the various forms of SOCE under study. As a consequence, generalizing the findings of these studies beyond the immediate participants is as problematic as claiming that the findings obtained from divorced clients who earlier participated in marital therapy provides a valid representation of outcomes for the therapeutic care of distressed marriages.

The Reincarnation of Shidlo and Shroeder (2002): New Studies Introduce Anti-SOCE Advocacy Research to the Next Generation

In what appears to be a renewed effort to supply an empirical foundation to legal and professional anti-SOCE (sexual orientation change efforts) activities, three new studies are being or have been published and are already being cited among gay and lesbian activists and their allies. Because this research is assuredly going to be uncritically trotted out in professional and lay discussions about SOCE, it is important that these studies be evaluated critically so that those concerned with clients' rights and therapeutic choice are knowledgeable about what these studies actually tell us. In what follows, I will analyze each study, providing a brief description and more in-depth critical discussion before closing with some general observations.

Flentje, Heck, and Cochran (2013)

Description of the Study

Flentje and colleagues (2013) set out to study what the process of reorientation therapy entails. Specifically, they wanted to find out about "typical modalities and interventions" of such psychological care. They surveyed 38 individuals who had gone through at least one "episode" of reorientation therapy and later reclaimed a LGB identity. According to the authors, the results revealed that frequently used reorientation interventions had a strong emphasis on religious practices—including negative messages about LGB individuals—and employed techniques that emphasized change over validation. Some unethical practices were also noted.

Among the professional and policy recommendations the authors draw from their conclusion is the endorsement of legal efforts, such as SB1172 in California, that prohibit licensed therapists from engaging in change-oriented intervention with minors (though as of this writing, the California law is still being litigated and therefore is not

in effect). Further, the authors explicitly suggest that therapists and clients pay close attention to when they might report licensed SOCE practitioners to state licensing boards: "Regardless of state legislation and the client's age at the time of the reorientation therapy experience, if ethically questionable or unethical behavior on the part of a licensed provider is identified, clients could be informed of and supported in their rights to report such behaviors to state licensing boards" (p. 274).

Analysis of the Study

Flentje and colleagues (2013) provide two paragraphs of limitations to their research. Yet any nonpartisan critique would, of necessity, make clear that a two-paragraph statement is woefully inadequate to provide sufficient insight into the scientific merit of this study. Most prominent of the concerns regarding the work of Flentje and colleagues are multiple facets of the study's sample.

Recruitment. Through various list-servs—that were were not fully described but were designed to locate "ex-ex-gays"—the researchers specifically sought individuals who identified as "ex-gays" at the time of their reorientation treatment and who, at the time of the data collection, had identified as lesbian, gay, or bisexual (LGB). Such a recruitment method introduced obvious bias into the study and probably insured that the researchers obtained the results for which they were looking.

Participants rated themselves as being "exclusively homosexual" (n = 22) or "predominately homosexual" (n = 16), indicating that the sample represented the most subjectively unalterable end of the same-sex attraction spectrum. In this context, it is worth remembering the concerns noted by the American Psychological Association's 2009 task force report, with the small modification of substituting "opponents" for "proponents," "succeeded" for "failed," and "benefited" for "harm" in the original text:

> Study respondents are often invited to participate in these studies by LMHP [licensed mental health professionals] who are [opponents]

> of SOCE, introducing unknown selection biases into the recruitment process . . . because study recruiters were open [opponents] of the techniques under scrutiny; it cannot be assumed that the recruiters sought to encourage the participation of those individuals whose experiences ran counter to their own view of the value of these approaches. [Opponents] of these efforts may also have limited access to the research for former clients who were perceived to have [succeeded] in the intervention or who experienced it as [beneficial] (p. 34).

Perhaps the most effective way of clarifying the significance of this limitation is to provide a simple analogy. Imagine researchers who set out to investigate the modalities and interventions associated with martial therapy by recruiting a sample of former marital therapy clients who had subsequently divorced their spouses. How scientifically justified would it be for these researchers to offer their findings as a description of all marital therapy and to call for increased ethical and legal scrutiny of licensed therapists who offer such ineffective practices? Yet this is essentially parallel to how Flentje and colleagues approach their subject matter. The illegitimacy of such conclusions should be obvious to anyone who is fair-minded and not already predisposed against SOCE.

Treatment Setting and Provider Type. As if the recruitment problem was not bad enough, concerns multiply when the authors detail the setting and type of counselor participants reported as providing their SOCE. The majority of therapy “episodes” (56.1%) were provided by religious or pastoral counselors. Another 16.8% were administered by peer counselors. Only 34.6% of therapy “episodes” were actually provided by a licensed mental health professional. The failure of this study to disentangle religious providers from licensed therapists is a serious limitation that makes it inappropriate to draw from the findings any definitive conclusions regarding professionally conducted SOCE.

The authors do report that ethically questionable interventions occurred during 13 different courses of therapy reported by 10 different participants. Their discussion of these interventions may provide useful insight into how Flentje and colleagues (2013) framed their findings to cast the worst possible light on professionally conducted SOCE. They noted that nine of these 13 "episodes" where ethically problematic interventions occurred "included a licensed or licensable professional as one of the providers of therapy" (p. 266). Of course, the authors appear to have no way of knowing whether the licensed professional actually provided the ethically dubious intervention or whether it was provided by a religious counselor also involved in the participant's treatment. The authors then go on to describe a few of the ethically questionable practices, including aversive and holding therapies. The suspicion that these interventions were not provided by licensed professionals is given credence by the authors' earlier admission that no licensed therapist was described as utilizing aversion therapy, and the holding therapy was described as being provided by an "ex-gay layperson" to which the participant had been referred by his pastor.

Participant Demographics and SOCE Occurrence. Apart from being a very small (n = 38) and select sample, the sample composition was highly skewed toward male (n = 31) and Caucasian (n = 33) accounts and from a highly educated background (all but one having completed at least a four-year college education). The APA task force report (2009) noted similar concerns with the SOCE literature, raising particular concerns for generalizing findings to individuals who are less educated and less religious, not Caucasian, youth, and women.

Of interest to the question of SOCE harm, ten participants reported having attempted suicide. Of these, six participants reported a suicide attempt prior to their therapy, seven reported one or two suicide attempts during reorientation, and one indicated two suicide attempts following the conclusion of treatment. These findings suggest a significant portion of the sample was experiencing serious emotional distress

prior to SOCE, distress that cannot be definitively attributed to their therapy experience in the absence of longitudinal data.

The authors do acknowledge the fact that participant reports were retrospective and that this may have impacted the accuracy of their accounts. In fact, there is no indication in the study of how long ago participants actually sought SOCE, although it can be deduced from some of the statistics that at least some recollections are of SOCE that occurred at least 15 years prior. It is reasonable to assume that persons who decide to adopt a GLB identity following failed attempts to change their same-sex attractions and behaviors are not going to look back at those attempts with particular favor, and this could negatively color their recollections. The APA task force report (2009) expressed just such concerns with the SOCE literature, noting how retrospective accounts can introduce serious bias into research findings, and there is no reason to limit this caution to just favorable SOCE studies.

Another example of negative framing may be found in the authors' description of the costs and duration of participants' SOCE "episodes." Mean and median statistics suggest the inclusion of one or more significant outliers. Mean length in weeks of reorientation therapy was 40.5, though the median was only 26, with a standard deviation of 42.6. More problematic was the description of financial costs, with the mean total cost for a single episode of SOCE reported to be $2,195 and the median cost being $130, with a standard deviation of $5,267 and a range from $0 to $26,000. This suggests the presence of outliers as well as the ill-advised combining of intensive week or weekend SOCE experiences with SOCE provided via hourly psychotherapy sessions. Similarly, the costs of all SOCE per participant were $7,105 and the median costs $2,150, with a standard deviation of $11,384. These costs were reported to range between $0 and $52,000, again indicating at least one severe outlier.

It is curious that when the authors attempt to make the case against SOCE in the discussion section they choose to cite the inflated mean figure for total costs rather than

the more appropriate (and less dramatic) median statistic. Clearly, a more scientifically honest approach to the issue of cost would have been to use the median or to recalculate the statistics after removing the outliers.

Conclusion

Sample and other methodological limitations render the Flentje et al. (2013) research inappropriate for making any definitive claims about the general practice of SOCE, particularly in its professionally administered form. In this regard it resembles the earlier research by Shidlo and Schroeder (2002), whose methodological shortcomings it clearly repeats, only this time accompanied by unjustified implications regarding SOCE harm, benefit, and professional practice. Flentje and her colleagues sadly failed to be as forthcoming as Shidlo and Schroeder were when the latter acknowledged in italics that "*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).

Dehlin, Galliher, Bradshaw, Hyde, and Crowell (2014)

Description of the Study

A study by Dehlin and colleagues (2014) appears to be methodologically superior to the Flentje et al. (2013) research and is being published in an APA-affiliated journal. This research employed a web-based survey to contact 1,612 current or former members of The Church of Jesus Christ of Latter-day Saints (LDS, or Mormon) who had engaged in an effort to cope with (understand, accept, or change) their same-sex attractions. A diverse sample was sought, including participants who reported past engagement in change-oriented intervention. Results indicated that private and religious change methods were far more frequent than therapist-led or group-based efforts, and these methods were

reported to be the most damaging and least effective. When sexual orientation change was identified as a goal (compared to intervention where change was not a goal), reported effectiveness was lower for almost all interventions.

While some beneficial SOCE outcomes were noted (such as acceptance of same-sex attraction and reduced depression and anxiety), overall findings were said to support the conclusion that sexual orientation is highly resistant to explicit change attempts and that SOCE are overwhelmingly reported by participants to be either ineffective or damaging. The most ineffective/harmful methods cited by participants in this study were individual effort, church counseling, and personal righteousness (fasting, prayer, and scripture study). Generally, this study's conclusions were consistent with the APA task force's report: SOCE is judged as not likely to be effective, SOCE benefit is related to methods not specific to change-related intervention, and forms of therapy focused on helping the gay person accept a gay lifestyle for him/herself are endorsed.

Analysis of the Study

Although the Dehlin et al. (2014) study has the appearance of providing strong support for the APA's skeptical stance on SOCE effectiveness, attention to details of the study bring such a conclusion into question. Several aspects of the study's methodology and conclusions need to be discussed.

Author Affiliation. To their credit, the study's authors make clear that they are all "LGBTQ allies" who affirm the APA's position supporting religious beliefs and practices, and that all the authors "have been active in supporting the LGBTQ community, online, and national/international engagement." Four of the five authors were raised LDS, and two remain active in the church. All are said to work closely with LGBTQ Mormons in professional and/or personal roles. These disclosures do not include any indication as to how many of the authors, if any at all, had attempted change earlier in their lives, but it would not be surprising if some of them had personal experiences with failed SOCE.

Such personal commitments are not surprising and quite common among researchers in the study of sexual orientation. This raises the risk that the authors are known in some fashion by some of the participants, which can cause participants to respond in socially desirable ways (APA, 2009).

The authors' anti-SOCE affinities also increase the likelihood of groupthink and the risk of failing to recognize important alternatives, resulting in tainted conclusions and social-policy recommendations (Chambers, Schlenker, & Collisson, 2012; Redding, 2013). The only way around these difficulties is a bipartisan research program that brings together investigators from both sides of the issue, something that to date opponents of SOCE have shown no inclination of doing (Rosik, Jones, & Byrd, 2012).

Sample Recruitment. The importance of author affinities is also evidenced in evaluating how the authors obtained their sample. Dehlin and colleagues (2014) emphasize that they sought out a diverse sample by including recruitment through LDS groups that would be supportive of SOCE. Interestingly, the National Association for Research and Therapy of Homosexuality (NARTH) was not contacted as a source for soliciting LDS participants—a curious omission. In the final sample, 21% of participants were solicited through online and print media that were not particularly conservative sources (e.g., *Huffington Post*, Religion Dispatches.org, *Salt Lake Tribune*, and *San Francisco Chronicle*). Another 21% of the sample was obtained through LDS-affiliated LGBTQ support groups, purportedly across the spectrum of beliefs regarding SOCE. Unfortunately, the authors do not break down this figure by specific support group organization, which would have made transparent just how much their claims to have avoided recruitment bias were actually successful. What they did indicate was that Evergreen International (a group more favorable to SOCE) refused to advertise the study, and one is left wondering if the affinities of the authors had something to do with this decision. Word of mouth (electronic social media) led 47% of participants to involvement in the study, which, given the author affinities, cannot be assumed to be equally divided

among opponents of SOCE and those sympathetic to it. Finally, 5% of the sample was solicited through LGBTQ support organizations, which were described as being very helpful in promoting awareness of the study, which is not unconnected to questions about the potential bias introduced through word-of-mouth recruitment. In summary, despite Dehlin et al.'s promotion of their study as involving an ideologically diverse sample, the information the authors provide do not guarantee—and in fact raise serious questions about—the diversity of this sample in evaluating SOCE and generalizing participants' SOCE experiences.

Additional light may be shed on this concern by highlighting the fact that 25% of participants self-described as disaffiliated LDS and another 10% were reported to have been forced to leave the LDS church. In addition, 36% of participants were inactive church members. Only 29% were still actively engaged with their church. What this suggests is that the sample consisted overwhelmingly of participants who were moderately to highly disaffected with the LDS church, which again raises concerns about the representative nature of the sample and the response bias this disaffection may have introduced against SOCE specifically and conservative values in general. Add to this the concerns associated with retrospective, self-report surveys (APA, 2009) and SOCE experiences highly skewed (76%) toward the accounts of male participants, and there is justifiable concern with the reliability and generalizability of the study's findings.

Definition of Variables. Another questionable and outcome-biasing feature of Dehlin et al.'s study is likely to be the manner in which they defined (or operationalized) their primary outcome measure. Participants were asked to rate their SOCE experiences on a five-point scale, from 1 = *highly effective*, 2 = *moderately effective*, 3 = *not effective*, 4 = *moderately harmful*, and 5 = *severely harmful*. This is a highly unusual rating scale in that it is anchored by terms that are actually measuring different dimensions—effectiveness and harm. I cannot think of another example where a key outcome measure was defined using terms that are not antonyms (opposites) but rather meshed-together

endpoints from two different qualities—in this case conflating harm and effectiveness. To be consistent with most research, Dehlin and colleagues should have provided participants with two scales—one anchored by *highly effective* on one end and *highly ineffective* on the other end and the other by *significantly beneficial* on one end and *significantly harmful* on the other.

Note also that the midpoint of the scale is *not effective*, which is far from the typical neutral rating one would expect to find at the center point of a scale. This also is hard to fathom and clearly promotes a biasing effect toward SOCE as lacking effectiveness. This is because of the well-known midpoint response bias, wherein respondents often tend to chose a middle response when they are rushing, when they are uncertain, or when they have no opinion. Far preferable for this research would have been seven-point scales for both effectiveness and harm that would have allowed for more nuanced responding (such as the inclusion of *slightly harmful* or *slightly beneficial* and *slightly effective* or *slightly ineffective* options) and included truly neutral midpoints (such as *neither harmful nor beneficial* and *neither effective nor ineffective)*. As it stands, the conflation of harm and effectiveness in the response scale used in this study creates significant uncertainties about what the results actually mean. Certainly, outcomes would have been more favorable had Dehlin and colleagues (2014) defined the midpoint as *not harmful* rather than *not effective*, which would have been an equally arbitrary methodological decision.

Ideological confounds are also quite possible in the authors' choice to measure psychosexual health in part through utilizing Rosenberg's (1965) measure of self-esteem. Some scales define their construct in a manner that is inherently biased against religious values (Rosik, 2007a, 2007b), which is always a methodological concern when surveying conservatively religious individuals. Consequently, scores may reflect differences between humanistic values and theistic beliefs (for example, elevation of the self versus the virtues of humility and self-negation) more than the construct purportedly assessed

by the instrument, which in the present case was self-esteem. Such appears to be the case with this measure of self-esteem, where research has suggested that when antireligious humanistic dimensions of the Rosenberg scale were statistically controlled, the self-esteem ratings of conservatively religious persons were significantly improved (Watson, Morris, & Hood, 1987). The implication for the Dehlin et al. (2014) study is the distinct possibility that self-esteem levels were suppressed and might actually have been higher than indicated for participants who remained conservatively religious and therefore were more likely to report positive SOCE experiences. As it stands, the authors reported that they failed to find significant quality of life or self-esteem differences between participants who had attempted SOCE and those who did not, a discovery not loudly trumpeted in the article.

Positive Outcomes. In spite of these problems with scale definitions and their potential biasing toward ineffective SOCE ratings, some SOCE methods actually did receive mildly positive endorsements. Of interest is the fact that these slightly positive ratings were found for therapist-led, group therapy, group retreat, and psychiatry methods, while personal righteousness, individual effort, and church counseling methods received slightly harmful ratings. Once again, given the scale ambiguities, we cannot be sure this average rating signifies less effectiveness or more harm. Psychotherapy was found to have moderate or greater effectiveness by 44% of respondents who sought it, with respective effectiveness ratings of 48% for psychiatry, 41% for group therapy, and 48% for group retreats. Of contextual importance is the finding that professional SOCE methods were reported far less frequently by participants than religiously oriented methods, meaning that aggregate results concerning change in Kinsey scores and psychosexual health likely provide an unrealistically negative view of professional SOCE.

Overrepresentation of Nonprofessional SOCE Methods. Dehlin and colleagues (2014) report that religious and private forms of SOCE were far more

prominently reported in their sample than were professional methods. Whereas 85% of participants indicated engaging in either religious or private individual SOCE methods, only 44% reported some form of therapist or group-led SOCE. Engaging in "personal righteousness" (such as prayer, fasting, studying scripture, or an improved relationship with Jesus) was reported twice as much as pursuing professional psychotherapy. Yet the authors report that group-related and therapist-led methods tended to be rated by participants as the more effective and least damaging forms of SOCE. Furthermore, SOCE "methods most frequently rated as 'effective' tended to be used the least and [for the] shortest duration, while methods rated most often as 'ineffective' tended to be used most frequently and for the longest duration" (p. 6). The authors also contend that this "effectiveness" represented not orientation change but orientation acceptance, decreased psychological distress, and improved family relationships.

It is worth mentioning here that Dehlin and colleagues (2014) speculate about the reliance of participants on private and religiously-oriented SOCE methods and suggest this may be due in part to the refusal of licensed therapists to engage in SOCE. This is a tragically ironic observation in that psychologists and other mental health professionals have abandoned religiously conservative persons with SSA conflicts who wish to pursue change. Rather than engage with therapists who respect their self-determined goals and are trained to provide SOCE with an awareness of professional and ethical issues, psychology may be forcing these individuals to "white knuckle" their struggle alone or rely on untrained religious practitioners, where the risk of harm may be significantly greater. Incredibly, if the present study's findings are to be believed, arguments offered in favor of California's proposed legal prohibition of SOCE with minors have specifically suggested licensed therapists refer these minors to religious counselors—a practice more likely to harm these minors than were these therapists to actually provide the SOCE.

This overrepresentation of purportedly ineffective/harmful individual (conducted alone by oneself) and religious-oriented SOCE methods makes the study's findings

regarding Kinsey ratings and psychosocial health inappropriate as a measure of professionally conducted SOCE. These general results summed over all SOCE forms therefore are likely to be skewed in an adverse direction, and again might conceal potential positive outcomes of professional SOCE.

Type of SOCE Provider. Another critical concern with the study by Dehlin and colleagues (2014) is the likelihood of provider confounds. In other words, the study combined religious and professional SOCE providers. The results (ambiguous as they may be already given the scaling problems) clearly implicated SOCE provided by religious authorities (such as LDS bishops) as being associated with greater ratings of harm as compared to SOCE provide by licensed therapists. While it would be understandable to conclude from such findings that conservative clergy or pastoral counselors may do more harm than good when working with persons with same-sex attraction struggles, such a conclusion would be a highly inappropriate generalization from this research. The reason for this has to do with how the LDS church selects its ecclesiastical leaders. The typical LDS church bishop does not obtain theological or pastoral graduate education but is instead chosen from among male members in good standing with the church who have shown themselves to be competent and successful with their families and vocations. Dehlin et al.'s results in this regard may well reflect the not particularly surprising discovery that religious individuals in conflict about their same-sex attractions are at a greater risk of harm when their SOCE provider is, for example, a plumber or a banker, however well-intended the provider might be.

Kinsey Ratings. Dehlin et al. (2014) indicate that individuals engaged in SOCE did not on average report different Kinsey attraction, behavior, and identity scores from participants who had not engaged in SOCE. Yet current ratings on these dimensions are not a direct measure of SOCE outcomes and do not tell us very much about the effects (or lack thereof) of the SOCE experience. Presumably, a significant period of time may have elapsed between the end of SOCE and the survey administration, and

many factors unrelated to SOCE could impact these ratings. Only a pre- and post-test design can take into account pre-SOCE levels of these dimensions and enable tentative conclusions about causality, as the authors admit: “It is not possible to determine causality and directionality of these relationships without the use of methodologies such as randomized clinical trials or longitudinal studies” (p. 10). What these ratings probably do reflect is that most of this sample (91%) had adopted a GLB identity since the time of their SOCE, a finding entirely consistent with participants being overwhelmingly disaffected (73%) with their church. This raises the same concerns noted above for Flentje et al.’s (2013) study of “ex-ex-gays.”

Conclusion

While Dehlin et al.’s (2014) study is clearly an improvement over the Flentje et al. (2013) research, it nonetheless suffers from many of the same limitations and in this regard may be more pernicious, as the findings will certainly be offered by opponents as evidence of professionally conducted SOCE harm and ineffectiveness. Dehlin and colleagues encourage such a usage in their discussion about the study’s implications for counseling, asserting that the findings support the APA and other professional associations’ conclusions about SOCE and advocating for the LDS church and affiliated therapists to adopt acceptance-based forms of therapy.

Consideration of this critical review would instead suggest that the findings of Dehlin et al.’s (2014) study cannot be definitively or legitimately generalized beyond the sample population examined. It is a sample purported to be more representative but which, in fact, is overwhelmingly represented by currently LGB-identified persons who are disaffected with the LDS church and who most commonly engaged in SOCE alone or with religious leaders unlikely to have formal psychological or even pastoral training. Questionable measurement (scaling) of the outcome variable also raises questions about the internal validity of the findings. The results of Dehlin et al.’s study therefore may be

useful in anti-SOCE advocacy, but they do not shed much light on the risk of harm or effectiveness of SOCE offered by licensed mental health professionals. In fact, authors more sympathetic to SOCE might have argued that the data point to the need for religious conservatives with SSA conflicts to have greater access to professionally guided forms of SOCE. At most, Dehlin and colleagues have provided evidence that some prior participants of SOCE who are now opposed to the goals of SOCE may look back on their experience as harmful or not effective.

Bradshaw, Dehlin, Crowell, Galliher, and Bradshaw (2014)

Description of the Study

No doubt aware of the limitations of the Dehlin et al. (2014) study regarding therapist-led SOCE, this mostly same team of authors analyzed the subsample of respondents who reported participation in psychotherapy for their SSA conflicts. This sample was comprised of 868 individuals (672 men and 194 women). The authors reported that such counseling was largely ineffective, with less than 4% of participants reporting any modification of SSA, 42% indicating their change-oriented therapy was "not at all effective," and 37% finding it to have been moderately to severely harmful. Affirming psychotherapeutic approaches were often found to be beneficial in reducing depression, increasing self-esteem, and improving relationships. The authors conclude that there is a "very low likelihood" of sexual orientation modification and advise highly religious sexual minority persons to consider this before engaging in reorientation therapy.

Analysis of the Study

The Bradshaw et al. (2014) study is not a new study in the sense that it uses the same data set employed by Dehlin et al. (2014). Rather, it examines the specific subgroup

of participants who reported having engaged in SOCE via psychotherapy. This means that many of the methodological problems noted for Dehlin et al.'s research persist as well as a few new concerns.

Sampling Procedures. As noted above, the concerns associated with this research group's first study remain present for this article as well and in some cases are given further delineation. The overrepresentation of men and their experiences continues, with the added observation by the authors that the women participants showed great Kinsey scale variability and more bisexuality. Furthermore, male participants were three times more likely than women to make explicit statements that change had not occurred. The potentially biasing effect of a largely LDS-disaffected sample is suggested in the finding that participants no longer associated with the church were significantly more likely to describe their therapy experiences as "severely harmful." While this could signal a tendency to minimize harms suffered among those still trying to be faithful to the church, it could just as well reflect a tendency to emphasize harms by those who now feel an affinity to an LGB community that may be hostile to certain beliefs/practices of the LDS church related to sexuality in general and SOCE in particular.

Bradshaw et al. (2014) also observed that categorical change (change from no opposite-sex attraction to only opposite-sex attraction) was not reported by participants; rather, when change was indicated it was more toward bisexuality. Moreover, these authors noted that bisexuality was underrepresented in the sample. This is a concern in that bisexuality is likely to be more responsive to change-oriented intervention than an exclusively homosexual orientation (Whitehead & Whitehead, 2010), and this could have reduced reports of positive SOCE outcomes in comparison to what might have been obtained with a more representative sample. Finally, the likely recruitment problem favoring participants allied to GLB organizations and communities unsympathetic to SOCE continues to loom in the background of Bradshaw et al.'s work, making strong conclusions against change-oriented psychological care scientifically and professionally inadvisable.

Measurement Concerns. Outcomes are again measured with the problematic scale that conflates two different dimensions (harm and effectiveness). The discussion of these concerns noted above concerning the Dehlin et al. (2014) study will not be repeated here, but their salience can be seen in the author's report that 42% of psychotherapy SOCE participants viewed their experience as *not at all effective*, 21% as *moderately harmful*, and 16% as *severely harmful*. This reporting sounds as if the results are independently derived from two different measures, as they clearly should have been. The fact that they are taken from three neighboring points on a single scale certainly creates the likelihood of a loss of important nuance in the data, thereby unduly inflating participant ratings of harm and ineffectiveness in their evaluations of professional SOCE. Again, these outcomes surely would have been different had Bradshaw et al. (2014) defined the midpoint as *not at all harmful*.

It should also be mentioned that the authors indicate their survey took, on average, more than an hour to complete. This fact makes for a greater risk of significant midpoint response bias (which would bias the overall effectiveness rating downward) as participants seek to get through an unusually long survey process as quickly as possible.

In addition, Bradshaw et al. (2014) trichotomize the goals of psychotherapy-related SOCE into change, acceptance, and understanding. Yet these are by no means mutually exclusive goals, and it is reasonable to believe that most therapists providing SOCE are also promoting goals of acceptance (e.g., of the reality of clients' SSA) and understanding (e.g., promoting the clients' self-discovery of the origins of their SSA). Thus, this forced-choice categorization appears by definition to mischaracterize professional SOCE, again with a likely accompanying loss of data precision that could lend useful refinement to the study's findings.

Confounding of SOCE Forms. Another serious potential concern in Bradshaw et al.'s (2014) study is the admission by the authors that participants engaged on average in 3.7 additional forms of SOCE interventions. Moreover, "It became clear

that participants often viewed their SOCE holistically, as a composite of all the interventions in which they had engaged, including, especially, private efforts made concurrently with professional counseling" (p. 12). Thus participants engaged in multiple therapy efforts that were not differentiated in their overall rating scores. Open-ended responses suggested that some participants applied the outcome ratings narrowly to therapist-led SOCE, while others rated the benefit or harm of their experience across all SOCE forms utilized. Consequently, the results of this study cannot be reliably linked to professional SOCE, as they may well be adversely distorted by participants' evaluative inclusion of the more deleterious forms of SOCE in their ratings. To employ these ratings as a pure reflection of professional SOCE as Bradshaw et al. have done is to engage in scientifically unjustified speculation.

It is also possible that many of the 93 participants who reported an aversive therapy emphasis in their SOCE experienced this intervention in the context of religious forms of SOCE or engaged in it years ago when aversive treatments were common to a broad range of clinical concerns within the field of psychology. The fact that contemporary SOCE practitioners have long eschewed the use of aversive techniques with unwanted SSA (NARTH, 2010) would seem to make dubious the assumption that recent professional forms of SOCE are behind this figure. Furthermore, as noted previously, Flentje et al.'s (2013) study found that no aversive treatments were reported by participants in their professional SOCE experiences.

Additional Signs of Bias. While not a methodological issue per se, Bradshaw et al.'s (2014) discussion of SOCE provides not-so-subtle indications of their partisan sentiments. For example, Bradshaw and colleagues dismiss Spitzer's (2003) research, citing Spitzer's "repudiation" of his findings but fail to note that several of his participants subsequently affirmed their change and repudiated Spitzer's action (Armelli, Moose, Paulk, & Phelan, 2013). Similar to how the APA's (2009) task force report dismissed the Jones and Yarhouse (2011) study in a footnote, Dehlin et al. (2014), dismiss Jones and Yarhouse in a

sentence, despite this study's clearly more rigorous methodology. Bradshaw and colleagues seem eager to point out the demise of Exodus International and admissions of lack of change by its former president. This is a curious non sequitur in that Exodus was a religious ministry promoting religious forms of SOCE while the present article was addressing only SOCE delivered through licensed mental health providers. Finally, the authors reveal their etiological commitments when they affirm that SOCE requires a disregarding of the "large body of evidence" demonstrating "a biological origin for sexual orientation" (p. 24). Such a definitive commitment to biological origins is not in keeping with the current APA opinion (APA, 2008; Just the Facts Coalition, 2008), which states:

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, 2008; emphases added).

Conclusion

Bradshaw and colleagues (2014) conclude their article with the following statements:

> For adherents to this line of reasoning [i.e., that change can occur], the claim of a successful sexual orientation change by a few individuals is sufficient to generalize to the population at large. The clear evidence, however, is that dutiful long-term psychotherapeutic efforts to change are not successful and carry significant potential for serious harm, and that LGBQ Latter-day Saints find greater satisfaction in counseling approaches that result in acceptance or accommodation. (p. 24)

As is evident, the authors first create a straw argument whereby all SOCE proponents assume that change for some patients means all patients can change. They cite no literature to back up this accusation but then proceed to challenge this false portrayal by citing the results of their study. However, as I have attempted to make clear, this study's serious methodological weaknesses make the authors' broad generalizations scientifically unjustifiable. That Bradshaw and colleagues would make such unqualified conclusions places their work firmly within the advocacy research tradition of Shidlo and Schroeder (2002).

General Discussion

There is little doubt that some consumers of professional SOCE experience their therapy as ineffective and/or harmful. To state otherwise would be to claim a standard of outcome unattained by any other approach to psychological care (Lambert, 2013; Lambert & Ogles, 2004). Therapists who engage in SOCE no doubt find agreement in the desire to minimize the potential for harm and increase the likelihood of successful outcomes through a commitment to high ethical and practice standards. The issue in question, however, is the prevalence of harm and the degree of effectiveness in professional SOCE. I have offered here an admittedly critical review of three recent studies because is it likely the limitations of this research will be glossed over (if mentioned at all) by activists and professional mental health associations eager to demonize change-oriented care and further restrict therapeutic choice for clients with unwanted same-sex attractions.

While these studies do appear to document some experiences of harm and unsuccessful SOCE as well as suggest that nonprofessional forms of SOCE may carry a higher risk of adverse outcomes, an objective methodological analysis indicates that the findings simply cannot support any conclusions beyond these broad observations. In fact, were we to apply the overly rigorous methodological standards of the APA (2009)

task force to these studies, it would have to be concluded that they do not meaningfully advance the discussion on the issues of SOCE harm and effectiveness. Foremost in preventing this research from furthering our understanding of SOCE outcomes are sampling and measurement concerns that virtually guaranteed that reports of SOCE harm would be inflated and accounts of success would be suppressed.

The aforementioned analogy remains apt. The central limitations of these studies are captured well by imagining a project wherein researchers surveyed religiously conservative former marital therapy patients who had subsequently divorced in order to determine the treatment's effectiveness and harm and then used these results to make sweeping conclusions about this therapeutic modality. Would this be a fair and scientifically justified use of the findings? I might add that the marital therapist is not trying to cure an illness here, but rather is frequently attempting to help clients live their lives in a manner consistent with their religious beliefs about the sanctity of marriage (Dollahite, Hawkins, & Parr, 2012). Furthermore, these religious clients' deeply held moral values may lead them to remain in a distressful marriage and pursue therapy long after other clients would have opted for divorce. Yet the choices of such clients to seek marital therapy are not *ipso facto* assumed by the profession to be based on *internalized divorce-negativity* or *cultural maritalism* and thereby invalidated, despite the additional emotional stress that may come from remaining in their marriages. Outside of political advocacy calculations, an evenhanded scientific assessment (not to mention common sense) would suggest that professional SOCE clients and therapists be given a similar benefit of the doubt and allowed space within the mental health professions to provide such psychological care to those who seek it (cf. NARTH, 2012; Rosik, 2013).

Shidlo and Schroeder (2002) explicitly recruited former SOCE clients who felt harmed by their experience, with predictable findings. But at least they were willing to explicitly and emphatically emphasize their inability to generalize beyond their sample. Flentje and colleagues (2013), Dehlin and colleagues (2014), and Bradshaw and

colleagues (2014) have reincarnated Shidlo and Schroeder's methodology in a less overt manner through sampling that may appear more diverse but that functionally is quite similar in its effect. Moreover, the authors of these new studies are far more willing to draw conclusions and make recommendations that they have no assurance can actually be supported beyond their own study samples.

No doubt NARTH and other SOCE proponents would welcome this research were it utilized to offer guidance within the bounds of its limitations—such as the need for therapists to (1) provide SOCE within the ethical standards of their profession, (2) recognize the limitations of our current scientific understanding of sexual orientation change, and (3) offer up-to-date education on sexual orientation and SOCE to conservative religious communities. Sadly, the authors of the studies examined in this review have largely not chosen such a scientifically accurate and measured approach but rather offered what appear to be advocacy-emboldened recommendations that support the further professional marginalization and legal prohibition of professional SOCE. This only serves to fuel the polarization around SOCE that constitutes an ongoing disservice to individuals with unwanted same-sex attractions who seek professional psychological care.

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