

Ideological Gatekeeping or Quality Control?
One Author's Experience with Peer Review at the
American Psychologist

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Ideological Gatekeeping or Quality Control? One Author's Experience with Peer Review at the *American Psychologist*

In this article I utilize my recent experience with the peer review process at the American Psychological Association's flagship journal, the *American Psychologist*, to provide readers with an opportunity to evaluate for themselves the integrity of this practice. My colleague, Paul Sullins, Ph.D, and I submitted a short comment to the journal challenging the global characterization of sexual orientation change efforts (SOCE) as invariably harmful in a published summary of the APA's *Guidelines for Psychological Practice with Sexual Minority Persons*. We cited emerging research as the basis for rethinking this conventional wisdom. Here I present in full both the comment we submitted and the responses received from the reviewers that formed the basis for rejection of the comment. These are followed by a second rendering of the reviewers' feedback, but this time interspersed with my observations. Readers are encouraged to come to their own conclusions as to whether the basis given for the rejection best represents considerations aligning with either ideological gatekeeping or objective quality control.

Keywords: SOCE research, peer review, *American Psychologist*

Early in 2022, the *American Psychologist* published an executive summary of the American Psychological Association's (APA) *Guidelines for Psychological Practice with Sexual Minority Persons* (Nakamura et al., 2022). These Guidelines include an extensive discussion about sexual orientation change efforts (SOCE) with the unequivocal message that SOCE are harmful for sexual minority persons across the lifespan. However, being aware of an emerging literature that suggests much more uncertainty and lack of nuance is present in the SOCE research, I determined to see if reviewers at the *American Psychologist* would see fit to allow a professional exchange on this topic. This journal is the flagship journal of the APA, distributed to every dues-paying member. The authors' page of the journal specifically encourages submissions referred to as "comments," which must address an article published in the journal within three months of the target article's publication. A comment is limited to 1,000 words and 10 references and, if published, usually is responded to in a rejoinder by the authors of the target article. This means that space is very limited and

one's presentation and argumentation must be concise and tightly focused.

In a single afternoon, I wrote a draft of a comment, which was honed somewhat in subsequent weeks. I also pursued Dr. Paul Sullins to be a co-author with me since he is a prominent researcher in the emerging literature I would cite. He graciously reviewed the draft comment for his suggestions. Finally, on Friday, April 8, 2022, I submitted the manuscript to the *American Psychologist* and waited with what I confess was a degree of pessimism to see what would happen. Below is the full text of that submission:

Sociopolitical Diversity Can Improve Our Understanding of Sexual Orientation Change Efforts: Comment on Nakamura et al., 2022

In this comment we focus specific attention on Guideline 4 of the American Psychological Association's *Guidelines for Psychological Practice with Sexual Minority Persons* (Nakamura et al., 2022) as pertains to sexual orientation change efforts (SOCE). We organize our discussion by first outlining the gist of this guideline, then report on new research that bears importantly on the

conclusions of this guideline, and close with some recommendations for improving the future study of SOCE. We state at the outset our support for the APA mandate to promote client self-determination while not doing harm to clients. All psychologists who work with sexual minorities should be conversant with the *Guidelines*, and we particularly resonated with the admonition to “. . . be aware that important within-group differences exist and that there is not a universal sexual minority experience” (p. 2).

Guideline 4 emphatically affirms the definitiveness of universal SOCE harm. SOCE “. . . practices are ineffective and cause substantial harm . . .” (p. 4). The summary then goes on to assert that “. . . sexual minority persons who have undergone SOCE are twice as likely both to contemplate suicide and to report having attempted suicide compared to sexual minority peers who did not undergo SOCE” (p. 4). The research from which these findings were derived is that conducted by Blosnich et al. (2020), who utilized data from the Generations study, a national representative sample of 1518 sexual minorities. The findings were described by Blosnich and colleagues as supporting the conclusion, among similar others, “. . . that SOCE is a stressor with particularly insidious associations with suicide risk” (p. 1027), which “. . . may compound or create problems, such as . . . suicidal ideation and suicide attempts” (p. 1028). This study also features prominently in other APA publications (e.g., Glassgold, 2022).

Although psychologists should seek to alleviate the suffering of sexual minorities, we are concerned that guidance to achieve such aims needs to be based on robust empirical data that has been subjected to meaningful critique. We submit that this appears not to have been the case for Blosnich et al. Specifically, Sullins (2021) reanalyzed Blosnich, but unlike the original

study, controlled for pre-SOCE suicidality using information obtainable from the Generations dataset. Sullins’ reanalysis discovered SOCE was not positively associated with any form of suicidality. For example, whereas Blosnich et al. reported an adjusted odds ratio (AOR) of 1.92 (95% CI 1.01 – 3.64) for suicidal ideation between SOCE exposure and non-exposure groups, Sullins’ reanalysis revealed an AOR of .44 (.20 – .94). For suicide attempts, while Blosnich et al. reported an AOR of 1.75 (.99 – 3.08), Sullins found controlling for pre-SOCE suicidality reduced this AOR to .74 (.36 – 1.43). In a second study of the Generations data, Sullins (2022) also reported that, on average, sexual minority persons who had undergone failed SOCE therapy did not suffer higher psychological or social harm.

The attenuation of Blosnich et al.’s results is both striking and concerning. Most if not all of the SOCE research alleging harm fails to control for pre-SOCE levels of distress, a limitation that should encourage scientific humility in both conclusions from and applications of this literature. We have no doubt that certain SOCE practices are harmful to sexual minorities generally, and we have no interest in defending such activities. However, Sullins’ work along with other recent studies suggest there remains room for a much finer resolution in our understanding of SOCE beyond a simple harm versus no harm narrative. Sexual minorities are an incredibly heterogeneous group of people and SOCE covers an exceedingly broad and largely unspecified array of practices and beliefs. We believe there is value in research that can shed light on which SOCE methods are harmful for which sexual minorities rather than simply foreclosing access to all speech-based, voluntarily pursued practices that might be considered SOCE (e.g., MASKED FOR REVIEW).

Toward this end, we encourage SOCE researchers to attend to several emerging considerations in their work in order to more clearly discern which findings from this literature are reproducible and which are not. Sullins' work indicates that accounting for pre-SOCE levels of health and distress is an indispensable methodological requirement, even if assessed retrospectively. A movement away from reliance on simplistic SOCE exposure versus non-exposure dependent variables in favor of investigating specific varieties of SOCE methods under specific conditions (e.g., voluntary versus coercive) would also be highly recommended. SOCE research will also benefit from moving beyond recruiting only LGB+-identified sexual minorities to including those who do not identify as LGB+, who appear to represent a more politically and religiously conservatively sub-group that has been largely invisible within much of this literature (Lefevor et al., 2020; MASKED FOR REVIEW). Gaining access to networks inhabited by non-LGB+-identified sexual minorities will likely involve recruitment of and collaboration with sociopolitically conservative researchers and religious representatives in order to gain the trust and hence participation of these individuals.

Although challenging for a profession often not aligned with conservative social values, we think such "adversarial collaboration" among research psychologists is essential for mitigating the impact of confirmation bias and capturing the most ecologically valid and replicable picture of SOCE experiences (Duarte et al., 2015). The willingness to pursue sociopolitical diversity among research investigators and participants is an important marker of professional psychology's dedication to the pursuit of truth, even and especially when this involves an admittedly controversial and politically charged topic such as SOCE.

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Editorial Response

On April 27, 2022, I received an e-mail from an editor with the journal rejecting our submission. I present it here along with the reviewers' comments in their entirety. I reserve judgment on whether this rejection was justified, and whether it signified ideological gatekeeping around issues of sexual orientation or a defensible rejection of substandard scholarship. Instead, my preference is to provide readers with an intellectual exercise wherein they can decide what might have been the motives behind the editorial decision to dismiss the comment. I have no way of definitively knowing these motives, so I remain ultimately agnostic on the matter. However, after presenting the complete text of the editor's and reviewers' responses, I will then present a second version of these interspersed with my thoughts and observations. Before reading this latter version, my encouragement is for readers to pause and come to their own conclusions and, only after doing this, continue on to reading my commentary.

The Text of the Decision E-mail from the *American Psychologist*

Dear Dr. Rosik,

Thank you for submitting your manuscript Sociopolitical Diversity

Can Improve Our Understanding of Sexual Orientation Change Efforts: Comment on Nakamura et al., 2022, for review to the *American Psychologist*. Two reviewers, each an expert in the area of your work, have kindly provided reviews of your paper. I have also read your work. Based on the reviewers' comments and recommendations to me, and my own reading of the paper, I have reached an editorial decision. I regret to say that although the topic of the paper is important, the concerns about the paper preclude its publication in *American Psychologist*. The reviewers find the evidence for the position espoused in the comment is suspect and counter-evidence is not cited.

While it is always difficult to receive a negative outcome for a submission, I hope that you will find the reviews helpful in pursuing this work. Thanks again for letting us consider your paper.

Sincerely,

[Name withheld], [An] Editor,
American Psychologist

Reviewers' Comments:

Reviewer #1: Please evaluate the Comment on the basis of the following criteria:

1. Is the goal of the comment clear? Yes. The authors have a clear goal of supporting sexual orientation change efforts (SOCE) and disagree with the APA's Guidelines for Psychological Practice with Sexual Minority Persons. However, they cite as supporting evidence, several studies built on flawed logic.

2. Does the comment relate clearly to the original article?

Somewhat. The authors zero in on the specific “Guideline 4” of the APA document, ignoring other guidelines in the document. It is unclear if they support the other guidelines or object to the entire spirit of the document.

3. Does the comment add new information to the scholarly discussion of the topic? No. The evidence that the authors use to support their argument is potentially not peer-reviewed (as one citation of an article by Sullins 2021 had a footer that explicitly said the document was not peer-reviewed) or conducted to peer-review at open access journals that have questionable rigor.

4. Is the information provided important? No. The authors attempt to deride the APA’s stance against SOCE by suggesting previous peer-reviewed research on the topic is flawed while citing their own list of highly suspect, allegedly peer-reviewed research. They try to appeal that the science behind the harms of SOCE is equivocal and, therefore, the APA’s stance is mistaken. To uphold a practice that is condemned by multiple professional associations is not important information; it is information designed to obfuscate.

5. Is the Comment written clearly? Partially. It is clear that the authors are trying to achieve a publication in the American Psychologist to detract from the APA’s stance on SOCE. However, there are parts of the letter that are unclear. For instance, the authors write, “We have no doubt that certain SOCE practices are harmful to sexual minorities generally, and we have no interest in defending such activities.” The author should offer examples of what they deem as harmful SOCE

practices. It is unclear to the reader what kinds of practices they are referencing because all forms of SOCE are condemned by multiple professional associations. If the authors believe there are nuances of SOCE and some practices that are indefensible while others are defensible, then they should provide clear examples of what they mean. Additionally, the authors write, “However, Sullins’ work along with other recent studies suggest there remains . . .” The authors should supply citations of these other recent studies.

6. Is the tone of the Comment constructive and collegial? It is collegial, but I did not find it constructive. The authors do not suggest what they think the Guideline should be or whether the APA should reverse its stance on SOCE. Their suggestion of “sociopolitical diversity” is not constructive because it is unclear what journals are to do about this. The authors are using claims of “sociopolitical diversity” and “adversarial collaboration” to manufacture opportunities to peddle support of SOCE within journals that have actual rigorous peer review.

7. How likely is it that the Comment will be cited in future publications? I think it is likely this will be cited in future open access publications that seem to be the premier venues for publishing studies that support SOCE. I strongly believe this letter will find its way into the policy arena as well, as advocates of SOCE continue to fight against SOCE bans under the ruse of “patient choice.” They will undoubtedly trumpet a letter in the American Psychologist because it seems that

most of their other work cannot find homes except for paid open access journals.

Reviewer #2: *This article is a comment on the Nakamura et al. (2022) American Psychologist paper that provides an executive summary of the 2021 revision of the APA Guidelines for Psychological Practice with Sexual Minority Persons. The comment focuses on Guideline 4, which asserts that psychologists understand that sexual minority orientations are not mental illnesses and that efforts to change sexual orientations cause harm. It is the latter point that the authors challenge.*

The authors' argument that sexual orientation change efforts (SOCE) are not harmful rests on an unpublished article that has apparently not been peer-reviewed (Sullins, 2021). The authors' argument is therefore weakened.

Moreover, new research studies emerge every year documenting the harmful effects of conversion therapy. Here are several recent ones:

Forsythe, Anna et al. (2022). Humanistic and economic burden of conversion therapy among LGBTQ youths in the United States. JAMA Pediatrics, 176(5), 493–501. <https://doi.org/10.1001/jamapediatrics.2022.0042>.

Higbee, Madison et al. (2020). Conversion therapy in the Southern United States: Prevalence and experiences of the survivors. Journal of Homosexuality. Advance online publication. <https://doi.org/10.1080/00918369.2020.1840213>

Przeworski, Amy et al. (2021). A systematic review of the efficacy,

harmful effects, and ethical issues related to sexual orientation change effects. Clinical Psychology: Science and Practice, 28, 81–100. <https://doi.org/10.1111/cpsp.12377>

Overall, then, the authors of this comment use a single, unpublished study to challenge a single study supporting the assertion that SOCE are harmful. The evidence base, however, does not rely on that single study, but instead rests on multiple studies, all of which replicate the finding that SOCE are harmful.

Having read the entire text of this communication, I now encourage readers to reflect for a few moments on their reaction to both our original comment and the reviewers' feedback: To what extent does the feedback seem to be reasoned and measured and reflect a fair critique of the failings of our comment? To what extent does it seem to suggest an ideologically closed perspective that is gatekeeping preferred narratives about SOCE? To what extent might both options be at play? What considerations bring you to your conclusions?

Feedback with Commentary

Now that you have had some opportunity to reach your own conclusions relatively free from persuasion, I again present the reviewers' comments, but this time interspersed with my own observations. Again, the reader can determine whether my thoughts are a reasoned and legitimate concern for a premature foreclosing on some change-allowing therapies, or whether I am simply a partisan hack with no real interest in acknowledging the validity of the conventional wisdom that all SOCE causes harm.

Reviewer #1: Please evaluate the Comment on the basis of the following criteria:

1. *Is the goal of the comment clear? Yes. The authors have a clear goal of supporting sexual orientation change efforts (SOCE) and disagree with the APA's Guidelines for Psychological Practice with Sexual Minority Persons. However, they cite as supporting evidence, several studies built on flawed logic.*

In my own therapy work with couples, I frequently warn about the dangers of assuming you know the motives of the other and caution that one's assumptions are more likely to conform to one's own preconceived beliefs rather than the ultimate reality. Here I judge that the reviewer has committed such an error. I neither support all interventions that have been described as falling under the rubric of SOCE (i.e., aversive techniques are a clear example of this), nor do I disagree completely with the APA's guidelines. In fact, in our comment we specifically point out one area of agreement, and there are many more, such as respecting the dignity and autonomy of sexual minority persons. Furthermore, while we are accused of using flawed logic, these flaws are not specified, so it is conveniently impossible to know what the reviewer has in mind. One would hope in the interests of helping the authors improve their manuscript that the reviewer would at least specify one example of their flawed thinking.

2. *Does the comment relate clearly to the original article? Somewhat. The authors zero in on the specific "Guideline 4" of the APA document, ignoring other guidelines in the document. It is unclear if they support the other guidelines or object to the entire spirit of the document.*

From this feedback I take it that potential contributors must agree with "the entire spirit" of the Guidelines or any critique of any portion of it will be deemed illegitimate, no matter how sound or scholarly the argument is. Yet it is unclear how it might be possible to raise serious and important questions about the Guidelines' view of SOCE harms and not run afoul of this "entire spirit" directive. Beyond this, it would have been impossible to address all the guidelines in 1000 words. The purpose of "zeroing in" on Guideline 4 was precisely to limit ourselves to a clear focus that could be suitable to expound upon within such a strict word limitation. Again, I'm not sure how we could have satisfied this reviewer and stayed within the space limitations of the comment format.

3. *Does the comment add new information to the scholarly discussion of the topic? No. The evidence that the authors use to support their argument is potentially not peer-reviewed (as one citation of an article by Sullins 2021 had a footer that explicitly said the document was not peer-reviewed) or conducted to peer-review at open access journals that have questionable rigor.*

There is truth to this concern in that Sullins' main study (Sullins, 2021) had yet to be peer-reviewed at the time of our submission. The unfortunate back story is that Sullins' reanalysis has been held up in peer review for over a year (though it appears it may finally be getting published in a prestigious journal and hence worth the delay). It would have been ideal for his reanalysis to have had a peer-reviewed background for our reviewers. Moreover, I included reference to some of my research in our comment, which is peer-reviewed, but since the review process is supposed to be

blind, I masked references to my studies, so the reviewers had no clue. I decided that I could not mask Sullins' study, since it was key to our comment and to mask his work as well would mean reviewers would have no idea what the research we referred to actually was. So, it was an unfortunate set of circumstances we were working under, i.e., a time limitation for submitting the comment and a laborious and not finalized review process at another journal involving research central to our argument. Such are the vagaries of the scholarly peer review process.

I would also add that a lack of peer-review status has not seemed to be an impediment for research that is taken seriously by academics in this literature. For example, the study published in book form by Bell, Weinberg, and Hammersmith (1981) was, to my knowledge, never peer-reviewed but is still cited today as support for the notion that family dynamics or childhood trauma never play a role in the development of same-sex sexuality (e.g., Przeworski et al., 2021, noted by the second reviewer below). If in fact studies purporting to challenge aspects of the conventional wisdom on matters pertaining to sexual orientation and gender identity may have a more difficult road to publication in an APA journal, then it is also hard to miss the irony in this reviewer's concern. Namely, research that is counter to preferred narratives and official policies within the APA is less likely to be published in their family of journals (as well as most other professional association-affiliated journals), and hence these researchers are more willing to turn to open access journals to bypass such gatekeeping. Yet by forcing such researchers into doing so, this APA reviewer determines their research is *ipso facto* of substandard rigor and unworthy of publication. Maybe it is sour grapes, but it is hard not to feel some resonance here with the old adage, "heads I win, tails you lose."

4. Is the information provided important? No. The authors attempt to deride the APA's stance against SOCE by suggesting previous peer-reviewed research on the topic is flawed while citing their own list of highly suspect, allegedly peer-reviewed research. They try to appeal that the science behind the harms of SOCE is equivocal and, therefore, the APA's stance is mistaken. To uphold a practice that is condemned by multiple professional associations is not important information; it is information designed to obfuscate.

According to the Merriam-Webster dictionary, *deride* means (1) "to laugh at or insult contemptuously" or (2) "to subject to unusually bitter or contemptuous ridicule or criticism." By that standard, unless I am seriously missing something, I find it a stretch to characterize the comment as deriding the APA's stance. Disagreeing in part, certainly, but deriding, I cannot find evidence for this being an overt or covert element of our comment. It is discouraging to me as someone who appreciates measured, academic discussions to have our comment dismissed on this basis. There was no intent on our part to personally offend a reader, but it seems the reviewer may have experienced the information in that fashion.

Nor do I find the reviewer really interacting with the substance of our argument. If we are really incorrect in our view, for example, that most if not all of the SOCE research purporting harms does not account for pre-SOCE levels of distress, then that should be easy to prove in a sentence by citing the literature that does precisely dispute our claim. Nothing along these lines is offered. Instead, in what appears closer to actual derision, the reviewer describes the

work of Sullins and others as “highly suspect, allegedly peer-reviewed research.”

Then there is the accusation that our intent (again reading our motives) is to “obfuscate” what “multiple professional associations” have condemned. However, our comment only made the minimalist claim that the SOCE literature has yet to arrive at a complete accounting of SOCE practices and there is a likelihood based on the emerging research we cite that some methods associated with SOCE may be experienced as beneficial by some sexual minorities. Again, appeals to authority, including mental health associations, do not constitute a substantive refutation of the concerns we expressed in our comment. This seems to be a tribal rather than scientific argument that is difficult to overcome, i.e., settled science is what organizations such as the APA say it is. In this characterization, science is not about exploring and potentially challenging conventional wisdom in a field, but rather about providing more evidence in favor of the APA’s position. What is sought is not a clearer and more nuanced picture of the truth, but rather a stronger bulwark for defending the practices and policy positions that have received the imprimatur of the APA. Is it scientifically likely that one side on such complex and controversial issue as SOCE has it completely correct and the other side has without exception gotten it wrong? To learn from each other, both sides have to be willing to listen to reasoned arguments and data.

5. Is the Comment written clearly? Partially. It is clear that the authors are trying to achieve a publication in the American Psychologist to detract from the APA’s stance on SOCE. However, there are parts of the letter that are unclear. For instance, the authors write, “We have no doubt that certain SOCE practices are harmful

to sexual minorities generally, and we have no interest in defending such activities.” The author should offer examples of what they deem as harmful SOCE practices. It is unclear to the reader what kinds of practices they are referencing because all forms of SOCE are condemned by multiple professional associations. If the authors believe there are nuances of SOCE and some practices that are indefensible while other are defensible, then they should provide clear examples of what they mean. Additionally, the authors write, “However, Sullins’ work along with other recent studies suggest there remains . . .” The authors should supply citations of these other recent studies.

Due to the space limitations of a comment, we did not offer an example of a harmful practice, but this would have been easy to do, e.g., aversive cognitive and behavioral techniques. Similarly, it would have been simple to give examples of potential beneficial SOCE practices, such as behavioral reductions in same-sex behavior or choosing not to identify as LGB. These complaints seem fairly trivial and ones that would normally result in a request to make minor revisions rather than an absolute rejection. The “other recent studies” referenced were those I have conducted (Rosik et al., 2021; Rosik et al., 2022), and since the review process required a blinded manuscript, I had to indicate these references were “masked for review.” Hence, this was another unfortunate double-bind, though again easily fixable and hardly a convincing rationale for rejection over revision.

6. Is the tone of the Comment constructive and collegial? It is collegial, but I did not find it

constructive. The authors do not suggest what they think the Guideline should be or whether the APA should reverse its stance on SOCE. Their suggestion of “sociopolitical diversity” is not constructive because it is unclear what journals are to do about this. The authors are using claims of “sociopolitical diversity” and “adversarial collaboration” to manufacture opportunities to peddle support of SOCE within journals that have actual rigorous peer review.

For the record, I am in agreement with a lot of the APA’s position on SOCE that makes for good clinical practice, such as not overstating the claims of change, not guaranteeing orientation change, determining client motivations for change, looking at the impact of discrimination and minority stress experiences, etc. Our comment was again only asking the APA to consider the possibility Guideline 4’s universal and non-specific characterization of all SOCE methods as harmful, for all sexual minorities may need to be reconsidered. We had neither the word space nor the inclination to challenge anything more about the Guideline.

In addition, it is hard not to experience the terminology of “manufacture opportunities,” “peddle support,” and “actual rigorous peer review” as being contemptuous, but I cannot definitively read the reviewer’s mind. I would say that the reviewer seems to have completely missed the point of advocating for sociopolitical diversity, which is the admission that ideological monocultures make for suboptimal science due to such well-established problems as confirmation bias, groupthink, and motivated reasoning. This seems to me to be a constructive recommendation. What journals dedicated to improving replicable science can do is to encourage and publish research on SOCE that reflects such diversity, either between or

within the research articles it features. I fear this may no longer be part of the definition of “rigorous peer review” as pertains to SOCE within APA journals, regardless of scientific merit.

7. How likely is it that the Comment will be cited in future publications? I think it is likely this will be cited in future open access publications that seem to be the premier venues for publishing studies that support SOCE. I strongly believe this letter will find its way into the policy arena as well, as advocates of SOCE continue to fight against SOCE bans under the ruse of “patient choice.” They will undoubtedly trumpet a letter in the American Psychologist because it seems that most of their other work cannot find homes except for paid open access journals.

The reviewer is of course correct in assuming the recent research our comment alludes to will be finding its way into the scientific and policy conversations about SOCE. We are committed as social scientists to assisting in this endeavor, both for the advancement of scientific truth as well as to protect the interests of traditionally religious and other sexual minorities who are being prohibited from finding professional care in which to explore the fluidity of their same-sex attractions, behaviors, and identities and/or their experience of gender.

It is a concern that something as foundational to psychotherapy as patient choice can be placed in scare quotes and summarily dismissed as a “ruse.” Also disconcerting is the implied disparagement of open access journals, i.e., that research published in these journals is not as scholarly as that found in APA-related journals, since authors pay to have their work published in

the former (this is done in order to offset costs associated with the publishing service and make articles free and immediately available to anyone in the world). I have heard this viewpoint expressed before in efforts to delegitimize research in open access journals. I consider this a form of *ad hominem* argument, only in this instance not against a person but rather a publication medium. What is similar is that the reviewer's criticism does not address the quality of the argument (in this instance Dr. Sullins' research), but dodges that responsibility by attacking the messenger (i.e., open access journals).

It is also telling that the APA often cites open access journals in their official documents. In fact, the full version of the *APA Guidelines for Psychological Practice with Sexual Minority Persons* includes three citations from *PLOS One*, which describes itself as a peer-reviewed open access scientific journal (i.e., Fitzgerald-Husek et al., 2017; Flynn et al., 2017; Friedman et al., 2014). Furthermore, the one open access journal we did cite in the comment was for Sullins' study published in *Frontiers of Psychology*, which in 2020 had an impact factor of 2.99 (an impact factor is the average number of times articles from a two-year timeframe have been cited in indexed journals). This number is higher than 43.3% (26/60) of the impact factors associated with APA-affiliated journals that listed this statistic for 2020. The quality of research published in many open access journals seems to stand up quite well to comparisons with non-open access journals such as those published by the APA, the reviewer's disparagement notwithstanding. If indeed journals published by mental health associations serve gatekeeping functions in arenas such as SOCE where these associations have established strong ideological and policy commitments, then one can expect open access journals to be the

primary publishing venue for research that brings new insights and clarity to these topics.

Reviewer #2: *This article is a comment on the Nakamura et al. (2022) American Psychologist paper that provides an executive summary of the 2021 revision of the APA Guidelines for Psychological Practice with Sexual Minority Persons. The comment focuses on Guideline 4, which asserts that psychologists understand that sexual minority orientations are not mental illnesses and that efforts to change sexual orientations cause harm. It is the latter point that the authors challenge.*

The authors' argument that sexual orientation change efforts (SOCE) are not harmful rests on an unpublished article that has apparently not been peer-reviewed (Sullins, 2021). The authors' argument is therefore weakened.

As I noted previously, our argument is perhaps weakened by the fact Sullins' reanalysis has languished in the peer review process for over a year. Weakened is not the same as nullified. Given the limited timeframe for submitting a comment following publication of the target article, we had no real choice but to proceed ahead of the peer review process for the reanalysis. However, I am sure that these reviewers are able scholars who could easily critique a reanalysis and point out the serious flaws, should they exist. The fact that the reviewer bases the rejection of the comment predominantly on the basis of a lack of peer review of Sullins' reanalysis is taking the easy way out. A critique and rejection primarily on the merits of the research would seem a much more sound and

convincing basis for the reviewer's judgment.

Moreover, new research studies emerge every year documenting the harmful effects of conversion therapy. Here are several recent ones:

Forsythe, Anna et al. (2022). Humanistic and economic burden of conversion therapy among LGBTQ youths in the United States. JAMA Pediatrics, 176(5), 493–501. <https://doi.org/10.1001/jamapediatrics.2022.0042>

Higbee, Madison et al. (2020). Conversion therapy in the Southern United States: Prevalence and experiences of the survivors. Journal of Homosexuality. Advance online publication. <https://doi.org/10.1080/00918369.2020.1840213>

Przeworski, Amy et al. (2021). A systematic review of the efficacy, harmful effects, and ethical issues related to sexual orientation change effects. Clinical Psychology: Science and Practice, 28, 81–100. <https://doi.org/10.1111/cpsp.12377>

These studies deserve a more complete critique than I can provide here, but a few observations in the present context are in order. The Forsythe et al. (2022) study commits the very methodological errors our comment sought to bring to light. First, the study's participants were all LGBTQ-identified individuals. Second, the authors repeatedly cite Blosnich et al. (2020) in their arguments. They do acknowledge the possibility of selection bias such as that discovered in Sullins' reanalysis of Blosnich et al., i.e., that preexisting distress leads to the pursuit of SOCE. However, they dismiss this by asserting “. . . such an argument assumes that they freely seek SOGICE” (sexual orientation and gender identity change

efforts; p. 499), adding, “With such prevalent pressure to change orientation or identity, it is unlikely that LGBTQ individuals who undergo SOCE differ from their peers except for the extent of the pressure or coercion they receive” (p. 499). Of course, Sullins' reanalysis, based on a nationally representative sample obtained through the gay-allied Williams Institute at UCLA, clearly takes precedence over the speculations of Forsythe et al.

In fact, as a third and final observation, the Forsythe et al. paper is rife with speculation. The authors confess, “The model made several assumptions because of the limited availability of data” (p. 494), “. . . including that the risks of adverse outcomes was the same across different sexual orientations and gender identities and for various SOGICE modalities” (p. 499). This includes the lumping together of religious and licensed mental health providers (74% and 26% of the sample, respectfully), as well as the conflation of electroconvulsive practices with contemporary speech-based therapies. I counted the presence of at least 16 different assumptions embedded within their model. This raises a real risk of modeling that ultimately is more reflective of conjecture than established, real-world facts.

The Higbee et al. (2020) study commits the same error of including only sexual minorities who are LGBTQ-identified, which eliminates by definition those who may have had beneficial experiences with sexual attraction fluidity exploration and hence did not adopt such a sexual identity label. Moreover, the authors were exceedingly conscious of their decision to exclude those who did not identify as LGBQ. They acknowledge, “. . . we chose to only include sexual orientation in our analysis because the other variables often measure individuals who identify as heterosexual but engage in same-sex sexual activity rather than individuals with a *solidified* LGBQ+ sexual

identity” (p. 8, my emphasis). This exclusion likely places severe limitations on interpretation of the study’s results.

Another concern is the authors’ causative assumptions from correlational data, concluding that, “The finding that respondents who undergo conversion therapy before age 18 are significantly more likely to experience serious mental illness further substantiates the scientific consensus around conversion therapy as a psychologically harmful practice” (p. 13). This is precisely the kind of erroneous thinking that Sullins’ reanalysis so effectively challenges, as Higbee et al. have no idea of the pre-SOCE distress levels of their participants. Other problems with this study are fairly “run of the mill” for this literature, such as a definition of conversion therapy that almost exclusively highlights obsolete practices such as using hypnosis to induce vomiting and paralysis, administering electric shocks, and “corrective rape.”

Finally, Przeworski et al. (2020) offer a systemic review of the SOCE literature. As noted by Schumm et al. (this issue), Przeworski et al.’s review primarily relied on studies that were very old and/or based on small samples, which is a poor basis for drawing conclusions about contemporary forms of sexual attraction fluidity exploration in therapy (SAFE-T). The research in this review of SOCE is thus subject to the same methodological concerns we were attempting to address, i.e., a lack of assessment for pre-SOCE distress. To a significant degree this review is already outdated and needs to take into account the research we refer to in our comment. To summarize and return to the second reviewer’s feedback, it appears all three of these papers serve effectively to highlight our concerns rather than refute them.

Overall, then, the authors of this comment use a single, unpublished

study to challenge a single study supporting the assertion that SOCE are harmful. The evidence base, however, does not rely on that single study, but instead rests on multiple studies, all of which replicate the finding that SOCE are harmful.

We actually cited two studies (while others were masked), and the second was the aforementioned reanalysis published in *Frontiers of Psychology* and dismissed (but at least recognized) by the first reviewer. The main point of our comment remains. If the SOCE evidence base is largely suffering from at least a few very serious methodological oversights, it does not by virtue of its sheer volume or consistency in potentially faulty findings negate those concerns. Nowhere does the reviewer challenge the new research on empirical grounds but relies instead on an implied version of the “settled science” argument. I think this is most unfortunate for furthering an understanding of SOCE that better represents the experiences of ideologically and religiously diverse sexual minorities.

Conclusion

I have attempted to give the reader an opportunity to come inside the inner sanctum of the academic peer review process through a detailing of my experience with the *American Psychologist* peer review process. While I definitely have an opinion, the subjective aspects of the process mean I cannot be definitively certain our comment was rejected out of a gatekeeping function rather than a lack of scholarly quality. Unfortunately, the failure of the reviewers to interact with the merits of the specific methodological issues we raise is reason for concern. It may take significantly more time and effort before our research findings gain sufficient traction to be taken seriously, but I

remain hopeful that this endeavor will have an impact. The integrity of the science around change-allowing therapies and the necessity for sexual minorities to have the option of exploring their sexual orientation and/or gender fluidity is at stake.

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