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Commentary on Rosik et al. (2026)

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I enjoyed reading this study on SOCE. I'll say upfront that this isn't a primary area of specialty for me, though I follow the various debates around sex, sexual orientation and gender with some interest. I felt that, overall the authors gave a fair overview of prior studies without personal bias.

I wondered...it has seemed to me that the opposition to SOCE is typically rooted in the belief that sexual orientation is immutable (which I generally buy). At the same time, and largely from the same "side", I sometimes hear arguments about the fluidity of sexual orientation (edit: the authors themselves refer to this later in the discussion), kind of the "love the one you're with" approach, and how people are perhaps more bisexual than we often admit. Again, I'm not sure I buy that argument, but does there seem to be some disconnect between making that argument and opposing SOCE? If sexual orientation is fluid...shouldn't SOCE work?

I'm curious too...there seems to be an assumption in the opposition to SOCE that all "conversion therapy" is the same. But I could see some differences between a gentle, client-guided approach as opposed to a "you're going to hell if you don't..." approach. Granted, perhaps no approach works, but I was curious if there's been any thinking around this?

I appreciated the adversarial collaboration in this case. The "research team" section gets a little close to a "positionality statement", which I generally see as pointless, but I think it kind of makes sense for this particular article.

Regarding effect sizes, and with some admitted hubris, I wouldn't use Cohen's suggestions as they are too generous. This is a wide-standing problem in academic psychology as we are too easy on ourselves, and use these horrible relative effect size interpretation guides that make us sound better than we actually are. Use Ferguson (2009, Professional Psychology: Research and Practice which very specifically speaks to clinical relevance as that comes up in the discussion)'s non-relative effect size interpretation guide instead. Sorry for the self-citation. If you've got another source with similarly rigorous recommendations go for it (but not Cohen and not, for the love of God, Ozer and Funder).

Effect sizes again, there's a problem with larger samples (and, yes, yours is large enough) with noise effects sometimes becoming "statistically significant". So, I wouldn't rely on p-values. Instead, I would use a smallest effect size of interest (SESOI) or $r = .10$ in order to avoid false positives ($r = .20$ is probably minimal for "clinical significance", see Ferguson, 2009 above). No need to cite this, but Ferguson & Heene (2022) explains how this happens with simulations and why a SESOI is needed. I'd report your eta squared out to the third decimal as it's hard to tell if those .01s are just above or just below that SESOI. Either way, I think I'd be more cautious in interpreting those as hypothesis supportive. We've got a real bad habit of using incredibly weak (and possibly false positive) findings as "hypothesis supportive" and that's resulted in widespread miscommunication both within and outside of the field (and unfortunately even the open science folks are guilty of this). Basically, if your eta squared is actually .014 I'd say go ahead and proceed albeit with caution (and it still may not be clinically significant even if it's not likely a noise false positive finding), but if you rounded up from, say .009, I would not interpret that as hypothesis supportive even if $p < .05$.

For the section that begins "Sexual Identity Labeling, Pre-Existing..." please put effect sizes in the text. I had difficulty parsing them from the table. For things like Chi Square they can generally be converted into r for ease of understanding. Again, watch your descriptors (don't use Cohen's recommendations...I'm guessing none of these would be a "medium" effect size as per Ferguson, 2009). And, again, watch the SESOI.

My observation of the discussion is that it makes the mistake so many social science papers do...it once again ignores effect sizes. Note: I have no stake in this particular issue. But I worry generally when social science papers find weak effect sizes, which may or may not be false positives due to statistical noise, and immediately switch to the binary and emotional language of "harm" and "damage" which, I think, is emotive and misleading perhaps even outright manipulative, not strictly scientific.

Let's assume for the moment that none of your findings are noise (all are reasonably above the SESOI), but they are all small effects (per Ferguson, 2009). Would it not be possible to say that SOCE treatments may be more likely to result in negative than positive outcomes (which would have been my a priori stance, FWIW), but that these negative effects are actually much more modest than people have thought (for instance, the reference to "torture" the authors mention in the literature review). I think, again taken at face value and ignoring the potential "noise" issue, that there's an argument that SOCE treatments are pretty much useless, but at the same time some of the narrative around them being "dangerous" may actually be overblown. Even just saying SOCE is useless seems like a good reason not to advocate it for me. Starting to talk about "harm" and "damage" I think risks making the authors here sound shrill...or at least typical of the effect size ignorance that predominates social science and, perhaps at least as much as the "replication crisis" keeps psychology as a pseudoscience of low reliability and deservedly low repute.

I do appreciate the authors say "Finally, we note that although we found statistical significance between our groups, the group differences may not be clinically significant." But this should be a central argument, not a "finally" after language about harm and damage.

I appreciate the authors talk about their process. I think that's helpful for other research teams considering similar projects. Note again where the authors say "In the present study, for example,

we experienced differences over the degree to which statistical versus clinical significance should be stressed in the interpretation of our results.” The reality is by this point we know that “statistical significance” is prone to high error as well as miscommunication (we’ve literally known this for decades at least since the APA’s Wilkinson task force in 1999). Thus, I’m weighing in on the side of the “clinical significance” folks here, as I would for any manuscript on any topic (and with no attachment to SOCE on either side). You really need to focus almost exclusively on clinical significance, even more so for an article that is very specifically related to clinically significant arguments, and I don’t think I could endorse publication of this article without that (again, same as I would argue for any topic). Basically, you can’t use words like “harm” and “damage” while minimizing any critical thinking about whether your results are clinically significant or now (and you very clearly minimize any such critical thinking in this current draft).

The discussion is very, very, very, very, very, very, very long (I admittedly began to skim). Most people aren’t going to read that. Cut it down, focus on clinical significance, and keep your arguments succinct.

Lastly, I read your note included with the submission. I think it’s gross the way you were treated. But sadly typical. I’m sorry that happened. Even though I just asked you to cut down your discussion...I would like to see you discuss this whole episode for a paragraph or two in your discussion. I think it’s an important part of the record. From what I can see, I agree you should likely have been offered the opportunity for a correction, not a retraction. I’m beginning to think we’ve become too retraction-happy, particularly with controversial topics and these are being used for scientific censorship.

I hope these comments are helpful.