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COMMENTARY

Critiques Strengthen and Improve the Original Findings: Rejoinder to Commentaries on Sullins (2022b)

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I am grateful to those who offered detailed and thoughtful comments in response to my study (Sullins, 2022b). Rosik's (2022) hopeful call for renewed scientific rigor in the study of sexual orientation change efforts (SOCE) contrasts sharply with comments from four other teams of scholars, each of which I respond to individually below. Blosnich et al.'s (2023) and Rivera and Beach's (2022) detailed analyses have helped to sharpen my thinking and to present revised and alternative results that confirm and state more strongly those of my original study. Haldeman and Glassgold's (2023) egregious misrepresentation of the research literature has allowed me to correct the false research narrative a little more fully in response. Strizzi and Di Nucci's (2022) appeal to ethics to call for censorship of positive or even neutral SOCE findings has enabled me to restate the perhaps forgotten ethical principles involved—principles which affirm that ending therapeutic discrimination against those who seek to desist from an unwanted sexual orientation is important for ending discrimination against all sexual minority persons. Together, the critical responses of these scholars illustrate the strong set of interests and institutions that may likely prevent Rosik's (2022) proposed awakening. Despite the demonstrable evidence of rational and ethical fallacies that underlie the false narrative that SOCE is invidious and harmful, scholarly discrimination against SOCE therapies seems poised to continue for some time to come.

Response to Blosnich et al. (2023)

Ironically, after a detailed analysis proposing improvements in my timing of SOCE exposure relative to suicidality, Blosnich et al. (2023) insist that the "Generations data do not allow timing of SOCE exposure." They allege that I created this variable, yet earlier in their Commentary they reported that the dataset included the question, "About how old were you the last time you received treatment to change your sexual orientation?" (Blosnich et al., 2023; see Meyer, 2020, p. 194) This question elicited a variable reporting one's age at last SOCE treatment, which yields information about the timing of SOCE exposure. Just because the data do not tell us when SOCE began doesn't mean that the information on when it ended does not exist and cannot be used to make reasonable estimates regarding the relative timing of SOCE and suicidality. Blosnich et al. (2023) demonstrate that it can be done by actually doing it, at length, in their Commentary. They can't reasonably have it both ways. Either the data *do not* allow timing of SOCE exposure, in which case they have no basis to critique the flaws they see in my attribution of timing, or else they *do* allow timing of SOCE exposure, even if imperfectly, in which case they should have accounted for such temporality in their original study. Clearly the data do include a question on timing, of which they make good use to critique my categorizations, but did not disclose in Blosnich et al. (2020). At this point, I must agree with Blosnich et al. (2023) that "[r]esearchers ought to use the data that are available" and not pretend that variables they may wish were not there did not exist.

Remarkably, Blosnich et al. (2023) persist in their refusal to recognize the necessity of time order to establish causation. They state that establishing temporal order would permit "more accurate causal inferences" compared to "lifetime associations," implying against all reason that the latter are also somehow causal. By their backwards logic, lung cancer could cause habitual smoking. They even go so far as to advise that I misclassified suicide attempts that predated

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SOCE as not attributable to SOCE when the respondent reported a later attempt following SOCE. They do not seem to comprehend that the first attempt, before SOCE, could not possibly have resulted from SOCE, no matter what disposition one makes of the subsequent attempt. The presence of a pre-existing suicide attempt, moreover, makes it less likely, not more likely, that the subsequent attempt is attributable to SOCE.

On the other hand, the authors do present a convincing case, based on a review of the SOCE literature, that it would take a lag of up to four years of age before the last SOCE treatment, rather than the one year (12–24 months) that I used, to reasonably "indicate a probable pre-SOCE suicide attempt." I take their point and appreciate the correction regarding the probable duration of SOCE; however, this difference in measurement is hardly a "fatal flaw that renders the conclusions of [my] paper invalid" (Blosnich et al., 2023). The classification in question affects only one of the three models presented as alternatives to Blosnich et al.'s (2020) original analyses (Treatment Initiation Model, Table 2), and revising the "before SOCE" category as they recommend in that model does not change any of my conclusions regarding the invalidity of Blosnich et al.'s (2020) conclusions regarding SOCE and suicide. Table 1 presents the models revised according to Blosnich et al.'s (2023) recommendation.

For brevity, I will confine the discussion to the three forms of suicidal morbidity included in Blosnich et al. (2020): suicide ideation, suicide planning, and suicide attempts. As already noted, I categorized a first suicide attempt prior to SOCE as "before SOCE" regardless of any subsequent attempts. Beside the reason I noted, this categorization was consistent with Blosnich et al.'s (2020) own categorization, which collapsed all multiple suicide attempts into a single one, without addressing time span between attempts. Revising the duration of SOCE to less than or equal to four years before the last SOCE experience reduced the "before SOCE" category for suicide attempts to 13 cases, not 11 as Blosnich et al. (2023) reported; this does include two persons who reported a subsequent suicide attempt at the same age that they completed SOCE, which Blosnich et al. (2023) may have inadvertently counted as excluded due to SOCE time span. Expressions of suicide ideation "before SOCE" were reduced from 58 to 39, and suicide planning from 36 to 24. After these revisions, only instances of suicidality expressed at an age at least four years less than the respondent's age at the last SOCE exposure were considered to have probably occurred before SOCE began, as Blosnich et al. (2023) recommend.

Row 3 of Table 1 shows the effect of this revision on the Treatment Initiation Model (Table 2, Model 2) in my paper (Sullins, 2022b). This model most closely replicated Blosnich et al.'s (2020) models, adding only a consideration of time order relative to SOCE. For reference, Table 1 also presents Blosnich et al.'s (2020) results (Row 1) and the unrevised findings from Sullins (2022b) (Row 2). For all three forms of suicidality examined, the revised risk estimates were indeed larger with the revised model (Row 3) than with the

Table 1 Adjusted odds ratios (AORs) for suicidality by experience of sexual orientation change efforts (SOCE): Probability sample of sexual minorities, USA, 2016–2018 (N = 1,518)

	Suicidal Ideation AOR or % (95% CI)	Suicide Planning AOR or % (95% CI)	Suicide Attempt AOR or % (95% CI)
"Before SOCE" N (4 year span)	39	24	13
1. Per Blosnich et al. (2020) All lifetime suicidality	1.92 (1.01, 3.64) *	1.75 (1.01, 3.06) *	1.75 (.99, 3.08)
2. Treatment Initiation Model – (Table 2) per Sullins (2022a, 2022b)	.72 (.35, 1.50)	.88 (.49, 1.56)	.96 (.49, 1.90)
3. Treatment Initiation Model – (Table 2) per Blosnich et al. (2022) (4 year SOCE duration)	1.01 (.52, 2.00)	1.13 (.64, 2.00)	1.25 (.67, 2.36)
4. Treatment Initiation Model – (Table 2) with 6-year SOCE duration	1.27 (.65, 2.47)	1.46 (.81, 2.60)	1.57 (.87, 2.81)
5. Improved Model (Table 6) –	.84 (.49, 1.43)	.82 (.50, 1.35)	1.12 (.61, 2.05)
6. Relative risk of suicidal expression progressing to a suicide attempt(s) with intervening SOCE (Table 9)			
All SOCE	.20 (.05, .74) *	.13 (.03, .56) **	.58 (.11, 2.97)
SOCE as minor	.70 (.13, 3.86)	.65 (.06, 6.57)	–
SOCE as adult	.07 (.01, .34) **	.05 (.01, .43) **	.63 (.09, 4.32)

Odds ratios were estimated from population-weighted logistic regression models, as described in the indicated tables in Sullins (2022a, 2022b). AOR significantly different from unity, by *t*-test: *$p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; **** $p < 0.0001$. Unless otherwise indicated, in all models only suicidality expressed at least 4 years before the respondent's age at the end of SOCE is classified as having occurred before SOCE

Table 2 Odds ratios for lifetime (Model 1) and past six year (Models 2–4) suicide ideation by SOCE exposure, estimated from propensity score matched samples: Probability sample of sexual minorities, USA, 2016–2018 (N = 1518)

	Model 1 – per Blosnich et al. (2020)	Model 2 – per Sullins (2022a, 2022b)			
	Odds ratio (Logit coeff., *p*-value)	Odds ratio (Logit coeff., *p*-value)	Odds ratio (Logit coeff., *p*-value)	Odds ratio (Logit coeff., *p*-value)	Odds ratio (Logit coeff., *p*-value)
Outcome:	Lifetime suicide ideation	Suicide ideation in the past 6 years	Suicide planning in the past 6 years	Suicide intention in the past 6 years	Suicide attempts in the past 6 years
ATT	1.10 (.0915, .0688)	0.90 (−.1037, .0641)	.88 (−.1301, .0086)	.91 (−.0894, 0368)	0.98 (−.0163, .6425)
ATU	1.11 (.1081, .0000)	0.86 (−.1472, .0000)	.87 (−.1358, .0000)	.91 (−.0938, .0000)	0.99 (−.0093, .3050)
ATE	1.11 (.1072, .0000)	0.87 (−.1447, .0000)	.87 (−.1354, .0000)	.91 (−.0936, .0000)	0.99 (−.0097, .2695)
Compounding Model (Table 2, Model 4)	–	0.92	.86	.74	.93
N (total; treatment; matched control)	1451/82/427	1451/82/427	1457/82/419	1457/82/419	1457/82/419
Mean standardized difference	.032	.032	.031	.031	.031
Cases excluded from common support	0	0	0	0	0
Variables with variance ratio > 2	0	0	0	0	0
Rubin's B	15.0	15.0	15.7	15.7	15.7

Values report population-weighted logit estimates comparing treatment and control groups. N, number of unweighted cases; SE, standard error; ATT, average treatment effect on the treated; ATU, average treatment effect on the untreated; ATE, population average treatment effect. "Significance test *p*-value" corresponds to a *t*-test of significance, i.e., that the coefficient is equal to zero; ATU and ATE tests report estimated variance. Matching made use of the following covariates: the sum of ACEs, education, sexual minority identity, sexual identity, race/ethnicity, and age. Persons who completed SOCE less than 7 years ago (n = 25) were excluded

unrevised model (Row 2); however, just as in the unrevised model, they were not significantly different from 1, thereby indicating no determinable association. Even expanding the presumed duration of SOCE to six years did not alter this result, as Row 4 demonstrates. This model (Row 4) assumed a duration of SOCE two years longer than the four years that Blosnich et al. advised would reasonably indicate a probable pre-SOCE expression of suicidality. This stricter classification further reduced suicide attempts "before SOCE" to just 6, instances of suicide planning to 17, and suicide ideation to 31. In sum, revising the category "before SOCE" as Blosnich et al. (2023) recommend, and even more strictly, did not alter the conclusion of my paper (Sullins, 2022b) relative to the claims of Blosnich et al. (2020), namely, "sexual minority persons were at no greater risk of initiating any of these forms of suicidality following or during SOCE than were those who had not experienced SOCE" (Sullins, 2022b).

Likewise, using Blosnich et al.'s (2023) recommended imputation of suicidality before SOCE did not materially alter the findings of the Improved Model (Table 6 of the original study Sullins (2022b)), which more fully adjusted for childhood differences between the SOCE and non-SOCE groups than did Blosnich et al. (2020), nor of the risk of progression to one or more suicide attempts following an initial expression of suicide ideation and/or planning (Table 9 of the original study Sullins (2022b)). The latter was still sharply lower with intervening SOCE than with no SOCE, just as the unrevised findings showed in my study (Sullins, 2022b).

It is not surprising that such sharp reductions in the presumed number of suicidal expressions before SOCE would still invalidate Blosnich et al.'s (2020) results, since those results were barely significant to begin with. The low end of the confidence interval for all AORs reported by Blosnich et al. (2020) was just 1.01. It is indeed strange that they would expend so much effort to show that presuming an increased duration of SOCE would reduce the number of suicide attempts classified as "before SOCE," since, as I reported in my paper (Sullins, 2022b) and show in Table 1 (Row 1), the overall risk of suicide attempts with SOCE exposure was not significantly elevated using their 2020 models to begin with. (Note: Blosnich et al. did not report overall suicide attempt risk. Table 1 presents my replication of that finding using the models reported in their paper, which yielded results identical to theirs for the overall suicide risks they did report.) For all suicide attempts (but not ideation or planning), one could theoretically reduce pre-SOCE suicide attempts to zero and the results would still contradict those of Blosnich et al. (2020).

In sum, the findings of my study (Sullins, 2022b) continue to invalidate the conclusions of Blosnich et al. (2020), even after revising the presumed duration of SOCE upwards to the 4 years they recommend in order to reasonably "indicate a probable pre-SOCE suicide attempt"—and even an additional two years beyond that. The conclusions of Blosnich et al. (2020) regarding the invidious harm of SOCE remain in the realm of contrived illusion, not observed reality, produced by their failure to apply the principle of causal time order, i.e., that a result cannot reasonably be attributed to a cause later in time. Their denialism regarding the Generations data's measure of SOCE timing—maintaining that the evidence either does not exist or cannot be used to do what they themselves use it to do in their Commentary—compounds the contrivance. While refusing to consider causal time order or acknowledge the evidence in front of them, their comment claims that they used "conventional statistical approaches" and the "data that are available." In reality, they did neither of these things, which renders their findings both false and misleading regarding the putative harm from SOCE therapy for sexual minority persons.

Response to Rivera and Beach (2022)

Rivera and Beach (2022) arguably present a more comprehensive refutation of Blosnich et al. (2020) than I did. They affirm the main point of my study (Sullins, 2022b), i.e., that "Blosnich et al.'s approach failed to properly account for temporal relationships," but argue that my use of the Blosnich et al. (2020) models was undermined by methodological problems. Eventually, they appear to realize that much of their critique would, if correct, also undermine Blosnich et al.'s (2020) analysis. I only replicated Blosnich et al.'s models in order to offer an "apples to apples" comparison, and have published the main point of my study using different, much simpler methods (Sullins, 2022a), so I could simply let Blosnich et al. (2020) defend their own methods. However, Rivera and Beach's critique is emphatically not correct, so I will offer a few words in defense of both Blosnich et al.'s (2020) methods and my replication of them.

Rivera and Beach (2022) present a "straw man" argument that grossly misrepresents my study (Sullins, 2022b), the data, and the supposed superiority of counterfactual analysis. Rivera and Beach misrepresent the "key results" of my study to be that "SOCE had a protective effect against suicidal ideation." Nowhere do I suggest that reduced suicidal ideation is a key result of my paper. As both the title and the abstract clearly state, the key result is that "experiencing SOCE does not result in higher suicidality." I also presented evidence that suggests that SOCE may reduce suicide attempts (not suicide ideation) in some circumstances, but this is not key to the purpose of the paper, which was to rebut the false narrative that SOCE induces higher suicide risk.

Rivera and Beach (2022) compound their misunderstanding in an extended critique of Model 2, Table 2 (Treatment Initiation Model) of Sullins (2022b), which they criticize for at-risk period bias by "counting only post-SOCE suicidal ideation among those who experienced SOCE" (Rivera & Beach, 2022). But Model 2 is only a preliminary model, even in that table. I myself criticized it for not addressing "the possibility that … suicidal behavior may also have been caused by the experience of SOCE therapy" (Sullins, 2022b) and then presented two subsequent models (Models 3 and 4) that progressively address time differential, the last of which (Model 4) included all SOCE participants "whether or not they expressed suicidality prior to SOCE." Rivera and Beach ignore this model, which has little or no differential at-risk period bias, and thus is untouched by their critique.

Even Rivera and Beach's (2022) analysis of the one model they did examine (Model 2, Table 2 of Sullins (2022b)) ignores contrary evidence that undermines much of their exposition of the at-risk period bias in that model. First, Rivera and Beach do not consider the fact that both Blosnich et al. (2020) and myself reported age-adjusted risks, which effectively, by design, equalizes most of any age-related at-risk period bias after age 17. Second, their critique assumes a constant risk of suicide ideation over the life course (including apparently in infancy), when this is manifestly not the case. A glance at the histogram for age at suicide ideation (Fig. 1) shows that suicide ideation risk was highly concentrated among the young (and nonexistent before age 5), not evenly distributed by age as they assume. While the median age at first suicide ideation for the non-SOCE group was just 14 (which Rivera and Beach report), it was a year higher (age 15) for the SOCE group, which significantly reduced the differential period, since the 90th percentile for both distributions was just age 22, not age 54 as Rivera and Beach imagine. Almost four-fifths (78%, SE 1.4) of reported suicide ideation occurred before age 18, the minimum age of the survey, thereby minimizing age-related risk period differences in the target model and survival risk bias in both Blosnich et al.'s (2020) models and my own. This

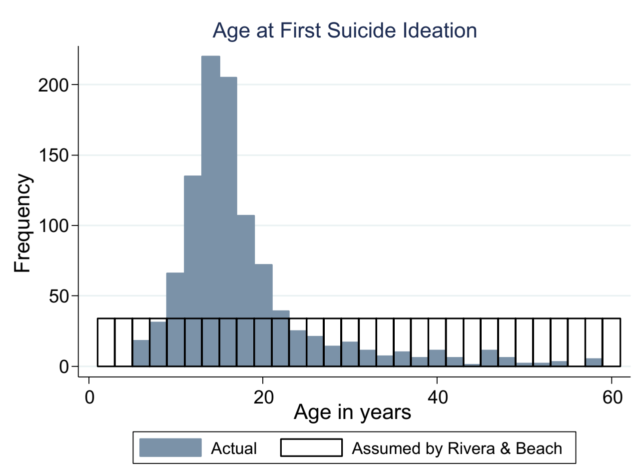

Fig. 1 Distribution of age at first suicide ideation: probability sample of sexual minorities, USA, 2016–2018 (N = 1518)

is not to deny that there is at-risk period bias in this model, which is why I presented better models following it, but only to make the point that such bias is not nearly as large a problem as Rivera and Beach suggest.

Rivera and Beach's (2022) insistence on the necessity of a counterfactual approach for my study (Sullins, 2022b) is emphatically not supported by the evidence, even the evidence they cite. Although this new method quickly became popular in medical studies in the 1990s, mostly using propensity score matching which mimics a random controlled trial, recently, more measured assessments have re-asserted the merits of traditional regression analysis. A review of 43 observational studies that analyzed "at least one association between an exposure and an outcome using both traditional regression and propensity score methods" found that both methods yielded the same results (not significantly different) 90% of the time. (Shah et al., 2005) A recent comparison of propensity score methods and covariate adjustment (standard regression) methods in four sets of observational data on cardiology treatment concluded that "propensity score methods are not necessarily superior to conventional covariate adjustment … which may be viewed as a suitable primary analysis method in many cases" (Elze et al., 2017, p. 366). Biondi-Zoccai et al.'s (2011) review of the question, which Rivera and Beach cite, likewise concluded that "propensity score methods are not meaningfully superior to standard multivariable approaches" (p. 738). The study also cautions against the "hype surrounding propensity scores."

Remarkably, given their censoriousness regarding the lack of a counterfactual approach, Rivera and Beach (2022) conceded that "multivariable adjustment can produce the same numeric estimate of effect or association as [counterfactual] approaches." They then faulted me for not using propensity score matching or a similar approach because "the newer approaches can perform better in situations with low events and high number of confounders…" But this is not such a situation. According to Biondi-Zoccai et al. (2011), the rule is that counterfactual models are preferred when the event per variable ratio (EPV) is less than 8. In the Blosnich et al. (2020) models I replicated, which predicted 1057 instances of suicidal ideation using six confounders, the EPV is 176. In the relatively small SOCE group alone, the EPV is 15. Biondi-Zoccai et al. (2011) explicitly refute Rivera and Beach's misplaced criticism and defend Blosnich et al.'s choice of method, which I replicated: "[L]ogistic regression (or Cox proportional hazard analysis) is the first choice approach when there are ≥ 8 events per confounder" (p. 738).

Further, Rivera and Beach (2022) are mistaken that counterfactual analyses must exclude post-exposure covariates. Numerous epidemiological studies match on current comorbidities that are related both to the probability of exposure and treatment outcome. Austin, a prominent counterfactual epidemiological methodologist, recently presented the following analysis as exemplary of best practices: the effect of high school noncompletion on lifetime mood or anxiety disorders by propensity score matching on an adjustment set that included current household income, urbanicity, employment status, smoking, and alcohol consumption, using retrospective cross-sectional data from the Canadian Community Health Survey (Austin et al., 2018)—all of which violates Rivera and Beach's self-declared, non-existent "rules" prohibiting the use of post-exposure covariates, varying at-risk periods, and cross-sectional data.

Despite their errors and misplaced arguments, however, the strongest counter-argument against Rivera and Beach's (2022) critique may be simply to agree with it. In addressing Blosnich et al.'s error, it made sense to restate their methods in my paper (Sullins, 2022b), but doing so was not essential to my argument. I could have made the same point with a counterfactual analysis, and for those who are convinced of the superiority of this method, I am happy to do so now. Table 2 presents findings for suicidal ideation from matched samples of SOCE (treatment) and non-SOCE (control) participants. To ensure a robust comparison, each SOCE case was matched with up to six non-SOCE cases nearest to it by propensity score within a distance no larger than two-tenths of a standard deviation (6 to 1 nearest neighbor caliper matching with replacement). To address temporal causation, both treatment and outcome variables were restricted to ensure that the latter occurred after the former: the outcome was restricted to suicide morbidity in the past six years, while the treatment group excluded those who completed SOCE less than seven years ago. This adjustment included 75% of SOCE participants and 67% of reported suicide ideation.

As Table 2 shows, the model that included lifetime suicide ideation without controlling for causal time order (Model 1), following Blosnich et al. (2020), predicted a 10% increased risk of suicide ideation with SOCE. By contrast, the model that was restricted to suicide ideation following SOCE (Model 2), accounting for causal time order consistent with Sullins (2022b), estimated a decline in suicide ideation risk of roughly 10%. The matching models also found reduced risk for suicide planning and suicide intention following SOCE, but not for suicide attempts. The resulting pattern of reductions in suicide risk following SOCE estimated by the matching models was similar to the results of the Compounding Model (Table 4, Model 2) in Sullins (2022b), which are included in Table 2 for comparison. Looking at the average treatment effect on the treated (ATT) from the counterfactual models, the odds ratio for suicide ideation following SOCE exposure was 0.90; the corresponding ratio from the logistic regression compounding model (Table 2, Model 4) in my original paper was 0.92; for suicide planning, matching estimated 0.88, regression 0.86; for suicide intention, matching yielded 0.91, regression 0.74; and for suicide attempts, matching yielded 0.98, regression 0.93. While none of the regression-based ORs were statistically significant, the matching models reported significantly reduced risk of suicide ideation, suicide planning, and suicide intentions following SOCE. Thus, the matching models suggest, even more strongly than in Sullins (2022b), that SOCE exposure results in reduced risk of suicidality in this population.

The counterfactual models presented in Table 2 met all the stipulations of Rivera and Beach's (2022) critique possible. All variables were fully balanced, as indicated by Becker and Ichino's (2002) *pscore* procedure using Stata. There was no difference in the at-risk period or survival risk between treatment and control groups. Although Rivera and Beach (2022) are mistaken that this is necessary in this class of models, all the model covariates reported conditions in childhood, substantially preceding SOCE treatment. Moreover, the models meet or exceed the other diagnostic metrics for acceptable matching models of this type. The 82 SOCE cases were matched with at least 419 non-SOCE cases, an average of 5.1 control cases for each treatment case, indicating a minimum of replacement. The mean standardized difference between treatment and control variables was just 0.03, well below the conventional 0.10 limit for such models. Rubin's B statistic indicated an acceptable match between the treatment and control group variance, well below the maximum permissible value of 25.

To illustrate the effectiveness of the sample matching, Table 3 presents the baseline difference between treatment (SOCE) and control (non-SOCE) groups for each independent variable before and after matching. Before matching, the *p*-values for *t*-tests of mean difference between the two groups ranged from 0.00 to 0.83, with seven significantly different characteristics. After matching, the difference *p*-values ranged from 0.71 to 1.0, with no significantly different characteristics. While the SOCE group was more male and experienced higher physical, emotional, and sexual abuse, parental IPV, and bully victimization in high school, after matching there was no significant difference between the SOCE and non-SOCE group on these characteristics. Matching also eliminated the age bias induced by the lookback restriction on SOCE exposure.

I am not suggesting that the counterfactual method presented here is superior to Blosnich et al.'s (2020) regression-based method, which I replicated in my study (Sullins, 2022b), nor that the matching estimates are more accurate. I think Blosnich et al.'s method is probably better, though each approach has its advantages. The point here is that, despite

Table 3 Comparison of baseline characteristics between treatment (SOCE) and control (non-SOCE) subjects in the original sample and in the propensity score matched sample

Variable	Original sample				Matched sample (caliper matching)			
	SOCE: Yes (82)	SOCE: No (1375)	Std Diff	Test $p>\|t\|$	SOCE: Yes (82)	SOCE: No (374)	Std Diff	Test $p>\|t\|$
	% or mean	% or mean			% or mean	% or mean		
Sex at birth (% male)	61.0%	45.7%	.31	.007	61.0%	58.5%	.05	.752
Age	42.0	36.3	.40	.001	42.0	41.2	.05	.736
Percent white	52.4%	62.2%	.20	.079	52.4%	54.5%	.04	.796
ACE: emotional abuse	81.7%	65.9%	.37	.003	81.7%	83.3%	.04	.786
ACE: parent incarceration	17.1%	13.4%	.10	.352	17.1%	18.9%	.05	.762
ACE: parent IPV	43.9%	31.8%	.25	.024	43.9%	43.9%	0	1.0
ACE: parent mental illness	50.0%	44.0%	.12	.286	50.0%	51.0%	.02	.897
ACE: physical abuse	56.1%	38.0%	.37	.001	56.1%	54.9%	.03	.876
ACE: parent substance abuse	56.1%	45.3%	.22	.056	56.1%	55.3%	.02	.917
ACE: parent divorce/separation	32.9%	34.1%	.03	.826	32.9%	34.4%	.03	.848
ACE: sexual abuse	62.2%	35.6%	.55	.000	62.2%	65.0%	.06	.707
Bullied in high school (1–4)	3.11	2.83	.27	.018	3.11	3.13	.02	.884
"Out" to most people in high school	14.6%	17.3%	.07	.533	14.6%	15.4%	.02	.885
Raised with no religion	12.2%	19.4%	.20	.108	12.2%	11.2%	.03	.841

SOCE, sexual orientation change efforts; "Std diff," absolute standardized difference (expressed in standard deviation units); ACE, adverse childhood experience; IPV, intimate partner violence. The propensity score matched sample was constructed using nearest neighbor matching on the logit of the propensity score with the six nearest matches within calipers of width equal to 0.2 of the standard deviation of the logit of the propensity score. Dichotomous variables are reported as percentages, continuous with mean and standard deviation. *t*-tests for the matched sample do not take into account that the variance is estimated. Values shown are for Model 2 the difference in analytical approach and corresponding differences in the actual estimates involved, the counterfactual models yield results that are very similar to those observed in the regression models presented in my study (Sullins, 2022b). When suicidality before SOCE was improperly included, estimated suicide risk following SOCE was elevated, but when pre-SOCE suicidality was not included, estimated suicide risk following SOCE was reduced. Rivera and Beach's (2022) contention that counterfactual models would lead to different results is simply mistaken. Whether demonstrated by means of regression models or counterfactual matching models, an examination of Blosnich et al.'s error regarding temporal causation leads to the same conclusion: exposure to SOCE does not increase suicide risk, and may even reduce it.

Response to Glassgold and Haldeman (2023)

The final two Commentaries to which I will respond do not engage the main point of my paper, but express concerns about the effect that my arguments may have on widespread institutionalized beliefs and declarations that SOCE is ineffective and/or harmful, and/or efforts to restrict SOCE, on the presupposition that my concerns about causal time order are false. This is, of course, not a presupposition I can share, nor is the requirement that a cause must precede an effect a disposable principle for those who want to assert the harmfulness of SOCE as a matter of scientific evidence. In view of this, a critique that reasserts, however forcefully, the body of beliefs and list of organizational resolutions based on the false research that has denied this principle—as do both Glassgold and Haldeman (2023) and Strizzi and Di Nucci (2022)—simply misses the point. No matter how many official reviews and pronouncements may concur, it is simply not the case that a suicide attempt made years before SOCE exposure can be a result of that future exposure. The fact that my study's findings, if correct, would falsify much of the correlational population evidence that claims that SOCE increases suicidal harm, may be understandably disturbing to those committed to those claims, but this does not constitute an argument against my study's findings. Referencing organizational resolutions also corrupts the scientific debate. If those organizations are truly scientific, their institutional resolutions should be downstream from the research process, and not be cited in an attempt to influence it.

Glassgold and Haldeman (2023) attempt to restate the narrative of SOCE harm and ineffectiveness in a Commentary that, instead, illustrates the bias and falsehoods that perpetuate it. They claim, for example, that I did "minimize the extensive SOCE literature on the risks of harms from SOCE." But this literature is not properly described as "extensive." Glassgold herself concluded, in the 2009 APA review of SOCE literature (American Psychological Association, Task Force on Appropriate Therapeutic Responses to Sexual Orientation., 2009, p. 83): "There are no scientifically rigorous studies of recent SOCE that would enable us to make a definitive statement about whether recent SOCE is safe or harmful and for whom" (p. 83). "No studies" is not "extensive." Since 2009, there have been just three or four population studies alleging suicidal harm from SOCE, all of which I reviewed in my study, minimizing nothing. My discussion in the paper agreed fully with that of Glassgold and Haldeman (2023) that "[t]he accumulation of population-based data verifying the harms of SOCE was…the impetus" for the APA's revised 2021 policies supporting SOCE bans. But while I raised the point to suggest that such organizations may want to reconsider those statements in light of the refuting evidence I presented, Glassgold and Haldeman raise the point to imply that my findings cannot be true because the APA policies based on those false earlier findings have already been authoritatively promulgated. As already noted, this is a case of the tail wagging the dog.

Glassgold and Haldeman (2023) also set out to "correct the narrative" regarding the efficacy of SOCE—an issue my paper did not address but which they see as related to harm—but instead they grossly misrepresented the narrative, to the detriment of SOCE, by means of conspicuous falsehood. Their Commentary asserted that "multiple extensive reviews" of SOCE research, including two by Glassgold, included "studies with strong experimental designs that could determine causal relationships between treatment and outcomes that found no experimental evidence of change in sexual orientation…" This remarkable statement explicitly contradicts what Glassgold (2022) concluded in her review of the SOCE literature last year: "I was unable to identify any methodologically sound studies to evaluate whether SOCE changes sexual orientation. For example, none of the published studies were experiments in which specific treatments were adequately tested" (p. 33). For earlier research, she restated the APA's 2009 finding that "substantial deficiencies existed in the design and analysis of research from the 1980s to 2008 (APA Task Force, 2009, pp. 26–35). Because of these deficiencies, none of the research from the 1980s to 2008 can make credible causal claims."

Here Glassgold and Haldeman's (2023) Commentary has disturbingly falsified the state of the evidence, to the detriment of SOCE, on the basis of an evidential claim which their own research has shown to be false. There have been no "studies with strong experimental designs that could determine causal relationships" of SOCE efficacy, as they falsely claim—a fact we know, if for no other reason, because Glassgold (2009, 2022) reported it in her reviews. Throughout their Commentary, Glassgold and Haldeman (2023) persisted in this false characterization of the research findings, referring at one point to SOCE's "demonstrated lack of efficacy," and, at another point, stating baldly as if it were a settled conclusion that "SOCE is ineffective." By the end of the Commentary, the lack of any "studies to evaluate whether SOCE changes sexual orientation" reported in Glassgold's (2022) review had become "extensive evidence that SOCE are not effective." On this openly false characterization of the state of the evidence, Glassgold and Haldeman (2023) came to the overwrought conclusion that further research on SOCE harm or lack thereof is an unnecessary "red herring" and that SOCE therapy should be coercively banned where possible. These conclusions are not merely unsupported by the evidence, fairly considered; they are based on untruth about the evidence.

To understand further the degree of misrepresentation taking place, it may be helpful to briefly examine Glassgold's (2022) review of recent SOCE literature. Under the heading of "Effectiveness," Glassgold reviewed the few studies on SOCE efficacy in just three paragraphs, the first sentence of which reiterates: "As noted earlier, I was unable to identify any methodologically sound studies to evaluate whether SOCE changes sexual orientation" (p. 33).

The remaining two paragraphs contrasted two studies which Glassgold interpreted as coming to different conclusions on the question of efficacy, and which received starkly different treatments from Glassgold. Jones and Yarhouse's (2007, 2011) study of mostly evangelical Christian SOCE alumni came to the guarded conclusion that "change of homosexual orientation appears possible for some" (p. 404) after 15% of their sample reported self-assessed change. Glassgold contrasted this study with three related publications from the same sample of Mormon SOCE alumni (Bradshaw et al., 2015; Dehlin et al., 2015a, 2015b), which tentatively concluded that their results suggested a "very low likelihood of modification of sexual orientation" (p. 391) after 3–6% reported changed sexual orientation. Four other studies were also mentioned briefly in passing but not discussed at length.

Glassgold evaluated these studies with extreme bias, systematically applying much higher standards of methodological rigor to studies that suggested that sexual orientation may change than to those that did not do so. For example, she rejected Jones and Yarhouse's (2011) use of qualitative coding of participant comments as a "subjective measure of change," but accepted and reported Bradshaw et al.'s (2015) findings, which used the exact same method. She rejected Jones and Yarhouse's (2011) sample design, a longitudinal study based on annual reassessments, as "unreliable" because, in part, a third of the sample was lost to follow-up after 6 years, but accepted that of Bradshaw et al. (2015), a retrospective study with 27% of the cases missing data. No mention was made of the fact that a 6-year longitudinal assessment is inherently more reliable, and accurate for measuring change over time, than is retrospective recall. Likewise, Glassgold dismissed Spitzer's (2003) retrospective study, which also concluded that some persons can experience a change in sexual orientation, due to unspecified "methodological limitations," but reported the findings of Bradshaw et al.'s (2015) study, which employed very similar retrospective self-report methodology.

Glassgold (2022) also dismissed Jones and Yarhouse's (2011) findings that "some participants felt the treatment had benefited them" for the odd reason that "impact (harm or benefit) of a specific type of effort is unknown" (p. 33). She did not explain why the lack of this level of detail would compromise this finding. On the other hand, she ignored Dehlin et al.'s (2015b) comparison of the effectiveness of a very similar range of SOCE efforts for the Mormon sample. This may have to do with the fact that Dehlin et al. (2015b) reported that "[t]he SOCE methods most frequently rated as effective were support groups, group retreats, psychotherapy, psychiatry, and group therapy." Thirty-nine to 48 percent of persons undergoing these methods rated them to be effective; 11 to 24 percent rated them "highly effective" (p. 100). Overall, at least a fifth of participants rated every SOCE method in the study as either "effective" or "highly effective" (p. 100). Remarkably, for a review supposedly focused on of SOCE effectiveness, Glassgold ignored these explicit effectiveness ratings, which are emphatically not consistent with her blanket conclusion in the Commentary that "SOCE is ineffective."

Most importantly, Glassgold's (2022) binary frame focused on sexual orientation change ignored the nuance and complexity of both sets of studies, whose findings were actually more complementary than contrasting. Both sets of studies agreed in finding that a small minority of persons self-assessed change in sexual orientation; that a higher proportion of persons did not perceive any change; that many of those who did not change sexual orientation attraction reported other benefits from SOCE; and that more persons reported benefit than harm. Pertinent to the present exchange, Bradshaw et al. (2015) found that a very small proportion (0.4%) of those receiving SOCE psychotherapy reported a suicide attempt, but over three times as many (1.3%) reported that SOCE helped them avoid suicide (p. 407). Both studies also reported that, in addition to those that reported a change in sexual orientation, a larger number of SOCE participants—23% of Jones and Yarhouse's (2011) sample, 42% of Dehlin's (2015a) sample—reported that they were helped by the SOCE experience to reconcile or manage their conflicting sexual attractions and religious convictions in various ways. Both sets of studies clearly stated that their non-random clinical samples cannot support the kind of general conclusions that Glassgold and Haldeman state in their Commentary, a limitation Glassgold noted in her review. These few, inconclusive studies form the bulk of what Glassgold and Haldeman's Commentary exaggerates by means of falsehood into "extensive evidence that SOCE are not effective."

Both Glassgold and Haldeman have done better work in the past, as I document for Haldeman in the next section; but the summary of the research on SOCE harm and effectiveness presented in their Commentary bears little relation to the actual evidence and a disturbingly negative relationship to the truth. It is disappointing, but perhaps should not be surprising, that a false research narrative would be perpetuated by falsehood.

Response to Strizzi and Di Nucci (2022)

Strizzi and Di Nucci (2022) denounce the knowledge that SOCE may reduce suicide as "irrelevant" and "secondary" and its publication as "egregiously problematic" and "unethical" because it may impede political efforts to restrict SOCE. Just as Blosnich et al. (2023) repudiated the necessity of causal reasoning, Strizzi and Di Nucci repudiate ethical reasoning that opposes their preferred outcome, on the grounds that the inconvenient truth thus revealed may harm the rights of a favored group. In their dubious ethical system, medical science should be based not on evidence but on political expediency, no matter how many more people may be put at risk of suicide. Evidence that challenges a widely favored political outcome, they assert, is "nefarious" and should be suppressed. If this view were to prevail, the imposition of such a test for orthodoxy on scientific inquiry would spell an end to the scientific enterprise, as only pre-approved ideas would be permitted to be discussed. This is exactly how a dark curtain falls on the formerly bright light of science.

It is not so simple a matter as declaiming, as if it were a universal truth, that same-sex attractions "are not considered pathologies" (Strizzi & Di Nucci, 2022). It depends who is doing the considering. Religious teachings subscribed to by over half the planet consider same-sex relations morally unhealthful in various degrees. Strizzi and Di Nucci pronounce that "it is unethical to treat something that is not a disorder or pathology." Would these public health experts then oppose abortion care, since pregnancy is not a disease? Would they outlaw all cosmetic plastic surgery? How about hair restoration or wrinkle reduction treatments? Or can they recognize that some conditions, normal in themselves, can be received by some persons as benign and by others as highly problematic?

Many who experience same-sex attractions tell us that they would like to be free of them. According to the Generations data, 10% (95% CI 8.5, 12.2) of sexual minority persons in the USA agreed with the statement, "If someone offered me the chance to be completely heterosexual, I would accept the chance." Experts may interpret such heterodoxy as itself being pathological, an expression of "internalized homophobia." For all I know, they could be right in many cases. But does this give them the right to coercively override the conscience of any who may disagree, by imposing laws and heavy penalties? What if the experts are also wrong in many cases?

Ignoring the contrary evidence I cited in the paper—or perhaps they consider that knowledge also unethical—Strizzi and Di Nucci (2022) defame SOCE therapies as nothing but coercive and ineffectual practices focused on eliminating same-sex sexual orientation. On the contrary, most SOCE therapy is freely chosen by religiously-committed persons or persons in a heterosexual relationship whose goal is greater personal wholeness, which may or may not involve a diminution of same-sex attraction or change of sexual identification. To allow individuals to freely seek to function more heterosexually is not to "seek to eradicate same-sex sexual orientations" from society any more than helping some persons learn to swim is an attempt to eradicate walking from society.

As far as human rights are concerned, Strizzi and Di Nucci (2022) ignore the fact those who want to change have rights, too. Tolerance must work both ways. For the same reasons that same-sex orientations should not be coercively changed, they should not be coercively prohibited from change. If it is true for heteronormative advocates, then it is equally true for sexual minority advocates, that love is love, and persons who love in ways with which they vehemently disagree should be permitted to live their lives in peace and dignity, without detraction or discrimination. It is a perverse form of bigotry that insists that tolerance of adopting a same-sex orientation requires intolerance of adopting a heterosexual orientation.

In their fervor for sexual minority rights, Strizzi and Di Nucci (2022) ignore the equally important issue of religious rights and the more fundamental question, for therapists, of patient's rights. Respecting such rights, even—maybe especially—when doing so contradicts the political views of the therapist, has long been a fundamental ethical principle of therapy, even with regard to conversion therapy. Twenty years ago, Haldeman (2002), editor of the recent book *The Case Against Conversion Therapy* (Haldeman, 2022), and no supporter of SOCE, addressed the human rights implications of conversion therapy in these words:

> The rights of individuals to their diverse experiences of religion and spirituality deserve the same respect accorded sexual orientation.…In some circumstances, it is more conceivable, and less emotionally disruptive, for an individual to contemplate changing sexual orientation than to disengage from a religious way of life that is seen as completely central to the individual's sense of self and purpose.…[R]eligion can serve as a central, organizing aspect of identity that the individual cannot relinquish, even if it means sacrificing sexual orientation in the process. (p. 262)

Diamond (2003), responding to the Spitzer study (2003) which first documented successful outcomes from SOCE, also advocated respect and clinical support for the freedom of choice for those who struggle to reconcile their experience of sexual orientation with conflicting religious convictions:

> I have come to know numerous men and women who have struggled with the gulf between their same-sex sexuality and their passionate devotion to the Mormon faith, both of which may be experienced as inextricably woven into one's deepest sense of self. As long as some individuals' chosen communities (whether based on faith, ethnicity, geography, etc.) invalidate the possibility of living openly with same-sex desires, clinicians must develop, analyze, test, and validate different approaches for helping members of those communities to make peace with, and decisions about, their irreconcilably conflicting life choices and chances. (p. 430)

In the same spirit, Haldeman (2002) advised that "gay-affirmative therapists need to take seriously the experiences of their religious clients, refraining from encouraging an abandonment of their spiritual traditions in favor of a more gay-affirming doctrine or discouraging their exploration of conversion treatments." (p. 263).

Regarding patient's rights, Haldeman (2002) reminded: "However this distinction between religious identity and sexual orientation may be viewed, psychology does not have the right to interfere with individual's rights to seek the treatments they choose" (p. 262). He added: "The reason the [American Psychological Association (APA)] does not ban conversion therapy outright is that the same arguments for diversity and autonomy [regarding sexual orientation] can be used to support those who seek to change their sexual orientation on the basis of religious belief and practice. Psychology's role is to inform the profession and the public, not to legislate against individuals' rights to self-determination" (p. 263).

Recently, as Strizzi and Di Nuzzi (2022) document, the APA and other professional organizations have begun to support bans on conversion therapy. However, as I showed in my study (Sullins, 2022b), these revised positions are based on Blosnich et al. (2020) and similar recent studies that falsely attribute harm to SOCE by ignoring time order. Haldeman and Diamond may have also subsequently changed their views for the same reason (Diamond & Rosky, 2016; Haldeman, 2022), yet they articulated ethical principles that nonetheless remain true today. These organizations and scholars, and Strizzi and Di Nucci, would be wise to reconsider and re-assert their former support for patients' rights to self-determination, including the right to freely seek their own autonomous, diverse goals in therapy.

In conclusion, Haldeman (2002) eloquently articulated the larger socioethical goal of therapy for persons who struggle with issues related to their sexual orientation, which may form the best corrective psychological science can offer to the censorious view of Strizzi and Di Nucci (2022):

> Optimal psychological functioning depends upon one's ability to integrate the various aspects of the self as fully as possible. In striving toward this goal for all patients, we move toward the most important work of all: not what changes sexual orientation, but what changes society so that we may all live and work together while respecting each other's differences. (p. 264)

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Data Deposit Information Meyer, I. H. (2020). Generations: A Study of the Life and Health of LGB People in a Changing. Society, USA, 2016–2019: Version 1. Electronic Data File. Inter-University. Consortium for Political and Social Research. https://doi.org/10.3886/ICPSR37166.V1

References

American Psychological Association, Task Force on Appropriate Therapeutic Responses to Sexual Orientation. (2009). *Report of the American Psychological Association Task Force on Appropriate Therapeutic Responses to Sexual Orientation*. https://www.apa.org/pi/lgbt/resources/therapeutic-response.pdf

Austin, P. C., Jembere, N., & Chiu, M. (2018). Propensity score matching and complex surveys. *Statistical Methods in Medical Research, 27*(4), 1240–1257.

Becker, S. O., & Ichino, A. (2002). Estimation of average treatment effects based on propensity scores. *The Stata Journal: Promoting Communications on Statistics and Stata, 2*(4), 358–377. https://doi.org/10.1177/1536867X0200200403

Biondi-Zoccai, G., Romagnoli, E., Agostoni, P., Capodanno, D., Castagno, D., D'Ascenzo, F., Sangiorgi, G., & Modena, M. G. (2011). Are propensity scores really superior to standard multivariable analysis? *Contemporary Clinical Trials, 32*(5), 731–740. https://doi.org/10.1016/j.cct.2011.05.006

Blosnich, J. R., Coulter, R. W. S., Henderson, E. R., Goldbach, J. T., & Meyer, I. H. (2023). Correcting a false research narrative: A Commentary on Sullins (2022) [Commentary]. *Archives of Sexual Behavior*. https://doi.org/10.1007/s10508-022-02521-2

Blosnich, J. R., Henderson, E. R., Coulter, R. W., Goldbach, J. T., & Meyer, I. H. (2020). Sexual orientation change efforts, adverse childhood experiences, and suicide ideation and attempt among sexual minority adults, United States, 2016–2018. *American*

Journal of Public Health, 110, 1024–1030. https://doi.org/10.2105/AJPH.2020.305637
Bradshaw, K., Dehlin, J. P., Crowell, K. A., Galliher, R. V., & Bradshaw, W. S. (2015). Sexual orientation change efforts through psychotherapy for LGBQ individuals affiliated with the Church of Jesus Christ of Latter-day Saints. *Journal of Sex & Marital Therapy*, *41*(4), 391–412.
Dehlin, J. P., Galliher, R. V., Bradshaw, W. S., & Crowell, K. A. (2015a). Navigating sexual and religious identity conflict: A Mormon perspective. *Identity, 15*(1), 1–22. https://doi.org/10.1080/15283488.2014.989440
Dehlin, J. P., Galliher, R. V., Bradshaw, W. S., Hyde, D. C., & Crowell, K. A. (2015b). Sexual orientation change efforts among current or former LDS church members. *Journal of Counseling Psychology, 62*(2), 95–105.
Diamond, L. M. (2003). Reconsidering "sexual desire" in the context of reparative therapy [Commentary]. *Archives of Sexual Behavior, 32*, 429–431.
Diamond, L. M., & Rosky, C. J. (2016). Scrutinizing immutability: Research on sexual orientation and US legal advocacy for sexual minorities. *Journal of Sex Research, 53*(4–5), 363–391.
Elze, M. C., Gregson, J., Baber, U., Williamson, E., Sartori, S., Mehran, R., Nichols, M., Stone, G. W., & Pocock, S. J. (2017). Comparison of propensity score methods and covariate adjustment: Evaluation in 4 cardiovascular studies. *Journal of the American College of Cardiology, 69*(3), 345–357. https://doi.org/10.1016/j.jacc.2016.10.060
Glassgold, J., & Haldeman, D. (2023). Evidence of sexual orientation change efforts ineffectiveness and risks of harm: A response to Sullins (2022) [Commentary]. *Archives of Sexual Behavior*. https://doi.org/10.1007/s10508-023-02554-1
Glassgold, J. M. (2022). Research on sexual orientation change efforts: A summary. In D. C. Haldeman (Ed.), *The case against conversion "therapy": Evidence, ethics, and alternatives* (pp. 17–50). American Psychological Association.
Haldeman, D. C. (2002). Gay rights, patient rights: The implications of sexual orientation conversion therapy. *Professional Psychology, Research and Practice, 33*(3), 260–264.
Haldeman, D. C. (Ed.). (2022). *The case against conversion "therapy": Evidence, ethics, and alternatives*. American Psychological Association.
Jones, S. L., & Yarhouse, M. A. (2007). *Ex-gays?: A longitudinal study of religiously mediated change in sexual orientation*. IVP Academic.
Jones, S. L., & Yarhouse, M. A. (2011). A longitudinal study of attempted religiously mediated sexual orientation change. *Journal of Sex & Marital Therapy, 37*(5), 404–427.
Meyer, I. H. (2020). *Generations: A study of the life and health of LGB people in a changing society, United States, 2016-2019. [ICPSR Codebook for Generations Waves 1 and 2 Merged Public-Use Data]* (Version v1) . Inter-University Consortium for Political and Social Research. https://doi.org/10.3886/ICPSR37166.V1
Rivera, A. S., & Beach, L. B. (2022). Unaddressed sources of bias lead to biased conclusions about sexual orientation change efforts and suicidality in sexual minority individuals [Commentary]. *Archives of Sexual Behavior*. https://doi.org/10.1007/s10508-022-02498-y
Rosik, C. H. (2022). A wake-up call for the field of sexual orientation change efforts research: Comment on Sullins (2022) [Commentary]. *Archives of Sexual Behavior*. https://doi.org/10.1007/s10508-022-02481-7
Shah, B. R., Laupacis, A., Hux, J. E., & Austin, P. C. (2005). Propensity score methods gave similar results to traditional regression modeling in observational studies: A systematic review. *Journal of Clinical Epidemiology, 58*(6), 550–559. https://doi.org/10.1016/j.jclinepi.2004.10.016
Spitzer, R. L. (2003). Can some gay men and lesbians change their sexual orientation? 200 participants reporting a change from homosexual to heterosexual orientation. *Archives of Sexual Behavior, 32*(5), 403–417.
Strizzi, J. M., & Di Nucci, E. (2022). Ethical and human rights concerns of sexual orientation change efforts: Commentary on Sullins (2022) [Commentary]. *Archives of Sexual Behavior*. https://doi.org/10.1007/s10508-022-02446-w
Sullins, D. P. (2022a). Absence of behavioral harm following non-efficacious sexual orientation change efforts: A retrospective study of United States sexual minority adults, 2016–2018. *Frontiers in Psychology*, *13*. https://www.frontiersin.org/article/https://doi.org/10.3389/fpsyg.2022.823647
Sullins, D. P. (2022b). Sexual orientation change efforts do not increase suicide: Correcting a false research narrative. *Archives of Sexual Behavior, 51*(7), 3377–3393. https://doi.org/10.1007/s10508-022-02408-2

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