Do Sexual Orientation Change Efforts (SOCE) increase suicidal risk for sexual minorities?: An exchange

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This document presents the text of a scholarly exchange on the question whether sexual minority persons who participate in SOCE therapy are thereby subject to increased suicidal risk. The documents included are unofficial versions, prior to final copy-edit and/or peer review, of studies and commentaries published in the American Journal of Public Health and the Archives of Sexual Behavior. Some are watermarked. The text is very similar to the final authenticated versions, but not exact. Readers are encouraged to acquire the final authenticated versions, which are the versions of record, and are available online, some at no charge, as indicated below and in the reference list.

Summary and Table of Contents

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Sexual Orientation Change Efforts, Adverse Childhood Experiences, and Suicide Ideation and Attempt Among Sexual Minority Adults, United States, 2016–2018

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Objectives. To examine how sexual orientation change efforts (SOCE) are associated with suicide morbidity after controlling for adverse childhood experiences (ACEs).

Methods. Cross-sectional survey data are from the Generations survey, a nationally representative sample of 1518 nontransgender sexual minority adults recruited between March 28, 2016, and March 30, 2018, in the United States. Self-identified transgender individuals were included in a separate, related TransPop study. We used weighted multiple logistic regression analyses to assess the independent association of SOCE with suicidal ideation and suicide attempt while controlling for demographics and ACEs.

Results. Approximately 7% experienced SOCE; of them, 80.8% reported SOCE from a religious leader. After adjusting for demographics and ACEs, sexual minorities exposed to SOCE had nearly twice the odds of lifetime suicidal ideation, 75% increased odds of planning to attempt suicide, and 88% increased odds of a suicide attempt with minor injury compared with sexual minorities who did not experience SOCE.

Conclusions. Over the lifetime, sexual minorities who experienced SOCE reported a higher prevalence of suicidal ideation and attempts than did sexual minorities who did not experience SOCE.

Public Health Implications. Evidence supports minimizing exposure of sexual minorities to SOCE and providing affirming care with SOCE-exposed sexual minorities. (*Am J Public Health.* 2020;110:1024–1030. doi:10.2105/AJPH.2020.305637)

Suicide has increased to a level that, along with drug overdose– and alcohol-related deaths, has reduced life expectancy for US persons for 3 consecutive years.[1] Suicidal ideation and suicide attempt (i.e., suicide morbidity) are strong predictors of death by suicide,[2] and suicide morbidity occurs more frequently among lesbian, gay, and bisexual (LGB or sexual minority) populations than among heterosexuals.[3,4] Identifying unique stressors that are associated with sexual minority individuals' suicidal ideation and suicide attempts can lead to tailored intervention and prevention efforts.

One stressor unique to sexual minorities is experiencing sexual orientation change efforts (SOCE), sometimes referred to as conversion or reparative therapy.[5] SOCE include a variety of approaches such as immersion in heterosexual-focused cognitive exercises, amplification of shame for same-gender attraction, and physical punishment (e.g., electric shock) intended to condition against mental or physical attraction to the same gender.[6–8] Negative outcomes of SOCE include increased distress, depression, hopelessness, and suicidal thoughts and behaviors.[6,8–10] SOCE have been practiced by religious counselors, medical professionals, and other health care providers for decades.[11] Despite several national professional organizations' official positions against SOCE (e.g., American Psychological Association,[11] American Medical Association,[12] National Association of Social Workers[13]), as of June 2019, only 18 US states (and Puerto Rico and Washington, DC) have laws that ban subjecting minors to SOCE.[14]

Minority stress theory describes stressors as unique in that they stem from homophobia and chronic in that they are present in day-to-day social interactions.[15] Minority stressors include prejudicial events and conditions that are expressed both interpersonally (e.g., violent attacks, discrimination) and structurally (e.g., laws allowing rejection of sexual minorities in housing and employment).[16] By its very nature and purpose, SOCE can be defined as a minority stressor because they promote heteronormativity as the only acceptable way of life and reinforce individual, family, and community rejection of LGB sexual orientation. By reinforcing stigmatizing societal attitudes and promoting self-rejection, professionals who engage in

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SOCE provide the exact opposite of recommended therapeutic approaches that should support self-acceptance.[17–19] Minority stress also affects sexual minorities through internalization of stigmatizing social attitudes and stereotypes. For example, LGB people internalize homophobic notions, contributing to adverse health outcomes.[17,20]

Related to stigmatization in their families, sexual minorities have a high prevalence of adverse childhood experiences (ACEs),[21] including physical and sexual abuse. Evidence shows dose–response relations of ACEs with suicidality,[22] which may partly explain disparities in poor mental health between sexual minority and heterosexual individuals.[23,24] Less is understood about how ACEs and SOCE may be associated among sexual minorities. For instance, it is plausible that sexual minority children may be less accepted by their parents and more likely to be subjected to SOCE. To date, no research has examined the relationship between ACEs and SOCE to our knowledge.

Regardless of its relationship with ACEs, there has been scant investigation of how experiencing SOCE is related to suicidal ideation and attempt among sexual minorities. Similarly, little is known about the independent associations of SOCE and ACEs and suicidal ideation and attempt. The dearth of inquiry stems mainly from a lack of data on experiencing SOCE among sexual minorities. Capitalizing on a novel probability-based national sample of sexual minority adults, we examined how experiencing SOCE is associated with suicide morbidity, after considering the effects of ACEs.

METHODS

We collected data as part of the Generations study, which was designed to examine health and well-being across 3 generations of nontransgender sexual minority people.

Generations contracted with Gallup to use an innovative 2-phase sampling approach. In phase 1, Gallup used a dual-frame sampling procedure, which included random-digit dialing to reach US landline and cellphone users (a random selection method was used for choosing a respondent in households reached on landline phones). Respondents screened at phase 1 were eligible to participate in phase 2 (a self-administered Web or paper questionnaire) if they identified as cisgender or gender nonbinary sexual minority (and not transgender); were in the age ranges for 1 of the 3 cohorts of interest in the Generations study (aged 18–25, 34–41, or 52–59 years); belonged to the racial and ethnic groups targeted (Black, Latino, or White, or had multiple racial and ethnic identities that included at least 1 of these; Table 1); completed at least sixth grade; and spoke English well enough to conduct the telephone interview in English.

The specific age groups were selected to represent people who came of age in distinct social historical periods relevant to lesbian gay bisexual transgender (LGBT) rights. The investigators identified the Pride generation as people who came of age in the 1970s and were aged 51 to 59 years at the time of recruitment, the Visibility generation as people who came of age in the late 1980s and 1990s and were aged 34 to 41 years at the time of recruitment, and the Equality generation as people who came of age in the 2000s and were aged 18 to 25 years at the time of recruitment.

Asian American and American Indian/Alaska Native sexual minority people were excluded because their low representation in the US population meant the researchers would have not been able to recruit sufficient numbers of respondents during the recruitment period to allow meaningful statistical analyses for these racial and ethnic groups. Education level was selected because respondents needed to be able to comprehend and self-administer the main study questionnaire. Respondents who identified as transgender, regardless of their sexual orientation, were invited to participate in a related TransPop study, which asked questions that were tailored to the transgender population.

In phase 1 366 640 respondents were screened in the brief telephone interview between March 2016 and March 2017. Of these respondents, 3.5% (n = 12 837) identified as sexual minority, transgender, or both. After applying the study inclusion criteria, 3525 were eligible to participate in the Generations study. The final cooperation rate[25] for the Generations study was 39%. The final sample included 1518 respondents, including 187 respondents from an enhancement recruitment period (April 2017 to March 2018) aimed at increasing the number of Black and Latino respondents. The entire sample was weighted for nonresponse using the US Census and for specific demographics of the LGBT population using Gallup data collected since 2012. More information about the study's methodology and rationale is available online at http://www.generations-study.com.

Measures

Demographic covariates included gender identity (man, woman, or nonbinary or genderqueer); sexual identity (lesbian, gay, bisexual, queer, pansexual, asexual, or other minority sexual identities); racial and ethnic identity (White, Black or African American, Hispanic or Latinx, or other racial and ethnic identity); educational attainment (high school diploma or less, some college, college degree, or more than a college degree); and age.

ACEs were measured using 11 items employed by the Centers for Disease Control and Prevention in population health surveillance.[26] The items are predicated with the statement "Now, looking back before you were 18 years of age. . ." and followed by several categories of negative experiences (e.g., living with anyone who was depressed, mentally ill, or suicidal; frequency of parents or adults in the home ever slapping, hitting, kicking, punching, or beating up each other; frequency of physical abuse). Three items specifically asked respondents about sexual abuse: How often did anyone at least 5 years older than you, or an adult, (1) ever touch you sexually, (2) try to make you touch them sexually, and (3) force you to have sex? These 3 sexual abuse items were combined into a cumulative measure of "any sexual abuse" if a respondent affirmatively answered 1 or more of the items.

Experiencing SOCE was measured by an item created by the survey team: "Did you ever receive treatment from someone who tried to change your sexual orientation (such as try to make you straight/heterosexual)?" Response options were: no; yes, from a health care professional (such as a psychologist or counselor who was not religious focused); and yes, from a religious leader (such as a pastor, religious counselor, priest). Because respondents could report experiencing both forms of SOCE, answers were combined in a

TABLE 1—Sociodemographic Characteristics of Individuals, by Experiencing Sexual Orientation Change Efforts (SOCE), Counts, and Weighted Proportions: Probability Sample of Sexual Minorities, United States, 2016–2018

	Overall Sample (n = 1518), No. (%; SE) or Mean (SE)	Experienced SOCE		
		No (n = 1410), No. (%; SE) or Mean (SE)	Yes (n = 108), No. (%; SE) or Mean (SE)	*P*
Gender identity				
Woman	750 (55.0; 0.016)	708 (94.3; 0.011)	42 (5.7; 0.011)	.15
Man	674 (37.6; 0.015)	616 (91.2; 0.014)	58 (8.8; 0.014)	
Nonbinary or genderqueer	94 (7.4; 0.009)	86 (94.2; 0.022)	8 (5.8; 0.022)	
Sexual identity				
Lesbian/gay	833 (46.9; 0.016)	757 (89.9; 0.014)	76 (10.1; 0.014)	.01
Bisexual	493 (40.6; 0.016)	476 (96.3; 0.011)	17 (3.7; 0.011)	
Other sexual identity	181 (12.5; 0.010)	166 (94.3; 0.018)	15 (5.7; 0.018)	
Racial/ethnic identity				
White	931 (59.5; 0.016)	871 (94.2; 0.010)	60 (5.8; 0.010)	.14
Black/African American	180 (13.5; 0.011)	162 (88.6; 0.029)	18 (11.3; 0.029)	
Latino/a	158 (10.8; 0.010)	145 (91.7; 0.027)	13 (8.3; s0.027)	
Other racial/ethnic identity	249 (16.2; 0.011)	232 (94.0; 0.019)	17 (6.0; 0.019)	
Educational attainment				
More than a college degree	288 (9.6; 0.006)	260 (90.6; 0.018)	28 (9.4; 0.018)	.08
College degree	429 (16.0; 0.009)	403 (95.5; 0.016)	26 (4.5; 0.016)	
Some college	492 (31.9; 0.014)	464 (94.5; 0.011)	28 (5.5; 0.011)	
High school diploma or less	309 (42.5; 0.017)	283 (91.8; 0.009)	26 (8.2; 0.009)	
Age, y	30.9 (0.37	30.7 (0.38	32.7 (1.43	.19

Note. Percentages and means were weighted. Sample size was n = 1518.

single category of having experienced SOCE by either or both sources.

Suicide morbidity was captured with several measures adapted from the Army Study to Assess Risk and Resilience in Service Members instrument,[27] which was adapted from the Columbia Suicide Severity Rating Scale (C-SSRS).[28] These measures included suicidal ideation (i.e., "Did you ever in your life have thoughts of killing yourself?"), having made a plan for suicide (i.e., "Did you ever think about how you might kill yourself [e.g., taking pills, shooting yourself] or work out a plan of how to kill yourself?"), and attempted suicide ("Did you ever make a suicide attempt [i.e., purposefully hurt yourself with at least some intention to die]?"). Individuals who reported at least 1 previous suicide attempt were then asked, "What were the most serious injuries you ever received from a suicide attempt?" The C-SSRS has 6 different categories of injury severity, but because of low frequencies in some categories, we combined information from these 2 items to create a 3-category suicide attempt variable: no attempt; attempt with no or minor injury (e.g., surface scratches, mild nausea, sprain, first-degree burns, flesh wound); and attempt with moderate or severe injuries (e.g., broken bones, second- or third-degree burns, stitches, bullet wound, major fracture, coma requiring respirator, or surgery).

Analyses

We summarized demographics for the overall sample using descriptive statistics. We examined ACEs as 8 dichotomous categories (yes or no) and in a count of ACEs endorsed by the respondents. We tested differences in sociodemographics, ACEs, and suicide morbidity between respondents who had experienced SOCE and respondents who did not experience SOCE. To better understand the relation between ACEs and experiencing SOCE, we used multiple logistic regression to assess the association of exposure to SOCE with ACEs after adjusting for demographics.

To investigate the independent associations of ACEs and SOCE with suicidal ideation, suicide planning, and suicide attempt, we first conducted separate multiple logistic regression models including covariates and ACEs followed by second models that added experiencing SOCE. For the 3-category variable of suicide attempt, we conducted multinomial logistic regression analyses, with "no attempts" set as the reference category; we followed the same method of having the first model include covariates and ACEs followed by a second model that added experiencing SOCE. We conducted all analyses using Stata/SE version 15 (StataCorp, College Station, TX). We weighted analyses to account for the complex sampling design and nonresponse. We reported all point estimates with 95% confidence intervals and assessed statistical significance at a *P* level of less than .05. All reported means and percentages are weighted.

RESULTS

Of the 1518 participants, 55% identified as women, more identified as lesbian or gay than bisexual (46.9% vs 40.6%, respectively), and about 60% identified as White (Table 1). Among men and women, sex assigned as birth was 100% concordant; for nonbinary individuals, 67.3% reported being assigned female sex at birth and 32.7% indicated being assigned male sex at birth (data not shown). Across the sample, 6.9% (n = 108) experienced SOCE from any source; of them, 80.8% reported SOCE from a religious leader, and 31.0% reported SOCE from a health care provider. Individuals with gay or lesbian identities were more likely to report experiencing SOCE than bisexually identified respondents or respondents with other sexual minority identities (e.g., queer, pansexual). The prevalence of experiencing SOCE did not significantly differ across the age cohorts of Generations: 6.2% among those aged 18 to 25 years, 8.3% among those aged 34 to 41 years, and 7.8% among those aged 52 to 59 years (*P* = .43; data not shown).

Participants had an average of 3 ACEs, and odds of experiencing SOCE were significantly greater among people who as

children lived with a parent or another adult who was depressed, mentally ill, or suicidal; lived in a household with parental intimate partner violence; or reported emotional, physical, or sexual abuse (Table 2) than among their counterparts. When ACEs were counted, there was a significant 25% increased odds of reporting SOCE experiences with each additional ACE experienced.

Sexual minorities who experienced SOCE had greater prevalence of all measures of suicide morbidity relative to sexual minorities without SOCE experiences (Table 3). Results of regression models with only ACEs and not SOCE and then with both ACEs and SOCE showed little change in estimates, and interaction tests of ACEs and SOCE were not significant (data not shown). Therefore, results of the full models are shown in Table 4. In the adjusted models, ACEs were positively associated with all measures of suicide morbidity. Compared with not experiencing SOCE, experiencing SOCE was associated with twice the odds of lifetime suicidal ideation, 75% increased odds of planning to attempt suicide, 88% increased odds of attempting suicide resulting in no or minor injury, and 67% increased odds of suicide attempt resulting in moderate or severe injury (the last did not reach statistical significance at $P < .05$).

TABLE 2—Prevalence and Adjusted Association of Adverse Childhood Experiences (ACEs) With Experiencing Sexual Orientation Change Efforts (SOCE), Counts, Weighted Proportions, and AORs: Probability Sample of Sexual Minorities, United States, 2016–2018

	Experienced SOCE			
ACEs	No, No. (%; SE) or Mean ❻SE	Yes, No. (%; SE) or Mean ❻SE	*P*	Multivariable,[a] AOR (95% CI)
Household substance use				
No	771 (94.4;0.010)	48 (5.6;0.010)	.11	1 (Ref)
Yes	639 (91.8;0.013)	60 (8.2;0.013)		1.56 (0.92, 2.65)
Parental separation or divorce				
No	928 (93.6;0.009)	72 (6.4;0.009)	.45	1 (Ref)
Yes	482 (92.3;0.015)	36 (7.7;0.015)		1.38 (0.83, 2.30)
Parental mental illness				
No	789 (94.0;0.010)	50 (6.0;0.010)	.23	1 (Ref)
Yes	621 (92.1;0.013)	58 (7.9;0.013)		1.76 (1.05, 2.94)
Incarcerated household member				
No	1218 (93.3;0.009)	90 (6.7;0.009)	.59	1 (Ref)
Yes	192 (92.1;0.023)	18 (7.9;0.023)		1.17 (0.57, 2.39)
Parental partner violence				
No	960 (94.5;0.009)	62 (5.5;0.009)	.02	1 (Ref)
Yes	450 (90.5;0.016)	46 (9.5;0.016)		1.86 (1.13, 3.05)
Emotional abuse				
No	478 (96.1;0.011)	22 (3.9;0.011)	.01	1 (Ref)
Yes	932 (91.9;0.011)	86 (8.1;0.011)		2.48 (1.31, 4.70)
Physical abuse				
No	870 (94.7;0.009)	49 (5.3;0.009)	.02	1 (Ref)
Yes	540 (90.9;0.014)	59 (9.1;0.014)		1.87 (1.11, 3.13)
Sexual abuse				
No	907 (95.4; 0.008)	47 (4.6; 0.008)	< .001	1 (Ref)
Yes	503 (89.1; 0.017)	61 (10.9; 0.017)		2.95 (1.75, 5.00)
No. of ACEs	3.3 ❻0.07	4.2 ❻0.31	.01	1.25 (1.10, 1.42)

Note. AOR = adjusted odds ratio; CI = confidence interval. Percentages and means are weighted. Sample size was n = 1518.

[a]All multivariable models were weighted and adjusted for age, gender identity, sexual identity, education, and race/ethnicity.

DISCUSSION

We found that about 7% of sexual minorities experienced SOCE. This compares with 17% reported by a previous study from the Multisite AIDS Cohort Study.[29] But that study is not directly comparable because its sample included men who have sex with men, was not representative of the US population, and had a mean age of 61.5 years, which is older than our sample. A study using a nonprobability sample of transgender and gender nonbinary individuals in the United States found that about 10% reported experiences of SOCE.[30] To our knowledge, our study is the first to publish data on SOCE in a nationally representative sample of nontransgender sexual minorities in the United States.

We found that sexual minorities who experienced ACEs were more likely to have experienced SOCE than were sexual minorities who did not experience ACEs. Even after adjustment for exposure to ACEs, which are known risk factors for mental health problems and suicide morbidity, experiencing SOCE was independently associated with suicidal ideation, suicide planning, and suicide attempts. We did not find a significant relation between experiencing SOCE and suicide attempt with moderate or severe injury, but it is noteworthy that the odds ratio estimate was in the same direction and of similar magnitude as the other significant associations. The relatively small sample may have hampered statistical power for the rare outcome of suicide attempts resulting in moderate or severe injury.

To date, the mental health harms of SOCE have been documented primarily via qualitative inquiry.[6–8] Our study adds to previous anecdotal findings with quantitative evidence showing the association between SOCE and suicide morbidity. The results of this study suggest that SOCE is a stressor with particularly insidious associations with suicide risk. The SOCE associations may be explained with the construct of perceived burdensomeness of the interpersonal theory of suicide,[31] which has been associated with suicide morbidity among sexual minorities.[32] Further research into this area may investigate the specific constructs and mechanisms (e.g., enacted stigma, internalized stigma, identity concealment) that could incite perceived burdensomeness and

TABLE 3—Prevalence of Suicide Morbidity, by Experiencing Sexual Orientation Change Efforts (SOCE), Counts, and Weighted Proportions: Probability Sample of Sexual Minorities, United States, 2016–2018

	Experienced SOCE		
Lifetime Suicide Morbidity	No, No. (%; SE)	Yes, No. (%; SE)	*P*
Suicidal ideation	967 (73.4, 0.014)	90 (84.0, 0.042)	.04
Made a suicide plan	763 (58.7, 0.016)	74 (71.7, 0.054)	.03
Attempted suicide			
No	1087 (73.8, 0.015)	65 (59.6, 0.060)	.02
Yes, no injury or minor injury	172 (13.4, 0.012)	23 (24.6, 0.053)	
Yes, moderate or severe injury	151 (12.8, 0.012)	20 (15.7, 0.042)	

Note. Percentages were weighted. Sample size was n = 1518.

create the risk of suicidal thoughts and behaviors among survivors of SOCE.

Limited evidence exists to guide clinical practice with individuals who have experienced SOCE. Many people participate in SOCE to conform to social expectations of family, culture, and religion.[6] Yet SOCE are ineffective and may compound or create problems, such as depression, guilt, intimacy avoidance,[5–8] and, as we have shown here, suicidal ideation and suicide attempts. Cognitive behavioral therapy may help resolve these outcomes by addressing the detrimental effects of minority stressors,[18] including the effects of SOCE. However, best practices for affirming care with sexual minorities who experienced SOCE are largely uncharted.

Health care and social service providers working with sexual minorities with histories of or active suicidal thoughts and suicide attempts should be aware that cumulative trauma assessments should include a history of SOCE experiences, which may have amplified internalized stigma. To better understand the impacts of SOCE as a unique minority stressor for sexual minorities, population health surveys that include items about stressful life experiences should also include items to assess experiences of SOCE.

Study Limitations

The Generations study team developed the SOCE measure, and although it seems

TABLE 4—Associations of ACEs and Experiencing Sexual Orientation Change Efforts (SOCE) With Suicide Morbidity, AORs: Probability Sample of Sexual Minorities, United States, 2016–2018

	Suicide attempt[a]			
	Suicidal Ideation (n = 1489), AOR (95%CI)	Suicide Planning (n = 1480), AOR (95%CI)	Suicide Attempt With No/Minor Injury (n = 1507), AOR (95%CI)	Suicide Attempt With Moderate/Severe Injury (n = 1507), AOR (95%CI)
Experienced SOCE	1.92 (1.01, 3.64)	1.75 (1.01, 3.06)	1.88 (1.01, 3.50)	1.67 (0.76, 3.64)
No. of ACEs	1.28 (1.17, 1.39)	1.27 (1.19, 1.37)	1.27 (1.17, 1.39)	1.38 (1.25, 1.52)
Age, y	0.97 (0.96, 0.98)	0.98 (0.97, 0.99)	0.99 (0.97, 1.00)	0.99 (0.97, 1.00)
Gender identity				
Female (Ref)	1	1	1	1
Male	1.06 (0.77, 1.45)	0.86 (0.64, 1.15)	1.19 (0.78, 1.82)	0.46 (0.28, 0.78)
Nonbinary/genderqueer	3.32 (1.32, 8.35)	2.22 (1.08, 4.56)	0.98 (0.35, 2.74)	1.70 (0.83, 3.50)
Sexual identity				
Gay/lesbian (Ref)	1	1	1	1
Bisexual	1.34 (0.93, 1.92)	1.16 (0.83, 1.61)	1.12 (0.69, 1.82)	1.53 (0.94, 2.49)
Other sexual minority	2.19 (1.27, 3.79)	1.87 (1.13, 3.09)	1.55 (0.74, 3.25)	0.96 (0.47, 1.96)
Racial/ethnic identity				
White (Ref)	1	1	1	1
Black/African American	0.55 (0.35, 0.85)	0.65 (0.43, 0.99)	1.43 (0.85, 2.39)	0.54 (0.26, 1.12)
Latino/a	0.55 (0.34, 0.89)	0.70 (0.45, 1.10)	1.02 (0.53, 1.98)	0.59 (0.26, 1.31)
Other racial/ethnic identity	0.93 (0.59, 1.48)	1.22 (0.81, 1.82)	0.85 (0.50, 1.44)	1.12 (0.68, 1.85)
Educational attainment				
Postgraduate (Ref)	1	1	1	1
College degree	1.01 (0.70, 1.46)	0.88 (0.62, 1.25)	1.53 (0.86, 2.73)	0.84 (0.45, 1.56)
Some college	0.90 (0.61, 1.33)	1.08 (0.75, 1.54)	1.56 (0.89, 2.73)	1.21 (0.68, 2.15)
High school diploma or less	1.02 (0.65, 1.60)	0.91 (0.60, 1.37)	1.54 (0.83, 2.84)	0.97 (0.51, 1.84)

Note. ACE = adverse childhood experience; AOR = adjusted odds ratio; CI = confidence interval. All multivariable models were weighted. Sample size was n = 1518.

[a]Estimated with multinomial logit model (no suicide attempts as reference category).

straightforward, no evidence of the measure's validity and reliability exists at this time. Additionally, people who experienced SOCE may continue to have negative feelings about their same-sex sexual orientation and may be more likely than others to hide their sexual minority identity; thus, our study recruitment method may have underrepresented SOCE exposure among sexual minorities.

Our measure of SOCE is limited in that it does not differentiate among the diverse experiences SOCE people may have had. Despite the strong associations of SOCE, further research is necessary to understand variability in SOCE experiences. For instance, our survey item broadly captured SOCE, but we are unable to determine if SOCE were received from a practitioner who solely focused on SOCE (e.g., conversion camps) or arose in the context of a generalized discussion with a mental health profession or religious leader. Thus, we cannot discern differential impact of various experiences of SOCE.

Similarly, our measure did not allow us to accurately time SOCE experiences as they related to ACEs exposure. To probe causal relationships, future survey items ought to attend to issues of the timing of ACEs and SOCE (e.g., age of first and last experiences) and the type and dosage of these stressful exposures (e.g., number of experiences). Other methodological limitations include that ACEs may be prone to recall bias, likely resulting in underestimates of the phenomena.[33] Additionally, other childhood adversities may not be captured in the ACEs inventory (e.g., community safety) that may be associated with SOCE or suicidal ideation or attempt. Last, data about mental health care utilization other than SOCE were not available, so we could not examine the relationship of non-SOCE mental health treatments, ACEs, and suicidality.

Other limitations include that because of their low base rates in the US population, our methodology did not allow us to recruit sufficient numbers of Asian and American Indian/Alaska Native sexual minorities to facilitate analyses of these groups. Our survey completion rate is lower than that of the 2017 Behavioral Risk Factor Surveillance System (BRFSS) survey (63.8%), but this may be because our comprehensive self-administered survey may be more demanding for respondents than the BRFSS interviewer-administered phone modality.[34]

Public Health Implications

Major professional medical and health services organizations condemn the practice of SOCE.[11–13] However, to date, 32 US states have no laws protecting minors from SOCE, and existing laws do not apply to adults or SOCE administered through religious leaders.[14] This religious exemption is particularly concerning because among the sexual minorities in this sample who experienced SOCE, 4 of 5 people received it from a religious provider. The landscape regarding legality of banning SOCE continues to evolve,[35] and despite both the lack of scientific evidence to uphold SOCE and the documented harm it can do, sexual minority people continue to be at risk for exposure to SOCE. Greater awareness of the harms of SOCE need to be conveyed to the general public, especially in areas that may have a greater prevalence of professionals who engage in SOCE. AJPH

CONTRIBUTORS

J. R. Blosnich conducted the analyses. J. R. Blosnich and I. H. Meyer conceptualized the study. All authors contributed to writing and reviewing article drafts.

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CONFLICTS OF INTEREST

The authors have no conflicts of interest to disclose.

HUMAN PARTICIPANT PROTECTION

Approval was received for this study by the institutional review boards of the University of California, Los Angeles and the University of Pittsburgh.

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ORIGINAL PAPER

Sexual Orientation Change Efforts Do Not Increase Suicide: Correcting a False Research Narrative

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Abstract

Sexual orientation change efforts (SOCEs) signify activities designed to change or reduce homosexual orientation. Recent studies have claimed that such therapies increase suicide risk by showing positive associations between SOCE and lifetime suicidality, without excluding behavior that pre-dated SOCE. In this way, Blosnich et al.'s (2020) recent analysis of a national probability sample of 1518 sexual minority persons concluded that SOCE "may compound or create…suicidal ideation and suicide attempts" but after correcting for pre-existing suicidality, SOCE was not positively associated with any form of suicidality. For suicidal ideation, Blosnich et al. reported an adjusted odds ratio (AOR) of 1.92 (95% CI 1.01–3.64); the corrected AOR was .44 (.20–.94). For suicide planning, Blosnich et al.'s AOR was 1.75 (1.01–3.06); corrected was .60 (.32–1.14). For suicide attempts, Blosnich et al.'s AOR was 1.75 (.99–3.08); corrected was .74 (.36–1.43). Undergoing SOCE after expressing suicidal behavior reduced subsequent suicide attempts from 72 to 80%, compared to those not undergoing SOCE, when SOCE followed a prior expression of suicidal ideation (AOR .17, .05–.55), planning (AOR .13, .04–.45) or intention (AOR .10, .03–.30); however, SOCE following an initial suicide attempt did not significantly reduce further attempts. By violating the principle that a cause cannot occur after an effect, Blosnich et al. misstated the correct conclusion. Experiencing SOCE does not result in higher suicidality, as they claim, and may sharply reduce subsequent suicide attempts. Restrictions on SOCE will not reduce suicidal risk among sexual minorities and may deprive them of an important resource for reducing suicide attempts.

Keywords Sexual orientation · Conversion therapy · Suicide · Sexual orientation change efforts (SOCEs) · Minority stress

Introduction

In the present century, suicide rates have trended downward globally (Roth et al., 2018) but have risen sharply in the USA (Curtin et al., 2016), particularly among younger Americans (Curtin, 2020), including younger sexual minority persons (Meyer et al., 2021). While evidence that sexual orientation is associated with higher completed suicide risk in the USA is mixed (Cochran & Mays, 2011; Erlangsen et al., 2020; Mathy et al., 2011), it is well established that lesbian, gay, and bisexual (LGB) youth are at higher risk than others of related behavior such as thinking about, planning or intending suicide, as well as suicide attempts (Haas et al., 2010; Hottes et al., 2016; Liu et al., 2020; for a review, see Moagi et al., 2021). In both the general population and among sexual minorities, suicide ideation is the most prevalent form of suicidal behavior, followed by making a plan for committing suicide, declaring or signaling unambiguous intention to commit suicide (as opposed to signaling a need for help), and making a suicide attempt. Although only suicide attempts are directly predictive of completed suicide (Harris & Barraclough, 1997; Kessler et al., 2005; Suominen et al., 2004), suicide prevention focuses on treatment following suicidal thoughts, plans or declarations of intention, which often predate an attempt and are "important in their own right as indicators of extreme psychological distress" (Kessler et al., 2005, p. 2487; van der Feltz-Cornelis et al., 2011).

Some sexual minority persons have attempted to resolve unwanted homosexual attraction, behavior, and/or identity by engaging in programs or interventions ranging from camps, intensive study, and aversion techniques to traditional talk

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therapy, collectively known as sexual orientation change efforts (SOCE), or sometimes "conversion" or "change-allowing" therapies. Most SOCE are pursued in religious contexts, by religious practitioners or for religious reasons, and most persons undergoing SOCE do so as youth or young adults. Shidlo and Schroeder's (2002) qualitative study of 202 SOCE participants found that the large majority (91%) reported undergoing individual psychotherapy (including cognitive/behavioral therapy or psychoanalysis). The average course of treatment consisted of 118 counseling sessions over 26 months; small proportions received (or also received) aversive conditioning (9%), hypnosis (4%), psychotropic medication (2%) or inpatient psychiatric treatment (1%) (p. 250). The reported results of such efforts usually entail partial movement toward less intense or exclusive homosexual fantasy and/or reduced same-sex and increased opposite-sex sexual expression, with small proportions reporting either complete resolution of unwanted sexual orientation elements or movement toward increased homosexual orientation (Bradshaw et al., 2015; Dehlin et al., 2015; Jones & Yarhouse, 2011; Karten & Wade, 2010; Spitzer, 2003; Sullins et al., 2021).

Both the efficacy and ethics of SOCE are contested. Spitzer's prominent 2003 study in this journal reporting change as described in the previous paragraph (Spitzer, 2003) prompted a storm of criticism and debate (Drescher & Zucker, 2006; Peer Commentaries on Spitzer, 2003) and Spitzer's (2012) eventual repudiation of the study. Subsequent studies yielded similar findings, however, as well as evidence of psychological benefit from SOCE (Beckstead & Morrow, 2004; Dehlin et al., 2015; Jones & Yarhouse, 2011; Karten & Wade, 2010; Pela & Sutton, 2021; Sullins, 2022; Sullins et al., 2021). Proponents, citing such reports, argue that persons who want to try to resolve same-sex attractions that trouble them should be free to seek therapy to do so. Those opposed to SOCE insist that the practice is "ineffective and may cause harm to patients and their families who fail to change" (Drescher et al., 2016, p. 7; see also Haldeman, 2022; Blosnich et al., 2020; Flentje et al., 2013), including an increased risk of suicidal behavior (Higbee et al., 2022; Ryan et al., 2020). A range of clinical and scholarly associations have issued cautionary or oppositional statements (Alempijevic et al., 2020; American Psychiatric Association, 2000; American Psychological Association, 2021; National Association of Social Workers, 2015). Legal battles between these positions have resulted in limited legislative restrictions on SOCE in 20 US states, injunctions against such bans in two states, and proposed protective legislation in two more states (Movement Advancement Project, 2020). The 2009 American Psychological Association (APA) Task Force on the topic concluded that the practice of SOCE has "become mired in ideological disputes and competing political agendas" (American Psychological Association, Task Force on Appropriate Therapeutic Responses to Sexual Orientation, 2009, p. 92).

The present study addresses the question of suicidal behavior risk due to SOCE by re-examining recent survey findings purporting to demonstrate increased suicidal behavior among former SOCE participants (Blosnich et al., 2020). The question of SOCE efficacy is not at issue; since minority sexual orientation was a screening criterion for survey participation, the data included only persons for whom, by definition, the stated aims of SOCE were not achieved.

Until recently, claims of elevated suicidal risk from SOCE exposure were based primarily on small-sample qualitative studies (Beckstead & Morrow, 2004; Dehlin et al., 2015; Flentje et al., 2013; Haldeman, 2002, 2012). None of these studies included any measures of suicidality but inferred its presence and scope from narrative comments. The causal connection to SOCE was presumptive and speculative. Dehlin et al. (2015) summarize: "No known study to date has drawn from a representative sample of sufficient size to draw conclusions about the experience of those who have attempted SOCE. Furthermore, no known study to date has provided a comprehensive assessment of basic demographic information, psychosocial well-being, and religiosity, which would be required to understand the effectiveness, benefits, and/or harm caused by SOCE" (p. 96).

Some recent studies have begun to overcome these limitations, employing improved study design, samples, and measures. Salway et al. (2020) analyzed survey questions on SOCE participation and suicidal ideation, estimating subgroup differences in a large non-probability sample (n = 8,388) of Canadian sexual minority men. Ryan et al. (2020) reported on similar questions about SOCE experience and suicidal behavior in a retrospective study of 254 sexual minority young adults. Meanley et al. (2020) examined a clinical sample of 1,156 older men with AIDS in four cities, which was roughly representative of urban men who have sex with men (MSMs). Finally, Blosnich et al. (2020) examined a national probability sample of sexual minority persons, gathered via random telephone sampling, which collected extensive measures of the nature and timing of SOCE exposure as well as multiple forms of suicidal ideation, including thinking about, intending, planning, and attempting suicide. All of these efforts improved the state of knowledge by (1) using larger, more objective samples, and for Blosnich et al.'s (2020) study, a national probability sample, (2) asking direct questions rather than inferring from incomplete open-ended comments, and (3) including a comparison group of non-SOCE participants.

Despite these improvements, all four studies also exemplify a serious error which may render their findings invalid: each reports an association of SOCE with suicidality as if the former caused the latter, without

examining the possibility that the suicidality may have preceded recourse to therapy. This problem is not trivial for these studies, since all four call for SOCE to be restricted due to its negative health outcomes, and in particular its invidious effect on suicide risk.

Ryan et al. (2020) did not measure the timing of SOCE relative to suicidality, but the remaining three studies all made use of data that could have addressed this question, but did not do so. The response set for Salway et al.'s (2020) questions pertaining to SOCE exposure and having thought about or attempted suicide ("no; yes, some time ago; yes, last 12 months; yes, both prior to and last 12 months") permitted at least a crude specification of time order, but instead of making use of this information Salway et al. collapsed all "yes" responses to both suicide questions into a single variable indicating "having ever thought about or attempted suicide " (p. 3). Meanley et al. (2020) had access to 32 years of longitudinal data measuring depression and other negative psychosocial health conditions for men at least 40 years old who reported an average age of 24 at the initiation of SOCE therapy. A third of their respondents initiated SOCE after age 34, suggesting that pre-existing psychopathology was a real possibility, yet they did not take steps to address this important potential confounder.

Blosnich et al. (2020) made use of data from the Generations Study, a well-crafted survey of a population-based sample (N = 1518) of the sexual minority population in the USA administered by the Williams Institute from 2016 to 2018 (Meyer, 2020). Detailed follow-up questions determined the respondent's age when both SOCE and suicidality were reported, as described further in the Measures section below. Despite the availability of such comprehensive information on the timing of both suicidality and SOCE, Blosnich et al. did not attempt to determine to what extent the former may have preceded the latter.

The present study amends this lack by replicating and then adjusting Blosnich et al.'s (2020) findings to account for suicidality that may have preceded SOCE. The working hypothesis of this analysis is that a substantial portion of suicidal experience occurred prior to undergoing SOCE, thus appropriately moderating some or all of the observed effect of SOCE therapy on suicidality. Depending on the extent of the moderation, this will result in one of three possible outcomes relative to Blosnich et al.'s claim that SOCE "may compound or create…suicidal ideation and suicide attempts" (p. 1028): (1) the positive effect of SOCE on suicidality will be reduced (weak hypothesis outcome); (2) there will no longer be an observed association between SOCE and suicidality, indicating that SOCE has no effect on sexual minority suicide (moderate hypothesis outcome); or (3) the effect of SOCE on suicidality will be negative, indicating that exposure to SOCE significantly reduces suicidality (strong hypothesis outcome).

Method

Participants and Procedure

The data for this study were collected as part of the Williams Institute's Generations Study, an epidemiological study designed to examine the health and well-being of three generational cohorts of non-transgender sexual minority persons in the USA (Meyer, 2020). The cohorts consisted of persons aged 52–59 (Pride Generation) in 2016, whose sexual coming of age took place around the time of the Stonewall riots and the start of the gay liberation movement; persons aged 32–41 years (Visibility Generation) in 2016, whose early life experiences coincided with the beginning of the AIDS epidemic and greater visibility and social acceptance for LGB people; and those aged 18–25 in 2016 (Equality Generation), whose early life experiences were affected by the growing focus on LGB marriage and employment equality. Eligibility was also restricted to the three largest US racial and ethnic groups (Black, Latino, or White, although multi-ethnic identities that included one of these was also included) (Krueger et al., 2020).

Participants were screened by the Gallup Organization using daily random digit dialing (both landline and cell phones) for one year beginning March 2016. Recruitment for Black and Latino participants extended for an additional year, until March 2018. Respondents who identified as "lesbian, gay, or bisexual" but not transgender (who were recruited into a separate companion study) were invited to complete a self-administered online or paper questionnaire, which required 5th grade English proficiency. Calls to 366,640 Americans resulted in a sample of 3,525 eligible participants (1%), of which 1,518 (43%) completed usable interviews. Statistical weighting adjusted for the complex survey sample design, differential non-response, the extended sample of Black and Latino respondents, and known characteristics of the sexual minority population as reflected in prior data collected by Gallup and the US Census. The resulting data are designed to be generalizable to the US population of sexual minority adults and have formed the basis for several prior studies and estimates for this population (Meyer et al., 2021; Nock et al., 2009; Rothblum et al., 2020). More information about the study's methodology and sample characteristics is available online at http://www.generations-study.com and in several published reviews and methodological reports (Krueger et al., 2020; Meyer, 2020; Meyer et al., 2020).

Measures

Measures closely followed those reported by Blosnich et al. (2020). Model covariates included gender identity, with categories of man, woman, and non-binary; sexual identity, with categories of lesbian, gay, bisexual, queer, pansexual, asexual, and other; race and ethnicity, with categories of White, Black, Hispanic, and other; educational attainment, with categories of high school or less, some college, college degree, and more than a college degree; and age in years.

For SOCE participation, respondents were asked, "Did you ever receive treatment from someone who tried to change your sexual orientation?" and if so, their age when such treatment last occurred. The specific type of treatment was not characterized further, although follow-up questions asked whether the treatment was from a religious leader or healthcare professional. Four questions addressed lifetime suicidal behavior: "Did you ever in your life have thoughts of killing yourself?" "Did you ever think about how you might kill yourself (e.g., taking pills, shooting yourself) or work out a plan of how to kill yourself?" and "Did you ever make a suicide attempt (i.e., purposefully hurt yourself with at least some intention to die)?" An additional question on suicide intention was not analyzed in Blosnich et al.'s study. Response options for each question were "No," "Yes, once," and "Yes, more than once." Follow-up questions for the yes responses asked how old the respondent was when they engaged in the behavior or in both the first and most recent of multiple instances of the behavior (Blosnich et al., 2020).

Adverse Childhood Experiences (ACEs)

The ACE score was expressed as the additive index of eight indicators of childhood experiences identified by the Centers for Disease Control and Prevention (CDC) to be negatively related to adult health outcomes: sexual abuse; physical abuse; emotional abuse; substance abuse in the household; intimate partner violence in the household; mental illness in the household; a family member imprisoned; and parental separation or divorce. Three ACEs figured prominently in the analysis. Sexual abuse was measured by three questions: "(Before 18 years of age) How often did anyone at least 5 years older than you, or an adult, ever touch you sexually?", "(Before 18 years of age) How often did anyone at least 5 years older than you, or an adult, try to make you touch them sexually?", and "(Before 18 years of age) How often did anyone at least 5 years older than you, or an adult, force you to have sex?" The response options were "Never," "Once," or "More than once," and responses to the three questions were combined into a single measure of sexual abuse. Mental illness and emotional abuse were assessed by similarly worded questions asking whether before age 18 the respondents had "live[d] with anyone who was depressed, mentally ill, or suicidal," or how often they had been sworn at, insulted or put down. Indicator variables coded one for the presence or zero for the absence of each of these experiences.

Childhood Bully Victimization

Respondents were asked "How often, if ever, were you bullied before you were 18 years old?" The mean of the 4-point response scale (often, sometimes, rarely, never) was reverse coded so that higher scores indicated more frequent childhood bully victimization.

Statistical Analysis

The analysis proceeded in three stages. The first goal was to replicate the findings of Blosnich et al.'s (2020) logistic regression models showing a positive association between SOCE and suicide. This involved reconstructing, as closely as possible, the same variable classifications reported in that study. Second, the models were adjusted to account for the timing of suicidality relative to SOCE. Third, the analysis was extended beyond Blosnich et al.'s models in order to examine the relationship of SOCE and suicidality more fully. The extended models included additional covariates and employed ordered logistic regression to examine repeated instances of suicidal behavior. All analyses used survey weights to allow for generalization to the US population of sexual minority adults, ages 18–27, 32–43, and 50–61. Comparative results using unweighted data are presented in a supplement (Tables S1–S9). All models presented, including those that replicated Blosnich et al.'s findings, were certified for proper model specification using the Pregibon/Tukey goodness of link test (Pregibon, 1980) and for acceptable fit to the data using the Hosmer and Lemeshow goodness of fit procedure for complex sample designs (Archer et al., 2007). Analyses were performed using SPSS 25 and Stata 13 statistical software. As a secondary analysis of pre-existing public data, the present study's methods were certified to be exempt from human subject ethical review under 45 CFR 46.104 by the Catholic University of America Institutional Review Board in ethical certification decision number 21-0016.

Results

Blosnich et al. (2020) reported sociodemographic characteristics of the sample—sexual identity, gender identity, racial identity and educational attainment— by SOCE experience (p. 7). A total of 69.0% (95% CI 57.0–78.8) of those receiving SOCE therapy or efforts did so from a religious leader, "such as a pastor, religious counselor or priest;" 19.2% (95% CI 11.8–29.5) from

a healthcare professional "such as a psychologist or counselor who was not religious-focused;" and 11.9% (95% CI 5.7–23.1) from both. Those experiencing SOCE did so at a young age, 18.2 years (95% CI 16.9–19.3) on average, evenly split among those who underwent SOCE as a minor (49.9%, 95% CI 37.8–62.0), at an average age of 14.5 years (95% CI 13.6–15.4), and as an adult (50.1%, 95% CI 38.0–62.2), at an average age of 21.8 years (95% CI 20.4–23.2). Suicidal morbidity also tended to be expressed (or in cases of multiple instances, to begin to be expressed) at a young mean age, when the respondent was still a minor: 15.8 years (15.4–16.2) for suicidal thoughts; 17.0 years (95% CI 16.5–17.5) for suicide planning; and 17.4 (95% CI 16.7–18.0) years for suicide attempts.

Table 1 reports descriptive statistics for the timing of lifetime suicidal behavior relative to SOCE. Unweighted counts are shown with population-weighted percentages unadjusted for covariates. The lines labeled "Total suicide behavior" follow Blosnich et al.'s categorizations, which included all suicidal behavior regardless of when it occurred. For SOCE participants, three rows partition total suicide behavior by its timing relative to SOCE (before, during, after). The results show that Blosnich et al.'s categories included a substantial amount of suicidal behavior that preceded SOCE therapy. Of the 89 SOCE participants who reported ever having thoughts of suicide, 58 (65%) of them did so before they underwent SOCE. Likewise, almost half of reported suicide planning (49%) and suicide attempts (48%) occurred prior to SOCE. For every type of suicidal behavior, Blosnich et al.'s inclusion of pre-SOCE suicide behavior inflated the prevalence among SOCE participants to a rate higher than that among those who had never undergone SOCE (for example, 84.9% for suicidal ideation with SOCE compared to 73.4% with no SOCE). When only suicidality during or after SOCE is considered, the unadjusted prevalence in the SOCE group was no longer significantly higher than in the non-SOCE group for any form of suicidality, and was significantly lower for suicide ideation and planning.

Most persons who engaged in suicidal behavior reported multiple instances of suicidal thoughts (73.4%, 95% CI 70–77) and planning (63.9%, 95% CI 60–68), and a third reported multiple suicide attempts (34.2%, 95% CI 29–40). The bottom panel of Table 1 documents the relation of repeated suicidal behavior to the experience of SOCE. "Before intervening SOCE" reports behavior expressed before SOCE that recurred during or after SOCE treatment; "During/after SOCE" reports behavior that began following SOCE. Together, these offer crude measures of the propensity of SOCE treatment to curb or instigate suicidal behavior. With the exception of suicide ideation expressed before SOCE, these measures suggest that SOCE treatment may be more effective than its absence both in ameliorating prior and reducing subsequent suicidal behavior. With the exception of suicide ideation before intervening SOCE, less suicidal behavior recurred or began following SOCE than if no SOCE had occurred. Both effects were strongest for the most serious suicidal behavior: suicide attempts. The prevalence of repeated suicide attempts following SOCE (9.8%) was less than one-third the prevalence with no SOCE experience (36.1%).

Table 1 Prevalence of lifetime suicide morbidity by sexual orientation change efforts (SOCEs), in percent: Probability Sample of Sexual Minorities, USA, 2016–2018 (n = 1518)

Suicide behavior first occurred	Suicide ideation *N* (%, S.E.)	Suicide plan *N* (%, S.E.)	Suicide intention *N* (%, S.E.)	Suicide attempt *N* (%, S.E.)
SOCE (n = 108)				
Before SOCE	58 (55.0,6.1)	36 (33.2,5.9)	31 (31.3,5.9)	20 (17.8,4.4)
During SOCE	11 (7.7,2.8)	13 (10.6,3.8)	11 (11.9,4.3)	7 (4.0,1.6)
Subtotal Before + During	69 (62.8, 6.0)	49 (43.8, 6.2)	42 (43.2, 6.2)	27 (21.8, 4.7)
After SOCE	20 (22.1,5.2)	24 (28.2,5.8)	17 (21.5,5.5)	15 (15.8,4.6)
Total suicide behavior (first instance)	89 (84.9, 4.1)*	73 (72.0, 5.4)*	59 (65.1, 5.5)*	42 (40.4, 6.0)*
No SOCE (n = 1410)				
Total suicide behavior (first instance)	967 (73.4, 1.4)	763 (58.7,1.6)	524 (42.3, 1.7)	323 (26.6, 1.5)
Percent repeated instances/attempts				
Before intervening SOCE	45 (79.4, 6.8)	19 (55.0, 10.6)	15 (46.3, 11.3)	5 (28.1, 12.2)
During/After SOCE	17 (56.5, 11.9)	10 (29.0, 9.6)**	12 (50.3,11.9)	2 (9.8, 7.6)**
No SOCE	691 (73.1, 1.8)	451 (62.1, 2.1)	265 (55.1, 2.6)	104 (36.1, 3.3)

Percents shown are population-weighted. "Before SOCE" occurred at least a year before SOCE occurred; "During SOCE" occurred in the same year as SOCE; "After SOCE" occurred in a year more recent than SOCE. Asterisks report significant difference from the corresponding "No SOCE" category by t-test, $p < .10$; *$p < .05$; **$p < .01$; ***$p < .001$. Missing data for age at SOCE reduced usable cases by 2

Table2 Adjusted odds ratios (AORs) for lifetime 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 intention AOR or % (95% CI)	Suicide attempt AOR or % (95% CI)
"Experienced SOCE"				
1. Per Blosnich et al.	1.93 (1.02, 3.67)*	1.75 (1.01, 3.06)*	2.50 (1.56, 4.00)****	1.75 (.99, 3.08)
2. Treatment Completion Model	.44 (.20, .94)*	.60 (.32, 1.14)	.86 (.47, 1.57)	.74 (.36, 1.43)
3. Treatment Initiation Model	.72 (.35, 1.50)	.88 (.49, 1.56)	1.38 (.81, 2.34)	.96 (.49, 1.90)
4. Compounding Model	.92 (.52, 1.61)	.86 (.52, 1.42)	.74 (.36, 1.43)	.93 (.50, 1.73)

Odds ratios were estimated from population-weighted logistic regression models. ACEs, age, gender identity, sexual minority identity, race and educational attainment were included in the models but are suppressed in the table. AOR significantly different from unity, by t-test: *p<.05; **p < .01; ***p < .001; ****p < .0001

Table 2 presents adjusted odds ratios (AORs) estimated from logistic regression models which are adjusted, following Blosnich et al., for age, gender identity, sexual minority identity, race/ethnicity, educational attainment, and the number of ACEs. Four models are presented. The AORs for Model 1 (labeled "Per Blosnich et al.") replicate those reported in Table 4 of Blosnich et al.'s study. The AOR for suicide planning was identical to Blosnich et al.'s (both 1.75), as were the covariate coefficients (not shown). The AOR for suicidal ideation differs trivially (Blosnich et al. reported 1.92, the present study estimated 1.93) due to missing data on age at SOCE. The AOR for suicide intention was large, positive and significant; Blosnich et al. did not examine this outcome. The AOR for suicide attempts estimated in the present study (1.75) was midway between two AORs that Blosnich et al. reported according to the severity of injury (1.67 and 1.88). Although Blosnich et al. interpret it causally, Model 1 is only an association model which measures the covariation of SOCE exposure with lifetime suicidality, regardless of when either of these conditions occurred.

Models 2 through 4 present true treatment models, which conceive of SOCE as an intervention which may initiate or aggravate suicidality in those who experience it. Models 2 and 3 impose a standard treatment and response analysis, comparing the risk of becoming suicidal after being exposed to SOCE with the risk of becoming suicidal in the absence of SOCE exposure. Since respondents were asked for dates only by year, we cannot know whether suicidal behavior expressed in the same year as SOCE exposure occurred before or after SOCE began, so Models 2 and 3 express disparate assumptions on this point. Model 2 ("Treatment Completion") assumes that same-year suicidality all occurred before SOCE participation, and is thus considered pre-existing suicidality. This model controls for the existence of suicidality that pre-existed the completion of SOCE participation, but does not address the possibility that, in addition to suicidal behavior resulting from the completion of SOCE therapy, suicidal behavior may also have been caused by the experience of SOCE. Model 3 ("Treatment Initiation") examines this latter possibility, expressing the assumption that same-year suicidality all occurred after SOCE began and may thus be a result of the SOCE experience. This assumption is a more restrictive test of the possibility of SOCE-induced suicidality, as indicated by the higher predicted AORs for Model 3 for all four outcomes. With one exception, the estimated AORs for both Models 2 and 3 were not significantly different than unity for any outcome, indicating that 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. The exception is the Model 2 AOR for suicide ideation (0.44, 95% CI 0.20-0.94), which was significantly lower than one, indicating that, after accounting for pre-existing suicide ideation both before and during SOCE therapy, the risk of suicidal thoughts following SOCE therapy was significantly reduced by more than half.

Adjusting for suicidal behavior that pre-dates treatment addresses the possibility that SOCE may have created suicidality where there was none before, but not the possibility that SOCE may have compounded suicidality that was initially expressed prior to SOCE. Model 4 ("Treatment Compounding") addresses this question. This model compares the risk of expressing any suicidality following SOCE, regardless of when the suicidality in question may have begun, with the risk of ever expressing suicidality for persons never exposed to SOCE. This imposes an even more extreme test of SOCE causation, which posits that only persons who did not express suicidality following SOCE can be considered not to have been suicidal due to SOCE, whether or not they expressed suicidality prior to SOCE. Even by this very restrictive standard, none of the suicidal measures were positively associated with SOCE, as indicated by non-significant model AORs ranging from 0.86 to 0.93.

Preliminary investigation revealed that predicted outcomes were strongly different for those who had experienced SOCE as a minor (under age 18) than as an adult (age 18 years or over). Table 3 reports the results for the treatment initiation

Table3 Adjusted odds ratios (AORs) for suicidality after experiencing SOCE as a minor (n = 43) or an adult (n = 63): Probability Sample of Sexual Minorities, USA, 2016–2018 (N = 1,518)

Models	Suicidal ideation AOR or % (95% CI)	Suicide planning AOR or % (95% CI)	Suicide intention AOR or % (95% CI)	Suicide attempt AOR or % (95% CI)
Treatment initiation model				
SOCE as a minor (under 18)	1.04 (.36, 3.03)	1.12 (.50, 2.53)	2.66 (1.25, 5.68)*	1.73 (.75, 3.99)
SOCE as an adult (18 or older)	.30 (.09, 1.01)	.56 (.24, 1.31)	.51 (.22, 1.14)	.19 (.06, .66)*

Odds ratios were estimated from population-weighted logistic regression models. Reference category is "no SOCE". ACEs, age, gender identity, sexual minority identity, race and educational attainment were included in the models but are suppressed in the table. AOR significantly different from unity, by t-test: *p < .05; **p < .01; ***p < .001; ****p < .0001

model (Model 3 in Table 2). The two groups by age at the time of SOCE sharply and consistently partition the AORs shown in Table 2 into higher (or less reduced) odds of post-SOCE suicidal expression for those exposed as minors and lower (or more reduced) odds for those exposed as adults. According to the model shown in Table 3, minors, but not adults, were 2.7 times more likely to express suicidal intentions following SOCE, while adults, but not minors, were over 5 times less likely to attempt suicide following SOCE. Adults may also have been less likely to have thoughts of suicide after SOCE (p = .053 for this AOR of 0.30).

To clarify these results further, the present study attempted to examine some pertinent distinctions and predictors in the relation of SOCE and suicidal expression. This required improving the precision of some elements of the predictive model presented by Blosnich et al., after correcting for the absence of adjustment for pre-existing suicidality. Table 1 notes that a majority of those who reported suicidal behavior other than suicide attempts reported doing so more than once; up to four instances of each suicidal behavior was recorded in the data file. Blosnich et al. collapsed these multiple instances of suicidal behavior into indicator variables signifying only that the respondent had ever engaged in the respective behavior. Persons who reported multiple suicide attempts, for example, were coded the same as those who reported only a single attempt. To capture this additional variation, the four suicidality measures were disaggregated into variables reporting both single and multiple instances of suicidal behavior. These ordinal outcomes required the use of ordered logistic regression. This method permitted the conditions of SOCE prior to any suicidality (the compounding model) and SOCE intervening between prior and posterior suicidal expressions (the treatment models) to be included, with some restrictions, in a single model. Table 4 compares for each outcome the model AOR resulting from the inclusion of repeated suicidality with that of the treatment (initiation) model. For all four suicidal outcomes, including repeated suicidality results in reduced AOR estimates, suggesting that part of the difference in the odds of suicidality between SOCE and non-SOCE participants may be attributed to the fact that SOCE participants were less likely to engage in repeated suicidal behavior.

To control for childhood conditions that predict suicidality, Blosnich et al. (2020) included the summary index of indicator variables for 8 ACEs which are known to negatively affect later life outcomes. However, as they reported (p. 1027), not all of the ACEs were related to suicide and/or SOCE treatment. The present study examined the contribution of each ACE measure to model fit, as indicated by the likelihood-ratio Chi-square test, which compares models with and without the variable to see if the former explains the variation in the data more fully or accurately than the latter. For each suicidal outcome, only 3 of the 8 ACEs significantly improved the fit of the model predicting suicidality conditioned by SOCE: emotional abuse, household mental illness, and sexual abuse. After including these three ACEs, the difference chi-square for the model also including all of the remaining five ACEs ranged from 2.71–10.18, yielding p-values (with 5 degrees of freedom) of 0.0703-0.7749 (see Table S7). The summary index of all 8 ACEs was therefore replaced with one including only the three significant ACEs. This resulted in modest further reductions in the predicted AOR for SOCE exposure, as Table 4 shows, suggesting that SOCE participants may vary from other sexual minorities in their exposure to the three significant ACEs predicting suicidal expression among sexual minorities. An additional measure of childhood distress, bully victimization, which also improved both model fit and the prediction of differences due to SOCE, was also included in the improved model.

Table 5 presents the prevalence by SOCE exposure and zero-order AORs for suicide attempts for the predictor variables in the improved model. In addition to improving model fit, the summary index of the three significant ACEs was much more strongly associated with suicide attempts, at an AOR of 2.0, than was the sum of all 8 ACEs, at 1.3.

Table4 Adjusted odds ratios (AORs) for lifetime suicidality by experience of sexual orientation change efforts (SOCE), showing the effect of model improvements: Probability Sample of Sexual Minorities, USA, 2016–2018 (N =1,518)

	Suicidal ideation AOR or % (95% CI)	Suicide planning AOR or % (95% CI)	Suicide intention AOR or % (95% CI)	Suicide attempt AOR or % (95% CI)
"Experienced SOCE"				
Per Blosnich et al.	1.93 (1.02, 3.67)*	1.75 (1.01, 3.06)*	2.50 (1.56, 4.00)****	1.75 (.99, 3.08)
Accounting for suicidality prior to SOCE	.72 (.35, 1.50)	.88 (.49, 1.56)	1.38 (.81, 2.34)	.96 (.49, 1.90)
Accounting for repeated suicidality	.66 (.35, 1.24)	.61 (.38, .98)*	1.25 (.75, 2.09)	.83 (.46, 1.51)
Including only significant ACEs	.62 (.33, 1.17)	.58 (.36, .92)*	1.15 (.68, 1.94)	.75 (.43, 1.31)

Odds ratios were estimated from population-weighted logistic regression models. ACEs, age, gender identity, sexual minority identity, race and educational attainment were included in the models but are suppressed in the table. AOR significantly different from unity, by t-test: *p<.05; **p < .01; ***p < .001; ****p < .0001

Each of the three component ACEs strongly and significantly predicted suicide attempts. All the predictors except household mental illness varied significantly by SOCE participation. The significance p-value of the unweighted difference for "All SOCE" for this variable was 0.053.

Table 6 presents the improved model results. The "All SOCE" AORs for this model were lower for all four outcomes than the corresponding AORs in the treatment initiation model (Table 2), which added to Blosnich et al.'s models only a control for pre-existing suicidality. This provides a crude confirmation that the improved model actually did improve measurement. The ratio of the SOCE odds ratios for the improved model with the treatment initiation model (Table 2), respectively, predicting suicide ideation,

Table5 Population prevalence of childhood predictors of suicidality, by SOCE participation as a minor (n=43) or an adult (n=63): Probability Sample of Sexual Minorities, USA, 2016–2018 (N = 1,518)

Variable (range)	No SOCE Mean (95% CI)	All SOCE Mean (95% CI)	SOCE as adult Mean (95% CI)	SOCE as minor Mean (95% CI)	Suicide Attempts AOR (95% CI, p)
Sum of ACEs (0–8)	3.27 (3.13, 3.41)	4.18 (3.58, 4.78)**	3.92 (2.94, 4.90)	4.51 (3.77, 5.26)**	1.32 (1.23, 1.41), < .001
Emotional abuse (0,1)	.69 (.66, .72)	.83 (.74, .92)*	.76 (.62, .90)	.92 (.84, 1.00)****	2.93 (2.06, 4.18), < .001
Parent mental illness (0,1)	.46 (.43, .49)	.54 (.42, .66)	.53 (.37, .69)	.59 (.42, .66)	2.71 (2.00, 3.68), < .001
Sexual abuse (0,1)	.35 (.32, .38)	.58 (.46, .70)***	.56 (.40, .73)*	.59 (.41, .76)**	2.03 (1.48, 2.77), < .001
Sum of above 3 ACEs (0–3)	1.50 (1.44, 1.56)	1.95 (1.71, 2.18)***	1.86 (1.50, 2.21)	2.10 (1.79, 2.40)***	2.00 (1.70, 2.34), < .001
Childhood Bully Victimization (0–1)	2.92 (2.85, 2.99)	3.13 (2.87, 3.39)	2.95 (2.55, 3.35)	3.33 (3.01, 3.64)*	1.65 (1.40, 1.94), < .001

Odds ratios were estimated from population-weighted ordered logistic regression models adjusted for age, gender identity, sexual minority identity, race and educational attainment. Different from "No SOCE" by F-test: *p < .05; **p < .01; ***p < .001; ****p < .0001

Table 6 Adjusted odds ratios (AORs) for suicidality after experiencing SOCE as a minor (n = 43) or an adult (n = 63), showing improved model results: Probability Sample of Sexual Minorities, USA, 2016–2018 (N = 1,518)

Models	Suicidal ideation AOR or % (95% CI)	Suicide planning AOR or % (95% CI)	Suicide intention AOR or % (95% CI)	Suicide attempt AOR or % (95% CI)
All SOCE	.55 (.31, .998)*	.53 (.33, .84)**	1.10 (.65, 1.87)	.69 (.38, 1.23)
SOCE as minor	.63 (.29, 1.34)	.56 (.32, 1.00)*	1.52 (.75, 3.07)	.98 (.51, 1.88)
SOCE as adult	.39 (.19, .81)*	.38 (.21, .68)**	.53 (.29, .98)*	.24 (.10, .57)**
Three ACEs	1.55 (1.34, 1.80)****	1.64 (1.42, 1.89)****	1.62 (1.40, 1.87)****	1.82 (1.54, 2.16)****
Childhood bully victimization	1.68 (1.47, 1.91)****	1.41 (1.24, 1.61)****	1.43 (1.23, 1.66)****	1.42 (1.20, 1.68)****

Odds ratios were estimated from population-weighted ordered logistic regression models. Reference category for SOCE is "no SOCE". The following variables were included in the model but are suppressed in the table: prior suicidal behavior, age, gender identity, sexual minority identity, race and educational attainment. AOR significantly different from unity, by t-test: *p<.05; **p<.01; ***p<.001; ****p<.0001

planning, intention, and attempts was 0.76, 0.60, 0.80, and 0.72, suggesting that roughly a quarter of the SOCE-related suicidality reported in the treatment initiation model can be accounted for by the measurement improvements included in the improved model.

In the improved model, compared to persons not experiencing SOCE, suicidal expression was significantly reduced for adults following SOCE for each of the four outcomes examined. Suicide planning was also reduced for those undergoing SOCE as minors. Overall, sexual minorities were only about half as likely to engage in thoughts or plans of suicide following SOCE as those who had not experienced SOCE. The strongest effect of SOCE was on suicide attempts among adults, who were less than one-fourth as likely to attempt suicide following SOCE as were adults who had not undergone SOCE. For all four outcomes, the AORs for "All SOCE" were reduced from those shown in the bottom row of Table 4. The difference was due to the use in the Table 6 models of a summary measure for household mental illness, emotional abuse, and sexual abuse, and the inclusion of bully victimization.

Table 7 disaggregates the three significant ACE suicide predictors in order to examine their relative strength. For all outcomes, household mental illness predicted suicide behavior most strongly, followed by emotional abuse, and then by sexual abuse, which was the weakest predictor of the three. Persons who experienced household mental illness were about twice as likely to engage in suicidal behavior. All three ACEs predicted suicide attempts a little more strongly than the other three suicidal behaviors. This effect was especially pronounced for sexual abuse, which was significantly associated with suicide intentions and attempts but not with suicidal ideation or planning. On the other hand, having been bullied as a child was a little more strongly associated with thoughts of suicide than with any of the other less frequent suicidal behaviors.

Table 8 Adjusted odds ratios (AORs) predicting SOCE participation as a minor (n = 43) or an adult (n = 63): Probability Sample of Sexual Minorities, USA, 2016–2018 (N= 1,518)

	SOCE as minor	SOCE as adult
ACE: Emotional Abuse	3.34 (1.15, 9.71)*	1.24 (.59, 2.60)
ACE: Mental Illness	1.33 (.65, 2.74)	1.42 (.73, 2.79)
ACE: Sexual Abuse	2.41 (1.17, 4.97)*	1.88 (1.05, 3.36)*
Bully Victimization	1.34 (.83, 2.14)	.99 (.68, 1.44)
Sex (ref = female)	2.66 (1.25, 5.64)*	1.32 (.69, 2.51)
Current age	.97 (.94, 1.00)*	1.04 (1.01, 1.06)**

Odds ratios were estimated from population-weighted logistic regression models. The following variable was included in the model but is suppressed in the table: race. AOR significantly different from unity, by t-test: *p < .05; **p < .01; ***p < .001; ****p < .0001

Table 8 presents the direct associations of the improved model predictors with SOCE participation as a minor or an adult. Those who had experienced SOCE as a minor were more likely to be male, to have experienced emotional or sexual abuse, and to have undergone SOCE more recently. Those who had undergone SOCE as an adult were also more likely to have been sexually abused but to have undergone SOCE less recently.

Suicidal behavior, as noted above, can be progressive, leading from less to more serious forms before resulting in a suicide attempt. In the Generations data, less frequent forms were strongly nested inside more frequent ones. Almost all of those who reported making a suicide plan (97.6%, 95% CI 96.3, 98.5) or declaring intent (98.3%, 95% CI 96.4–99.3) also reported thinking of suicide. The proportion of suicidal behaviors that were followed by a suicide attempt rose from 36% for suicidal ideation (36.4, 95% CI 32.9, 40.2) to 44% (43.5, 95% CI 39.4, 47.7) for suicide planning to 58% (57.8, 95% CI 52.9, 62.7) for suicide intention. Of those reporting suicide attempts, 99% (98.5%, 95% CI 96.6, 99.3)

Table 7 Adjusted odds ratios (AORs) for suicidality after experiencing SOCE as a minor (n = 43) or an adult (n = 63), showing the effect of individual ACEs: Probability Sample of Sexual Minorities, USA, 2016–2018 (N = 1,518)

Models	Suicidal ideation AOR or % (95% CI)	Suicide planning AOR or % (95% CI)	Suicide intention AOR or % (95% CI)	Suicide attempt AOR or % (95% CI)
All SOCE	.60 (.33, 1.10)	.56 (.35, .90)*	1.23 (.66, 1.92)	.70 (.39, 1.27)
SOCE as minor	.68 (.32, 1.45)	.59 (.33,1.06)	1.56 (.77, 3.17)	1.01 (.53, 1.93)
SOCE as adult	.41 (.19, .86)*	.40 (.22, .72)**	.53 (.29, .99)*	.25 (.10, .57)**
ACE: Emotional Abuse	1.68 (1.26, 2.25)****	1.69 (1.25, 2.29)**	1.57 (1.15, 2.14)**	1.75 (1.19, 2.59)**
ACE: Mental Illness	2.06 (1.57, 2.70)****	2.04 (1.56, 2.67)****	1.87 (1.40, 2.48)****	2.22 (1.60, 3.07)****
ACE: Sexual Abuse	1.00 (.75, 1.34)	1.22 (.91, 1.61)	1.42 (1.06, 1.90)*	1.52 (1.09, 2.12)*
Childhood bully victimization	1.68 (1.47, 1.93)****	1.41 (1.23, 1.61)****	1.43 (1.24, 1.66)****	1.42 (1.20, 1.69)****

Odds ratios were estimated from population-weighted ordered logistic regression models. Reference category for SOCE is "no SOCE". The following variables were included in the model but are suppressed in the table: pre-existing suicidality, age, gender identity, sexual minority identity, race and educational attainment. AOR significantly different from unity, by t-test: *p < .05; **p < .01; ***p < .001; ****p < .0001

also reported thinking of suicide, 94% (93.9, 95% CI 90.5, 96.3) making a suicide plan, and 92% (91.9, 95% CI 87.6, 94.8) declaring suicidal intent beforehand. Since suicide attempts most strongly predict completed suicide (Harris & Barraclough, 1997; Suominen et al., 2004), a primary goal of prevention is to reduce the number of attempts following ideation, planning or declaration of intention. (Turecki & Brent, 2016).

Table 9 presents findings for the relation of SOCE to suicidal progression, showing the AOR for suicide attempts following an initial expression of suicidal behavior that conditioned on the presence or absence of intervening SOCE treatment. The results indicate that SOCE experience was associated with a very strong reduction in the risk of a suicide attempt following suicide ideation, planning or intention, but not following a suicide attempt prior to SOCE. Following initial thoughts of suicide, those not receiving SOCE therapy were almost six times as likely (5.9, the inverse of the AOR of 0.17 shown) to attempt suicide as were those exposed to SOCE. The corresponding elevation in the risk of a suicide attempt in the absence of intervening SOCE was similarly large following initial suicide planning (4.3) or intention (12.5). After an initial suicide attempt, however, SOCE participation was not associated with any difference in the risk of a subsequent attempt.

Partitioning the results by age, shown in the second and third rows of Table 9, reveals that most of the reduction in suicide attempt risk following SOCE was confined to those undergoing SOCE as adults rather than as minors. Adults undergoing subsequent SOCE after suicidal thoughts, plans or attempts were far less likely to attempt suicide, compared to adults who did not undergo SOCE. Stating the inverse for ease of interpretation, Table 9 reports that following initial thoughts of suicide, adults who did not receive subsequent SOCE therapy were over 16 times more likely to attempt suicide than were those who had undergone SOCE, and over 33 times more likely to attempt suicide following the making of a suicide plan or the declaration of a suicide intention. On the other hand, minors who experienced SOCE following suicidal thoughts, plans or intentions did not experience significant reductions in the risk of suicide attempts, and neither minors nor adults undergoing SOCE after a suicide attempt were less susceptible to a further attempt than were those who did not undergo SOCE.

Discussion

A fundamental principle of the scientific method is that temporal precedence, in which the cause precedes the effect in time, is necessary to establish a real-world (nomothetic or efficient) cause-effect relationship (Babbie, 2012, p. 92; Hausman, 1998; Hume, 1748, sec. VII). For this reason, one of the earliest lessons learned by any student of modern statistics is that "correlation alone is not causation." To complete the causal inference, one must also establish which of two or more correlated elements came first, and in observational data, eliminate other possible causes.

Blosnich et al.'s (2020) study of SOCE and suicidality offers a cautionary example of the harm that can result when this principle is ignored. In their analytic claim that "sexual minorities who experienced SOCE reported a higher prevalence of suicidal ideation and attempts than did sexual minorities who did not experience SOCE" (p. 1024), over half (55%, unweighted) of the cases they report as having "experienced SOCE" actually consisted of persons who expressed suicidality before ever experiencing SOCE treatment. Most of the suicidality did not follow SOCE in time but preceded it. Consequently, Blosnich et al. falsely concluded that SOCE treatment has an "insidious association with suicide risk" and that it "may compound or create … suicidal ideation and suicide attempts." We might call this the fallacy of association.

Correcting Blosnich et al.'s analysis for time order revealed substantially different results in the present study. After controlling for pre-existing conditions, there no longer remained any positive association of SOCE with suicidality in the Generations data (moderate hypothesis outcome). Where

Table 9 Adjusted odds ratios (AORs) of suicide attempts following previous suicidality (suicidal ideation, planning, intention or previous suicide attempt) by experience of intervening SOCE as minor (*N*=43) or adult (*N*=63): Probability Sample of Sexual Minorities, USA, 2016–2018 (*N*= 1,518)

Suicide attempts following	Suicidal ideation (*N* = 1026) AOR (95% CI)	Suicide planning (*N* = 792) AOR (95% CI)	Suicide intention (*N* = 556) AOR (95% CI)	Suicide attempt (*N* = 336) AOR (95% CI)
All SOCE	.17 (.05, .55)**	.13 (.04, .45)**	.10 (.03, .30)****	.55 (.15, 2.05)
SOCE as minor	.36 (.32, 1.86)	.52 (.10, 2.78)	.30 (.07, 1.31)	1.18 (.21, 6.81)
SOCE as adult	.06 (.01, .30)**	.04 (.00, .34)**	.04 (.01, .21)****	.68 (.15, 3.12)

AORs and percentages estimated from population-weighted ordered logistic regression models adjusted for ACEs, bullying victimization, age, gender identity, sexual minority identity, race, and educational attainment. Covariates are suppressed in the table. AOR significantly different from unity, by *t*-test: *$p < .05$; **$p < .01$; ***$p < .001$; ****$p < .0001$

there was a significant association, suicidality following SOCE was reduced, not increased. For adults undergoing SOCE, the overall odds of suicide ideation were reduced by over two-thirds (AOR of 0.30) and suicide attempts were reduced by four-fifths (AOR of 0.19) in Blosnich et al.'s own models, adjusted only for prior suicidality and age differences.

Modest improvements in measurement revealed that adults undergoing SOCE therapy experienced significant collective reductions in risk of all forms of suicidal behavior (strong hypothesis outcome). Such persons were from half to three-quarters less likely (AORs of 0.53 to 0.24) to engage in suicidal behavior following SOCE than were comparable persons who had not undergone SOCE. Importantly, adults who underwent SOCE therapy after initial expressions of suicidal behavior also experienced sizable reductions in the risk of a subsequent suicide attempt. When followed by SOCE treatment, adult suicide ideation, planning or intention was 17 to 25 times less likely to lead to a suicide attempt. Similar strong reductions in suicide risk were not observed for those undergoing SOCE as minors (with one exception: suicide planning). However (again with one exception: suicide intentions among those undergoing SOCE as minors), under no conditions examined in this study was SOCE associated with an increase in suicidality.

The cause of the SOCE-related reduction in suicide risk cannot be determined from these cross-sectional data, and any consideration of the question is necessarily speculative. Four adverse childhood conditions—household mental illness, emotional abuse, sexual abuse, and bully victimization—predicted about a tenth of the variation in sexual minority suicidal behavior and were more prevalent among persons who subsequently participated in SOCE. Minors undergoing SOCE were over three times as likely to have suffered emotional abuse as were those who did not undergo SOCE as a minor. They were also much more likely to be male. The same distinctions are not true for those who underwent SOCE as an adult. These differences may help account for the weaker observed association of SOCE with reductions in suicidality among those who were minors rather than adults at the time they experienced SOCE. The possible persistence of higher rates of declaration of suicidal intentions among minors, but not adults, undergoing SOCE may account, in part, for the prevalence of anecdotal declarations of post-SOCE suicidality by persons, predominantly males, who were exposed as minors. Together, these findings suggest that differential exposure to childhood conditions that are associated with suicidality may account for about a quarter of the overall association of SOCE with suicidality. These findings are only preliminary and beyond the scope of this study. Further study of these effects, apart from the question of SOCE therapy, may be of value for better understanding the precipitating influences on suicidal behavior among sexual minorities.

One thing that cannot be the cause of reduced SOCE-related suicidality in this study, however, is successful SOCE therapy. Since those who may have attained the goal of SOCE—to adopt heterosexual identity, orientation or sexual function—were systematically screened from the survey sample used in this study, it cannot be the case that the reduction in suicidality was related to resolving distress due to unwanted homosexual orientation. Since other studies have found that those experiencing successful SOCE outcomes tend to report more positive psychological benefits (Dehlin et al., 2015; Lefevor et al., 2019; Shidlo & Schroeder, 2002; Sullins et al., 2021), it is possible that the findings of the present paper understate the beneficial effect of SOCE treatment on suicidality and affect. Blosnich et al.'s (2020) study, and many other studies in this area, acknowledge this limitation. Alternatively, the suicidal participants who subsequently underwent SOCE in this sample may have felt more confirmed in or accepting of their homosexual or other minority sexual orientation by their unsuccessful SOCE experience (Dehlin et al., 2015), resulting in diminished suicidality.

Robust research supports the proposition that therapy that supports client self-determination promotes higher efficacy and lower harm than practices that impose the therapist's values on the client (Israel et al., 2008; Michalak et al., 2004; Moore et al., 2021; Ng et al., 2012; Ryan & Deci, 2000). Israel's review of LGBT client reports, for example, concluded that "therapists who supported client autonomy and accommodated client needs were more helpful than those who did not," finding that almost a third (31%) of unhelpful therapy situations "were characterized by therapists imposing their values, judgment, or decisions on clients." (Israel et al., 2008, p. 300) In the present context, this suggests that clients who may seek therapy to help retain and enjoy same-sex attractions and those who may do so to try to change or manage them in some other way should both be able to find support for their respective goals. While abundant research has explored the problem of clients facing antigay bias, almost none has examined the similar problem of clients facing anti-ex-gay bias. Further research that includes both persons for whom SOCE was unsuccessful and persons for whom SOCE may have been successful would add greatly to our understanding of the full effects of these practices in therapy.

Reduced suicide risk following SOCE may also be due to unobserved factors, such as childhood family solidarity or high religiousness, that may be related both to the propensity to seek SOCE therapy and to lower risk of suicidality. Sexual minority persons who have recourse to therapy following suicidal behavior may also enjoy higher overall social support or less social isolation, factors which are known to reduce

suicide risk (Turecki & Brent, 2016). Therapy-seeking by adults in distress may select for qualities of resilience or resourcefulness that also inhibit suicide behavior. In this case those undergoing other forms of therapy such as gay affirming therapy would likely experience similar reductions in suicide risk.

Surprisingly, given the multiplicity of claims about suicidal outcomes following SOCE, none of the randomized efficacy studies of gay affirming or neutral therapy identified in the most recent review (O'Shaughnessy & Speir, 2018) have assessed outcome suicidality, either ignoring the question altogether (Fals-Stewart et al., 2009; Pachankis et al., 2015; Parsons et al., 2014) or excluding from the study sample persons with prior suicidality (Carrico et al., 2006; Reback & Shoptaw, 2014; Shoptaw et al., 2008) or with current or past psychopathology that likely included suicidality (Antoni et al., 2000; Carrico et al., 2005; Gayner et al., 2012; Shoptaw et al., 2005). More recently, a well-designed study by Pachankis and colleagues examined outcomes following a gay affirming minority stress reduction intervention targeting women with unhealthy depression, alcohol use and suicidal behavior. The study found, however, that the treatment "was associated with only small reductions in minority stress processes and did not affect suicidality" (Pachankis et al., 2020). Although future research may well find such an effect, at this time there is no evidence to support the claim that gay-affirming therapy leads to reductions in suicide risk such as the present study has found following SOCE.

The possibility of a selection effect for reduced suicidality following SOCE is supported by the fact that, in the present study, adults, for whom SOCE treatment was more effective in reducing suicidality, also experienced more suicidality prior to SOCE. The proportion (S.E.) of each suicidal behavior that was expressed prior to undergoing SOCE therapy among those participating in SOCE as adults (compare to Table 1) was: ideation, 77% (7.5); planning, 66% (9.5); and attempt, 78% (10.0). This suggests that these adults may have disproportionately selected into SOCE treatment following suicidal behavior. If this was the case, then by ignoring time order Blosnich et al. (2020) may have simply reversed the direction of causation. For sexual minority adults, experiencing SOCE did not lead to higher suicidality, but experiencing suicidality may have led to higher SOCE participation. As a recent clinical study of SOCE therapy outcomes has observed, rather than demonstrating that "professional psychological [SOCE therapy] instigates suicide…[t]he [Blosnich 2020] study instead seems to communicate that individuals who experience distress are more likely to seek assistance" (Pela & Sutton, 2021).

The findings of this paper are consistent with at least two other studies that have examined the timing of suicidal morbidities relative to the SOCE experience. Beckstead and Morrow (2004), for example, found that "at least" 20 (40%) the 50 SOCE participants in their study reported experiencing significant suicidal ideation before therapy; three (6%) reported attempting suicide. At least 8 (16%) participants reported no further suicidal ideation after therapy. This qualitative account documents that suicidality was generally high in this population prior to undergoing SOCE, although prevalence is difficult to infer further.

Shidlo and Schroeder (2002) reported that, of 202 total participants, 59 (29%) reported a history of suicide attempts, but only 11 (5.4%) had attempted suicide after conversion therapy. Twenty-five participants (12.4%) had attempted suicide prior to conversion therapy; of these, 22 (88%) did not re-attempt suicide following conversion therapy. Twenty-three (11.4%) participants reported a suicide attempt during conversion therapy, of which 20 (87%) also did not re-attempt after therapy. Altogether, 88% of clients in Shidlo and Schroeder's sample who were suicidal before or during conversion therapy did not subsequently exhibit further suicidality, and only 5 persons (2.5% of total participants) without a previous history of suicidal attempts initiated such behavior following conversion therapy (p. 254).

Unfortunately, recourse to the fallacy of association is hardly unique to Blosnich et al.'s (2020) study. Despite presenting only global associations between SOCE and negative psychosocial conditions, Meanley et al. (2020) nonetheless incorrectly concluded: "These findings support classifying conversion therapy as a sexual minority stressor that contributes to psychosocial health inequality." Although they addressed a number of limitations in their study design that precluded attributing causality to SOCE, and were studying older adults using decades of longitudinal data, they never considered the extent to which poor psychosocial health may have predated SOCE.

Ryan et al. (2020) measured recent suicide ideation and lifetime suicide attempts with no attempt to determine how much of these behaviors may have predated SOCE. Nonetheless, they concluded negatively that "attempts to change sexual orientation during adolescence were associated with elevated…suicidal behavior" (p. 164). Like Blosnich et al., their use of the fallacy of association was expressed in an equivocal use of the term "experienced SOCE" in which suicidality that may have been expressed prior to SOCE was attributed to the effect of SOCE treatment (p. 166).

Salway et al. (2020), who alleged strong psychological harm from SOCE solely based on global associations, reported: "We are unable to know whether SOCE preceded the psychosocial health outcomes identified by participants…" This is not true: their measures segregated earlier SOCE exposure ("prior to 12 months ago") and recent suicidality ("in the last 12 months"), which would have

given them a direct measure of the amount of suicidality that followed SOCE. Moreover, since in their sample 22% of those exposed to SOCE reported having been exposed within the past 12 months, and they reported suicidality in terms of lifetime prevalence, it is likely that a substantial proportion of the reported suicidality preceded SOCE exposure. Salway et al. (2020) continued by speculating that "reverse causation is unlikely given that the major drivers of seeking SOCE correspond to environmental attitudes—for example, family religiosity—rather than intraindividual factors" (p. 507). These comments exemplify the logical fallacy of begging the question, that is, assuming what one is trying to prove; in this case, assuming away the possibility that parents, regardless of their religiosity, might be more likely to seek therapy for a child who is suicidal, and characterizing SOCE participants as "survivors" (pp. 502, 507) even while conceding that suicidal ideation may have preceded SOCE. On the basis of such specious findings, Salway et al. proposed "eradicating" SOCE by amending the Canadian criminal code (p. 507).

A disturbing feature of this research is that, at least for some, including Blosnich et al.'s (2020) study, the choice to ignore time order in attributing causation was not inadvertent but intentional. Salway et al. (2020) simply rejected the problem of the fallacy of association as unimportant: "Even if loneliness, depression, anxiety, or suicidal ideation preceded SOCE attendance, the history of these factors minimally suggests that SOCE survivors should be assessed for any current, ongoing mental health struggles" (p. 4). Blosnich et al. similarly dismissed an editorial letter's objection that "the attribution of increased suicidality to SOCE is quite speculative without a control for pre-SOCE suicidality" (Rosik et al., 2021) with the confused declaration that the possibility "that those in the sexual orientation change efforts (SOCE) group may have been more distressed than their counterparts at the outset…does not contradict our conclusion in that people who experienced SOCE…had a greater prevalence of suicidal behavior than their counterparts" (Blosnich et al., 2021). Blosnich et al. are simply mistaken: as the evidence in the present paper shows, controlling for pre-SOCE suicidality emphatically contradicts their conclusion. Neither Salway et al. (2020) nor Blosnich et al. (2020) seem to recognize that to account for pre-existing conditions does not merely propose an alternative interpretation for the same empirical conclusions (which both studies characterize as "reverse causation"), it results in quite different empirical conclusions. They seem unaware, or perhaps they disagree, that to assume that an effect can precede a cause is not merely a theoretical disagreement that challenges their conclusions but is a logical fallacy that invalidates them altogether.

The four recent studies employing the fallacy of association discussed above, by Salway et al. (2020), Blosnich et al. (2020), Ryan et al. (2020), and Meanley et al. (2020), comprise the most frequently cited population evidence for the conclusion that SOCE therapy increases the risk of suicidal behavior. In its 2009 review of the literature the APA concluded that there was insufficient evidence to determine that SOCE was harmful (American Psychological Association, Task Force on Appropriate Therapeutic Responses to Sexual Orientation, 2009), calling for caution but not prohibition of SOCE therapy. By 2021 the APA issued a resolution which clearly stated that SOCE "puts individuals at significant risk of harm" ("APA Resolution") (American Psychological Association, 2021) and resolved to support policies and laws that "oppose, prohibit or aim to reduce SOCE". To support the stronger, definite conclusion that SOCE generated suicidal harm, the APA Resolution cited Ryan et al. (2020) five times and Blosnich et al. (2020) three times (American Psychological Association, 2021, pp. 5 & 6). The APA Resolution was followed by the publication in early 2022 of an edited volume titled "The Case Against Conversion Therapy: Evidence, Ethics, Alternatives" (Haldeman, 2022), which set forth in greater detail the evidence that led to this change in policy. In the volume, Judith Glassgold, the chair of the APA Task Force that produced the 2009 statement, reviewed the evidence for harm from SOCE published since that time (Glassgold, 2022). In support of the claim that "SOCE has a significant association with suicide risk" (Glassgold, 2022, p. 34), Glassgold cited only three studies: Blosnich et al. (2020), Salway et al. (2020), and Meanley et al. (2020). Glassgold noted that Blosnich et al. (2020) used a nationally representative sample and reported its findings in detail (Glassgold, 2022, p. 33).

Likewise, the 2021 review of SOCE research by a team of scholars at Coventry University led by Adam Jowett, commissioned by the British National Equalities Office pursuant to the consultation on a proposed SOCE ban in that country, cited Salway et al. (2020), Blosnich et al. (2020), and Ryan et al. (2020) as "stronger evidence from subsequently published studies [since the 2009 APA review] that have comparison groups of LGBT people who have not undergone conversion therapy and demonstrate … [that] exposure to sexual orientation change efforts is consistently associated with higher likelihood of suicidal thoughts and suicide attempts compared to LGB people who have not undergone conversion therapy." Like Glassgold, Jowett et al. (2021) cited Blosnich et al.'s (2020) findings of increased suicidal odds in detail, with the note that "[p]articular strengths of this study include its random (probability-based) sample" (p. 45). Jowett et al.'s (2021) review exemplifies the problem with reliance on research that employs the fallacy of association. Jowett et al. (2021) cautioned against "making *causal* interpretations from [associational] studies. For instance, an alternative explanation could be that LGBT people with mental health problems are more likely to seek out conversion therapy" (p. 45, emphasis in original). They then continued: "However, one study controlled for adverse

childhood experiences (e.g., physical or sexual abuse) that are also associated with suicidal thoughts (Blosnich et al., 2020). Meanwhile, another study found that associations with negative health outcomes were markedly stronger for those who had experienced both parental attempts to change their sexual orientation and conversion therapy from a therapist or religious counsellor compared to those who had experienced just one of these (Ryan et al., 2018)" (p. 45, citations in original). Here Jowett et al. misinterpreted (understandably, given the equivocal language of the studies themselves) the pre-existing negative or adverse health associations reported in these studies to be outcomes following SOCE, and thus incorrectly concluded: "On the basis of this evidence, alternative explanations for this finding are less plausible than the conclusion that conversion therapy has a negative impact on mental health." The corrective evidence presented in the current study suggests that this conclusion should be reversed.

At least one prominent scholar of sexual minorities has recognized the disabling problem presented by ignoring the fallacy of association. Bailey (2020) has recently criticized much of the research on sexual minorities for presenting evidence "exclusively in the form of associations" which ignores the possibility that "the increased prevalence of mental health problems in [non-heterosexual] persons is, at least in part, the cause, rather than the effect, of increased self-reported experiences of stigmatization, prejudice, and discrimination." As a result, he concluded that "minority stress research has not generated findings uniquely explicable by the model, and it has ignored the model's serious limitations." Bailey reiterated his concern from an earlier review that "it would be a shame—most of all for gay men and lesbians whose mental health is at stake—if sociopolitical concerns prevented researchers from conscientious consideration of any reasonable hypothesis" (Bailey, 1999).

Conclusion

Thirteen years ago, the APA Task Force on sexual orientation therapies recognized the problem of bias in the use of evidence in this controversial area of study. One of the best responses to the misleading use of evidence shown by Blosnich et al. (2020) and other studies employing fallacies of association may be to recall and affirm some of the Task Force's pertinent recommendations:

1. "Actively oppose the distortion and selective use of scientific data about homosexuality by individuals and organizations seeking to influence public policy and public opinion and take a leadership role in responding to such distortions.…
2. Encourage advocacy groups, elected officials, policymakers, religious leaders, and other organizations to seek accurate information and avoid promulgating inaccurate information about sexual minorities" (American Psychological Association, Task Force on Appropriate Therapeutic Responses to Sexual Orientation, 2009, p. 92).

The consequences of flawed inference are not merely theoretical, however. By ignoring time order, Blosnich et al. (2020) have mistakenly attributed causation to what may be, in part, a cure of suicidal distress, with potentially harmful consequences for sexual minority persons. Imagine a study that finds that most persons using anti-hypertension medication have also previously had high blood pressure, thereby concluding that persons "exposed" to high blood pressure medication were much more likely to experience hypertension, and recommending that high blood pressure medications therefore be banned. This imagined study would have used the same flawed logic as Blosnich et al.'s (2020) study, with invidious consequences for persons suffering from hypertension.

After accounting for pre-existing suicidal behavior, sexual minorities who underwent SOCE treatment were not at higher risk of suicidality. Indeed, some of them may have been placed at much lower suicidal risk. Judicial or legislative restrictions on SOCE participation could deprive sexual minorities of an effective resource for reducing suicidality, thereby putting them at substantially higher suicide risk.

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Declarations

Conflict of interest The author has no financial or proprietary interests in any material discussed in this article.

Ethical Approval As a secondary analysis of pre-existing public data, the Institutional Review Board of the Catholic University of America reviewed and certified, in ethical certification decision number 21-0016, the present study's methods to be exempt from human subject ethical review under 45 CFR 46.104.

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COMMENTARY

Correcting a False Research Narrative: A Commentary on Sullins (2022)

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Sullins' (2022) report about the relationship of sexual orientation change efforts (SOCE) and suicidality among sexual minority persons suffers from a fatal flaw that renders the conclusions of the paper invalid. In Blosnich et al. (2020), we demonstrated that SOCE was associated with higher lifetime prevalence of suicide ideation, suicide planning, and suicide attempt with no/minor injury. Sullins critiqued our research because we did not consider the temporal order of SOCE and suicidality, something we clearly discussed in our paper. Sullins used the same *Generations* data to suggest a different outcome by attempting to create the temporal order of SOCE and suicidality. However, the same limitations that prevented us from assessing temporal order also undermined his findings: no data in the *Generations* study are available to assess the timing of SOCE initiation, so there is no way to establish temporal order. The only difference between Sullins' and our analysis is that Sullins ignored this significant limitation and proceeded to conclude not only that SOCE was not associated with suicidality but that it was protective. Sullins claimed to correct a "false research narrative" in Blosnich et al. (2020). However, the false narrative that requires correction is Sullins' own conclusions based on misplaced certainty in his faulty methods.

Both Blosnich et al. (2020) and Sullins (2022) used the same *Generations* dataset (information about the study's methodology and rationale is available online at http://www.generations-study.com). Sullins used various suicidal outcomes, but for sake of clarity, we focus this commentary on the outcome of suicide attempt. In the *Generations* data, suicide attempts can be timed according to the respondent's self-reported age of attempt. Suicide attempt was asked with one item: "Did you ever make a suicide attempt (i.e., purposefully hurt yourself with at least some intention to die)?" If respondents reported one attempt, they were asked the age of that sole attempt ("About how old were you?"). If a respondent indicated multiple suicide attempts, then they were asked to report their age for both first and last attempt ("About how old were you the very first time?" and "About how old were you the most recent time?"). For SOCE exposure, the only information available on timing in the *Generations* dataset comes from one question that asked, "About how old were you the *last* time you received treatment to change your sexual orientation?" [emphasis added]. Using these questions, Sullins created "pre-SOCE suicidality" variables among which he claims to categorize a suicide attempt *prior* to SOCE by cross-referencing the age of suicide attempt (or age of first suicide attempt, if more than one suicide attempt was reported) with the age of *last* exposure to SOCE. Sullins then used this "pre-SOCE suicidality," which is a misleading variable name, in analyses that exonerate SOCE as harmless.

Sullins asserted that if SOCE exposure occurred after a suicide attempt, then SOCE could not have caused the suicide attempt. He underscored this point in the discussion to explain to the reader the importance of temporal precedence—that is, a cause must precede the effect in time. But as we show here, Sullins' categorization is faulty and therefore the entire premise of his analytical approach is highly suspect.

Sullins mistook the time of *last* exposure to SOCE to be the time of exposure to SOCE as a whole. This is patently and demonstrably wrong for two reasons consistently demonstrated in the research literature: (1) SOCE exposure can be prolonged in duration and (2) most people who experienced SOCE have been exposed to multiple SOCE attempts. In terms of duration of SOCE exposure, Nicolosi et al. (2000)

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found that average duration of SOCE among their sample of 882 individuals exposed to SOCE was 3.4 years. Spitzer (2003) documented an average SOCE duration of 4.7 years for 79% of his sample of 200 individuals previously exposed to SOCE but were no longer involved in SOCE at the time of interview data collection. Import2021antly, for the remaining 21% of individuals in Spitzer's sample who were still undergoing SOCE at the time of interview data collection, the mean duration of SOCE was 15.0 years. Shidlo and Schroeder (2002), whose work Sullins cites, found an average duration of over two years. Regarding number of SOCE attempts, Spitzer (2003) reported that 90% of the participants had more than one type of SOCE. Salway et al. (2021) found that nearly 66% of people exposed to SOCE reported two or more attempts at SOCE. Clearly, the age of *last* exposure to SOCE is rarely, if ever, the correct estimate for age of initial exposure to SOCE. To estimate temporal order, the ages of first *and* last exposure to SOCE are necessary, but the age of first exposure to SOCE was not collected by the *Generations* survey.

For his analyses, Sullins appears to subtract age of suicide attempt from age of last SOCE exposure, completely ignoring the frequency and duration of SOCE. Using this approach, Sullins divides the sample into three groups according to whether they had their (first) suicide attempt before, during, or after SOCE. The respondents who were categorized by Sullins as having had a "pre-SOCE suicide attempt" are those for whom the difference between ages of last SOCE exposure and suicide attempt was one year or more. For example, a respondent who reported a suicide attempt at age 15 and the last SOCE exposure at age 17 was categorized by Sullins as someone who had a suicide attempt before SOCE exposure. Accordingly, Sullins concludes such a respondent's suicide attempt was not predicated on exposure to SOCE. Yet, as we show in Table 1, research evidence does not support Sullins' conclusion. A person whose age of *last* SOCE exposure at age AQ4 6 17 could have started their SOCE at age 15 or earlier, which means their suicide attempt at age 15 could have coincided with SOCE or occurred after a previous SOCE exposure.

Nonetheless, Sullins categorized 20 respondents as having had a "pre-SOCE suicide attempt," which he interpreted to mean that SOCE could have not been a cause in their suicide attempts. Using the knowledge from existing studies on frequency and duration of SOCE, we re-examined the data in *Generations.* We found that of the group of 20 respondents Sullins defined as people with "pre-SOCE suicide attempts," at least 65% could have been misclassified (Fig. 1). If we assumed a SOCE exposure duration of two to four years, nine respondents could be reclassified as having a suicide attempt during SOCE. Furthermore, four respondents who were classified as having a "pre-SOCE suicide attempt" reported multiple suicide attempts. Although these four respondents reported their *first* suicide attempt prior to *last* SOCE, they reported their last suicide attempt *during or after* exposure to *last* SOCE. For example, one respondent with multiple suicide attempts indicated age of last SOCE at 24 and their first suicide attempt at age 22; Sullins presumably classified this respondent as "pre-SOCE suicide attempt." However,

Table 1 Summary of studies reporting on number of episodes/types of sexual orientation change efforts (SOCE) and duration of SOCE

Authors	Year published	Sample size exposed to SOCE	Country	Number of episodes/types of SOCE	Duration of SOCE
Byrd	2000	79	US	NR	4.2 years (mean)
Nicolosi et al.	2000	882	US	NR	3.4 years (mean)
Shidlo and Schroeder	2002	202	US	58.4% ≥ 2 types	26 months (mean)
Spitzer	2003	200	US	90% ≥ 1 type	4.7 years (mean for 79% of sample no longer in SOCE at time of interview) 15.0 years (mean for 21% of sample still in SOCE at time of interview)
Beckstead and Morrow	2004	50	US	NR	4 years (mean)
Flentje et al.	2014	38	US	3 (mean)	40 weeks/episode (mean)
Bradshaw et al.	2015	898	US	NR	4.3 years for men; 5.0 years for women (mean)
Dehlin et al.	2015	1060	Global	2.6 types (mean)	4.7 years (mean for SOCE-related psychotherapy)
Salway et al.	2021	910	Canada	65.1% reported≥2 SOCE attempts	23.8% reported duration > 1 year
Meanley et al.	2019	219	US	NR	23.5% reported duration > 1 year
Goodyear et al.	2022	22	Canada	NR	72.5% reported duration ≥ 1 year
Kinitz et al.	2022	22	Canada	NR	4.7 years (mean)

NR = not reported

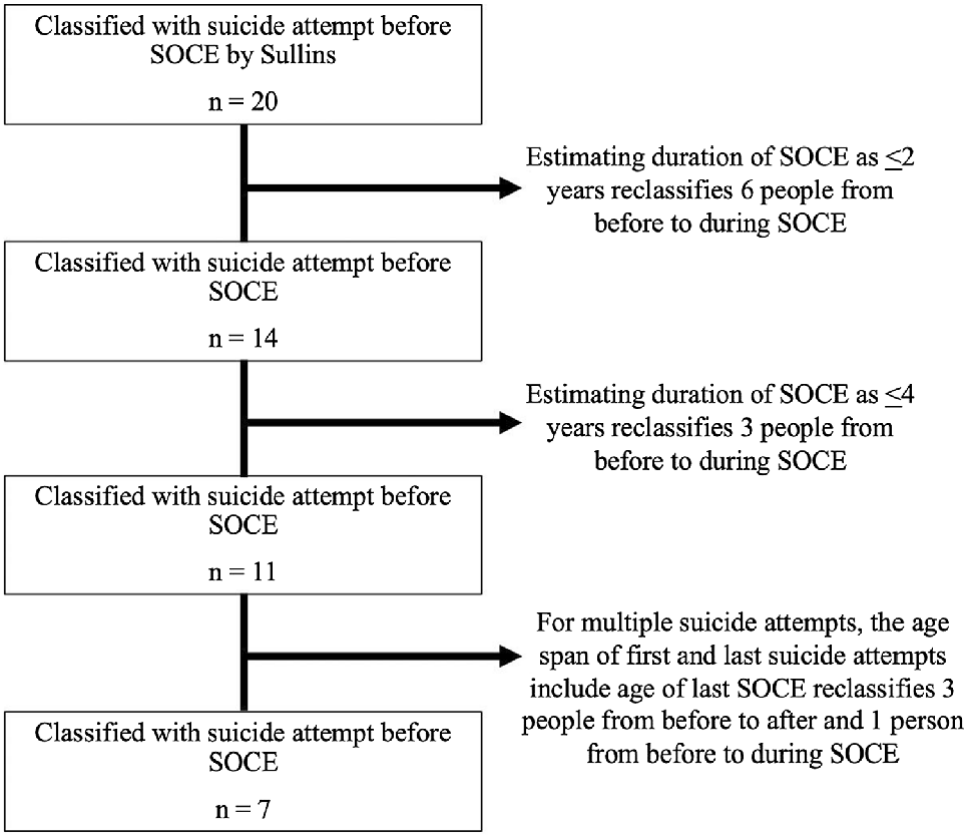

Fig. 1 Mistaken classifications of Sullins' (2022) temporal categorization of suicide attempts as occurring before sexual orientation change efforts (SOCE)

Sullins ignores that this respondent reported their last suicide attempt at age 24, which was during the respondent's *last* SOCE exposure. Taken together, if we estimate an average SOCE duration of four years, as research evidence suggests, and correct Sullins' oversight about individuals with multiple suicide attempts, of his original group of 20 respondents with alleged "pre-SOCE suicide attempt," 13 may have been misclassified, leaving only seven with a probable pre-SOCE suicide attempt (Fig. 1).

As discussed by Blosnich et al. (2020), *Generations* data do not allow timing of SOCE exposure. Sullins made temporal categorizations by presuming information that does not exist in the dataset and by ignoring research evidence that strongly suggests his temporal estimates are flawed. With unfounded categorization of *Generations* data, Sullins concluded that SOCE could not cause the suicide attempt and went further to conclude that it might *lower* the likelihood of a suicide attempt. As we have shown here, if we were to join Sullins in guessing exposure to SOCE, we would determine that most suicide attempts ought to be classified as having occurred during or after SOCE, not before SOCE. We are not suggesting, however, that is what researchers should do. Researchers ought to use the data that are available, not create data they *wished* they had. The risk in presuming data is that a researcher's bias would influence the estimates they create—thereby constructing misleading research findings.

Sullins critiqued our paper by writing that we did not determine to what extent suicidality may have preceded SOCE exposure. He is correct—as clearly explained in that paper, we did not estimate temporal order because the data did not allow for this. Instead, based on the data available, we used conventional statistical approaches to assess lifetime associations without making assumptions that are not supported by the data. Further research would be needed to establish temporal order for more accurate causal inferences.

Sullins' (2022) analyses are predicated on a fabricated classification of temporal order. We stand by our former critique of Sullins' problematic use of *Generations* data (Meyer & Blosnich, 2022) and underscore that Sullins' (2022) analyses and conclusions are invalid.

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Declarations

Conflict of interest The views expressed are those of the authors and do not necessarily reflect the position or policy of the institutions, National Institutes of Health, or the United States Government.

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SULLINS RESPONSE TO BLOSNICH ET AL. (2023)[2]

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.

[2] A freely available formatted read-only copy of the authenticated version of Sullins's rejoinders is available at https://rdcu.be/c6LRY .

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 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 of Sullins (2022b)), 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 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), 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). 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.

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COMMENTARY

Unaddressed Sources of Bias Lead to Biased Conclusions About Sexual Orientation Change Efforts and Suicidality in Sexual Minority Individuals

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Introduction

We are writing this Commentary in response to Sullins' (2022) paper "Sexual Orientation Change Efforts Do Not Increase Suicide: Correcting a False Research Narrative" which analyzed data from the Williams Institute's Generations Study to answer the question: Do sexual orientation change efforts (SOCE) have an effect on suicidality of those who experience it? Sullins concludes after conducting some statistical analyses that "Experiencing SOCE does not result in higher suicidality…and may sharply reduce subsequent suicide attempts." We strongly disagree with these conclusions. Sullins' work has glaring methodological flaws inconsistent with expected practices in doing causal inference using observational data (Hernán et al., 2016). Given these flaws, we call for the retraction of this work.

The key results of Sullins' paper can be found in Table 2 where it was reported that SOCE had a protective effect against suicidal ideation based on significantly lower odds for suicidal ideation among those who experienced SOCE (aOR: 0.44, 95%CI: 0.20, 0.94). This finding contrasted with the findings of a prior paper (Blosnich et al., 2020). Blosnich et al. found that SOCE was associated with significantly higher odds of suicidal ideation (aOR: 1.92, 95%CI: 1.01, 3.64).

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The source of the contrasting results lies in how the analyses differ in how outcomes were counted into the analysis. Blosnich et al. counted suicidal ideation at any time of the participants life (from birth up to time of the survey). Thus, Blosnich et al.'s approach failed to properly account for temporal relationships. Due to this, it is unclear if SOCE caused the increased suicidal ideation observed in the SOCE-exposed individuals. To address this problem, Sullins tried to "account for the timing of suicidality relative to SOCE," which translated to counting only post-SOCE suicidal ideation among those who experienced SOCE in their analysis. While the underlying idea of only counting outcomes after the exposure is correct, the implementation in the analysis was wrong.

Estimating Effects of Interventions and the Counterfactual Framework

In epidemiologic work, valid estimates of causal effects of single exposure interventions (such as SOCE in this case) draw on the counterfactual framework (also called potential outcomes framework) (Höfler, 2005). This framework is described as follows:

> Counterfactual theory deals with hypothetical scenarios about the occurrence of an outcome in contrasting states of exposure by posing "What if?" questions… When thinking about the causal effect of an exposure on an outcome, an example of a counterfactual question is "Would the outcome have been different if exposed individuals had not been exposed in the first place?" (Bours, 2021)

We can apply the counterfactual design in the following example by comparing two scenarios. The first scenario involves recruiting a group of 100 people and subjecting them all to SOCE on January 1, 2019. We then measure the

proportion of individuals with suicidality outcomes by June 1, 2019. The second scenario requires going back in time (or traveling to a parallel dimension) and recruiting the same 100 people, but this time not allowing them to experience SOCE on January 1, 2019; we would then assess their suicidality by June 1, 2019. Knowing the rates of suicidality between the two scenarios, we can then interpret the difference in rates as the average population effect of SOCE.

Unfortunately, time travel is not a feasible research methodology. Indeed, given a set of 100 individuals, we can only observe one scenario for each person: The individual is either exposed to SOCE or not. Researchers must then rely on statistical approaches to generate estimates of counterfactuals to the observed: What is the outcome if those observed to be exposed to SOCE were not exposed? What is the outcome if those observed not to be exposed to SOCE were exposed instead? Over the years, researchers have developed many approaches to estimate these scenarios and to calculate causal effects. The easiest example of an approach relying on counterfactuals is the randomized trial where randomization permits average effect of a treatment (Greenland & Brumback, 2002). Randomized trials, however, cannot always be implemented, especially if prior evidence suggests lack of equipoise and there is high risk of harm due to the intervention (Nardini, 2014).

Observational studies (or natural experiments) can also be used to derive causal estimates provided that appropriate design and techniques are utilized and that some additional assumptions are made (Dreyer et al., 2010). While there are many assumptions to consider, we focus on the ones that Sullins' paper failed to account for properly. First, Sullins failed to correctly count the outcomes for similar time periods (or at-risk periods) in the control (no SOCE) group. Second, Sullins did not address confounding and the balance between the exposed and unexposed was not achieved. Finally, Sullins failed to use an appropriate data source, leading to selection or survivor bias. We elaborate each point in the sections below.

Unequal At-Risk Periods Distorted the Outcome Measurement

The first flaw in Sullins' analysis is that the unexposed had a longer period at risk than the exposed (Fig. 1A and B). For exposed individuals, only suicidality events that occurred after SOCE were counted. For the unexposed individuals, however, Sullins lacked an appropriate index date and did not attempt to account for this issue. Among those exposed to SOCE, follow-up started at age of SOCE and ended at age of survey; the average "at-risk" period in this group was 14.62 (SD: 11.9) years. Those who did not experience SOCE, however, were followed for their entire lifetime, for an average period of 30.63 (SD: 13.25) years.

Thus, Sullins compared the odds of suicidal ideation after experiencing SOCE among those who experienced SOCE to the lifetime odds of suicidal ideation among those who did not experience it. Given the longer risk period of unexposed (lifetime) vs. exposed (starting only after later adolescence), it is unsurprising that he calculated an "odds ratio" that was "protective." In truth, however, the reported odds ratios are not interpretable in the epidemiologic sense and do not serve as evidence of association or causation. Selecting the index date for unexposed groups is not a trivial problem in observational research and requires careful design and statistical modeling (Hernán et al., 2016). These techniques would often match an exposed individual to an unexposed individual and use the index date of the exposed for the pair so that both exposed and unexposed would have similar at-risk periods. The implementation and alternatives are numerous and beyond the scope of this letter but are discussed elsewhere (Thomas et al., 2020).

Inappropriate Covariate Adjustment

The second flaw occurred due to some questionable analytical decisions that hindered achievement of balance between groups. The goal of achieving balance is to mitigate confounding so that the statistical model can obtain unbiased estimates of the effect of an intervention (albeit with additional assumptions) (Greenland & Morgenstern, 2001). One approach to achieve balance is covariate adjustment (also known as multivariable adjustment) (Elze et al., 2017). This is the approach used both by Sullins and Blosnich et al.

The challenge observational researchers face is how best to identify which variables to include as covariates since they are potential confounders (VanderWeele, 2019; Witte & Didelez, 2019). In general, one should avoid adjusting for variables that occur after the outcome. Variables that occur after the outcome would be hard to justify as a confounder because confounders need to be present before the outcome occurs. In addition, if the goal is to estimate the total effect of an intervention, it would also be prudent to exclude variables that occur after the exposure but may have occurred before the outcome (VanderWeele, 2019). These variables lie along the pathway between exposure and outcome and may mediate the effect of the exposure. Including these variables in the model takes aways some of the calculated effect of the exposure leading to inaccurate estimate of the total effect. There are cases where mediation or post-treatment confounding need to be addressed in the analysis, but these are not important for the paper we are examining.

Back to the main paper, there are two variables that Sullins and Blosnich et al. adjusted for in the models that are

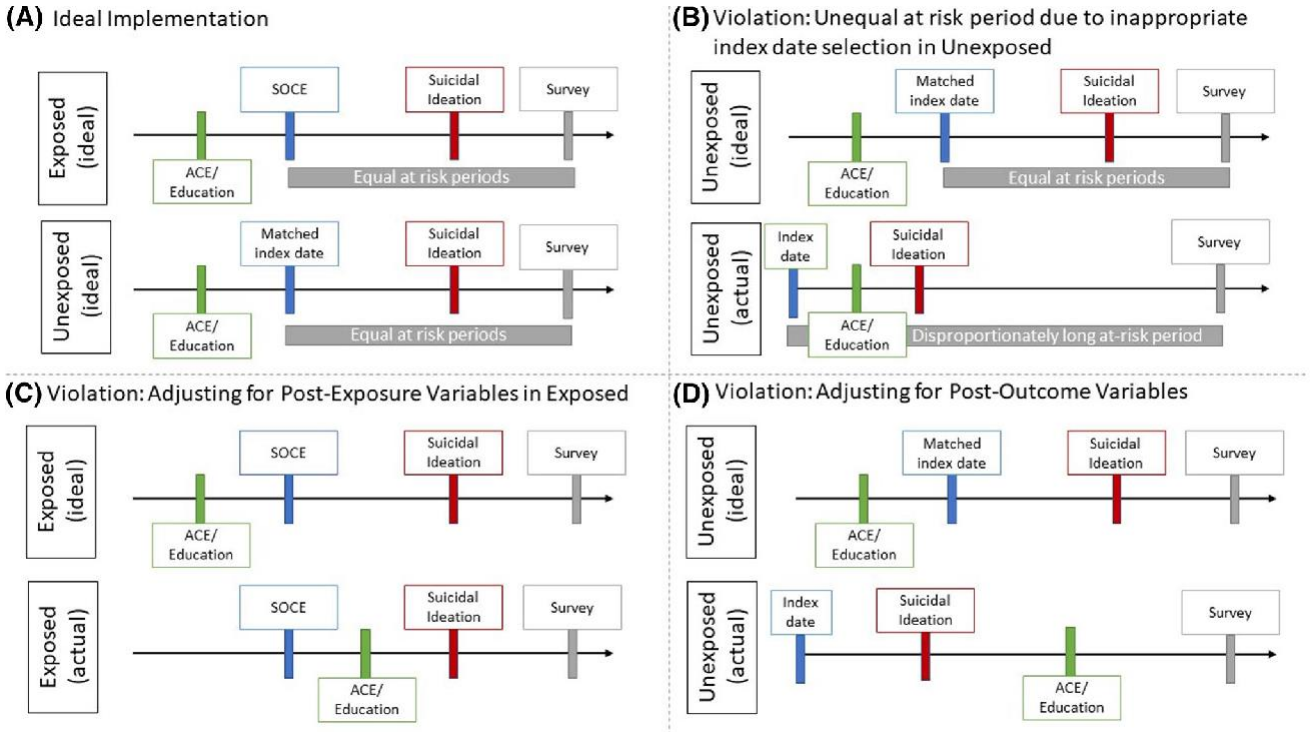

Fig. 1 Timing of Key variables in Analyzing Suicidality using the Generations Study and Resulting Analytical Issues. ACE—adverse childhood event, SOCE—sexual orientation change effort. **A** shows the timing of variables in an ideal setting. We see that the starting index date for unexposed is matched to the SOCE start date in the exposed, so the groups have comparable at-risk periods. In this setting, the ACE/education covariates included in the analysis were those that occur prior to the index date for follow-up. Figures B, C, and D show deviations from this ideal that occurred in the Sullins' paper and the resulting violations. **B** shows that instead of matching the index date in unexposed, the index date in unexposed was from birth resulting in a wider at-risk period for unexposed than exposed. **C** shows that instead of using pre-SOCE covariates, ACE/education after SOCE was included in the statistical model. Finally, **D** shows two violations, failure to match the index date (as seen in B) as well as including covariates that were measured after the outcome

problematic: adverse childhood events (ACEs) and educational attainment. ACEs ask about experiences from birth up to 18 years old. Education is a time-varying exposure and could end at several points (e.g., high school at 18, Bachelor's at 22) but was measured only at the time of the survey. The timing of ACE and education measurement creates issues in the analysis.

Looking at the exposed to SOCE, the median age of SOCE is 17 years. However, values of ACE and education were measured at 18 years or later. As a result, the model used values that occurred after the exposure and violates the principle of excluding post-exposure confounders. Ideally, values of ACE and education before or at the same time of SOCE should be used but these are not available in the dataset (Fig. 1A and C).

A related issue also appears among unexposed. In this group, the median age for first suicidal ideation (outcome) was 14 years old. Again, ACE and education values refer to those at age 18 or older. So, for the unexposed, the model violated the rule of not adjusting for variables that occurred after the outcome. The solution is to adjust using earlier values at the appropriately selected index date (Fig. 1A and D).

As a minor note related to achieving balance, Sullins failed to utilize techniques that are better for estimating causal effects such as propensity score matching, inverse propensity weighting, and g-computation (Blakely et al., 2020; Stuart, 2010). While multivariable adjustment can produce the same numeric estimate of effect or association as these newer approaches, the newer approaches can perform better in situations with low events and high number of confounders and can be less sensitive to errors in specifying the statistical models (Biondi-Zoccai et al., 2011; Blakely et al., 2020; Fu et al., 2019).

Survivor and Selection Bias

Even if these analytical fixes are done, however, we contend that the Generations Study—the data source used by Sullins—cannot be used to answer the question that Sullins wanted to answer. This critique applies to Blosnich et al.'s

work as well, but this team was not explicitly attempting to measure cause and effect. Both papers used the first wave of the Generations Study, which is a survey of non-transgender sexual minority persons in the USA from three generational cohorts. As a survey, first and foremost among eligibility criteria is that a person must live long enough life to become eligible to be interviewed. The survey is thus susceptible to survival-related selection bias, especially since it was interested in the effect of an early life exposure on a distal outcome (Howe & Robinson, 2018). Given the scientific question at hand, even Wave 1 data of the Generations Study thus represent people who survived and were able to participate in the study. Since we do not have information about these individuals, statistical modeling cannot be used to calculate an effect of SOCE that is generalizable to the target population (Howe et al., 2016). Examining the effect of exposures that occur during childhood or adolescence would likely require cohorts that start data collection early on in life and continue on to adulthood (Howe & Robinson, 2018). These cohorts should have repeated measures not just of the outcome and exposure but also of confounders.

A subtle additional complication is that the selection in the Wave 1 sample leads to a situation where an association between SOCE and suicidality is going to be artificially present due to "collider bias" (Cole et al., 2010; Hernán et al., 2004). In this form of bias, associations can be detected from the data even though, in truth, the exposure and outcome are not related. The straightforward solution is to not condition on survival. This means setting up a different cohort where everyone is followed up from childhood up to adulthood, regardless of survival or loss to follow-up. Thus, the Generations Study Wave 1 data are unusable for the scientific question at issue in the papers by Sullins and by Blosnich et al., since any resulting detected "association" cannot be easily attributed as arising from this bias or from a true causal relationship.

Conclusion

As researchers, we are often tasked with answering difficult questions about health and well-being of minoritized populations. We should do so with care and respect. The Generations Study represents a generous contribution to research by lesbian/gay/bisexual individuals who may have had to recall stressful life events in the process of participating in the study (e.g., conversion therapy, recalling suicidal ideation, or attempts). The least researchers should do is to conduct high-quality research when working with data shared by sexual minority individuals. Fortunately, the increase in popularity in causal inference across fields including epidemiology, sociology, and psychology gives us tools to better utilize these data sources (Gangl, 2010; Ohlsson & Kendler, 2020). Several papers, such as the ones we cited in prior sections, have laid out theory and guidelines on how to do this properly with appropriate data sources. Increasingly, more papers have successfully applied these methods to show benefit or harm of interventions, broadly defined (Graetz et al., 2022; Ioannou et al., 2022; Madenci et al., 2021; Rudolph et al., 2021). Sullins' work cannot be counted as one of these well-thought-out papers. Rather, it is an example of how incorrect application of statistics can lead to horribly wrong and unsupported conclusions.

As a last note, we stress that even the best and most rigorous quantitative analyses cannot be the answer to a complex research question on suicidality in sexual and gender minority populations. At best, a rigorous quantitative analysis answers a very specific question for a very specific population and context (Hammerton & Munafò, 2021; Vandenbroucke et al., 2016). Sullins' claim that this paper corrects a false narrative about SOCE is itself a false narrative. In the face of multiplicity of evidence stemming from quantitative and qualitative analysis and narratives of people who have been harmed by conversion therapy, a quantitative paper needs to provide excellent and robust evidence derived from rigorous methods to counter this overwhelming body of evidence of harm (Jones et al., 2022; Jowett et al., 2020; Streed et al., 2019; Wright et al., 2018). Sullins' paper is not that paper.

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Declarations

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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 of Sullins (2022b) (Treatment Initiation Model) 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 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 co-morbidities

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 .90; the corresponding ratio from the logistic regression compounding model (Model 4, Table 2 of Sullins (2022b)) in my original paper was .92; for suicide planning, matching estimated .88, regression .86; for suicide intention, matching yielded .91, regression .74; and for suicide attempts, matching yielded .98, regression .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 .03, well below the conventional .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 .00 to .83, with seven significantly different characteristics. After matching, the difference *p*-values ranged from .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 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.

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LETTER TO THE EDITOR

Evidence of Sexual Orientation Change Efforts Ineffectiveness and Risks of Harm: A Response to Sullins (2022)

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Sullins (2022) argued that sexual orientation change efforts (SOCE) are not linked to an increase in suicide based on an attempt to reanalyze correlational relationships of a dataset. However, his claims about this study (Blosnich et al., 2020) were flawed and do not withstand scrutiny and are based on a misinterpretation of the underlying data (Blosnich et al., 2023). Sullins' paper appears to minimize the extensive SOCE research literature of the risks of harms from SOCE, which include suicidal ideation and attempts (American Psychological Association (APA), 2009; American Psychological Association (APA), 2021a; American Psychological Association (APA), 2021b; Cramer et al., 2008; Glassgold, 2022; Panozzo, 2013; Przeworski et al., 2021; Serovich et al., 2008; US Substance Abuse and Mental Health Services Agency, 2015). This response is an effort to correct the narrative of the research on effectiveness and harms of SOCE.

Multiple research reviews evaluating the research literature from 1960 to the present (about 300 studies) have found that SOCE are ineffective at changing sexual orientation and pose a risk of harm (APA, 2009; APA, 2021a; APA, 2021b; Cramer et al., 2008; Glassgold, 2022; Haldeman, 1994; Panozzo, 2013; Przeworski et al., 2021; Serovich et al., 2008; US Substance Abuse and Mental Health Services Agency, 2015). Included in these reviews are studies with strong experimental designs that could determine causal relationships between treatment and outcomes that found no experimental evidence of change in sexual orientation and did document harms (APA, 2021a; Glassgold, 2022; Przeworski, et al., 2021). Glassgold's (2022) summary of these studies and a review of 16 subsequent research studies from 2008–2021 found similar results—no evidence of effectiveness and evidence of harm. When all the methodologically diverse studies are taken together, despite the limitations of specific designs, the evidence is strong that SOCE can be harmful to sexual minority people of all ages. This includes mental and behavioral risks as well as an increased risk of suicidal ideation and attempts (APA, 2021a; Glassgold, 2022; Przeworski et al., 2021). Sullins (2022) inaccurate interpretation of research data and broad claimed that SOCE pose no additional risk of suicidality is not supported by the research. SOCE adherents have not provided any source of supporting evidence of SOCE efficacy and instead criticize these systematic reviews. Because of its demonstrated lack of efficacy and potential to cause serious harm, the major medical and mental health professional and scientific association reject efforts to change sexual orientation.[1]

Numerous jurisdictions in the US and countries abroad have banned the use of SOCE with minors. The first such ban, which was enacted in 2012 in California, rejected the concept of "conversion therapy" as the term incorrectly suggests that it is a legitimate form of treatment. Instead, it was declared a public health concern serious enough to warrant legislative action. Since then, 20 other US states and the District of Columbia, numerous local jurisdictions, and a number of foreign countries have banned SOCE. The accumulation of population-based data verifying the harms of SOCE was also the impetus for the American Psychological Association most recent SOCE policy and Guidelines for Practice

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with Sexual Minority Persons. Both these document warn psychologists that SOCE is ineffective and puts individuals at significant risk of harm (APA, 2021a; APA, 2021b).

Given that there is no evidence of SOCE's effectiveness and no scientific basis to its claims regarding change to same-sex sexual orientation, suggestions that harms be more rigorously studied are red herrings. There are numerous ethical problems with studying potentially harmful treatments in human subjects (Whitney, 2021), especially given the regulations for *Protection of Human Subjects* developed by the U.S. Department of Health and Human Services (2018).

In fact, the existing research on the potential risks of SOCE is consistent with current and emerging approaches to evaluating the harms that can occur in health treatments and are adequate to make the case for risk of substantial harm (Frieden, 2017; Whitney, 2021). Researchers have proposed criteria for identifying potentially harmful behavioral health treatments as those that (1) cause psychological or physical harm to the client or others; (2) result in harmful effects that are long-lasting; (3) have had independent research teams find and replicate the harmful effects (Lillenfield, 2007). Ineffective treatments—those that may not directly cause harm but do not improve the health or well-being of the individual receiving treatment—may also be considered harmful in so far that they deprive an individual of needed care and waste time and resources (Beckstead & Morrow, 2004; Dmidjian & Hollon, 2010). The existing studies and reviews of SOCE cited above find evidence consistent these criteria for potentially harmful treatments (Mercer, 2017; Przeworski, et al., 2021).

Sullins' claims (2022) of lack of SOCE harms do not match the extensive evidence that SOCE are not effective and pose a risk of harm (Glassgold, 2022). Providing SOCE is contraindicated by the evidence and by ethical principles (APA, 2009; APA, 2017; APA, 2021a, APA, 2021b; Lilienfeld, 2007; Mercer, 2017). Thus, Sullins' misinterpretation of research data and claims pose risks to patients. There are other approaches to address distress related to sexual orientation that are based on scientific evidence and do not make unsupported claims or pose risk of harm that can be the basis of future research (APA, 2009, 2021b).

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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; Dehlin et al., 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.

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COMMENTARY

Ethical and Human Rights Concerns of Sexual Orientation Change Efforts: Commentary on Sullins (2022)

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In this brief commentary, we present several reasons why the publication of "Sexual Orientation Change Efforts Do Not Increase Suicide: Correcting a False Research Narrative" by Sullins (2022) in the *Archives of Sexual Behavior* is egregiously problematic. As we understand that Blosnich et al. (2020) are currently preparing a commentary addressing the methodological concerns of Sullins (2022), we will focus on the ethical and human rights issues associated with Sullins' paper, which argues that sexual orientation change efforts (SOCE) do not result in higher suicidality. Our main argument here is that the obvious and very serious ethical and human rights concerns related to SOCE transcend any methodological analysis by Sullins (2022) or indeed anyone else, for the simple reason that the problem with SOCE is not just about outcomes and well-being but primarily about rights and autonomy so that a methodological analysis seeking to undermine causation is just irrelevant.

SOCE are also known as conversion therapy, change allowing therapy, reparative therapy, reorientation therapy, and reintegrative therapy (among others), though these efforts ought not to count as therapeutic (Grey et al., 2022). The purpose of SOCE is to change an individual's same-sex sexual orientation to an other-sex sexual orientation typically from gay/lesbian to straight (Glassgold, 2022). These types of efforts seek to eradicate same-sex sexual orientations and promote heterosexual orientations. This is already in and of itself a violation of both sexual rights and human rights, independent of any positive or negative consequences on well-being.

Concretely, the applicable international human rights laws include non-discrimination, right to health, prohibition of torture and ill-treatment, right to freedom of conscience and religion and freedom of expression, and rights of the child when minors are the targets of these efforts (United Nations, 2020). All of these rights and protections can be jeopardized by SOCE practices. Further, it is unethical to treat something that is not a disorder or pathology (Bhugra et al., 2016; United Nations, 2020). Same-sex sexual orientations are normal and are not considered pathologies (Bhugra et al., 2016); thus, SOCE are clearly unethical.

The question of whether sexual orientations can change during the course of one's life is secondary and unnecessary for granting legal and human rights protections (Diamond & Rosky, 2016). The argument is that same-sex sexual orientations need not and ought not to be coercively changed and efforts to do so violate individuals' human rights. Compounding these serious concerns, SOCE do not work in achieving their aims. This represents additional considerations regarding the ethical treatment of patients. There is no evidence that SOCE are effective in changing sexual orientation (Glassgold, 2022). As Sullins (2022) admits, "the question of SOCE efficacy is not at issue; since minority sexual orientation was a screening criterion for survey participation, the data included only persons for whom, by definition, the stated aims of SOCE were not achieved." Providing treatment to patients under the known false promise of its efficacy (i.e., offering a treatment that is known to not work) is fraud (United Nations, 2020). Moreover, legal precedent was set in the USA in 2015 with *Ferguson v. JONAH*, ruling that as same-sex orientations are not illnesses, SOCE practices constitute consumer fraud (Streed et al., 2019). Therefore, SOCE ought not to count as treatment and, even if it were, it would be fraud. It is as easy as that.

Sullins (2022) does acknowledge that the ethics of SOCE are contested, and that cautionary and oppositional statements have been issued by professional organizations but still concludes, "judicial or legislative restrictions on SOCE participation could deprive sexual minorities of an effective resource for reducing suicidality, thereby putting them at substantially higher suicide risk." These conclusions bring themselves serious political, ethical, and human rights risks.

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While the majority of reputable healthcare organizations denounce, condemn, and/or call for a ban on these efforts based on ethical and human rights concerns in line with those described here (Grey et al., 2022; see supplemental material[1]); and the United Nations (2020) has called for an international ban on SOCE practices, a statement implying that judicial and legislative restrictions on SOCE, which are unquestionably known to be harmful and constitute human rights violations (see supplemental material), may increase the risk of harm, is nonsensical and misleading. The potential for these conclusions drawn by Sullins (2022) to be used nefariously in political and legislative debates can put sexual minority individuals in real danger if legislation allowing for these harmful practices is implemented or just debated. The ethical concerns and human rights violations associated with SOCE presented above render the relationship between SOCE and suicidality along with the question of causality secondary. Any work advocating human rights violations, which also constitute unethical and fraudulent "treatment" of individuals or patients, has no place in scientific discourse.

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[1] The supplemental material consists of the policy and/or position statements condemning sexual orientation change efforts from 53 organizations (American Academy of Child and Adolescent Psychiatry, 2018; American Academy of Family Physicians, 2007; American Academy of Nursing Position Statement on Reparative Therapy, 2015; American Association for Marriage and Family Therapy, 2009; American Association of Sexuality Educators, Counselors and Therapists, 2017; American Counseling Association, 2017; American Group Psychotherapy Association, & International Board for Certification of Group Psychotherapists, 2002; American Medical Association, 2018, 2022; American Mental Health Counselors Association, 2014; American Psychiatric Association, 2018; American Psychoanalytic Association, 2012; American Psychological Association, 2021; Bhugra et al., 2016; Canadian Psychological Association, & Canadian Association of Social Workers, 2018; Veltman, & Chaimowitz, 2014; Clinical Social Work Association, 2016; De Groot, 2022; European Association of Psychotherapy, 2017; Homosexuality and Adolescence, 1993; International Federation of Social Workers, 2018; Memorandum of Understanding on Conversion Therapy in the UK, Version 2, 2022; National Association of School Psychologists, 2022; National Association of Social Workers, 2015; Pan American Health Organization, 2012; Society for Adolescent Health and Medicine, 2013; Substance Abuse and Mental Health Services Administration, 2015; United Nations, 2020).

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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 United

States 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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COMMENTARY

A Wake-Up Call for the Field of Sexual Orientation Change Efforts Research: Comment on Sullins (2022)

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Keywords Sexual orientation change efforts · SOCE · Conversion therapy · Suicide

The field of sexual orientation change efforts (SOCE) research has a serious problem. It is currently functioning in an academic sociopolitical monoculture that brings into question the replicability of its findings and likely limits the validity of its conclusions (Duarte et al., 2015).[1] Although I have expressed these concerns for years (e.g., Rosik et al., 2012, 2021a, 2022), Sullins' (2022) reanalysis of Blosnich et al. (2020) is a test of the field's scientific integrity that cannot be ignored. I expect there will be an intense push to delegitimize Sullins' findings; however, my recommendation to the field is to practice greater humility and do better, more nuanced research. Here's why.

I will focus my attention on a few critical methodological vulnerabilities in the current SOCE literature. Sullins' work highlights the importance of assessing for pre-SOCE distress in whatever health indicators are being studied. Essentially, this indicts the entire body of the recent literature, as I document in Table 1. (This also applies to Gender Identity Change Efforts [GICE], also noted in the table.) No matter how intense the advocacy interests of researchers are, correlation still gets us no closer to causation. If this critical shortcoming is mentioned at all as a limitation by SOCE researchers, it is typically given one sentence and ignored when clinical or policy implications of the findings are discussed. This is no longer acceptable in light of Sullins' reanalysis.

As if that is not a sufficient indictment on its own, I have documented another critical methodological deficiency to which Sullins alluded that likewise casts a shadow on the current SOCE literature. Two decades ago, Shidlo and Schroeder (2002) observed, "…we have found that conversion therapists and many clients of conversion therapy steadfastly reject the use of *lesbian* and *gay*. Therefore, to have used gay-affirmative words would have been inaccurate and unfaithful to their views" (p. 249, authors' emphases). This is a concern precisely because contemporary SOCE research has almost exclusively surveyed LGB + -identified sexual minorities (see Table 1). It is true that many sexual minorities come to adopt an LGB + identity following unsuccessful SOCE. It is also true that some sexual minorities have reported they dropped an LGB + identity when they experienced change. In addition, there appears to be a sizable but mostly invisible subgroup who have never adopted an LGB + sexuality label, largely for religious reasons (Lefevor et al., 2020; Rosik et al., 2021a). There is evidence this is the subgroup that most often pursues SOCE, so routinely omitting them from SOCE research is a fundamental problem.

Again, this oversight is often a byproduct of an ideological monoculture, wherein researchers understandably utilize the LGB + -allied networks, venues, and organizations easily accessible to them. Few SOCE researchers are known to and trusted by conservative religious institutions and networks. Consequently, the literature almost completely ignores those sexual minorities who have prioritized their religious identities over LGB + identities, are embedded in more traditional faith communities, and may have had different experiences

[1] For example, the National Association of Social Workers (NASW) since 2014 has endorsed 754 candidates to federal office in the United States. The political affiliation of these candidates has been 753 Democrat and 1 Republican (NASW, 2022). Other mental health associations are very likely to be similarly distributed in their sociopolitical leanings. Social psychologist Jonathan Haidt has referred to such numbers as reflecting a "statistically impossible" lack of diversity in a country equally divided between the political left and right (Tierney, 2011).

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Table 1 Inclusion of pre-SOCE or pre-GICE health distress and non-LGBT + -identified sexual minorities in recent research critical of SOCE and GICE

Study	Pre-SOCE or GICE distress controlled?	Purposely Includes non-LGBT + ?	%LGBT +
Blosnich et al. (2020)	No	No	> 87.5% LGB
Chan et al. (2022)	No	No	> 93.7% LGB +
Dehlin et al. (2015)	No	No	> 90% LGB +
del Rio-Gonzalez et al. (2021)	No	No	100% LGBT +
Flentje et al., (2013, 2014)	No	No	100% LGB
Government Equalities Office (UK) (2018)	No	No	> 94% LGB +
Green et al. (2020)	No	No	> 77.9% LGB; 100% T
Higbee et al. (2022)	No	No	100% LGBT +
Jones et al. (2022)	No	No	> 86.6% LGBT +
Lee et al. (2021)	No	No	100% LGB
Maccio (2010)	No	Some	86.9% LGB
Mallory et al. (2018)	No	No	100% LGBT
Meanley et al. (2020)	No	No	100% GB
Ogunbajo et al. (2022)	No	No	100% GB
Ozanne Foundation Advisory Board (2018)	No	Some	> 74.9% LGB +
Ryan et al. (2020)	No	No	100% LGBT +
Salway et al. (2020)	No	No	> 95.8% LGBT +
Salway et al. (2021)	No	No	> 98.5% LGBT
Turban et al. (2019)	No	No	> 78.3% LGB + ; 100% T
Veale et al. (2021)	No	No	100% T or NB

LGBT = lesbian, gay, bisexual, transgender; NB = non-binary. Percentages less than 100 usually reflect the presence of an "other" category that may or may not have included non-LGBT + identified sexual minorities; hence, these figures are likely to be conservative estimates of LGBT + percentages

and outcomes from their change efforts (Rosik et al., 2021b, 2022). These sexual minorities are often excluded by design, as when an LGB + identity is required for study participation. For example, Higbee et al. (2022) indicated they chose "…to only include sexual orientation [i.e., identity, not attractions or behavior] in our analyses because the other variables often measure individuals who identify as heterosexual but engage in same-sex activity rather than individuals with a solidified LGBQ + sexual identity. Experiences of same-sex attraction and same-sex activity tend to include substantially higher percentages of the general population than LGBQ + self-identification" (p. 619). The situation may well be akin to assessing the benefits and harms of marital therapy using only participants recruited through divorce support groups. The SOCE experiences of LGB + -identified persons are of course important to document, but they must not be overgeneralized in a rush to advocate for certain policy prescriptions.

Other methodological weaknesses in the SOCE literature are worth mentioning more briefly. First, SOCE is typically operationalized in an exceedingly coarse fashion, such as someone who "attempts to change" or "tried to change" participants' sexual orientation or gender identity (e.g., Salway et al., 2021). Moreover, research frequently uses language that characterizes change exploring therapy as coerced, again inserting a bias from within a monoculture that may assume no sexual minority person could ever freely choose to explore their capacity for change based on self-knowledge, desire to preserve marriage to a loved one of the opposite sex, intent to hold one's family together, or appreciation of the beauty of one's faith that gives much meaning to life (L. Haynes, personal communication, September 17, 2022). Green et al. (2020) disqualified 105 participants who reported they experienced SOCE but did not indicate someone tried to "make" them change. These researchers justified this exclusion by saying "…it was assumed that these young people may not have understood the intended meaning of conversion or reparative therapy" (p. 1222). Such imprecise and inconsistent operationalizations of SOCE appear plausibly inclusive of wildly varying practices such as aversive behavioral techniques, encouragement to modify risky sexual behaviors, watchful waiting, or even generic prayers for healing. Researchers cannot precisely know what participants envisioned as SOCE or GICE, and hence they can have no real understanding of the source of their findings. Giving sexual minorities the opportunity to evaluate specific methods they have experienced for dealing with their distress is a more

granular approach to SOCE that will likely reveal greater complexity than current narratives allow (Rosik et al., 2022). Another concern with this literature is that SOCE is almost always studied in a binary, retrospective fashion (i.e., exposure to SOCE versus no exposure), which limits what can be gleaned from these studies. Inclusion of sexual minorities still engaged in self-determined, speech-based forms of personal change efforts could assist in identifying the variety of psychological trajectories that may be associated with SOCE, although, as noted, successful recruitment of such individuals may depend upon the establishment of sociopolitically diverse research teams. Relatedly, adding a comparison group of sexual minorities who received non-SOCE psychotherapy as minors and/or young adults would be one way of coming closer to discerning SOCE-originating distress without having to resort to prospective research designs.

Finally, research on SOCE too often relies upon single-item and/or non-standardized measures of psychological distress. Studies of change efforts should more regularly utilize psychometrically established health scales with available normative information in order to avoid questionable interpretations (Reyna, 2017). Relatively few studies have used such instruments (e.g., Chan et al., 2022; Veale et al., 2021), but even these have failed to provide basic descriptive information about group scores such as means and *SDs*, making it impossible to discern the complete meaning of the results. This is crucial because highly significant SOCE group differences may occur on one end of a health-related scale. Such differences should not be interpreted as contrasting well-being and ill-being but rather as averages and gradations of well-being or ill-being that may be so subtle as to not be clinically significant when comparing groups. Giving attention to such details can assist researchers in discerning where statistical significance may not reflect policy-relevant clinical significance (Hojat & Xu, 2004). This will more fully and accurately capture the heterogeneous experiences of sexual minorities who pursue SOCE.

As Sullins pointed out, the American Psychological Association prominently featured Blosnich et al. (2020) in their recent critique of "conversion therapy" (Glassgold, 2022; Haldeman, 2022). A few years ago, Ferguson (2015) questioned the social science behind the APA's policy statements and resolutions on such topics as abortion and media violence. In an observation germane to most mental health associations, he asserted "…policy statements such as these may create a 'tail wags the dog' effect in which science is selected to support a preexisting policy instead of science being carefully and objectively communicated to policymakers and the general public" (p. 536). The only exception to this concern Ferguson highlighted happened to be the APA's policy statements on SOCE, which he described as "grounded in empirical data and practice within the field." Sullins' research undermines such unwavering confidence in the unique stature of the SOCE literature as being scientifically unassailable.

Sullins' work should instigate a reexamination concerning the conventional wisdom of universal harms associated with SOCE and GICE and be a loud wake-up call to researchers and policy makers about the perils of social science conducted within sociopolitical monocultures that cultivate and incentivize confirmation bias and groupthink. Duarte et al. (2015) point out that "…certain assumptions, theories, and findings can become the entrenched wisdom in a field, not because they are correct but because they have consistently undergone less critical scrutiny. When most people in a field share the same confirmation bias, that field is at a higher risk of reaching unjustified conclusions" (p. 23). Sullins has shed an important spotlight on the fact there are potentially narrative-altering limitations within the current SOCE literature, and I have attempted to provide some further examples in this comment. Attending to these methodological weaknesses holds promise in clarifying what SOCE practices are harmful or helpful and for whom. We should all desire a body of research on SOCE that has been subject to meaningful critique in order to give some assurance it is broadly generalizable, accurate, and reproducible. Currently, as Sullins has shown, this appears not to be the case.

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Declarations

Conflict of interest I have coauthored work with Dr. Sullins in the past (Rosik et al., 2021c; Sullins et al., 2012).

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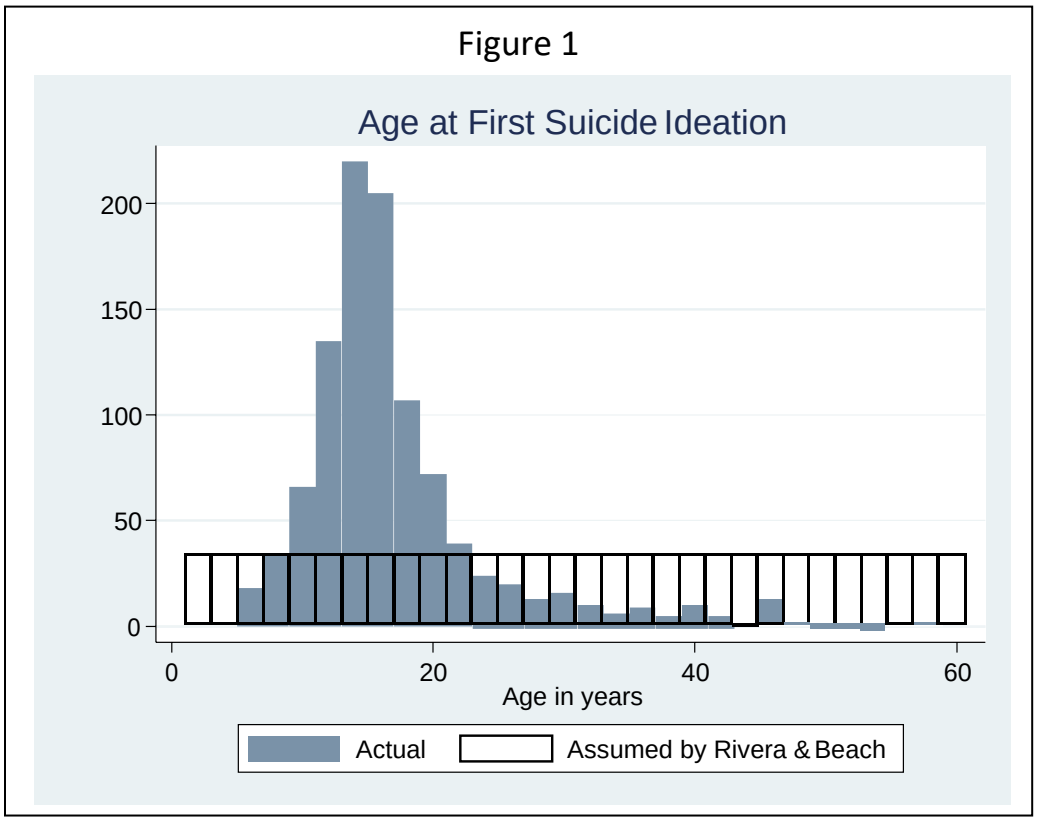
https://doi.org/10.1007/s10508-022-02408-2

FIGURES

Figure 1



TABLES

Table 1. Adjusted odds ratios (AORs) for suicidality by experience of sexual orientation change efforts (SOCE): Probability Sample of Sexual Minorities, United States, 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 (2022)	.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 (2022). 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, United States, 2016-2018 (N=1518)

	Model 1 – per Blosnich et al. (2020)	Model 2 – per Sullins (2022)			
	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.

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.