Conversion Therapy Bans, Suicidality, and Mental Health

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This paper provides the first causal evidence of the effect of state-level, statutory bans on conversion therapy practices (also called Sexual Orientation and Gender Identity and Expression Change Efforts). These practices cause serious psychological harm and impose economic costs of treatment in the hundreds of millions of dollars. Leveraging variation by state and year in statutory bans on conversion therapy for minors, I estimate a series of treatment-timing robust difference-in-differences models and show that these bans lead to modest reductions in deaths by suicide and improvements in self-reported mental health, mostly driven by young males under the age of 25, suggesting that these bans may be an effective lever by which the market externalities imposed by conversion therapy practices may be internalized. I present these results across the backdrop of a sustained increase in national youth suicide mortality rates.

JEL Codes: I3, I18, I19, K38
Keywords: conversion therapy, suicidality,

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1. Introduction

It is well-documented in the economics literature that individuals that sexual and gender minorities (SGM) face worse health outcomes, lower self-rated physical and mental health, and report greater rates of risky behaviors health behaviors like smoking, alcohol consumption (see Meyer 1995; Hatzenbuehler, et al. 2008; Bostwick, et al. 2010; Carpenter, et al.; 2021 and others) compared to their heterosexual and cisgender counterparts. Of particular concern are the disparities in mental health, where SGM experience 2-4 times the risk of depression and anxiety disorders and nearly 5 times the risk of suicide (Cochran, et al. 2003).

In many cases, these disparities manifest early, often in adolesence and throughout early adulthood during critical formative periods when SGM youth navigate important interpersonal and developmental milestones (Russell and Joyner, 2001; Marshall, et al. 2008) while also navigating issues of gender identity and/or sexual orientation that may deviate from societal or familial expectations. These stressors, taken together with external stressors (e.g. anti-LGBTQ+ laws, practices, and rhetoric) likely play a significant role in the observed mental health disparities between SGM and cisgender, heterosexual individuals.

One stressor unique to SGM is Sexual Orientation or Gender Identity Change Efforts (SOGICE), more commonly known as conversion therapy[1].

[1] Both LGBTQ advocacy organizations and mental health professional organizations (GLAAD, 2022; Glassgold et al. 2009) recommend caution when using the term “conversion therapy” to describe sexual orientation or gender identity change efforts as these terms have most often been used to insinuate that sexual

Conversion Therapy is a widely-discredited set of pseudo-therapeutic interventions aimed at enforcing heterosexual attraction and cisgender identity or expression (Mallory, Brown, and Conron, 2019). This practice is grounded in the belief that non-heterosexual and/or non-cisgender identities are inherently pathological, requiring intervention on the part of clinicians (or sometimes spiritual leaders) to suppress them or remove them altogether (Meanly, et al. 2020). The practice has been classified by the International Rehabilitation Council for Torture Victims as a form of torture (Perez-Sales, 2020)

Exposure to conversion therapy dramatically worsens the relative risk of poor mental health and suicidality, even among other SGM. A recent study (Blosnich, et al. 2020) showed that sexual minorities exposed to conversion therapy reported double the odds of lifteime suicidal ideation relative to those who had not been exposed to conversion therapy, 75% increased odds of planning suicide, and 88% increased odds of an attempt which caused an injury, and a 2019 study from the Williams institute suggests that transgender individuals may be as much as twice as likely to have undergone conversion therapy.

It is difficult to know the reach of conversion therapy, but the Williams Institute (Mallory et al. 2019) estimates nearly 700,000 survivors ages 18-59 have

and gender minority status is disordered and that the practices themselves are therapeutic. I make the decision to use the term "conversion therapy" in this paper after carefully weighing these recommendations against the paucity of work on these practices in the economics literature, and thus their relative unfamiliarity to readers who may only know them as conversion therapy. Future economics work should similarly show care when referring to these practices as "therapy."

been subject to these practices, including 350,000 of whom underwent these practices as adolescents. The Trevor Project's 2019 National Survey on LGBTQ Youth Mental Health reported that of the 34,000 surveyed youth, over two thirds were encouraged by people they knew to change their sexual orientation or gender identity. As of July 2022 (Movement Advancement Project, 2022), 20 states and the District of Columbia have banned conversion therapy for minors (though both New York and DC's bans apply to adults as well), however no previous study has evaluated the causal effects of these bans on mental health or suicidality. This study fills that gap by providing the first evidence of the effect of state-level statutory bans on conversion therapy.

1.2 Conversion Therapy in the United States

The early 20th century saw the first clinical efforts to understand and change sexual orientation as early practicioners of psychoanalysis Sigmund Freud, Edmund Bergler, and Sandor Rado first began theorizing about the etiology of homosexuality (Drescher, et al. 2016). Interventions born of these theories were not faith-based, as would become common by the turn of the century, but rather pathologized homosexuality as a developmental disorder arising from disordered parenting or adverse childhood experiences.

These theories were subsequently challenged in the mid 20th century by the work of early sexologists like Alfred Kinsey and Evelyn Hooker, whose pioneering field work interviewing non-patient, non-institutionalized subjects, revealed that homosexuality and bisexuality were far more common than initially thought (Kinsey, Pomeroy, and Martin, 1953; Hooker, 1957). Concurrently, Clellan Ford and Frank Beach's (1951) joint cross-cultural and ethological studies offered evidence that homosexual behavior was not only, as Kinsey and Hooker suggested, a common variation of human sexual behavior, it was also quite common in nature. However, this new scientific consensus was slow to penetrate American psychiatry at the time, where homosexuality remained pathologized in both the first and second editions of the Diagnostic and Statistical Manual (DSM-I [1952], II [1968]).

However, following the 1969 Stonewall riots, early LGBTQ+ civil rights activists and their allies disrupted the 1970 and 1971 meetings of the American Psychiatric Association (Drescher, 2015). These protests led to informational panels at subsequent meetings, and ultimately to the APA's decision to de-classify homosexuality as a mental disorder in 1973, and remove it from the DSM altogether by 1987 with the new DSM III-R. The World Health Organization (WHO) followed suit by removing it from the International Classification of Diseases by 1990.

As the scientific consensus shifted, debates about homosexuality (and later, transgender identity and expression) moved more into the domain of religion and politics, and the scientific establishment turned its eye to the effects of conversion therapy. Though the APA's position since 1973 was that homosexuality was not a mental disorder, by 1998 the APA expanded its position to suggest that conversion therapy was based on developmental theories of "questionable scientific validity," encouraging the National Institutes of Mental Health and the academic research community to investigate the potential risks of undergoing conversion therapy (APA, 2001).

By the early 2000s, psychologists began to document the ineffectiveness and harms of conversion therapy, including severe mental health distress (Beckstead and Morrow, 2004; Shidlo and Schroeder, 2002). In 2009, the American Psychological Association's Taks Force on Appropriate Therapeutic Responses to Sexual Orientation (APA, 2009) recommended mental health professionals provide assistance to those living with distress over their sexual identities with more scientifically-rigorous modalities.

1.3 Conversion Therapy Bans in the United States

As of the time of this paper, a total of 20 states, the District of Columbia, Puerto Rico, and 100 municipalities have banned conversion therapy for minors, either legislatively or by executive order, and a recent study (Flores et al. 2020) found that a majority of Americans support banning the practice. However, the movement to ban conversion therapy in earnest began with California's *SB-1172*, which was signed into law on September 30, 2012.

Although the law was originally slated to go into effect on January 1, 2013, a district court judge granted a temporary injunction preventing the law from going into effect. This was subsequently overturned in the federal appellate court of the 9th Circuit, and the law went into effect on August 29, 2013, just 10 days after New

Jersey's conversion therapy ban (AB 3371) was signed into law and went into immediate effect. Following the resolution of the legal challenges to SB-1172 and the passage of AB 3371, and increasingly public outcry over LGBTQ+ youth suicides, the Obama administration called for a ban on conversion therapy in 2015, after which over 2/3 of the current conversion therapy bans were signed into law or issued via executive order.

Table 1 lists each state that banned conversion therapy and the date on which each ban went into effect.

Table 1: States Fully Banning Conversion Therapy and Effective Date of Ban

State	Year Passed	Effective Date	Ban Type
New Jersey	2013	19-Aug-13	Legislative
California	2012	29-Aug-13	Legislative
Oregon	2015	18-May-15	Legislative
Illinois	2015	1-Jan-16	Legislative
Vermont	2016	1-Jul-16	Legislative
New Mexico	2017	7-Apr-17	Legislative
Connecticut	2017	10-May-17	Legislative
Rhode Island	2017	19-Jul-17	Legislative
Nevada	2017	1-Jan-18	Legislative
Washington	2018	7-Jun-18	Legislative
Hawaii	2018	1-Jul-18	Legislative
Delaware	2018	23-Jul-18	Legislative
Maryland	2018	1-Oct-18	Legislative
New Hampshire	2018	1-Jan-19	Legislative
New York	2019	25-Jan-19	Legislative
Massachusetts	2019	8-Apr-19	Legislative
Maine	2019	17-Sep-19	Legislative
Colorado	2019	2-Aug-19	Legislative

Utah	2020	21-Jan-20	Executive Order
Virginia*	2020	1-Jul-20	Legislative
Minnesota*	2021	15-Jul-21	Executive Order

*Note: These states not in sample period

2. Literature Review

While LGBTQ+ civil rights have been at the epicenter of political debates in the United States since the 1960s, only more recently have economists and other social scientists documented how the evolving legal environment born from these debates have affected SGM economic and health outcomes (Badgett, Carpenter, and Sansone, 2021). While sentiment toward SGM has improved markedly in the past few years for both sexual minorities (Anderson and Fetner, 2008; Masci et al. 2019) and gender minorities (Lewis, et al., 2017; Jones, et al. 2018), this growing body of work documents that these improvements are positively impacted by the passage of pro-SGM legislation such as same-sex marriage legalization (Aksoy et al. 2020; Kreitzer et al. 2014; Tankard and Paluck 2017), the repeal of laws that criminalize homosexuality (Kenny and Patel, 2017), and court decisions banning employment discrimination (Deal, 2022).

Positive changes in attitudes toward SGM are often cited as a key mechanism for the concurrently observed effects of pro-LGBTQ+ policies on SGM health, especially mental health. For instance, bans of legal same-sex marriage are associated with poorer sexual minority mental health (Herdt & Kertzner, 2006; Hatzenbuehler et al. 2010), while legal recognition of same-sex marriage is associated with improved SGM mental health (Riggle, Rostosky, & Horne, 2019; Wight, LeBlanc, & Badgett, 2013). Mann (2022) documents marked improvements in self-reported mental health, particularly among likely sexual minority men following the passage of employment non-discrimination acts, while Mann, Campbell, and Hien (2022) show that state-level ordinances that provide access to gender-affirming care for low-income transgender individuals through state Medicaid programs improve transgender mental health.

This work contributes to the growing body of LGBTQ+ economic research that estimates the direct effects of LGBTQ-targeted policy on the health outcomes of SGM.

3. Data

3.1 Mortality Data

To estimate the effect of conversion therapy bans on deaths by suicide, I use publicly available data from the 2007-2020 US Vital Statistics National Center for Health Statistics Multiple Cause of Death Files to calculate morality rates (NCHS, 2007-2020) for deaths by suicide (both by firearms and other means), non-injury deaths due to malignant neoplasms (cancer), and deaths due to major cardiovascular diseases. These data are abstracted from death certificates filed with the vital statistics centers of each state and the District of Columbia and include all deaths which occurred in the United States from 2007-2020. I calculate mortality rates at the state level, using the number of suicide and non-injury related deaths per 100,000 population. I use sex and age group-specific populations to calculate suicide rates when stratifying by those respective populations.

The public-use mortality files suppress counts of deaths fewer than 10 for confidentiality reasons. This results in significantly left-censored data for some age groups in which death by suicide is relatively rare (for example, nearly 15% of data on gun-related deaths by suicide for young men under the age of 25 is suppressed). Therefore, I only use data that is twice stratified (by age and gender, by age and method, or by gender and method) for these age groups, which ensures less than 1% suppression in all cases. Similarly, since some 5-year and 10-year age bands for young populations result in more than 5% data suppression. Rather than introduce potential measurement error via imputation, I restrict my main analysis to deaths by suicide under age 25.

I supplement each state by year observation with aggregated time-varying state-level characteristics from the American Community Survey (ACS) including state racial and ethnic composition, average income and education, and the state unemployment rate. Finally, I include data on the state-level LGBTQ+ legal landscape for each year: sodomy law repeals, state same-sex marriage legislation, employment non-discrimination acts, and LGBT hate crime legislation. I combine these data to create a balanced state-by-year panel for years 2007-2020.

3.2 Google Trends

Since it is virtually impossible in the current data landscape to observe take-up of conversion therapy and the salience of conversion therapy bans, I turn to publicly available web search data from Google Trends to uncover how conversion therapy bans affected web search intensity for conversion therapy, consumer-related conversion therapy terms, and homophobic search terms overall.

Google Trends is a publicly available tool developed by Google that provides reports of the relative popularity of web-searches in Google Search from 2004 onward. Users may create time-series reports of the relative (to all other Google searches) popularity of a given search term (Cebrian and Domench, 2022) in a given region normalized between 0 and 100. This relative search intensity can be a useful proxy for socially sensitive topics like conversion therapy as those using Google's search engine have no incentive to lie or obfuscate potentially unpopular views (Stephens-Davidowitz, 2014), but also because Google Trends data can be useful in uncovering mechanisms that can affect health decisions (see Oster, 2018; Carpenter and Lawler, 2019).

I collect data on Google search intensity from years 2004-2020 for two sets of search terms: search intensity for "conversion therapy" itself, consumer related search terms such as organizations that advocate for conversion therapy or communities/movements that support conversion therapy (see Appendix Table 1 for a list of these terms. For the second I use principal component analysis to convert intensity into an index following Sansone, 2019; Mann, 2022; and Nikolaou, 2021. I standardize this index for ease of interpretation following Banerjee, Duflo, and Sharma, 2021.

3.3 The Behavioral Risk Factor Surveillance System

To examine the effect of conversion therapy bans on self-reported mental health, I draw from the 2007 to 2019 waves of the Behavioral Risk Factor Surveillance System (BRFSS). The BRFSS is an annual cross-sectional telephone-based health survey conducted by the Centers for Disease Control and Prevention (CDC). The BRFSS asks respondents to rate their mental health asking the following question: "Now thinking about your mental health, which includes stress, depression, and problems with emotions, for how many days during the past 30 days was your mental health not good?" Respondents are then asked to state the number of days for this question. I both use this variable and following Carpenter, Eppink, Gonzales, and McKay, 2021, I create an indicator variable for if respondents report at least 14 bad mental health days in the past 30 days, a measure often used by the CDC to indicate frequent mental health distress (Cree, et al. 2020).

3.4 Empirical Framework

The primary purpose of this study is to identify the causal impact of conversion therapy bans on suicidality and mental health. I begin by exploiting variation in the rollout of state-level bans on conversion therapy for minors to estimate a difference-in difference model with a baseline specification which takes the following form:

$$y_{st} = \beta_1 CTBan_{st} + \beta_n X_{st} + \mu_s + \tau_t + \epsilon_{st} \quad (1)$$

Where y_{st} is the outcome of choice in state *s* and year *t* and $CTBan_{st}$is an indicator variable equal to one if a conversion therapy ban in state *s* is in effect in year *t* and zero otherwise. I include a vector of time-varying, state-level covariates X_{st} which includes state racial and ethnic composition, average income and education, and the state unemployment rate as well as the state-level LGBTQ+ legal landscape for each year: sodomy law repeals, state same-sex marriage legislation, employment non-discrimination acts, and LGBT hate crime legislation. I include state and year fixed effects, μ_s and τ_t, to adjust for time-invariant state characteristics and time-specific shocks, respectively. I estimate ordinary least squares regressions and weight observations by each sub-group population, clustering my standard errors at the state level.

A growing body of literature (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021) urges caution in the estimation of difference-in-differences models with heterogeneous treatment timing arising from potential bias introduced by improper comparisons between early and later-treated units. Therefore, the estimated parameter of interest in equation (1), β_1, may be estimated with bias, particularly under heterogeneous treatment effects across time and treatment cohorts, where a cohort is a set of states that ban conversion therapy in a common year. I utilize three difference estimators from the literature to ensure my estimates are recovered with the least amount of bias: Gardner's (2021) two-stage difference-in-differences estimator, Callaway and Sant'Anna's (2020) doubly robust estimator, and Cengiz, et al.'s (2019) stacked difference-in-differences estimator. I present these estimates together with my baseline estimate to provide a plausible range of the "true" average treatment effect. For a summary discussion of each of these approaches, see Baker, et al. 2022.

Further, the validity of this quasi-experimental design relies on the absence of selection bias, usually referred to as the parallel trends assumption—that, absent conversion therapy bans, trends in suicide mortality would not have been statistically different across treatment cohorts.

While this counterfactual is inherently unobservable, I test for parallel pre-treatment trends by employing the following event study specification:

$$y_{st} = \sum_{j=-5;j\neq-1}^{5} \beta_t CTBan_{st} \ (t = k + j) + \beta_n X_{st} + \mu_s + \tau_t + \epsilon_{st} \quad (2)$$

Where y_{st} indicates the suicide rate in state *s* and year *t* and the summation term indexes the year relative to the conversion therapy ban (the ban occurs in *j*=0 and *k* represents the year that each state banned conversion therapy). I specify the reference period in these models at j=*-1*. To reject the null hypothesis that selection bias is present, I examine pre-treatment estimates of parameter β_t (such that *j*<0). If these estimates are not statistically different from zero, I interpret this as evidence of no selection bias / parallel pre-treatment trends. That is, states that banned conversion therapy would have continued a similar trajectory in deaths due to suicide had those bans never occurred.

As previously mentioned, new developments in the DiD literature have highlighted that under heterogenous treatment timing and heterogeneous treatment effects across cohorts and time, the estimation strategy discussed in equation (2) is prone to bias. While this bias is most exacerbated when large proportions of the sample are treated and/or treatments occur early in the sample timeframe (neither is the case in this context), I utilize the doubly robust method of Callaway and Sant'Anna (2021), which allows for estimation conditional on covariates. This approach identifies what the authors call the "group-time average treatment effect on the treated." I then use this method to construct event study plots, which I test for support of parallel trends.

4. Results

In this and following sections, I present a collage of evidence of the effect of conversion therapy bans on several outcomes. I begin by presenting descriptive statistics and unadjusted trends in state-level aggregated deaths by suicide, especially those among young men (who account for most deaths by suicide among those under age 25). Next, I present event study and difference-in-difference models that compare suicide-related mortality between states that have and have not banned conversion therapy. Following this, I present event study and difference-in-difference results from the BRFSS, comparing self-reported mental health among residents in states that have and have not banned conversion therapy. Finally, I present suggestive evidence from Google Trends that these bans were (a) salient and (b) led to meaningful changes in the search intensity for conversion therapy and related terms.

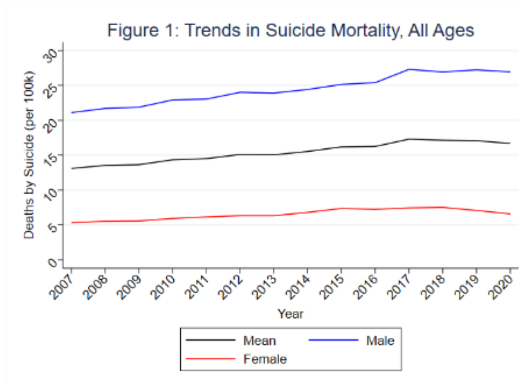
I estimate statistically significant reductions in suicide mortality rates following conversion therapy bans, driven mainly by reductions in suicide among young men under the age of 25. I do not find any sustained reduction in suicide among young women or among older populations. These effects on suicide related mortality mirror the estimates of the effect on self-reported mental health where again I observe improvements in self-reported mental health among young men under the age of 25, suggesting that the reductions I observe in deaths by suicide are being driven by improvements in mental health following these conversion therapy bans.

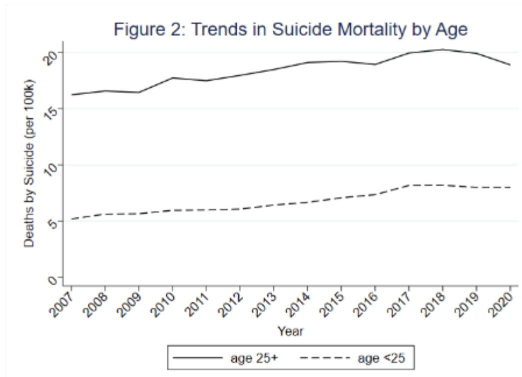
Finally, I present the results from several sensitivity tests, illustrating the robustness of my results. I re-estimate my models across a variety of placebo tests, demonstrate the robustness of my results to the use of linear and nonlinear estimation methods, and are not being driven by a single treatment cohort. Further, I discuss the limitations of my design and the conditions and/or specifications under which my results do not retain statistical significance.

4.1 Trends in Suicide-Related Mortality

Suicide is a very serious public health concern and a leading cause of death in the United States (CDC, 2022). In 2020, it was the twelfth-leading cause of death across all age groups, and nearly 46,000 people died by suicide, twice the amount that died due to homicide. Suicide is third leading cause of death for those age 15-24 and second leading cause of death for those 10-14. While female individuals are more likely report suicidal ideation and attempt, males are much more likely to die by suicide across all age groups. For example, in 2020, the age-adjusted suicide rate for males (22 per 100,000) was 4 times higher than that among females (5.5 per 100,000). The age-adjusted suicide rate has also increased across the study period of 2007 to 2020, with the largest sustained period of increase (about 21%) between 2007 and 2018 (author’s calculation).

I next present unadjusted trends in the rate of deaths by suicide. Figure 1 plots these average trends and splits them by sex. Since those most at risk of exposure to or to be survivors of conversion therapy are young people, I focus on this group. Figure 2 compares the calculated suicide rate among those under age 25 relative to all others. I document modest, persistent increases in population-level deaths by suicide among males and across age cutoffs.

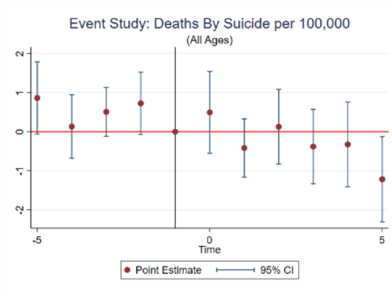




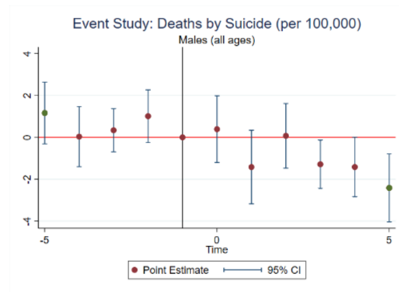
4.2 Impact of Conversion Therapy Bans on Deaths by Suicide

To test for the presence of pre-treatment parallel trends, I begin by presenting graphical evidence derived from equation (2). Figure 3 panel (a) displays the event study for the entire population. Panel (b) displays the event study restricting the sample to men. Panel (c) displays the event study when restricting the sample to men under age 25. Panel (c) provides strong evidence that there were parallel trends in suicide mortality for young men regardless of whether a state subsequently took up a conversion therapy ban.

Figure 3



(a)



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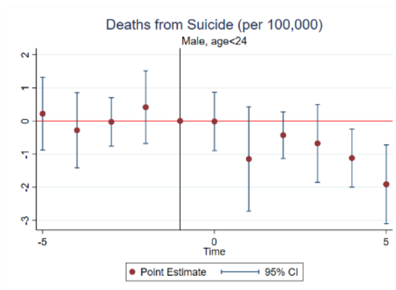


(c)

I present first my main results from equation (1) in Table 2, which summarizes the estimated effect of conversion therapy bans on deaths by suicide for the U.S. population. Column (1) presents the results from equation (1) without accounting for heterogeneous treatment timing. Columns (2)-(4) present estimates from three different treatment timing-robust models as discussed in section 3.4. All coefficient estimates presented in Table 2 and for the rest of this section represent the difference in the rate of deaths by suicide.

Table 2: Effect of Conversion Therapy Bans: All Suicide Deaths per 100,000

	(1)	(2)	(3)	(4)
	TWFE	C&S	Gardner	Stacked

		Doubly Robust DD	2SDD	DD
Conversion Therapy Ban	-.910074***	-.493288**	-.5284631*	-.561601**
	(0.32465)	(0.2439021)	(0.2982141)	(0.2149132)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
State Controls from ACS	☑		☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	13.15	13.15	13.15	13.15

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Table 3, I report the same results after restricting the sample to deaths by suicide under age 25. In Table 4, I report these results again after restricting the sample to deaths by suicide under age 25 for men. I present estimates and event studies for other subgroups in Appendix Figures 1a and 2a and show that conversion therapy bans had no effects on suicide mortality for older cohorts (ages 55+) or for women (of any age), respectively.

My estimates suggest that banning conversion therapy for minors lead to statistically significant reductions in deaths by suicide, driven mainly by men under the age of 25. This amounts to a reduction of about 4% relative to states that did no ban conversion therapy (about half a death fewer per 100,000 overall). This effect was slightly larger for young people under age 25, where I estimate about a 5.5%

reduction (about one quarter death fewer per 100,000), and was most pronounced for young men, where I estimate about an 8% reduction (a little over one fewer death per 100,000).

Table 3: Effect of Conversion Therapy Bans: Suicide Deaths per 100,000 (age<24)

	(1) TWFE	(2) C&S Doubly Robust DD	(3) Gardner 2SDD	(4) Stacked DD
Conversion Therapy Ban	-1.06056**	-.793288**	-.7606813*	-.727900**
	(0.4260652)	(0.3439021)	(0.3796913)	(0.3115808)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
State Controls from ACS	☑		☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	9.95	9.95	9.95	9.95

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Indeed, these effects are driven almost exclusively by reductions in suicide mortality for men (about a 6.5% reduction regardless of age), with no comparable statistically significant reductions in deaths by suicide for young women or women overall. This is consistent with evidence from psychological and epidemiological evidence that the subgroup most likely to take up or be pressured into conversion therapy are young men (Salway et al. 2021).

Table 4: Effect of Conversion Therapy Bans: Suicide Deaths per 100,000 (Male, age<24)

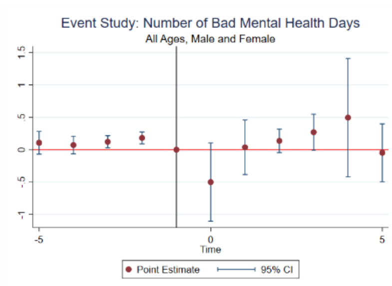
	(1) TWFE	(2) C&S Doubly Robust DD	(3) Gardner 2SDD	(4) Stacked DD
Conversion Therapy Ban	-.7873202**	-1.043888**	-1.072155*	-1.188784**
	(0.3852917)	(0.5150635)	(0.5656459)	(0.3740859)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
State Controls from ACS	☑		☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	14.54	14.54	14.54	14.54

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.3 Impact of Conversion Therapy Bans on Self-Reported Mental Health

Since reduced suicide mortality is only one potential vector of improved mental health following conversion therapy bans, I next present the secondary results from equations (1) and (2) using data from BRFSS. In Figure 4, I present event study plots for the number of bad mental health days. Panel (a) presents the event study for the whole population, panels (b) and (c) by gender. In Figure 5, I present event studies for males and females after restricting the sample to those under 25. Significant pre-trends are clearly observable throughout Figures 4 and 5, which threaten the causal interpretation of estimated DiD coefficients for the full population.

Figure 4



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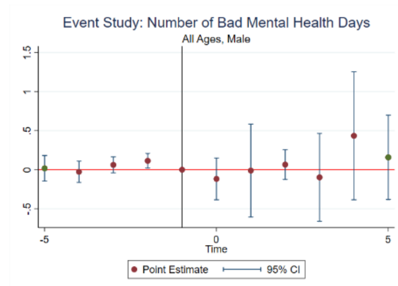

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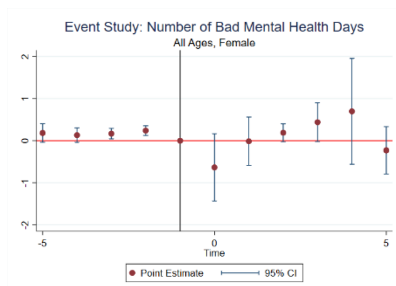
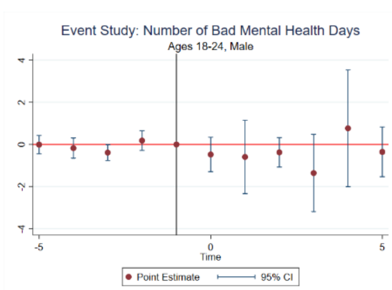
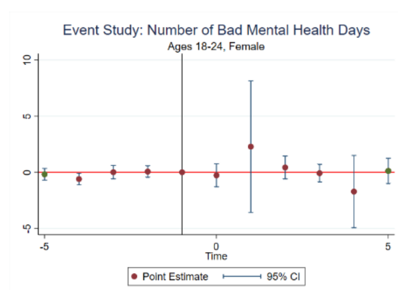


(c)

Figure 5



(a)

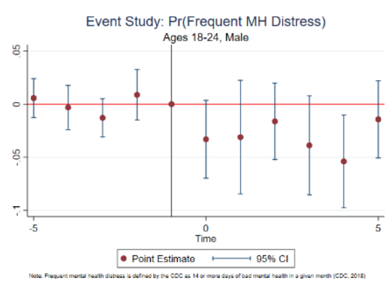


(b)

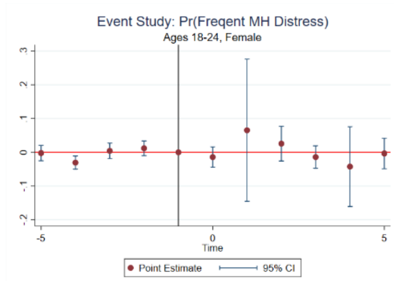
However, since suicidal ideation and attempt are likely a function of psychological distress rather than a modest increase in the number of bad mental health days, I re-estimate these event studies with the outcome of probability of frequent mental health distress. Figure 6 presents the results of equation (2) with this outcome. Panels (a) and (b) display separate event studies for males and females under age 25. (See Appendix Tables 2 and 3 for these results for the full population disaggregated by gender.) The results in figure 6 show broad similarities to the event studies for the number of poor mental health days in the pre-policy period, but a clearer pattern of reduction is visible for males.

Figure 6

(a)



(b)



The DiD results presented in tables 5 and 6 suggest that while conversion therapy bans may have a small effect on the number of self-reported poor mental health days (about 0.3 fewer bad mental health days--less than 1% over the baseline), they do cause significant reduction in the probability of reporting 14 or more poor mental health days by about 1 percentage point (a reduction of nearly 10% over the mean) for young men under age 25. The effect on young women is insignificant and near zero, which comports with my findings in the suicide mortality data, which further reinforces that the mental health effects of these bans are mostly driven by young men.

Table 5: Effect of Conversion Therapy Bans
Ages 18-24, Male

	(1) # of Bad Mental Health Days	(2) **Pr**(Frequent MH Distress)
Conversion Therapy Ban	-. 0291185*	-.00937854***
	-0.0108076	-0.00155005
State FE	☑	☑
Year FE	☑	☑
Individual Controls from BRFSS	☑	☑
LGBT Policy Controls	☑	☑
Mean of Dependent Variable	3.44146	0.0967425
Observations	124,079	124,079

Standard errors in parentheses. All regressions weighted and clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Effect of Conversion Therapy Bans
Ages 18-24, Female

	(1) # of Bad Mental Health Days	(2) **Pr**(Frequent MH Distress)

Conversion Therapy Ban	-0.1013373	-0.005196
	0.1450226	0.008434
State FE	☑	☑
Year FE	☑	☑
Individual Controls from BRFSS	☑	☑
LGBT Policy Controls	☑	☑
Mean of Dependent Variable	4.903313	0.0983318
Observations	124,079	124,079

Standard errors in parentheses. All regressions weighted and clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

While the magnitude of this reduction in frequent mental health distress may seem large, it is comparable to other similar estimated effects. For instance, Carpenter, et al. 2021 estimate an effect size roughly twice as large (for women in same sex households) when estimating the effect of legal access to same-sex marriage on health outcomes using BRFSSS, and Mann, 2022 estimates an effect size roughly four times as large (for men in same sex households) when estimating the effects of employment non-discrimination acts on mental health using BRFSS. Though the policies evaluated in each of these works are qualitatively different than conversion therapy bans, they suggest that my estimates are likely conservative since data limitations constrict my ability to explicitly measure the effect of conversion therapy bans on SGM themselves.

Based on the most recent estimates of the LGBTQ+ population among millennials and Generation Z (Gallup, 2021), a conservative scale-up estimate (using the lower bound of the 95% confidence interval of the effect observed in Table 5) of the likely effect on sexual minorities is about a 65% reduction in the probability of frequent mental health distress. I offer suggestive evidence of this argument of a downward bias in Appendix Table 4 in which I turn to the BRFSS optional sexual orientation and gender identity (SOGI) module. Beginning in 2014, the BRFSS has offered states an optional questionnaire module in which they may directly ask participants to disclose their sexual orientation and gender identity. This optional module was taken up by 35 states (see Appendix Table 5 for a list of these states and the years over which they collected SOGI information) between 2014-2020. While the lack of coverage of the SOGI module over the entire sample period limits the causal interpretation of my results, the estimates presented in Appendix Table 4 suggest much larger improvements in self-reported mental: about 50% reductions over the baseline in both the number of bad mental health days (about 3.7 fewer) and the probability of frequent mental health distress (about a 25-percentage point reduction), which roughly approximate the results of the scale-up.

Taken together, these estimates suggest a plausible mechanism for the results observed in section 4.2: statutory bans on conversion therapy reduce the probability of severe psychological distress, which could lead to suicidal ideation and attempt.

4.4 Google Trends

One potential source of concern is that conversion therapy bans may be largely symbolic acts of legislation, and are not salient to both targeted groups or the general public. Figures 7 and 8 present event studies of Google trends data described in section 3.2. Figure 7 shows an event study plot documenting Google search intensity for the term “Conversion Therapy.” While this event study should not be interpreted as evidence for causality (as the parallel trends assumption is violated), it illustrates an intuitive narrative: search intensity for the term conversion therapy was lower in states that would eventually ban the practice up until the year conversion therapy bans passed, in which a sharp relative increase is clearly observable. This comports with the idea that in the year leading up to a ban, news outlets would be reporting on it, driving searches for the term online.

Figure 8 shows an event study plot documenting search intensity for an index of terms including consumer-related search terms, including organizations which facilitated conversion therapy. The term “conversion therapy” itself is not included in this index. Again, an intuitive pattern emerges: statistically insignificant coefficient estimates in the years leading up to a conversion therapy ban and an observable decline in search intensity for these terms following the bans, about a .15 standard deviation decrease (see Appendix Table 6) relative to states that did not ban conversion therapy. Again, while these results should not be interpreted causally, the patterns in search intensity contribute to the body of evidence I have presented that these bans had a measurable behavioral effect.

Figure 7

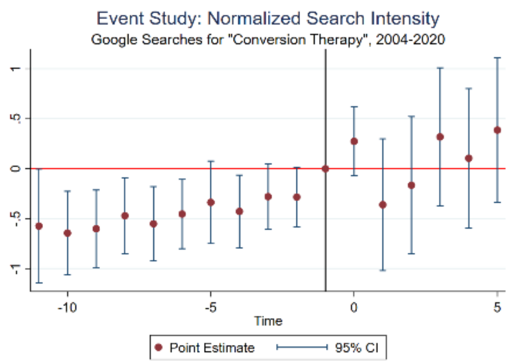
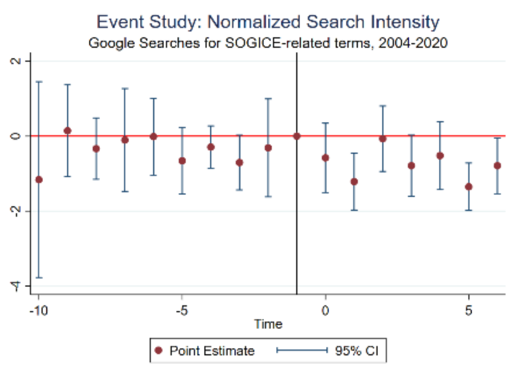


Figure 8



These results imply that conversion therapy bans were salient in states that passed them, which coincides with other evidence that providers were aware of them. For instance, in 2018 the Supreme Court cited the failed 2012 legal challenge to California's conversion therapy ban by practitioner Joseph Nicolosi in the *National Institute of Family and Life Advocates v. Becerra* case, which mandated faith-based "crisis pregnancy" center provide disclosures about state-provided abortion services. Nicolosi subsequently closed his practice to minors. Similarly, in 2015, the New Jersey Superior Court case *Ferguson v. JONAH* fined a conversion therapy-providing organization $72,400 and found that conversion therapy constituted consumer fraud. This fine was later increased to $3.5 million after the organization violated its settlement agreement, and the organization was required to permanently cease operations.

Since the effects of these bans were salient both to the general public and providers, conversion therapy bans are a plausible channel for the reductions I find in suicide mortality and improvements I find in mental health.

4.5 Robustness

Next, I consider a series of additional analyses to test the sensitivity of my results to a series of robustness exercises. To ensure that my results were not driven by a single treated state, I conduct a leave-one-out analysis and systematically exclude treated cohorts, re-estimating my primary specification. Iteratively dropping treated cohorts from my analysis does not meaningfully change my primary results. See Appendix Table 7 for the results of this exercise.

Next, I run a placebo test to ensure that my results are not spurious by re-estimating equation (1) for outcomes not likely to be affected by conversion therapy. In Appendix Figure 2 and Appendix Table 8, I present the results of these estimates showing that conversion therapy bans had no effect on non-injury deaths due to cancer or heart disease. This suggests that the policy's effects on deaths by suicide are not being driven by other unobserved aggregate mortality-reducing phenomena.

To test if variation caused by state-specific factors that trend linearly over time, I interact state fixed effects with year-specific indicator variables and re-estimate my primary specification. While I lose statistical significance across all my primary results under this specification, my results remain descriptively similar to my preferred specification. This is not surprising given the data limitations of the current study, in which state specific linear time trends would overfit the model.

Next, following Shahid, 2022, I estimate the effect of these bans on suicide mortality when the attempt is completed with a firearm compared to when the attempt is completed by some other mean. Clark, et al. 2020 find that sexual minority men are about half as likely to use firearms to complete suicide as non-sexual-minority men. While I find reductions in suicide mortality in both cases (see Appendix Tables 9 and 10), the reductions are larger among men who complete suicide by methods other than firearms (about a 5.5% reduction over the mean for firearm suicide mortality and about a 7% reduction over the mean for non-firearm mortality).

To ensure that my results are not being driven by assumptions of model linearity, I re-estimate equation (1) as a stacked poisson regression model, weighting by appropriate sub-group population. These results, displayed in

Appendix Table 11, are robust to nonlinear model selection and the estimated effects are quite similar (in percentage change) to those when the model is estimated using TWFE.

Finally, given concerns raised by MacKinnon & Webb (2018) about using cluster-robust standard errors when the number of treatment clusters is small, I use the authors' prescribed wild cluster bootstrapping procedure to re-estimate p-values. The results of this exercise are found in Appendix Table 12 and while I lose statistical significance for my results for young men, the estimated reductions in suicide for young people overall remain statistically significant at the 10% level.

5. Discussion and Conclusion

This study evaluates the impact on suicidality and mental health of banning the discredited practice of sexual orientation and gender identity/expression change efforts, sometimes referred to as conversion therapy. These practices are explicitly harmful to those who are subjected to them, dramatically increasing the probability of mental health disorders and of suicidal ideation and attempt. I find significant reductions in suicide mortality in states that ban this practice, especially among young men under the age of 25. These states saw reductions of about 8% in deaths by suicide for young men under the age of 25, about 1 fewer death per 100,000 relative to states that did no ban suicide. For comparison, Shahid, 2022 finds that the introduction of HIV-treating drugs at the height of the AIDS epidemic in the US reduced suicides by approximately 25% among likely-sexual minority men.

I find comparable effects of conversion therapy bans on self-reported mental health, again mainly concentrated among young men under age 25. In states that banned these practices, young men enjoyed significant reductions in the probability of frequent mental health distress relative to states that did not ban these practices, and with no detectable effect among women of similar age or older men. This is consistent with the literature that people most likely to take up or be forced into conversion therapy are young men. I offer suggestive evidence that the effects may be much larger for young people who explicitly identify as gender or sexual minorities.

Finally, I offer evidence that these bans were salient to the general population and that they changed internet search habits of those who live in states where the bans occurred relative to those in other states. Taken together, this evidence paints a clear picture the confesses with the broader psychological and epidemiological literature: conversion therapy practices are harmful, and banning them, while limiting the choice set of some practitioners who contest the immutability of sexual orientation and gender identity, has measurable population-level positive effects on suicide mortality and mental health, especially for young men. This is an important consideration given recent (and dramatic) increases in suicide mortality and self-reported poor mental health.

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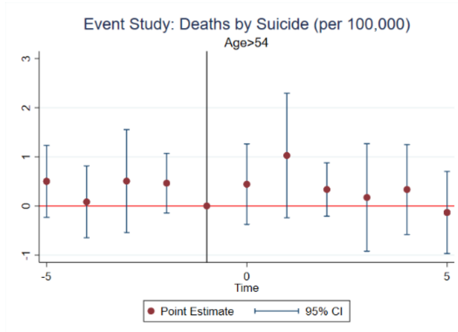
Appendices

Appendix Table 1: States Fully Banning Conversion Therapy and Effective Date of Ban

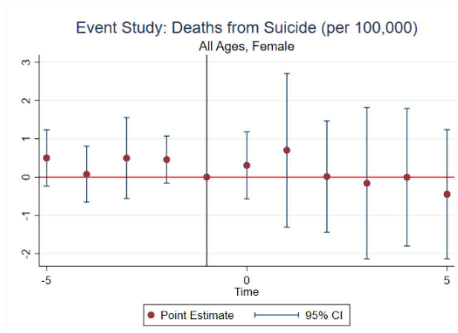
State	Year Passed	Effective Date	Ban Type
New Jersey	2013	19-Aug-13	Legislative
California	2012	29-Aug-13	Legislative
Oregon	2015	18-May-15	Legislative
Illinois	2015	1-Jan-16	Legislative
Vermont	2016	1-Jul-16	Legislative
New Mexico	2017	7-Apr-17	Legislative
Connecticut	2017	10-May-17	Legislative
Rhode Island	2017	19-Jul-17	Legislative
Nevada	2017	1-Jan-18	Legislative
Washington	2018	7-Jun-18	Legislative
Hawaii	2018	1-Jul-18	Legislative
Delaware	2018	23-Jul-18	Legislative
Maryland	2018	1-Oct-18	Legislative
New Hampshire	2018	1-Jan-19	Legislative
New York	2019	25-Jan-19	Legislative
Massachusetts	2019	8-Apr-19	Legislative
Maine	2019	17-Sep-19	Legislative
Colorado	2019	2-Aug-19	Legislative
Utah*	2020	21-Jan-20	Executive Order
Virginia*	2020	1-Jul-20	Legislative
Minnesota*	2021	15-Jul-21	Executive Order

*Note: These states not marked as treated in the sample period.

Appendix Figure 1



(a)



(b)

Appendix Table 2: Effect of Conversion Therapy Bans
All Ages, Male

	# of Bad Mental Health Days	**Pr**(Frequent MH Distress)
Conversion Therapy Ban	-.192881**	-.0044182**
	(0.9483237)	(0 .0019291)
State FE	☑	☑
Year FE	☑	☑
Individual Controls from BRFSS	☑	☑
LGBT Policy Controls	☑	☑
Mean of Dependent Variable	2.78711	.085744
Observations	2,142,371	2,142,371

Standard errors in parentheses. All regressions weighted and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 3: Effect of Conversion Therapy Bans
All Ages, Female

	# of Bad Mental Health Days	**Pr**(Frequent MH Distress)
Conversion Therapy Ban	-.0814669	-.0066069
	(0.0794031)	(0.0041823)
State FE	☑	☑
Year FE	☑	☑
Individual Controls from BRFSS	☑	☑
LGBT Policy Controls	☑	☑
Mean of Dependent Variable	3.820666	.1186232
Observations	3,100,259	3,100,259

Standard errors in parentheses. All regressions weighted and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 4: Effect of Conversion Therapy Bans: LGBT vs. Non-LGBT, under age 25

	(1) LGBT # of Bad MH Days	(2) LGBT Pr(Frequent MH Distress)	(3) Non-LGBT # of Bad MH Days	(4) Non-LGBT Pr(Frequent MH Distress)
Conversion Therapy Ban	-3.731807***	-.1255884***	.3715061	.0134074
	(0.6011279)	(0.0238072)	(0.5355287)	(0.0246599)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
Individual Controls from BRFSS	☑	☑	☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	7.598214	.254902	4.487043	.1310446
Number of Observations	5,291	5,291	42,051	42,051

Standard errors in parentheses. All regressions clustered at the state level.
[*] $p < 0.10$, [**] $p < 0.05$, [***] $p < 0.01$

Appendix Table 5:

States Releasing Sexual Orientation and Gender Identity Questions to the Public Use BRFSS Data File

- **2014:** Delaware, Hawaii, Idaho, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Minnesota, Montana, Nevada, New York, Ohio, Pennsylvania, Vermont, Virginia, Wisconsin, Wyoming.
- **2015**: Colorado, Connecticut, Delaware, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa (only to a random subset of its sample), Kansas, Maryland, Massachusetts, Minnesota, Missouri, Nevada, New York, Ohio, Pennsylvania, Texas, Virginia, West Virginia, Wisconsin.
- **2016**: California, Connecticut, Delaware, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kentucky, Louisiana, Massachusetts, Minnesota, Mississippi, Missouri, Nevada, New York, Ohio, Pennsylvania, Rhode Island, Texas, Vermont, Virginia, Washington, Wisconsin.
- **2017**: California, Connecticut, Delaware, Florida, Georgia, Hawaii, Illinois, Indiana, Iowa, Louisiana, Massachusetts, Minnesota, Mississippi, Montana, Nevada, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, Texas, Vermont, Virginia, Washington, Wisconsin.
- **2018**: Arizona (only to a random subset of its sample), Connecticut, Delaware, Florida, Hawaii, Idaho, Illinois, Kansas, Louisiana, Maryland, Minnesota, Mississippi, Missouri, Montana, Nevada, New York, North Carolina, Ohio, Oklahoma, Pennsylvania, Rhode Island, South Carolina, Tennessee, Texas, Vermont, Virginia, Washington, West Virginia, Wisconsin
- **2019:** Alaska, Arizona, Colorado, Connecticut, Delaware, Florida, Georgia, Hawaii, Idaho, Iowa, Kansas, Louisiana, Maryland, Minnesota, Mississippi, Montana, New York, North Carolina, Ohio, Oklahoma, Rhode Island, South Carolina, Tennessee, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin
- **2020:** Alaska, Arkansas, California, Colorado, Connecticut, Georgia, Hawaii, Idaho, Illinois, Indiana, Iowa, Kansas, Louisiana, Massachusetts, Michigan, Minnesota, Montana, New Jersey, New Mexico, New York, North Carolina, Ohio, Oklahoma, Rhode Island, South Carolina, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin

Appendix Table 6: Effect of Conversion Therapy Bans On Google Search Intensity

	(1) Search intensity for "Conversion Therapy"	(2) Search intensity for consumer-related CT terms
Conversion Therapy Ban	-.2601292**	-.1510363**
	(0.1249173)	(0.546488)
State FE	☑	☑
Year FE	☑	☑
Observations	800	800

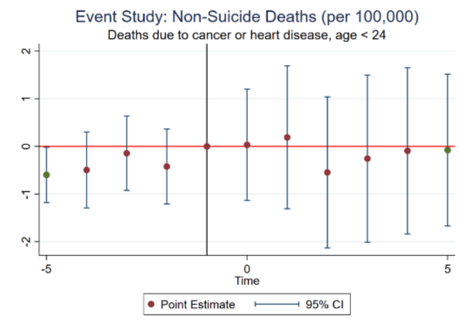
Standard errors in parentheses. All regressions weighted and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 7: Effect of Conversion Therapy Bans:
Leave-One-Out Analysis on Suicide Deaths per 100k age<24, Male

	(1) TWFE	(2) TWFE (Treatment Cohort 1 Dropped)	(3) TWFE (Treatment Cohort 2 Dropped)	(4) TWFE (Treatment Cohort 3 Dropped)	(5) TWFE (Treatment Cohort 4 Dropped)
Conversion Therapy Ban	-.7873202**	-.8597369*	-.7732241*	-.9244557**	-.872823**
	(0.3852917)	(0.3619338)	(0.4101149)	(0.4103481)	(0.4166467)
State FE	☑	☑	☑	☑	☑
Year FE	☑	☑	☑	☑	☑
State Controls from ACS	☑	☑	☑	☑	☑
LGBT Policy Controls	☑	☑	☑	☑	☑
Pre-Policy Mean	14.54	14.54	14.54	14.54	14.54

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Figure 2



Appendix Table 8: Effect of Conversion Therapy Bans: Non-Injury Deaths, Age<24

	(1) TWFE	(2) C&S Doubly Robust DD	(3) Gardner 2SDD	(4) Stacked DD
Conversion Therapy Ban	.1652108	-.0704945	.1836826	-.0582729
	(0.2463987)	(0.1998973)	(0.2247793)	(0.0392162)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
State Controls from ACS	☑		☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	4.87	4.87	4.87	4.87

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 9: Effect of Conversion Therapy Bans: Non-Firearm Suicide Deaths per 100,000 (age<24)

	(2) TWFE	(3) C&S Doubly Robust DD	(4) Gardner 2SDD	(6) Stacked DD
Conversion Therapy Ban	-.885726	-.3376604*	-.276946	-.207886*
	(0.799711)	(0.2011348)	(0.2918094)	(0.1238108)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
State Controls from ACS	☑		☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	3.63	3.63	3.63	3.63

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 10: Effect of Conversion Therapy Bans: Firearm Suicide Deaths per 100,000 (age<24)

	(2) TWFE	(3) C&S Doubly Robust DD	(4) Gardner 2SDD	(6) Stacked DD
Conversion Therapy Ban	-.106036*	-.0203685	-.0326	-.0499394
	(0.0533052)	(0.1175548)	(0.1892506)	(0.0926951)
State FE	☑	☑	☑	☑
Year FE	☑	☑	☑	☑
State Controls from ACS	☑		☑	☑
LGBT Policy Controls	☑	☑	☑	☑
Pre-Policy Mean	7.2	7.2	7.2	7.2

Standard errors in parentheses. All regressions clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix Table 11: Effect of Conversion Therapy Bans: Suicide Deaths per 100,000 (age<24)

	(1) Stacked DD	(2) Stacked DD Poisson
Conversion Therapy Ban	-.727900**	-.0488763**
	(0.3115808)	(0.0161183)
State FE	☑	☑
Year FE	☑	☑
Individual Controls from BRFSS	☑	☑
LGBT Policy Controls	☑	☑
Mean of Dependent Variable	14.54	14.54
Observations	1,036	1,036

Standard errors in parentheses. All regressions weighted and clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$