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Conversion therapy, suicidality, and running away: An analysis of transgender youth in the U.S.

Travis Campbell^{a,*}, Yana van der Meulen Rodgers^b

^a Southern Oregon University, United States

^b Rutgers University, United States

ABSTRACT

This study analyzes the relationship between conversion therapy and mental health and wellbeing of transgender youth in the U.S. We create a retrospective panel of transgender youth using the 2015 U.S. Transgender Survey to test how exposure to conversion therapy affects the likelihood of attempting suicide and running away from home. The empirical approach employs a difference-in-differences design. Results indicate that exposure to conversion therapy substantially increases the likelihood a transgender adolescent will attempt suicide and run away. The average treatment effect on treated (ATT) of conversion therapy on having attempted suicide is an increase of 17 percentage points, which amounts to a 55% increase in the risk of attempting suicide, and the ATT on the risk of running away is an increase of 7.8 percentage points, more than doubling the risk of running away. These effects are largest when exposure to conversion therapy occurs at a young age (11–14).

1. Introduction

Transgender individuals in the U.S. still face widespread stigma, discrimination, and violence (Winter et al., 2016; Carpenter et al., 2020). Problems of stigma and discrimination can lead to poor physical and mental health outcomes, and gender minority stress and gender dysphoria lie at the root of these inequities (Meyer, 2003; Turban et al., 2022). A major problem is the lack of family acceptance and support, which directly contributes to stress and dysphoria and also acts as a block against access to insurance and health care. In fact, Kundu et al. (2022) find that being insulted by one's parents or an adult during childhood ranks among the top three correlates of suicidal thoughts in the past year among a sample of sexual and gender minority young adults in Canada. Hostile parents, in turn, often encourage or coerce their transgender children into undergoing sexual orientation or gender identity change efforts, often referred to as "conversion therapy" or "conversion practices."¹ These efforts, usually led by counselors or religious advisors, aim to change an individual's sexual orientation or to discourage individuals, especially adolescents, from identifying as transgender or from expressing themselves in gender-diverse ways.

Most professional associations related to mental health and the health of young people have critiqued conversion therapy as being ineffective at best and harmful at worst, and 20 states have banned the practice (Mallory et al., 2019; MAP, 2022). However, there is very little quantitative evidence on the effects of conversion therapy on measures of mental health and wellbeing among trans and gender-diverse youth. Although quite a few studies across disciplines have been published on conversion therapy (e.g. James et al.,

* Corresponding author.

E-mail address: campbelt1@sou.edu (T. Campbell).

¹ This paper uses the term "conversion therapy" because it is somewhat more common in the literature. However, conversion therapy is not real therapy and it is not therapeutic. The American Psychological Association, the Endocrine Society, the American Academy of Pediatrics, the Substance Abuse and Mental Health Services Association, and the American Psychiatric Association have all formally endorsed gender-affirming care as the only acceptable approach to therapeutic care for transgender individuals (Bazelon, 2022).

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2016; Turban et al., 2020; Higbee et al., 2022), to our knowledge, none of them address important sources of selection into conversion therapy as explained below.

Our goal is to fill this gap by using data from the 2015 U.S. Transgender Survey (USTS) to examine the relationship between conversion therapy and the risk of attempting suicide and running away for transgender adolescents. Since unobservable characteristics such as socioeconomic status and upbringing may affect both conversion therapy exposure and measures of wellbeing, simple correlations that do not account for differences in characteristics between the treated and control groups prior to conversion therapy exposure may be misleading, and they cannot prove that young people who are subjected to conversion therapy are at a greater risk of poor wellbeing because of the therapy. For example, trans youth exposed to conversion therapy may have had other disadvantages (such as living in a hostile community or having lower socioeconomic status) that could explain both the conversion therapy participation and the poor wellbeing, or they may have had poor wellbeing that led their parents to consider conversion therapy. To identify the effect of conversion therapy on measures of wellbeing, we utilize a difference-in-differences (DID) approach. This approach allows us to estimate a substantive treatment effect of conversion therapy on the risk of attempting suicide and running away for transgender adolescents.

By comparing transgender adolescents who are exposed to conversion therapy (treated group) with those who are either never exposed or are exposed later on (control group), we find that conversion therapy increases the risk of attempting suicide by 17 percentage points, which amounts to a 55% increase in the risk of attempting suicide, and running away by 7.8 percentage points, more than doubling the risk of running away. The effects are largest when exposure to conversion therapy occurs at a young age (11–14). These estimates are compelling not only because they pass a battery of falsification and robustness tests, but also because the rate of attempting suicide and running away is remarkably similar in both level and trend between the treated and control groups over the five years preceding conversion therapy. Furthermore and most importantly, the estimates hold when we exploit only the precise timing of conversion therapy, limiting the control group to those who are exposed to conversion therapy one year after the treated group.

Our paper contributes to the literature on transgender well-being. Numerous studies have shown that transgender individuals are more likely to report poor general, physical, and mental health compared to their cisgender counterparts (Meyer et al., 2017; Cicero et al., 2020; Campbell and Rodgers, 2022). Mental health disparities are particularly stark, with considerably higher rates of depression, anxiety, PTSD, suicidality, and substance abuse in the U.S. transgender community (Haas et al., 2014; Marshall et al., 2016; Reisner et al., 2016; Downing, 2018; Lagos, 2018). Data from the USTS indicate that 40% of respondents had attempted suicide at least once in their lifetime, a rate that exceeds that of the general population by a factor of close to nine (James et al., 2016). Moreover, in a 2017 survey of U.S. high school students, 34.6% of the transgender students had attempted suicide compared to 5.5% of cisgender male students and 9.1% of cisgender female students (Johns et al., 2019). These kinds of mental health disparities also have repercussions for the labor market, with losses in human capital and labor productivity (e.g. Lerner and Henke 2008; Chatterji et al., 2011; Rivera et al., 2017; Dow et al., 2020).

For both indicators we examine, risk factors include depression, lack of family and community support, and victimization (Thompson, 2006). Although the magnitude of welfare losses seems at least an order of magnitude larger for suicide attempts compared to running away, we argue that running away is an important indicator of wellbeing because this action increases the subsequent risk of drug addiction, homelessness, sexual risk taking, dropping out of school, criminal activity, and additional depressive symptoms (Kaufman and Widom, 1999; Tucker et al., 2011; Rice et al., 2013; Aratani and Cooper 2015).

This study also offers timely evidence to inform the discussion of public policies relating to transgender health. Although a number of states have institutionalized bias against transgender individual (with, for example, provisions that exclude gender-affirming care from Medicaid coverage), several states have introduced such gender affirming policies as conversion therapy bans as well as gender marker laws that allow people to change their gender on official documents such as birth certificates and drivers licenses (Mann, 2021). These policy changes have placed transgender issues in the national spotlight and increased the need for empirical evidence on their possible welfare effects. However, relatively little is known about the effect of either affirming or exclusionary laws, given a relative dearth of data that allow the identification of transgender individuals. By using the U.S. Transgender Survey – the largest survey of transgender individuals of its kind – and recently-developed estimation techniques, this study aims to contribute new evidence on welfare outcomes associated with conversion therapy that should help to inform these policy discussions.

2. Background: conversion therapy

The underlying premise of conversion therapy – sometimes referred to as reparative therapy – is that lesbian, gay, bisexual, transgender, and queer (LGBTQ) individuals have a mental illness that needs to be cured, and the underlying objective is to change the sexual orientation or gender identity of that person. Efforts by health care practitioners and religious advisors to use conversion therapy to change someone's sexual orientation or gender identity date back to the 1890s and include a variety of techniques (Mallory et al., 2019). The most common technique now is talk therapy, but earlier forms of therapy based on pseudoscience included “aversive” techniques that entailed subjecting the individual to electrical shocks, nausea-inducing substances, paralysis, prompts to inflict self-harm, covert sensitization, and orgasmic reconditioning (Glassgold, 2009). Non-aversive techniques such as hypnosis and affection training were also used in an attempt to change thoughts and behaviors around sexuality and gender identity (Glassgold, 2009).

There is no credible peer-reviewed evidence showing that conversion therapy can actually change a person's sexual orientation or gender identity. In the case of sexual orientation, a systematic review of peer-reviewed articles about conversion therapy found 13 studies containing primary research. Twelve of those studies found that conversion therapy for sexual orientation was either ineffective or harmful, and the 13th study had enough problematic research design issues to render the claimed results implausible (What We Know Project, 2016). This conclusion about the ineffectiveness of conversion therapy in achieving its intended purpose builds on

earlier scholarship about the dubious ethics of conversion therapy as well as the potential for contributing to adverse outcomes such as anxiety and depression, suicidality, and social isolation (Halderman, 1994; Byne et al., 2012; Coleman et al., 2012; Wallace, 2013). In the case of gender identity, there are no peer-reviewed studies proving the efficacy of conversion therapy in achieving its intended goal, while two studies have shown a correlation between exposure to conversion therapy and adverse mental health indicators for transgender individuals (James et al., 2016; Turban et al., 2020).

Major medical associations have discredited conversion therapy. For example, in its 2018 policy statement on care for transgender and gender-diverse youth, the American Academy of Pediatrics wrote that conversion therapy is not only unsuccessful but also inappropriate, deleterious, unfair, and deceptive (Rafferty et al., 2018). Moreover, the Substance Abuse and Mental Health Services Association conducted a careful review of clinical guidelines and professional association reports and concluded that conversion therapy is “coercive, can be harmful, and should not be part of behavioral health treatment,” (SAMHSA 2015). Because advocates have deemed conversion therapy practices as harmful for LGBTQ minors, numerous states have passed legislation that prohibits licensed mental health professionals from subjecting LGBTQ minors to such practices. To date, 20 states plus the District of Columbia have complete bans on conversion therapy for minors (MAP, 2022). However, these bans are fairly recent, and religious providers are not covered by the bans. California was the first state to ban conversion therapy in 2012, followed by New Jersey and the District of Columbia within the next two years; most other states that do ban conversion therapy did not do so until 2017 and beyond. Another eight states have partial bans or legislation tied up in court, and 22 states have no laws on conversion therapy (MAP, 2022).

Transgender individuals make up a non-negligible part of the U.S. population. Among all individuals ages 13 and above, approximately 0.6% are transgender, which amounts to 1.6 million people (Herman et al., 2022). On average, the transgender population is younger than the overall U.S. population: of those individuals who identify as transgender, 18% are youths ages 13–17, while this age group constitutes less than 8% of the total U.S. population. These numbers have risen since 2017, the last time such estimates were generated, reflecting an increase in social acceptance among young people in being transgender, gender diverse, or non-binary (Herman et al., 2022).

Going to conversion therapy can influence mental health and wellbeing outcomes directly through the prolonging and intensification of gender dysphoria and gender minority stress since it is by construct a homophobic/transphobic institution in a hostile environment. It can also affect mental health and wellbeing indirectly through the denial of gender-affirming care. By implication, a transgender young person who has been placed into conversion therapy most likely is not receiving gender-affirming care, the latter being associated with improved health outcomes. A number of studies have found an association between access to gender-affirming care and improved health outcomes, including a lower incidence of suicide ideation and suicide attempts (e.g. Turban et al., 2022; Green et al., 2022). Mann et al. (2022) take this research one step further and find a positive effect of Medicaid coverage for gender-affirming care on measures of mental health for transgender beneficiaries.

Scholars and practitioners generally agree that support from family members is critical for preventing LGBTQ youth from experiencing negative outcomes (Ryan et al., 2009, 2010; Kosciw et al., 2020). Ridiculing a child, refusing to use their preferred name and pronoun, and preventing them from interacting with LGBTQ friends are forms of family rejection and trauma associated with poor mental health outcomes for LGBTQ youth. Requests for conversion therapy almost always come from parents and guardians rather than the trans youth themselves, making conversion therapy another form of family rejection (Glassgold, 2009). Adolescents do not have the legal authority to make their own medical decisions, so they often have no choice but to abide by their parents' demands to attend conversion therapy (Byne 2016). The Family Acceptance Project (2022:6) notes that sending LGBTQ children to mental health professionals and religious leaders to change their identity is usually perceived as “the most risk-inducing behavior that parents can engage in.”

3. Data description and methodology

3.1. United states transgender survey

Our analysis utilizes the 2015 wave of the U.S. Transgender Survey. The USTS – the largest survey of transgender people ever collected – has 27,715 respondents from all fifty states plus the District of Columbia, U.S. territories, and U.S. military bases abroad. The National Center for Transgender Equality conducted this survey of transgender adults (18 and older) in the summer of 2015 as a follow-up to their inaugural 2008–09 National Transgender Discrimination Survey (NTDS). It documents the lives and experiences of trans individuals, with detailed information on a range of indicators, including education, employment, race, family life, health status, marital status, and access to healthcare. As such, the USTS and NTDS have been used extensively to analyze the outcomes of transgender individuals (e.g. Bakko, 2020; Budge et al., 2016; Goldenberg et al., 2020).

We used two measures of mental health and wellbeing from the USTS for the empirical analysis: suicide attempts and running away. In the USTS, suicide attempts and running away are both self-reported, and for each outcome we code respondents who do not know the answer or refuse to respond as missing. For suicide attempts, respondents are asked “At any time in your life, did you try to kill yourself?” If the answer is yes, respondents are also asked about the number of attempts as well as the age when the first attempt was made. For running away from home, respondents are asked “Did you ever run away from home because you are trans?” If the answer is yes, respondents are also asked the age when they ran away. For the key independent variable to measure exposure to conversion therapy, respondents are asked “Did any professional (such as a psychologist, counselor, religious advisor) try to make you identify only with your sex assigned at birth (in other words, try to stop you being trans)?” If the answer is yes, respondents are also asked the age at which this first happened.

Our estimations include four indicators for social transitioning: ever feeling one's gender was different, ever thinking of oneself as

transgender, ever telling another that they are transgender, and ever living full-time as the gender of their gender identity. The following survey questions are used to measure these indicators: (1) “At about what age did you begin to feel that your gender was “different” from your assigned birth sex?” (2) “At about what age did you start to think you were trans (even if you did not know the word for it)?” (3) “At about what age did you first start to tell others that you were trans (even if you did not use that word)?” (4) “How old were you when you started to live full-time in a gender that is different from the one assigned to you at birth?” Because we know the ages when the respondents first experienced these events, we are able to construct cohort-specific variables for these social transitioning indicators by coding them as equal to one if the respondent first experienced each event by a particular year, and zero otherwise.

The USTS is an online survey that allows respondents to self-identify as transgender, so the survey includes people at various stages of transition, and it allows for a broad interpretation of trans – including transgender, trans, genderqueer, and non-binary identities (Shannon 2022). Although it is not a random sample, the USTS does contain vital information on mental health, wellbeing, and experience with conversion therapy that is not available elsewhere, including the Behavioral Risk Factor Surveillance System (BRFSS). While the BRFSS does include information about gender identity as well as self-reported health status, it does not have questions on conversion therapy, suicide attempts, or running away. A comparison of key demographic indicators in the USTS with the sample of transgender adults in the BRFSS indicates that the USTS sample is younger, more white, more educated, more likely to report good health, and more likely to have a job and an income that exceeds \$50,000 (Shannon 2022). These demographic differences indicate that treatment effect estimates using the USTS, even if internally valid, may not reflect the overall transgender population if the treatment effects are heterogeneous. We do not find evidence of heterogeneity by pre-exposure demographics (the estimates are notably precise), providing some assurance.

3.2. Data limitations

The USTS was not collected with a randomized sampling strategy, and it was only distributed online. To correct for discrepancies in the demographic composition of its sample, and to offset possible internet survey bias and other potential problems with survey access, the USTS team engaged in extensive outreach efforts to recruit respondents. These efforts included working with a network of advocacy groups and organizations to share information about the survey, holding survey-taking events, and offering cash-prize incentives (James et al., 2016). These efforts could also help to mitigate some of the possible undercounting of homeless transgender individuals, which matters for our study since we examine running away from home, a predictor of homelessness. This point is supported with data from the National Longitudinal Survey of Youth (NLSY), which is also subject to the same data limitation. Brakenhoff et al. (2015) find that 18% of NLSY respondents had run away from home before turning 17; 40% of those individuals had done so at least twice, and multiple runaway experiences more than doubled the odds of homelessness compared to those who never ran away.

Another limitation of the survey is that the question we use to measure exposure to conversion therapy does not explicitly use the term “conversion” or “therapy”, which could reflect efforts by the USTS team to use accessible language based on lived experiences (ICPSR 2015). A strength of the wording of this question is that it specifically asks whether a professional tried to stop the respondent from being trans, which helps to weed out other kinds of conversion efforts attempted by non-professional practitioners (Maccio 2010). However, a potential limitation of the wording is that it is broad enough to encompass counseling and techniques that may not be labeled as conversion therapy but still have the same intent. For example, someone who experienced a school counselor trying to police gender could answer yes to this question even though that respondent did not receive therapy per se.

Evidence from the Trevor Project (2019) indicates that this wording matters: in their survey of LGBTQ youth, two-thirds of respondents had experienced “someone” attempting to convince them to change their sexual orientation or gender identity, while this share dropped to 5% when the wording of the question was changed to specifically ask if they had experienced conversion therapy. In comparison, 13% of the USTS sample answered yes to the conversion therapy question, which suggests that the USTS question is reasonably close to capturing experiences with conversion therapy, even though it does not identify whether a therapeutic service was provided or what kind of a professional provided the conversion therapy. To help give further validity to using this question in the USTS, we examine the share of USTS respondents who report exposure to conversion therapy across regions. As shown in Online Appendix Figure A.1, which shows the share of respondents who had conversion therapy at some point in their lives by state of birth, conversion therapy is relatively common in the south and in rural western states such as Utah and Idaho. Conversion therapy is relatively uncommon in democratic-leaning areas such as the western coast and the northeastern US. These results are fairly intuitive and lend support to our assertion that this question yields a valid measure of exposure to conversion therapy.

There are four additional sampling issues. First, we cannot observe people for whom conversion therapy was successful. This should not be a problem for our estimates given the known low efficacy of conversion therapy in the case of sexual orientation (What We Know Project, 2016). Second, we cannot observe people who actually died by suicide. If conversion therapy increases the incidence and lethality of suicide attempts, then people who were most adversely affected by conversion therapy will die, selecting out of the sample. This selection on mortality would then disproportionately decrease the share of respondents who both had conversion therapy and attempted suicide afterwards, attenuating the estimates. The selection on mortality may be more severe for older cohorts, given the higher likelihood of prior attempts during adolescence, some of which resulted in mortality. This is demonstrated by the age profile of first suicide attempts in our data, and is confirmed with evidence in Salway et al. (2021). This problem of mortality selection implies that if anything, our results likely underestimate the effect of conversion therapy on having attempted suicide.

Third, the retrospective variables we use may be subject to recall bias– the (in)ability of respondents to accurately report their significant life events retrospectively. We can gage the presence of recall bias by testing for heaping in the distribution of the age and

implied calendar year when the events occurred. Results in Online Appendix B indicate that heaping by age is an issue, but it can be mitigated by looking only at recent life events. The only variable for which recall bias does not appear to be an issue based on the heaping analysis is first suicide attempt, our primary outcome variable. One could explain this with the argument that first suicide attempts are such a traumatic life event that respondents are able to recall their age at the time accurately, even if the event took place many years prior. The lack of heaping in the age distribution for suicide attempts also helps to dispel concerns that instead of a first suicide attempt, people are reporting the age at which they first began to be severely depressed. Our argument that recall bias is not an issue in the case of first suicide attempt is also supported with findings in [Beautrais et al. \(1997\)](#), a study that used parallel reports from subjects and significant others on precipitating factors and suicide attempts and found no evidence of recall bias.

A final limitation around the sampling is that the order of events is unclear when they occur at the same age (that is, the age at which a respondent said they first attempted suicide may be the same age at which the respondent was first exposed to conversion therapy. It is plausible that some parents sent their children to therapists or religious counselors as a result of a suicide attempt whether or not they knew that the professional would try to make their child identify only with their sex assigned at birth. Of the 27,456 respondents of the USTS who have non-missing data for both age of first suicide attempt and age when starting conversion therapy, 295 had first attempted suicide the same age at which they started conversion therapy. This is just 1% of the sample, so we are not too concerned about this issue. That said, recall bias could make this issue more problematic if, for example, those who cannot recall the exact timing of these events just record them as the same year in the survey. We thus acknowledge this imprecise timing of the life events surveyed as a data limitation beyond our control but one that should not be large enough to meaningfully affect our estimates.

3.3. Empirical methodology

Isolating the effect of conversion therapy from confounding factors is a difficult task, largely because the reasons for undergoing conversion therapy may also relate to a transgender person's risk of attempting suicide or running away. In particular, a transgender person going to conversion therapy may be predisposed toward attempting suicide when there are risk factors, especially a hostile family environment, that push the person into conversion therapy and also contribute to psychological distress. Although the USTS does include questions about how supportive or hostile the family is now and was in the past, these questions do not specify at what age the support or hostility began. This makes it difficult to separately identify the effect of a hostile family environment from the conversion therapy effect. Moreover, not everyone who enters conversion therapy does so because they have unsupportive parents. Among all USTS respondents who talked about their gender identity with a professional, 18% said that the professional attempted to stop them from being transgender, irrespective of the family environment ([James et al., 2016](#)).

If our assertion about risk factors is true, then people who undergo conversion therapy may be more prone to attempting suicide or running away than those who do not. This assertion is confirmed by [Figs. 1a and 1b](#), which report unweighted binned scatters of having attempted suicide or having run away by exposure to hormone therapy over time, where the x axis represents years before and after the start of conversion therapy (which is initiated in year 0). The figures also show the linear regression lines with a discontinuity in year -1 (one year prior to conversion therapy), and the figures compare those who were born in the same calendar year. [Figs. 1a and 1b](#) show that transgender youth who were exposed to conversion therapy were more likely to attempt suicide or run away than those who were not exposed, before the therapy had even begun. To make matters worse, leading up to their exposure, this difference was *growing*, nullifying any simple comparisons of suicidality by exposure to conversion therapy.

We tackle this selection issue by exploiting variation in the assignment and timing of conversion therapy across the lives of transgender youth in the U.S. As shown in [Figs. 2a and 2b](#), the age at which transgender individuals were first treated with conversion therapy and the calendar year in which the conversion therapy occurred both have considerable variation. We use a stacked DID design to exploit this variation. There are three main advantages to the stacked approach. First, stacking eliminates bias from using already-treated units as controls when the timing of exposure is staggered ([Goodman-Bacon, 2021](#)). Second, the stacked estimator is efficient and computationally inexpensive relative to alternatives ([Gardner, 2021](#)). Third, stacking allows researchers to construct credible control groups for each cohort individually using either theory or data-driven methods such as a synthetic DID approach, thereby reducing bias when exposure is not randomly assigned ([Arkhangelsky et al., 2021](#)). This third characteristic is particularly useful as synthetic DID weights eliminate the differential trends pre-exposure in attempting suicide or running away ([Figs. 1c and 1d](#)).

To ensure that proper comparisons are drawn, our final sample is a 'stack' of cohorts. A cohort is defined as one of every combination of age and calendar year of first exposure. Each cohort includes a treated group: all respondents who 1) report being the same age when exposed to conversion therapy, and 2) were also born during the same calendar year. Each cohort also includes a control group: all respondents who were 1) born during the same calendar year as the treated group, and 2) exposed after the event window or never exposed during the period of analysis.² Our baseline estimates focus on first exposures between ages 11 and 17, investigating the five years before and five years after. Cohorts with less than fifty control units are dropped, as are cohorts observed less than five years post-exposure, ensuring that composition changes will not create misleading dynamic treatment effects. Within each cohort, time is aligned with exposure, so event-time zero always denotes the first year of conversion therapy. Depending on the outcome, our final sample includes up to 247 cohorts, comprised of 1078 treated individuals and 24,192 controls, totaling approximately one million observations (again, individuals may appear in multiple cohorts).

This approach ensures that within each cohort, our synthetic DID estimates are robust to both misspecification ([Arkhangelsky et al.,](#)

² To be clear, individuals who meet these conditions for multiple cohorts are included as controls multiple times, and the same individual may appear as a control or as treated in different cohorts. Unlike the control group, an individual can appear as a treated unit only once by design.

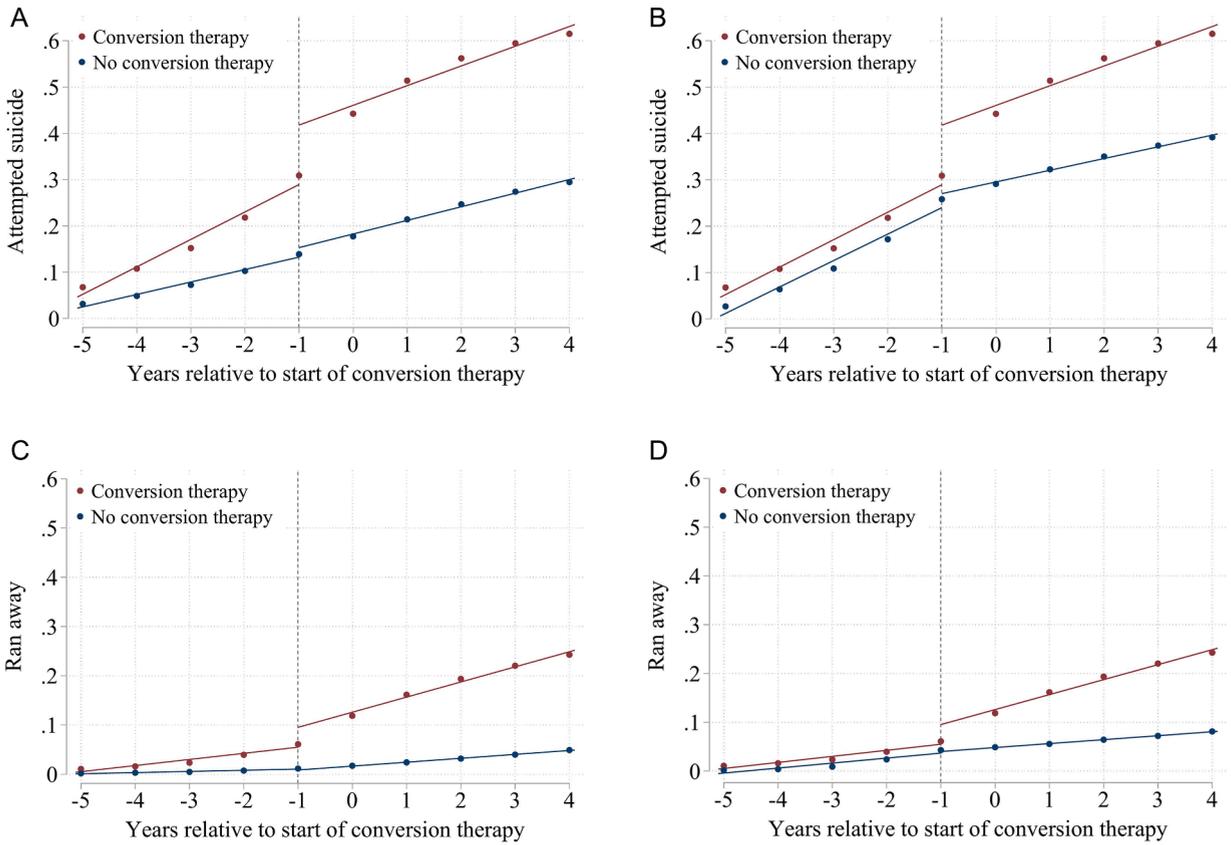


Fig. 1. Binned Scatter Plots of Attempting Suicide or Running Away by Treatment Status. *Notes:* Figs. 1a and 1b report unweighted binned scatters of attempted suicide or ran away by exposure to hormone therapy. Figs. 1c and 1d apply synthetic unit weights to the control group. All figures also show the linear regression lines with a discontinuity one year prior to conversion therapy. The control group includes both people who never report having conversion therapy and people who are exposed to conversion therapy after the five-year event window. The binned averages are found by averaging the outcome by treatment status and event time using our baseline stacked sample.

2021) and to bias caused by using already-treated units as controls (Goodman-Bacon, 2021). We efficiently aggregate the within-cohort estimates using a stacked DID approach with dynamic treatment effects.³ As is standard, we test for selection bias by allowing the trends in the outcome to deviate between the treated and control individuals for five years prior to conversion therapy.⁴ The baseline specification follows:

$$Y_{c,i,t} = \mu + \sum_{k=-5}^4 \beta_k D_{k,c,i,t} + X'_{c,i,t} \gamma + \alpha_{c,i} + \delta_{c,t} + \epsilon_{c,i,t} \tag{1}$$

where Y denotes an indicator for person i of cohort c for having attempted suicide or run away as of event-time t , D_k are leads and lags of an indicator variable for the year of conversion therapy exposure, X is a vector of the four cohort-specific controls for socially transitioning, $\alpha_{c,i}$ are cohort-specific individual fixed effects, $\delta_{c,t}$ are cohort-specific time fixed effects, and $\epsilon_{c,i,t}$ is the error term. The standard errors are clustered by individual, the level at which the conversion therapy occurs. Note that if we were to cluster by cohort-unit, the stacked difference-in-difference estimates would over-reject the null hypothesis due to inflating the sample size and not accounting for duplication. Hence to account for the sample size change from stacking, the robust standard errors are clustered by individual, not cohort-individual. This clustering strategy is now standard for the stacked procedure.

Because the main identifying assumption is parallel trends in the outcome and our data indicate that we lack parallel trends, we apply Arkhangelsky et al. (2021)'s synthetic unit weights to the control group. This strategy balances the trends in the outcome

³ We also provide estimates that manually aggregate the cohort-specific estimates, weighting by the sample share. The stacked estimates are slightly conservative.

⁴ For the overall effect of the conversion therapy, we estimate a separate stacked synthetic DID regression with a single treatment indicator, applying the product of the synthetic unit and synthetic time weights to further reduce bias (Arkhangelsky et al., 2021). These weights are referred to as synthetic DID weights.

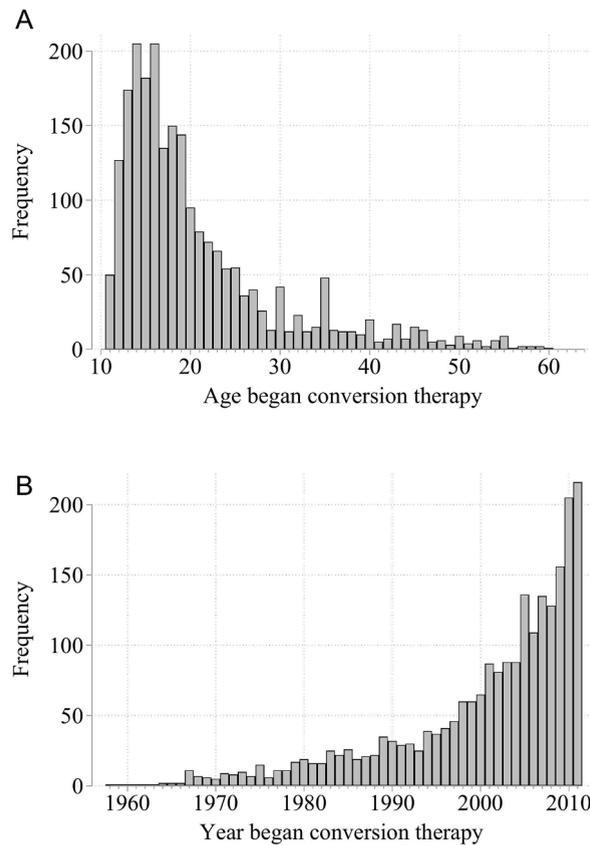


Fig. 2. Distribution of the Age and Year of First Exposure to Conversion Therapy. *Notes:* The figures depict variation in the timing of conversion therapy in our baseline analysis sample. Fig. 2a is a histogram depicting the distribution of the age each respondent reports being when they were first treated with conversion therapy. Fig. 2b is a histogram depicting the distribution of the calendar year of conversion therapy treatment. Respondents who were never treated with conversion therapy are omitted, as are those treated before age 11. The data for conversion therapy comes from the following question: How old were you the first time a professional tried to make you identify only with your sex assigned at birth (in other words, try to stop you being trans)?.

between treated and control individuals within each cohort. In other words, we re-weight the control individuals in each cohort so that their pre-exposure trends match the treated group. To do so, the procedure designs the unit weights in such a way that the treated units have an average outcome that is more or less parallel with the weighted average of the control units before starting conversion therapy.

One may be concerned that the above DID estimates make a suspect comparison even after reweighting the controls. Indeed, there may be fundamental differences in trends between transgender youths who undergo conversion therapy and those who do not, or those who do so five or more years later. To address this concern, we repeat this analysis with three important changes: one, we reduce the post-exposure window to one year rather than five years; two, we restrict the control group to individuals who are exposed to conversion therapy when one year older than the treated group; and three, to have enough control units, we allow individuals within cohorts to be born in different calendar years.⁵ If people who are exposed to conversion therapy at similar points in their lives are otherwise similar, then we can credibly estimate the immediate effect of conversion therapy—although the downsides are substantial. We can only estimate the effect after one year, and our sample size is severely limited. We refer to this second analysis as an *event study* since all individuals in the sample are exposed to conversion therapy at some point. By limiting the control group to those who successively undergo conversion therapy, the event study approach can estimate the immediate effect of conversion therapy using only variation in the precise timing of exposure, which is plausibly exogenous.

⁵ For the event study sample, a cohort is defined by the age when individuals were first exposed to conversion therapy. So, if the treated group underwent conversion therapy at age 15, then the control group underwent conversion therapy at age 16, regardless of calendar year of birth. The event time is aligned with the age of exposure in each cohort. Since people of the same age may have been born in different calendar years, cohort-calendar year fixed effects are added to the regression specification.

3.4. Covariate balance

Transgender youth exposed to conversion therapy are different than those who are not, as shown in [Table 1](#). The first two columns display the average pretreatment means by treatment status for the DID sample: youth who underwent conversion therapy (treated) and youth who did not (control). After reporting the difference between the treated and control groups, the next two columns display the average pretreatment means by treatment status for the one-year event study: people who were exposed to conversion therapy during the event window (treated) and people who were exposed to conversion therapy when one year older (control). To provide a thorough look at the covariate balance, we include the four social transitioning variables, several demographic indicators (race, sex assigned at birth, and region of birth), and several indicators that signal family support and access to resources (court name change, puberty blockers, hormone therapy, and surgical therapy).

[Table 1](#) shows that transgender youth who are exposed to conversion therapy are less likely to be white, assigned male at birth, or born in the Midwest, and they are also more likely to have been born in the South. These differences also hold up for the smaller event study sample but they are not statistically significant. Although there are virtually no differences between the treated and control groups in the indicators of parental support and access to resources, we do observe for both the DID sample and the event study sample that transgender youth who are exposed to conversion therapy are more likely to have socially transitioned (felt their gender was different, thought of themselves as transgender, told another person they were transgender, or lived full-time as their identified gender). This imbalance is perhaps intuitive: knowledge of transgender status is a necessary condition for conversion therapy administration, implying that youth who have socially transitioned may be selected into conversion therapy. To help mitigate selection bias, our baseline regression specification includes the four control variables for social transitioning. We also tackle the issue of drawing comparisons between individuals at similar stages in transitioning with inverse probability weights. The inverse probability weights reweight the control group for each cohort so that the control group is on average at a similar stage in their transition as the treated group is, on average.

The one-year event study approach further addresses this selection by exploiting variation in the timing of exposure to conversion therapy, which, on a small enough scale, is likely as-good-as-random. The event study sample restricts the control groups to only transgender youth who are exposed to conversion therapy when one year older than the treated groups. This restriction eliminates the demographic and regional imbalance and should mitigate most of the social transitioning differences.

4. Five-year effect of conversion therapy: a DID approach

The main results from the DID approach indicate that conversion therapy has deleterious effects on transgender youth. In particular, our DID estimates associate conversion therapy with a 17.0 (s.e.=1.0) percentage point increase in having attempted suicide and a 7.8 (s.e.=0.7) percentage point increase in having run away over the five years following first exposure ([Table 2](#)). Relative to the respective pretreatment means of 30.9 and 6.1, these suggest a remarkable increase of 55% in having attempted suicide and 128% in having run away. The estimates are precise, ruling out effects below an increase of 15 percentage points in having attempted suicide and 6 percentage points in having run away at 95% confidence.

The detrimental effects of conversion therapy surface immediately after exposure and accumulate with time ([Fig. 3](#)), which speaks to the intensification of gender dysphoria and gender minority stress when exposed to a hostile environment. For the five years preceding conversion therapy, the trend in having attempted suicide or run away for those exposed is indistinguishable from those who are not. During the first year of conversion therapy, the difference in having attempted suicide rises by approximately 12 percentage points and in having run away by about 4 percentage points. After five years, the difference in having attempted suicide rises by approximately 20 percentage points and having run away by about 12 percentage points. These numbers are striking and go a long way to challenge the views of healthcare practitioners, spiritual guides, and conservative politicians who continue to offer conversion therapy or block conversion therapy bans. This practice is clearly harmful, particularly for young people, and our results imply long-run rather than transitory effects.

4.1. Heterogeneous effects of conversion therapy

We test for heterogeneity in the effect of conversion therapy on attempting suicide or running away by the cohort's age during their first exposure. These estimates provide a falsification test: people who undergo conversion therapy as adults, rather than adolescents, arguably have more agency in the decision. As a consequence, conversion therapy's effect should diminish with adulthood, but not necessarily disappear. Our estimates pass the falsification test: conversion therapy has a much smaller effect on attempting suicide for adults aged 21 or older and does not have any effect on running away ([Fig. 4](#)). As might be expected, the effect on running away is very small when exposure to conversion therapy started at ages 19–20 and ages 21–22, and then disappears completely due to top-coding running away at age 21, which is discussed in [Appendix B](#). In contrast, the effect of conversion therapy is striking for earlier exposures (ages 11 and 12), exceeding a 20-percentage point increase in having attempted suicide. Note that the association of conversion therapy with a first suicide attempt during adulthood may be attenuated because the majority of first suicide attempts among transgender people occur during adolescence. Closely related, the adverse effect of conversion therapy on the extensive margin is the largest at age 11 and 12 (due to fewer 11 or 12-year-olds having previous suicide attempts) even if the overall effect of conversion therapy (increase in the number of suicide attempts) is comparable or even greater in individuals first exposed to conversion therapy at higher ages due to the intensive margin becoming more important for older individuals.

We do not find any evidence of heterogeneity by pretreatment demographics such as race, sex assigned at birth, or region of birth

Table 1
Covariate Balance Between the Treated and Control Groups Prior to Conversion Therapy.

	Difference-in-differences sample			Event study sample		
	Treated	Control	Difference	Treated	Control	Difference
Felt gender different	.850	.711	.139*** (0.010)	.836	.796	.040*** (0.005)
Thought transgender	.537	.363	.174*** (0.014)	.530	.463	.067*** (0.006)
Told others trans	.192	.079	.113*** (0.011)	.188	.146	.042*** (0.005)
Full time as identity	.026	.012	.014** (0.004)	.033	.026	.007** (0.002)
Court name change	.000	.000	-.000 (0.000)	.000	.000	.000 (0.000)
Had puberty blockers	.001	.000	.000 (0.000)	.001	.000	.001 (0.001)
Had hormone therapy	.001	.001	-.000 (0.000)	.001	.000	.001* (0.000)
Had surgical therapy	.002	.000	.001 (0.001)	.002	.001	.001 (0.001)
White	.792	.824	-.032* (0.03)	.783	.786	-.003 (0.006)
Assigned male at birth	.392	.545	-.152*** (0.016)	.359	.369	-.010 (0.007)
<i>Region of birth</i>						
Northeast	.220	.224	-.004 (0.014)	.220	.222	-.002 (0.006)
Midwest	.200	.250	-.049*** (0.014)	.205	.215	-.010 (0.006)
South	.276	.237	.039** (0.014)	.281	.274	.007 (0.006)
West	.253	.237	.016 (0.014)	.242	.236	.006 (0.006)
Other	.000	.000	.000 (0.000)	.000	.000	.000 (0.000)
F _{15, 1544}			27.6***			12.9***
People	1078	24,141		1313	1495	

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table displays average pretreatment means by treatment status: youth who underwent conversion therapy (treated) and youth who did not (control). Robust standard errors are clustered by individual and reported in parenthesis. The row F statistics reports the joint F-test for the difference in means. The control units are weighted by the synthetic unit weights.

Table 2
DID Estimates for the Average Five-Year Effect of Conversion Therapy on Attempting Suicide or Running Away for Transgender Youth.

Outcome:	(1)	(2)
	Attempted suicide	Ran away
Effect of conversion therapy	.170*** (0.010)	.078*** (0.007)
Pre-treatment average outcome	.309	.061
Treated individuals	1078	935
Control individuals	24,192	17,921
Number of cohorts	247	231
Observations	1,044,120	709,177
Cohort-individual fixed effects	✓	✓
Cohort-time fixed effects	✓	✓
Social transition controls	✓	✓

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the baseline DID estimates for exposure to conversion therapy. A separate cohort is defined for every combination of age and calendar year of first exposure. The event window includes five years before and after each cohort's exposure. For each cohort, the control group includes all individuals who are 1) born during the same calendar year as the treated group, and 2) exposed after the event window or never exposed during the sample. Cohorts with less than fifty control units are dropped. All regressions include cohort-individual and cohort-time fixed effects as well as cohort-specific controls for socially transitioning and are weighted by synthetic unit weights. Robust standard errors are clustered by individual and reported in parenthesis.

(Fig. 5), which shows that conversion therapy is equally bad for everyone and if anything, strengthens our results. A similar analysis also shows there is no evidence of heterogeneity by the calendar year of first exposure to conversion therapy (Online Appendix Figure A.2).

5. The effect of conversion therapy after one year, an event study approach

As discussed earlier, requests for conversion therapy almost always come from parents and guardians rather than young people themselves. Moreover, adolescents do not have the legal authority to make their own medical decisions, so they often have no choice but to abide by their parents' demands to attend conversion therapy (Byne 2016). Data from the USTS indicate that 14% of transgender respondents who had come out to their families were forced to go to conversion therapy (James et al., 2016). Because conversion therapy is often administered in the context of transgender children coming out to unsupportive parents, there are fundamental differences between adolescents exposed to conversion therapy and people who are not exposed, or are exposed at a much later age. This would imply that our DID estimates may be capturing both the effects of conversion therapy as well as the effects of living in a hostile family environment. Our event study estimates address this concern by comparing adolescents who undergo conversion therapy with those who undergo conversion therapy one year later, with the assumption that the exact timing of conversion therapy is

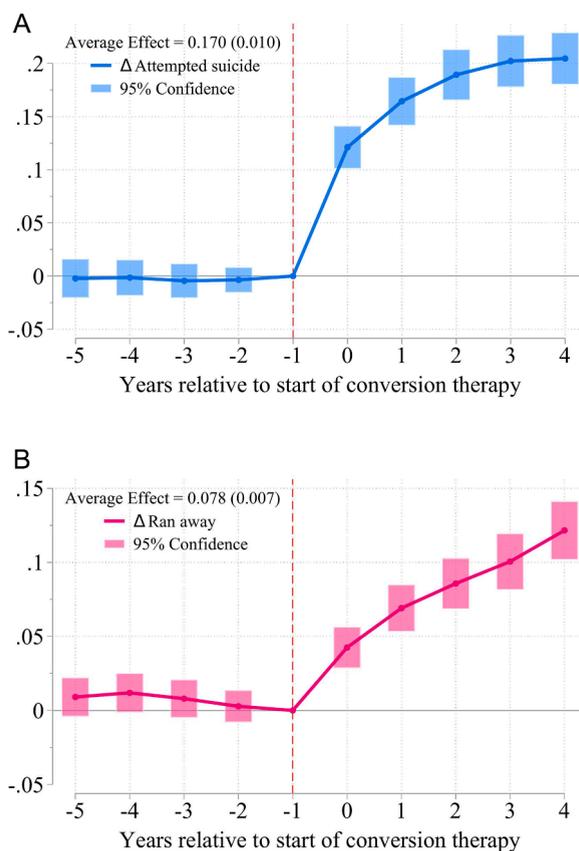


Fig. 3. Evolution of the Effect of Conversion Therapy on the Risk of Attempting Suicide and Running Away for Transgender Youth. *Notes:* The figures show the estimates of the stacked DID model given by Eq. (1). The outcome used in Fig. 3a is a dummy variable for attempting suicide (zero before the age of first attempting suicide and, if ever, 1 after) and Fig. 3b is dummy variable for running away (zero before the age of running away and, if ever, 1 after). All specifications include cohort-individual and cohort-time fixed effects as well as cohort-specific controls for socially transitioning and are weighted by synthetic unit weights. The shaded area in each figure is the 95% confidence interval based on robust standard errors clustered by individual.

as-good-as-random. Hence the covariates of the adolescents we are comparing are more similar in this event study compared to the DID approach since they have all undergone conversion therapy and have done so within a relatively small window of time. The downsides to this approach are that we can only estimate the effect of conversion therapy one-year post treatment, and the sample size is limited.

Conversion therapy is again found to be detrimental to the mental health and wellbeing of transgender youth (Table 3 and Fig. 6). For the five years preceding conversion therapy, the trend in having attempted suicide or run away between the treated and controls groups are indistinguishable. There is an abrupt change following conversion therapy. During the first year of conversion therapy, the difference in having attempted suicide rises by 4.9 percentage points and having run away by 2.8 percentage points relative to transgender adolescents who will be exposed when one year older. Relative to the pretreatment means of 35.6 and 5.9 respectively, we associate conversion therapy with an immediate 13.8% increase in attempting suicide and a 47.5% increase in running away.

The event study estimates are around half the size of the first year of the dynamic DID estimates that we had reported in Fig. 3, thus indicating that the DID estimates were indeed capturing some effects of socially transitioning in a hostile family environment. That said, the DID estimates still provide compelling evidence for the importance of supportive environments for the mental health and wellbeing of transgender youth. They also suggest that the effect of conversion therapy not only persists, but also grows with time; an insight we cannot assess with our event study approach.

6. Robustness checks

We subject the DID and the event study results to a battery of robustness tests. The estimates hold using alternative regression specifications, such as the controlling for legal name changes, medical transitioning (puberty blockers, hormone therapy, and surgical therapy); they hold without using synthetic DID weights; and they hold with cohort-individual-specific linear time trends (Online Appendix Table A.1 and Table A.2). We also address the issue of drawing comparisons between individuals at similar stages in transitioning with inverse probability weights. The inverse probability weights reweight the control group for each cohort so that the control group is on average at a similar stage in their transition as the treated group is, on average. This claim is evidenced by

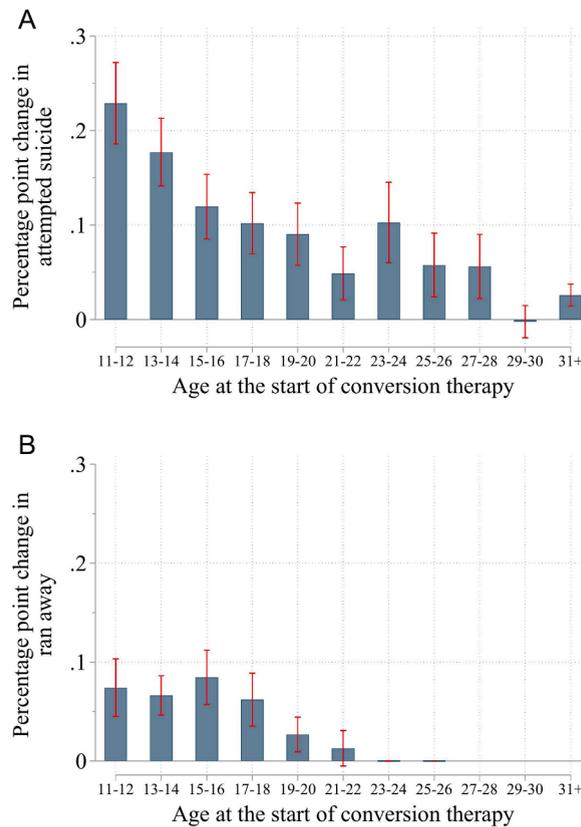


Fig. 4. Heterogeneity by Age at Exposure in the Effect of Conversion Therapy on the Risk of Attempting Suicide and Running Away for Transgender People. *Notes:* This figure depicts the baseline DID estimate for the overall effect of conversion therapy on the probability of attempting suicide or running away, subsetting by the cohort’s age when the treated group was first exposed to conversion therapy. The y-axis is the percentage point change in the outcome, and the x-axis is the subset used for the estimate. The blue bars in each figure depict the group specific estimate and the red bands depict 95% confidence intervals based on robust standard errors clustered at the individual level. All regressions include cohort-individual and cohort-time fixed effects as well as cohort-specific controls for socially transitioning, and are weighted by synthetic unit weights.

Appendix Figures A.3 – A.10, which show these weights successfully balance the observable characteristics; after applying the weights, the treated group and control group are at similar stages for all respective variables in the survey. Appendix Table A.1 and Appendix Table A.2 show that both the DID and event study estimates do not meaningfully change when using these weights.

The DID estimates are also robust to alternative choices made when constructing the stacked sample, such as the number of post-treatment years (Online Appendix Table A.3) or the minimum number of control units required for a cohort to be included in the sample (Online Appendix Table A.4). Most notably, our sample drops individuals who started conversion therapy before age 11. To test the robustness of using a different starting age, we conducted robustness checks that use alternative age thresholds (ages 9, 10, 11, 12, and 13) for the minimum age at which the treated group of each cohort started conversion therapy. Results for the DID estimates are found in Appendix Table A.5 and for the event study estimates in Appendix Table A.6. These alternative starting points make very little difference, likely because there are so few treated observations in these younger cohorts.

The stacked DID estimator may be inconsistent for the sample average treatment effect on the treated (ATT) when there is heterogeneity in treatment effects across cohorts or time, caused by weighting by both the variance of treatment and sample share within each cohort, rather than by only the sample share (Baker et al., 2022; Gardner, 2021). When we manually aggregate the cohort-specific estimates, weighting by only the sample share, both the estimates and standard errors are virtually unchanged, alleviating any such concerns (Online Appendix Table A.7 and Table A.8).

The histograms provided in Appendix B show that heaping by age is an issue, but it can be mitigated by looking only at recent life events, indicating that recall bias may be less concerning when events are recent. We hence test the robustness of our estimates to incrementally omitting cohorts by the number of years before the survey that they first had conversion therapy (Appendix Table A9 and A10). Our difference-in-difference estimates and event study estimates hold at all thresholds, even if we only consider cohorts that experienced conversion therapy five years before the survey, which is the strictest cutoff possible given our five-year event window, providing some assurance about recall bias affecting the results. However, the difference-in-differences estimate for effect on running away does become statistically insignificant at the strictest threshold due to a small sample size, as does the event study estimate for attempting suicide.

In another robustness check, we attempt to disentangle the effect of conversion therapy from a hostile family environment. To do

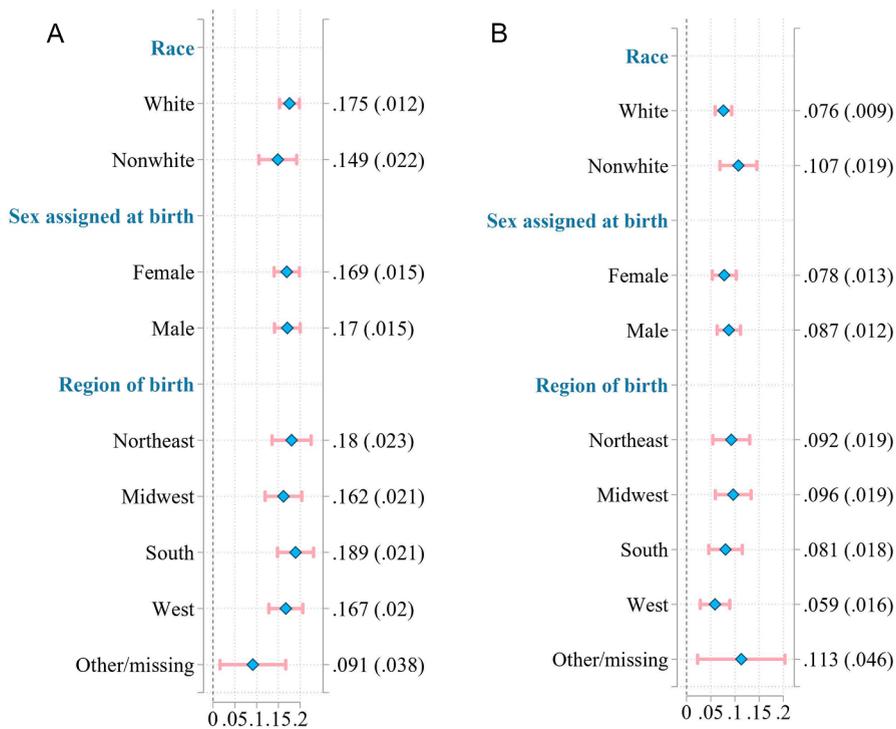


Fig. 5. Heterogeneity by Pre-Treatment Demographics in the Effect of Conversion Therapy on the Risk of Attempting Suicide and Running Away for Transgender Youth. *Notes:* This figure depicts the baseline DID estimate for the overall effect of conversion therapy on the probability of attempting suicide or running away, interacting the treatment indicator with pre-treatment demographics. The blue diamonds are the point estimates. The pink bars are the 95% confidence interval based on standard errors that are clustered by individual. Synthetic unit weights are applied. Point estimates and standard errors, in parentheses, are reported on the second y-axis. All regressions include cohort-individual and cohort-time fixed effects and include cohort-specific controls for socially transitioning.

Table 3

Event Study Estimates for the Average One-Year Effect of Conversion Therapy on Attempting Suicide or Running Away for Transgender Youth.

Outcome:	(1) Attempted suicide	(2) Ran away
Effect of conversion therapy	.049*** (0.014)	.028** (0.009)
Pre-treatment average outcome	.356	.059
Treated individuals	1313	1133
Control individuals	1495	1289
Number of cohorts	7	7
Observations	11,434	9064
Cohort-individual fixed effects	✓	✓
Cohort-time fixed effects	✓	✓
Social transition controls	✓	✓

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. This table reports the baseline event study estimates for exposure to conversion therapy. A separate cohort is defined for every age of first exposure. The event window includes five years prior to each cohort’s exposure, and one year after. For each cohort, the control group includes respondents who report being exposed to conversion therapy one year after the treated group. All regressions include cohort-individual fixed effects, cohort-age fixed effects, cohort-calendar year fixed effects, cohort-specific controls for socially transitioning, and are weighted by synthetic unit weights. Robust standard errors are clustered by individual and reported in parenthesis.

so, we estimate our baseline difference-in-difference specification using only respondents who report either having a supportive family environment, or do not report any form of family rejection for being trans, or both, in Appendix Table A.11. We also do this for our event study in Appendix Table A.12. This procedure is especially important for the difference-in-difference estimates because they primarily compare respondents who started conversion therapy to those who never will, which may not adequately account for differences in family support. This is the main explanation we have for why the first year of the dynamic difference-in-difference estimates is different than the one-year event study estimate.

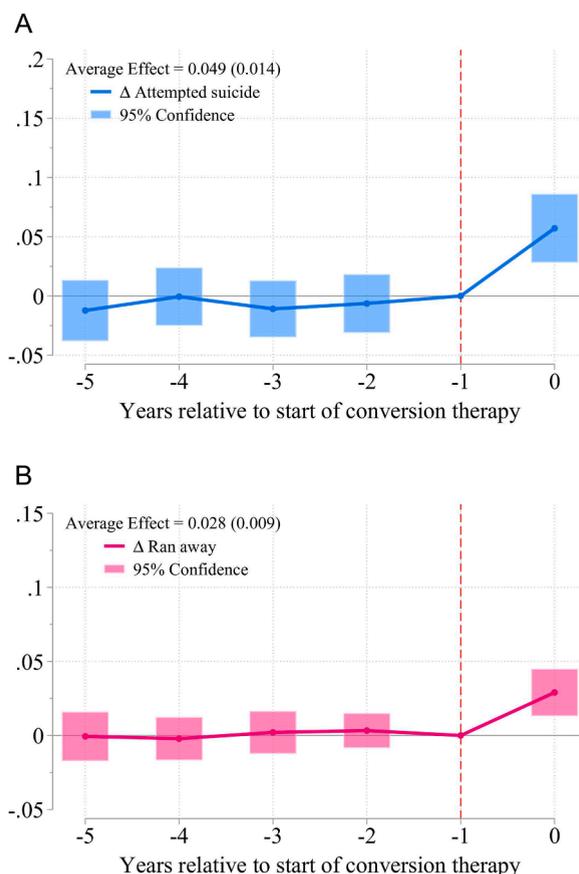


Fig. 6. Event Study Estimates of the Effect of Conversion Therapy on the Risk of Attempting Suicide and Running Away for Transgender People. *Notes:* The figures show the estimates of the stacked event study model. For each cohort, the control group includes only respondents who report being exposed to conversion therapy one year after the treated group was exposed. The outcome used in Fig. 6a is a dummy variable for attempting suicide (zero before the age of first attempting suicide and, if ever, 1 after) and Fig. 6b is dummy variable for ever having run away (zero before the age of running away and, if ever, 1 after). All regressions include cohort-individual fixed effects, cohort-age fixed effects, cohort-calendar year fixed effects, cohort-specific controls for socially transitioning, and are weighted by synthetic unit weights. The shaded area in each figure is the 95% confidence interval based on robust standard errors clustered by individual.

Under a strong assumption that we can correctly identify respondents who have had a supportive family for their entire past, this approach identifies the effect of conversion therapy on attempting suicide or running away independent of family support. As perhaps expected, the difference-in-difference estimates are smaller for these subgroups, although the estimates are still very large and precise, suggesting that conversion therapy may meaningfully affect the mental health of recipients independently of the family support mechanism. In contrast, the magnitude of the event study estimates is similar across subgroups, but statistical significance is sometimes lost, likely from the small sample sizes. A generous interpretation of these results is that the event study estimates control for differences in family support by using only the precise timing of conversion therapy, whereas the difference-in-difference estimates primarily compare those who have conversion therapy to those who never will, allowing for large differences in family support.

We also provide estimates that drop respondents with life events concurrent with conversion therapy. We provide the difference-in-difference estimate for this subsample in Column 5 of Appendix Table A.11. We also provide the event study estimate for this subsample in Column 5 of Appendix Table A.12. To create the subsample, we drop respondents who report starting conversion therapy at the same age as another retrospective life event, which include the following: 1) Felt their gender was different, 2) Thought of themselves as transgender, 3) Told others transgender, 4) Full-time as gender different than the one assigned at birth, 5) Went to court for legal name change, 6) Starting puberty blocking hormones, 7) Starting hormone treatment, 8) First surgery for gender transition. The difference-in-difference estimate for attempting suicide or running away is virtually unchanged, as is the event study estimate for running away. However, the event study estimates for attempting suicide slightly diminishes and is only statistically significant at 10% confidence.

Finally, to explore the extent to which our other retrospective variables correlate with the timing of conversion therapy, we conducted regressions in which the outcome is the conversion therapy dummy variable. The predictors include all variables that we have time variant information other than our two outcomes (attempted suicide and running away). The predictors include the following: ever felt gender was different than assigned sex at birth, ever thought of themselves as transgender, ever told other they are

trans, lives full-time as identified gender, went to court for legal name change, used puberty blocked, used hormone therapy, and had surgery for gender transition. The regressions use the stacked event study sample, implying that the regression coefficients measure the correlation between the variable and the timing of conversion therapy. As shown in Appendix Table A.13, the regression coefficients indicate that the social transitioning measures are highly correlated with the timing of conversion therapy. However, our synthetic unit weights substantially diminish the magnitude of the correlation. Moreover, the inverse probability weights successfully eliminate the correlation for all of these variables other than having surgery or going to court for a legal name change, which are both extremely rare in the youth sample.

7. Discussion

This study has used the 2015 U.S. Transgender Survey to estimate the relationship between exposure to conversion therapy and the risk of attempting suicide and running away from home among transgender adolescents. We constructed a retrospective panel of transgender youth and employed a stacked DID design to show that exposure to conversion therapy substantially increases the risk that a transgender adolescent will try to commit suicide or run away. The ATT of conversion therapy on having attempted suicide is an increase of 17 percentage points, which constitutes a 55% increase in the risk of attempting suicide relative to the average rate of suicide attempts. Moreover, the ATT on the likelihood of running away is an increase of 7.8 percentage points, which represents more than doubling in the risk of running away relative to the sample average. These effects are largest when exposure to conversion therapy occurs at a young age, and they not only persist but also grow with time. Although we are constrained by some formidable data and methodology limitations (especially the challenge of disentangling exposure to conversion therapy from growing up in a hostile family environment), we believe that the DID estimates provide strong evidence for the adverse effects of conversion therapy on the mental health and wellbeing of transgender youth.

Our estimates bolster the argument regarding the importance of parents accepting their children's gender identity and supporting their children. Given that parents and guardians are the primary agents who pressure adolescents into seeking help from a counselor or religious advisor, an important policy recommendation is to provide more professional support and counseling services for parents of transgender children. In addition, healthcare providers and religious leaders need training opportunities for providing gender-affirmative approaches in their practices and institutional settings. This study also informs policy makers about the potential benefits of implementing legal bans in states where conversion therapy is still practiced.

Our results build on previous evidence showing that efforts to promote more inclusion of transgender individuals contribute to improved wellbeing (Hughto et al., 2015). For example, having fully gender-concordant identity documents is associated with a 32% decrease in severe psychological distress and up to a 25% reduction in suicide risk (Scheim et al., 2020). Taking steps to bolster the welfare of transgender youth matters not only for individual wellbeing, it also matters for society at large and the macroeconomy. In particular, suicides entail not only extremely high emotional and social costs, but also losses in human capital and labor productivity. For example, evidence in Rivera et al. (2017) indicates that in 2013, Spain's economy lost 38,038 potential years of working life due to suicide deaths, which cost the overall economy an estimated 565 million Euro (roughly US\$ 425 million at the time). In the U.S., economic supports (through a higher minimum wage and earned income tax credits) are estimated to reduce suicides by 13,600 per year among low-educated working-age adults, which could contribute to an increase of \$1.1 billion annually in worker productivity (Dow et al., 2020).

Workplace losses due to disability arising from depression are also high, with the implication that efforts to improve the mental health of transgender youth may contribute to less absenteeism, greater work productivity, and higher labor force participation (Lerner and Henke 2008; Chatterji et al., 2011). More broadly, laws that promote inclusiveness of sexual orientation and gender identity minorities have repercussions for overall macroeconomic growth. Estimates in Badgett et al. (2019) indicate that adding an additional legal right for LGBT individuals related to anti-discrimination, social rights, and family law can boost a country's overall GDP/capita by approximately \$2000. As such, in the long run, taking steps to reduce the risk that transgender youth attempt suicide may have substantial repercussions that go far beyond mental health.

Disclosure campbell

This study was approved by the Institutional Review Board (IRB) of Rutgers University. The author declares that he has no relevant or material financial interests that relate to the research described in this paper.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at [doi:10.1016/j.jhealeco.2023.102750](https://doi.org/10.1016/j.jhealeco.2023.102750).

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