Anti-Discrimination Laws and Mental Health: Evidence from Sexual Minorities

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JOB MARKET PAPER

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Abstract

This paper provides the first causal evidence on the effect of employment based Anti-Discrimination Laws on mental health. Exploiting the roll out of Anti-Discrimination Laws at the state level within a difference-in-differences model that is robust to staggered timing I document that these laws reduce the number of poor mental health days by around 11% for male sexual minorities but have no significant impact on female sexual minorities. I leverage data from several sources to explore plausible mechanisms. I demonstrate that these mental health effects are unlikely a result of changes in labor market outcomes or health insurance coverage, and instead are likely driven by significant reductions in animosity and prejudice and improvements in workplace climate. I demonstrate that these changes in discrimination are gendered: with reductions in prejudice being principally driven by reductions in prejudice towards sexual minority men.

JEL Codes: I14, I3, J71, J78, K31
Keywords: Discrimination, Mental Health, Sexual Orientation.

Acknowledgements: I am grateful to Christopher Carpenter, Marcus Dillender, Michelle Marcus, Analisa Packham, Alex Hollingsworth, Sebastian Tello-Trillo, Patrick Button, Dario Sansone, Nigel O'Leary, Ian Burn, Mike Martell, Panka Bencsik, Gilbert Gonzales, Kirsty Clarke, and Coleman Drake, as well as seminar and conference participants at Vanderbilt University, Texas A & M University, Indiana University, Tulane University, and the American Society of Health Economists for helpful comments on an earlier draft. I thank Nhut Nguyen for sharing the Human Rights Campaigns Corporate Equality Index data. The views in this paper are solely the responsibility of the author and should not be interpreted as reflecting the views of Vanderbilt University or any other organization. All remaining errors are my own.

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1. Introduction

Over the past 60 years the US government has introduced a series of laws to reduce employment and wage discrimination experienced by marginalized groups, including policies to promote equal pay for women, prevent discrimination on the basis of race, prevent discrimination based on age, and protect people with disabilities in the labor market[1]. Historically, protection for sexual minorities varied across states with some states introducing anti-discrimination laws that protect sexual minorities, and others not. However, the recent 2020 *Bostock vs Clayton County* Supreme Court case ruled in favor of protecting sexual minorities from employment discrimination, introducing nationwide protection for sexual minorities. Proponents of these laws hope that the ruling will improve the lives of sexual minorities by reducing discrimination. Critics of these laws express concerns that anti-discrimination laws impose compliance costs on employers while offering little benefit to the groups they aim to help (Bader 2012; Clegg 2018; Sprigg 2013). If anti-discrimination laws fail to improve the lives of protected individuals, then this may suggest that these laws are inadequate and need to be either abandoned or retooled.

A large body of work has been devoted to exploring the effect of anti-discrimination laws on labor market outcomes, focusing broadly on unemployment spells and wages. While changes in these outcomes are important, one must consider the broader role of these policies in shaping the lives of the people they protect, or one fails to recognize the nuanced forms discrimination can take. In this paper I consider an alternative and important outcome – mental health. Mental health can be considered as a broad evaluation of a person's social, psychological, and emotional wellbeing and as such has been suggested as a key measure to evaluate policy (Frijters et al. 2020; Layard 2013). Poor mental health is the leading course of illness and accounts for around 50% of all disability among the working age population (Layard 2013). Additionally, poor mental health has substantial healthcare and productivity costs and in 2013 mental health disorders were the costliest health condition in the United States, costing around $201 billion (Roehrig, 2016)[2]. Poor mental health has substantial individual, societal and economic costs, with poor mental health leading to lower productivity and lower economic growth (Bubonya, Cobb-Clark, and Wooden 2017; Davlasheridze, Goetz, and Han 2018; Layard 2013). Evaluations of policies that can reduce these large direct and societal costs by reducing the incidence of poor mental health are important (Frijters et al. 2020; Layard 2013). However, prior

[1] Equal Pay Act, 1963; Title VII of the Civil Rights Act, 1964; Age Discrimination in Employment Act, 1967; Americans with Disabilities Act, 1990.

[2] Furthermore, these estimates are likely underestimates given the increased risks of harm from physical illnesses associated with poor mental health (Prince et al. 2007)

work has not been able to study the mental health effects of anti-discrimination laws (ADLs) given that mental health data were not routinely collected at the time that these laws (for non-sexual minorities, i.e., race, sex, and disability-based laws) were enacted. To overcome this, I analyze the effect of sexual orientation based ADLs which, for the most part have been introduced since the 1990s, which coincides with the routine inclusion of questions relating to mental health in health surveys, and as such I am able to provide the first analysis of the effect of these laws on the mental health of the people that these laws protect.

In this paper I exploit the rollout of state level sexual orientation based anti-discrimination laws across the US over the past 25 years paired with data on people that live in same-sex households from the Behavioral Risk Factor Surveillance System to identify the effect of the passage of these policies on self-reported mental health within a difference-in-differences model that is robust to the staggered rollout of ADLs. In doing so I provide the first evidence of the effect of minority-based employment protection policies on mental health and demonstrate that these policies have substantial effects. I demonstrate that sexual orientation based ADLs reduce the number of poor mental health days among men in same-sex households by around 11%, but do not significantly impact women in same-sex households. This core result is robust to numerous sensitivity tests. In addition, I explore three plausible mechanisms: changes in labor market outcomes, changes in health insurance coverage, and changes in prejudice and discrimination. I provide evidence that my main results are driven by reductions in prejudice and discrimination rather than by changes in labor market outcomes or health insurance coverage. In practice, I demonstrate that ADLs increase the firm level coverage of LGBTQ+ policies and practices, reduce the incidence of hate crimes among sexual minorities, and reduce sexual orientation based animosity (primarily towards male sexual minorities rather than female sexual minorities).

This new evidence contributes to four distinct literatures. First, I contribute to a literature on the effects of anti-discrimination laws, which has largely focused on labor market outcomes. Theoretical work indicates that anti-discrimination laws will likely lead to reduced terminations of minority employees, given that irrespective of whether discrimination motivates a termination, terminations still make employers open to the risk of legal action (Acemoglu and Angrist 2001). However, theoretical work also predicts that ADLs reduce the hiring of minority employees, given that the increased costs and risks involved with terminating employees increases the costs associated with hiring protected employees (Bloch 1994)[3]. This leads to ambiguity in terms of the predicted effect of these laws and indeed the existing empirical literature documents mixed conclusions for several groups. Regarding the labor market effects of ADLs for women, people with disabilities and age based laws, prior research has found mixed conclusions (Acemoglu and Angrist 2001; Adams 2004; Bailey, Helgerman, and Stuart 2022; Beegle and Stock 2003; Bell and Heitmueller 2009; Button 2018; DeLeire 2000; Hotchkiss 2004; Hyland, Djankov, and Goldberg 2020; Kruse and Schur 2003; Lahey 2008; Leonard 1989, 1990; Neumark and Stock 2006). Studies focused on race-based laws however, have documented that these policies improved economic outcomes for Black American's (Collins 2003; Leonard 1990; Neumark and Stock 2006) while studies focusing on the effect of sexual orientation based ADLs document small increases in the employment and wages of gay men, and small reductions in the employment and wages of lesbians (Burn 2018; Delhommer 2020; Gates 2009; Klawitter 2011; Klawitter and Flatt 1998; Martell 2013; Tilcsik 2011). A scant literature has considered the association between ADLs and health, finding that sexual minorities living in states with employment protections have lower psychiatric morbidity and lower levels of mood disorders than sexual minorities in states without these protections (Hatzenbuehler, Keyes, and Hasin, 2009, Blosnich et al., 2016), but these studies are unable establish causality. I build on this literature by studying the causal effect of anti-discrimination laws on mental health, for the first time.

Second, I contribute to the literature regarding the relationship between experiences of discrimination and health. Prior work has demonstrated that perceptions of discrimination among sexual minorities is associated with self-reported mental health (Mays and Cochran 2001), has exploited exogenous changes in attitudes towards marginalized groups to estimate the causal effect of discrimination on physical and mental health (Johnston and Lordan 2012), and has documented the existence of discrimination in access to health care providers (Button et al. 2020; Leech, Irby-Shasanmi, and Mitchell 2019) through the use of audit field experiments. I build on this work by demonstrating that laws that explicitly prohibit discrimination improve mental health, primarily through reducing discrimination.

Third, I contribute to the broad literature regarding the sexual orientation based health insurance disparity, and the effect of LGBTQ+ policies in reducing this disparity. Prior literature has

[3] It should be noted that discrimination in hiring is difficult to detect and associated with smaller damages than termination cases (Neumark and Button 2014; Posner 1995).

documented the health insurance coverage disparity between heterosexuals and sexual minorities (Ash and Badgett 2006; Gonzales and Blewett 2014; Gonzales, Henning-Smith, and Ehrenfeld 2021) and demonstrated that LGBTQ+ policies such as same-sex marriage legalization have helped to close the coverage gap (Buchmueller and Carpenter 2010; Carpenter et al. 2021; Dillender 2015; Gonzales 2015). However, no prior work has analyzed the effect of ADLs on the health insurance coverage of impacted individuals, such as sexual minorities, women, people with disabilities, or racial minorities. I build on this work by providing the first estimates of the effect of employment based ADLs on the health insurance coverage of those individuals protected.

Finally, I contribute to the literature regarding the effect of LGBTQ+ policies on discrimination towards sexual minorities. I study the effect of ADLs on prejudice and discrimination towards sexual minorities, providing the first evidence that ADLs improve the workplace climate for sexual minorities by increasing the firm level coverage of LGBTQ+ policies and practices, that ADLs reduce the incidence of hate crimes among sexual minorities, and that ADLs reduce sexual orientation based animosity. This builds on prior work that has demonstrated that ADLs improve attitudes towards sexual minorities (Deal 2022; Delhommer 2020), and work that has documented the effect of other LGBTQ+ policies on attitudes towards sexual minorities, sexual orientation-based animosity, and hate crime incidence (Aksoy et al. 2020; Blasco, Moreno Galbis, and Tanguy 2021; Nikolaou 2022; Sansone 2019).

This paper proceeds as follows. Section 2 provides institutional context underlying the difference-in-differences model. Section 3 provides a conceptual framework that discusses the mechanisms through which anti-discrimination laws may impact the mental health of people that are covered by these laws. In Section 4 I discuss data used for my main analysis which is followed by a discussion of the econometric approach (Section 5). My main results are provided in Section 6, while Section 7 provides a series of sensitivity analyses and robustness checks. In Section 8 I explore three plausible mechanisms: changes in labor market outcomes, changes in access to care, and changes in discriminatory attitudes and behaviors. Section 9 concludes.

2. Institutional Background

In 1974 Representatives Bella Abzug and Ed Koch introduced a bill which would have amended the Civil Rights Act of 1964 to protect sexual minorities from discrimination in employment, public accommodations, public education, the credit market, and protected them from hate crimes. However, the bill died. Nonetheless, state level coverage has grown rapidly since the early 80's. In 1982 Wisconsin became the first state to implement a sexual orientation based anti-discrimination law, following the District of Columbia's introduction of an ADL in 1977. However, this inspired a series of legislation and proposed legislation at the state level to prevent similar laws. Several laws were proposed to prevent gay and lesbian workers from working in specific environments (see for example Proposition 6 which would have barred sexual minorities from working in schools) and some states introduced laws that prevented the passage of local level non-discrimination acts. The battle for employment protection was a result of continued campaigning by rights activists despite years of failed efforts, in New York for example, an ADL was not introduced until 2002, over 30 years after an ADL was first introduced in a legislative debate there (Sears, Hunter, and Mallory 2009).

Despite backlash, ADLs gained progress during the 90's and 2000's and by 2019 25 states had passed ADLs, protecting sexual minorities from discrimination in employment (see Figure 1 and Figure 2). These policies aim to ensure that sexual minorities receive comparable pay, are not discriminated against during the hiring process or unfairly fired, that they receive the same benefits and training opportunities as their heterosexual counterparts and protect them from discrimination when it comes to any other aspect of employment, terms of employment or employment conditions. In addition to work that has documented disparities in these areas, analyses of the utilization of ADLs highlight that they are needed: sexual orientation based ADLs are utilized at similar rates to race and sex discrimination laws and these laws provide sexual minorities the ability to file such discriminatory claims (Ramos, Badgett and Sears, 2008; Sears, Hunter and Mallory, 2009; Baumle, Badgett and Boutcher, 2020).

The supreme court introduced federal employment protection for sexual minorities, ruling in favor of *Bostock* in the *Bostock vs Clayton County* case (6-3 decision). The court ruled on the 15th of June 2020 that discrimination based on sexual orientation is unlawful under interpretations of the term "sex": discriminators against sexual minorities accept behavior of employees of one sex (e.g., attraction to women among men) but not of employees of the other sex (e.g., attraction to women among women). As a result, the *Bostock* ruling found that sexual orientation employment-based discrimination is unlawful under Title VII of the Civil Rights Act of 1964, therefore outlawing discrimination towards sexual minorities under the same law that outlaws discrimination based on race, religion, sex, and national origin. This paper provides the first evidence of the potential mental health effects of this decision by exploiting pre-*Bostock* state level variation in the coverage of ADLs to estimate the effect of legislative protection on mental health.

3. Conceptual Framework

There are several reasons why one may expect ADLs to impact the mental health of sexual minorities. Given that ADLs aim to reduce discrimination towards sexual minorities one may expect that ADLs improve the labor market outcomes of sexual minorities, which may have downstream mental health effects. Alternatively, ADLs require that employers are not discriminatory in their offering of benefits such as health insurance to sexual minorities, which may lead to increases in employer-sponsored health insurance among sexual minorities, providing access to care, which may have positive effects on mental health. Finally, ADLs may improve the workplace climate and reduce discrimination towards sexual minorities, even if this is not visible in measures such as labor market outcomes or health insurance coverage. Given the well-established relationship between experiences of discrimination and mental health these reductions may lead to improved mental health. In what follows I discuss how these different mechanisms could results in ADLs improving the mental health of those individuals protected by ADLs.

First, ADLs may improve the mental health of sexual minorities by improving the labor market outcomes (and therefore socio-economic status) of sexual minorities, if the laws achieve their explicit aim. However, economic theory has highlighted that ADLs may not be an effective labor market policy to protect minority employees as theoretical predictions are ambiguous. In fact, theoretical work predicts that ADLs will reduce terminations but decrease new hiring of newly protected workers (Acemoglu and Angrist 2001; Bloch 1994), making it unclear whether ADLs can achieve their goal of reducing discrimination and improving the labor market outcomes of protected individuals. Nonetheless, much of the early work on the relationship between sexual orientation based ADLs and labor market outcomes documented positive associations between the existence of state level ADLs and the labor market outcomes of sexual minorities (Gates 2009; Klawitter 2011; Klawitter and Flatt 1998; Tilcsik 2011). Using quasi-experimental approaches more recent research has been able to untangle the causal effects of ADLs on the labor market outcomes of sexual minorities and has broadly documented that these effects are gendered. Martell (2013) used data on sexual behavior from the General Social Survey covering the period 1994-2010 with a sample size of around 200 sexual minorities to demonstrate that state level sexual orientation based ADLs are associated with higher wages for men who have sex with men. In a similar vein, Burn (2018) used ACS data covering the period 2008 to 2014, combined with the 1990 and 2000 US Census 5% sample and found that ADLs increased the hourly wages of cohabiting gay men (by around 2.5%) but reduced both intensive (0.733 hours) and extensive (1.7%) labor supply of cohabiting lesbians. Finally, using the American Community Survey (ACS) covering the period 2005 to 2016 Delhommer (2020) demonstrated that both local and state level ADLs are associated with small increases in labor force participation, employment, and earnings of gay men, but small reductions in labor force participation, employment, and earnings of lesbians. Given the large literature documenting the positive relationship between labor market outcomes and earnings and mental health and wellbeing (Ettner, Frank and Kessler, 1997; Theodossiou, 1998; Gardner and Oswald, 2007; Clark et al., 2008; Lindqvist, Östling and Cesarini, 2020) it may be the case that ADLs lead to improvements in mental health among gay men, but reductions among lesbians, in line with the impact of these laws on labor market outcomes.

Second, in addition to preventing discrimination in hiring, firing, and pay, these laws make it illegal to differentially offer benefits, such as health insurance, to sexual minorities and heterosexuals. As such, this may increase the health insurance coverage rates of sexual minorities and in turn, increase access to care. This may be particularly important given the wide literature that has documented the health insurance coverage disparity between heterosexuals and sexual minorities (Ash and Badgett, 2006; Gonzales and Blewett, 2014; Gonzales, Henning-Smith, and Ehrenfeld 2021a) and that sexual minorities have lower access to care and have less regular health check-ups. However, no prior work has analyzed the effect of ADLs on the health insurance coverage of impacted individuals, such as sexual minorities, women, people with disabilities, or racial minorities. If ADLs do indeed increase insurance coverage rates of impacted individuals this may be important for mental health outcomes. On the one hand, higher insurance coverage rates may lead to higher self-reported poor health as access to care may lead to increases in the diagnoses of ill-health. However, higher insurance coverage rates may also result in access to medicine and therefore be positively related to mental health outcomes given that individuals can access care to deal with ill-health. Indeed, prior work has shown a positive association between insurance coverage, access to care, and mental health (see *inter alia*: Finkelstein et al., 2012; Baicker et al., 2014; Ayyagari and Shane, 2015; Hampton and Lenhart, 2022).

Finally, even if ADLs do not change employer based discrimination (captured by labor market outcomes and health insurance coverage) they may play a key role in changing more nuanced forms of discrimination, in turn improving the social and workplace climate for sexual minorities, which may have positive mental health effects. There are several reasons why one may expect ADLs to reduce interpersonal discrimination and improve social climate for sexual minorities. First, these laws may lead to increased visibility and awareness of the LGBTQ+ community, given policy discussion in the news and media. Second, social norms may change and an awareness of the protection that sexual minorities have may lead people to re-evaluate their views given that the supportive legislative framework highlights that discriminatory behavior is no longer accepted. Third, the introduction of the law may lead firms to introduce policies and practices to support their sexual minority employees, through the introduction of firm diversity policies and diversity training. In line with these assertions recent research has found suggestive evidence that ADLs may improve attitudes towards sexual minorities. Delhommer (2020) for example documents that anti-discrimination laws are associated with increases in support for same-sex marriage in his analysis of Pew Research data covering the period 2005 to 2016, while Deal, (2022) finds support for the hypothesis that the *Bostock* ruling led to improved public support for sexual minorities, using data from the Democracy Fund + UCLA Nationscape survey. Importantly, a wide literature has documented the negative relationship between experiences of discrimination and mental health (Meyer, 2003; Johnston and Lordan, 2012; Perales and Todd, 2018; Hole and Ratcliffe, 2020; Armijos Bravo and Vall Castelló, 2021) and theoretical contributions suggest that the additional stress experienced by sexual minorities due to experiences and expectations of discrimination can explain the mental health disparity between heterosexuals and sexual minorities (Meyer 2003). As such, if ADLs do indeed reduce experiences or expectations of discrimination, it is likely that ADLs will have a positive impact on mental health.

4. Data

4.1. The Behavioral Risk Factor Surveillance System (BRFSS)

To estimate the effect of ADLs on the mental health of sexual minorities I utilize data from the Behavioral Risk Factor Surveillance System (BRFSS). The BRFSS is a nationally representative health survey that is conducted, via telephone by health departments at the state level. The Centre for Disease Control and Prevention (CDC) then compile the state level data annually into an individual-level cross-sectional dataset. The survey began in 1984, but originally was not conducted in all states. Since 1993 the BRFSS has been conducted in all 50 states (plus DC). I exploit data on people between the ages of 25 and 64, covering the period 1993 to 2019.

The survey primarily collects health related information but also asks participants numerous demographic questions, such as age, ethnicity, education, and marital status. Given its focus on health outcomes the BRFSS has been used extensively by health economists, including numerous studies analyzing population level mental health (see *inter* alia: Ruhm, 2000, 2005; Oswald and Wu, 2011; Alexander and Schnell, 2019; Bullinger, 2019; Mullins and White, 2019; Kuka, 2020).

4.2. Identifying sexual minorities in the BRFSS

I identify sexual minorities in the BRFSS following the approach introduced by Carpenter, et al., (2021) and Carpenter and Sansone (2021). Specifically, I use household composition to identify people that are likely in same-sex couples. When being interviewed one randomly selected adult is asked to state the number of adult males and adult females in their household. In addition, all respondents are asked their sex. This allows me to identify households that contain exactly two adult men or two adult women (referred to henceforth as same-sex households (SSH)), or households that contain exactly one adult man and one adult woman (referred to henceforth as different-sex households (DSH)). As highlighted by Carpenter, et al., (2021) and Carpenter and Sansone (2021) sexual minorities are more likely to live in a household comprised of two same-sex individuals than heterosexuals. This approach can be used as an indirect way to identify a large sample of sexual minorities that are in same-sex relationships.

Identifying sexual minorities this way has two advantages. First, it does not require sexual minorities to self-identify to interviewers: some sexual minorities may prefer to not disclose this information, leading to an undercount[4]; self-identification is also likely correlated with mental health and therefore this approach allows me to overcome selective disclosure biases. Second, the indirect approach allows me to identify sexual minorities in all states over a much longer period (1993-2019); a period that pre-dates questions used to identify sexual minorities in other surveys (such as sexual attraction, behavior and identity).

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[4] Indeed, prior work has documented that survey mode leads to differing estimates of the LGBTQ+ population due to differences in disclosure (Dahlhamer, Galinsky, and Joestl 2019). It is likely that this issue is more concerning in earlier years of the BRFSS given the historically higher incidence of prejudice and homophobia.

To use this technique, one must however make two data restrictions, as highlighted in Carpenter et al., (2021) and Carpenter and Sansone (2021). First, I restrict my sample to people over the age of 25. It is likely that the association between sexual orientation and household composition is weaker for younger populations given that younger people are more likely to live in same-sex households without being in romantic relationships (e.g., students or roommates). Second, I limit my analysis to the non-cellphone sample as the household composition questions were not administered to people who completed the survey via cellphone[5].

Since 2014 the BRFSS has included an optional module that includes sexual identity questions (the sexual orientation and gender identity (SOGI) module). This optional module has been administered at least once in 39 states. This data covering the period 2014 to 2019 is used to test the relationship between my indirect measure and direct questions on sexual identity. I find that among individuals in DSH's less than 2% identify as non-heterosexual, and less than 0.5% identify as gay or lesbian. Among men in same-sex households 33.51% identify as a sexual minority, with the majority of those identifying as gay (29.95% of the male SSH sample), while 16.67% of women in same-sex households identify as non-heterosexual, again with the majority identifying as lesbian (12.78% of the female SSH sample). These results indicate that my approach is better at identifying male sexual minorities than female sexual minorities, although, for both, the proportion that identify as a sexual minority is much larger than population estimates of sexual minorities (Badgett, Carpenter, and Sansone 2021).

These correlations are consistent with those documented in Carpenter et al., (2021) and Carpenter and Sansone (2021) who also demonstrate that people in SSH's are significantly more likely than those in DSH's to have had a HIV test, and significantly less likely to be married, which is consistent with previous work which has documented lower rates of marriage and higher rates of HIV testing among sexual minorities. The correlations documented in Table 1 clearly demonstrate that my indirect approach identifies a large population of people of which a non-trivial share are sexual minorities. While not all these individuals identify as a sexual minority, some of these may still be sexual minorities but may not wish to identify as such to interviewers. Furthermore, if these individuals are not sexual minorities, they should not be directly impacted by ADLs[6] thus results will be biased towards zero,

[5] The cellphone sample was not added until 2011.
[6] As tested later with falsification tests using DSH's.

ergo, allowing me to report conservative estimates of the effect of ADLs on mental health. This is the main disadvantage to using same-sex households to identify a sample of sexual minorities[7]. This bias is likely larger for women in SSH's than men in SSH's given that this indirect approach to identify sexual minorities is more successful in identifying likely male sexual minorities (see Table 1).

Figure 3 plots the proportion of people that are in DSH's (black), the proportion of women in SSH's (green), and the proportion of men in SSH's (blue), broadly demonstrating that the proportion of people in male and female SSH's has stayed fairly consistent over time. Around 3% of the population live in SSH's, which is in line with the broad literature regarding the size of the sexual minority population (Badgett et al. 2021). Additionally, the figure demonstrates that a slightly larger proportion of women are in SSH's compared to men, which is in line with the higher partnership rates of lesbians compared to gay men (Badgett et al. 2021). Finally, the figure demonstrates that a little less than 60% of the sample are in DSH's, which is closely aligned to estimates of the proportion of people that are married; the figure depicts a slight dip in the proportion of people in DSH's in recent years, which is in line with work documenting the changing proportion of people living with a spouse (Smock and Schwartz 2020).

4.3. Mental Health

My mental health measure relies on respondents' subjective assessment of their mental health (which includes stress, depression, and problems with emotions). Respondents to the BRFSS were asked to indicate for how many days during the past 30 days their mental health was not good. My outcome variables take the value of the number of days with poor mental health (ranging from 0 to 30). While I acknowledge that my outcome is a subjective self-report, prior work has verified the validity of this measure (CDC 2004; Pierannunzi, Hu, and Balluz 2013). In addition, in Section 7.3 I replace the outcome variable with a binary variable that takes the value 1 if the individual reported 14 or more days with poor mental health and 0 otherwise. This cut-off is generally used in public health as a measure of frequent mental distress in line with the CDC definition (CDC 2004) and is highly correlated with depression and other mental illness diagnoses (Cree et al. 2020).

[7] Another disadvantage is that I am unable to identify single sexual minorities or sexual minorities in opposite sex relationships.

It should be noted that while the mental health question is part of the core questionnaire and therefore asked in every year of the BRFSS, it was removed from the core questionnaire and instead asked as part of an optional module in 2002. Several states (29) did not administer the mental health question to respondents in 2002 as they chose to not include the healthy days optional module. This is not a problem in other years as the question was administered to all respondents, in all states, in all other years[8].

4.4. Mechanisms

In Section 8 and discussed in Section 3 I explore three plausible mechanisms: changes in labor market outcomes, changes in health insurance coverage, and changes in discrimination and prejudice. To explore these mechanisms, I make use of data from the BRFSS as well as three additional datasets.

The BRFSS is predominantly a health survey but does include some questions relating to labor market outcomes. BRFSS respondents are asked whether they are employed and are also asked their household income[9]. I make use of these two outcomes to explore the effectiveness of ADLs in improving the labor market outcomes of people in same-sex households. In addition, BRFSS respondents are asked whether they have health insurance coverage, I use responses to this question as an outcome variable to assess whether ADLs change the health insurance coverage of people in SSH's[10].

Unfortunately, the BRFSS does not include questions relating to experiences of discrimination, as such, I make use of several additional datasets to explore whether ADLs reduced discrimination and prejudice towards sexual minorities. In practice, I make use of three additional datasets: The Human Rights Campaigns (HRC) Corporate Equality Index (CEI), the FBI's National Incident Based Reporting System (NIBRS), and Google Trends.

Since 2002 the HRC, as part of its annual report, has compiled the Corporate Equality Index which is based on surveys of firms regarding their LGBTQ+ policies and practices. The CEI scores firms (between the value of -25 and 100) based on their coverage of LGBTQ+ specific policies and

[8] I deal with concerns regarding the unbalanced inclusion of the outcome variable across states in 2002 in Section 7.5.
[9] Unfortunately, the BRFSS does not include questions regarding individual income or income from wages.
[10] BRFSS respondents are not asked the source of their insurance coverage.

practices. To achieve the full score a firm must have workplace protection policies covering sexual orientation and gender identity, have inclusive family benefits (including domestic partner benefits), support an inclusive culture (including new hire training on nondiscrimination policies that cover LGBTQ+ people, and LGBTQ+ diversity training), and cover LGBTQ+ issues within their corporate social responsibility (including demonstrated efforts to include LGBTQ+ suppliers and demonstrated public support for LGBTQ+ equality initiatives). The CEI survey is sent to the managing partner or CEO and the head of human resources at all Fortune 1000 organizations; the HRC supplements this survey data with their own research into LGBTQ+ discrimination incidents reported in the media, organizational filings, and primary reports from LGBTQ+ employees. Prior work has typically used this data to assess the relationship between LGBTQ+ organizational climate and firm level outcomes such as performance (see *inter alia*: Do et al. 2022; Hossain et al. 2020; Johnston and Malina 2008; Nadarajah, Atif, and Gull 2022; Pichler et al. 2018).[11] I make use of this data to assess whether ADLs changed the firm level coverage of LGBTQ+ policies and practices, by estimating the effect of ADLs on the average CEI score of firms headquartered in states that have an ADL compared to those that do not have an ADL, within a difference-in-differences framework.

Additionally, I use data from the NIBRS of the FBI's Uniform Crime Reporting program to estimate the effect of the passage of an ADL on the incidence of LGBTQ+ motivated hate crimes. The NIBRS provides details on whether criminal offences are motivated by an offender bias (e.g., sexual orientation, disability, race, gender, etc). I create an outcome variable for each state by year cell that is equal to the hate crime rate (i.e.,$\left(\frac{LGBTQ{+}hate\ crimes_{st}}{population_{st}/\ 100{,}000}\right)$. I create a balanced state by year panel of the LGBTQ+ hate crime rate over the period 1998 to 2019[12].

Finally, I exploit google trends data to estimate the effect of ADLs on animosity towards sexual minorities. The advantage of using google trends data is that google searches are a way to explore

[11] While it should be noted that this data is not nationally representative, it is the leading source of data on LGBTQ+ firm level climate (Cook and Glass, 2016). Nonetheless, the data does not collect information on smaller firms or firms that refused to respond to the HRC survey. Given that there is a selection bias I restrict my analysis to a balanced sample of firms that responded in all years.

[12] The reporting of hate crime data by local authorities is voluntary. Using a balanced panel overcomes concerns that LGBTQ+ specific policies encourage agencies to voluntarily disclose their hate crime data. States not included in the dataset are those states that did not report hate crime data in at least one year over the period 1998 to 2019. These states are: Alabama, Alaska, Arkansas, Hawaii, Mississippi, Wisconsin, and Wyoming. This leaves 22 treated states (of which 14 introduce an ADL during the sample period) and 21 untreated states. In. practice, results from unbalanced models report extremely similar results. These results are available on request.

changes in attitudes to socially sensitive issues and are a good way to proxy animosity, given that searches reflect the wording that people use when they are often alone and in situations where there is no incentive to lie (Stephens-Davidowitz 2017). As such, prior work has used google searches to analyze the effect of other LGTBQ+ policies, such as same-sex marriage legalization on animosity towards sexual minorities (Nikolaou 2022; Sansone 2019).

Prior work has combined the intensity of three search terms (Faggot, Leviticus, and Sodomy) to identify the effect of LGBTQ+ policies on animosity towards sexual minorities (Nikolaou 2022; Sansone 2019). I instead estimate the effect of ADLs on each of these terms as well as the term Dyke, separately, as this allows me to explore whether ADLs have differential effects on animosity towards gay men and lesbian women, separately.

To be more specific, each of these terms have different connotations, and these connotations are largely gendered. For example, Leviticus is a book in the bible that states "You shall not lie with a man as with a woman, this is an abomination"; this reference has historically been used to legitimize homophobia among religious groups, especially towards gay men. Sodomy refers both to the "sin of Sodom" in Genesis chapters in the bible and sodomy laws which have historically been used to justify the prosecution of gay men. Faggot is an offensive, derogatory, and homophobic term used towards gay men. While, Dyke, is an offensive, derogatory, and homophobic term used towards lesbian women. As aforementioned, the first three terms are largely focused on gay men, i.e., Leviticus refers to men not "lying" with other men, Sodomy refers to sodomy laws which allow the prosecution of men who have sex with men, and the sin of Sodom which refers to male homosexuality, and Faggot is a term directed towards gay men. Dyke, unlike the other three terms, is explicitly related to female sexual minorities and is a derogatory term used towards lesbians. This allows me to identify the differential effect of ADLs on animosity towards gay men and lesbian women, for the first time. I collect data at the state by year level directly from google trends that cover the period 2004 to 2019 for each of these four terms and use these search intensities as outcome variable.

5. Econometric Framework

I begin my analysis of the impact of anti-discrimination laws on the mental health of sexual minorities within a two-way fixed effects differences-in-differences model that exploits temporal and geographic variation of anti-discrimination laws.[13] I specify estimation equation (1) in the following way:

$$y_{ist} = \alpha + \beta ADL_{ist} + x'_{ist}\gamma + S_s + T_t + \varepsilon_{ist} \tag{1}$$

Where my principal outcome variable y_{ist} is a variable that returns the value of the number of days with poor mental health in the past 30 days for individual i in state s at time t. The coefficient of interest β. ADL_{ist} is an indicator variable that takes the value 1 if the individual resides in a states that introduces an ADL during or before the respondent is interviewed, and zero otherwise: β therefore captures the effect of an ADL on the number of poor mental health days among sexual minorities. In addition, I include state fixed effects (S_s) and year fixed effects (T_t). In my preferred specification I additionally include a series of individual and state level control variables (x'_{ist}). More precisely, I include controls for respondent age (including its square), race, ethnicity, educational attainment, marital status, and parental status. In terms of state level covariates, I control for state population (log), the proportion of people in the labor force, and the unemployment rate, as well as a series of indicator variables that account for other state level LGBTQ policies: constitutional or statutory bans on same-sex marriage, same-sex marriage legalization, same-sex domestic partnership and civil union legislation, LGBTQ hate crime laws, and sexual orientation based health non-discrimination acts. I estimate all specifications for men and women, separately. Standard errors are clustered at the state-level (Bertrand, Duflo and Mullainathan, 2004). All specifications are weighed using BRFSS sampling weights following Simon, Soni and Cawley (2017). My identifying assumption is that the mental health of people in SSH's would evolve similarly in states that introduce an anti-discrimination law and those that do not, in the absence of the introduction of an ADL.

There may however be several concerns regarding the causal interpretation of results from Equation (1). First, the staggered timing of the policy may introduce heterogeneity bias and weighting problems in two-way fixed effects (TWFE) models, however, a recent literature has identified approaches to

[13] Seventeen states introduced sexual orientation based anti-discrimination laws during my sample periods Seven states and DC introduced the policy prior to the start of my sample period.

overcome these issues in both static and dynamic models (Roth et al. 2022)[14]. To alleviate concerns regarding the bias that may be present due to the staggered rollout of the policy, I provide results from the following Sun and Abraham (2021) model:

$$y_{st} = \sum_{j=-2}^{-6} \lambda_j D_{sj} + \sum_{j=0}^{5} \lambda_j D_{sj} + x'_{st}\gamma + S_s + T_t + \varepsilon_{st} \tag{2}$$

The interaction weighted (IW) estimator suggested by Sun and Abraham (2021) is a dynamic model which allows one to assess the dynamic effects of the policy as well as the plausibility of the parallel trends assumption, in a model that is robust to the issues present in traditional TWFE models. The IW estimator is depicted in Equation (2) which includes controls for lags and leads and uses never treated units as the comparison group. Always treated states are omitted following Sun & Abraham (2021). Lags and leads are captured by the dummy variables D_{sj} $(j = t - k)$, where k is the year that an ADL was introduced in state s, thus j is the period relative to the year that an ADL was introduced. Leads and lags are binned at distant relative periods, and the reference periods are $j = -1$ and $j = -6$, following Sun & Abraham's (2021) suggestions regarding omitting two periods to avoid multicollinearity.

Following Anand, Dague, and Wagner (2022) I present both static and dynamic Sun and Abraham (2021) models where event time is compressed to two periods (pre vs post) for the static model while dynamic effects are estimated by including both the leads and lags of the policy as depicted in Equation (2). While static Sun and Abraham (2021) models overcome the heterogeneity bias and weighting problems present in TWFE models there may remain concerns regarding the plausibility of the parallel trends assumption; dynamic models allow a visual interpretation of the plausibility of the parallel trends assumptions.

One may also be concerned that there are pre-existing differences in states that are treated and those that are not. As is usual I control for all time-invariant state characteristics using state fixed effects.

[14] Results from Goodman-Bacon's (2021) decomposition are provided in Appendix Table A2. These results demonstrate that around 63% of the unadjusted difference-in-differences estimate is composed of valid comparisons between treated and untreated units.

Additionally, I include an extensive list of LGBTQ+ policies as control variables. Furthermore, covariance balance tests presented in Table 2 demonstrate that the observable characteristics of people in SSH's in treatment and control states are remarkably similar at baseline. Differences in observable characteristics are small, and largely statistically insignificant[15].

The exact timing of the passage of ADLs was likely to be unknown, for example, in New York an ADL was not introduced until 2002, over 30 years after an ADL was first introduced in a legislative debate there (Sears et al. 2009). To offer further support that the main findings regarding the effect of ADLs on the mental health of sexual minorities are not driven by time-varying state characteristics I also present results from a triple-difference model (reported in Section 7.4) which compares the mental health differential between SSH's and DSH's across states and over time, while explicitly accounting for state-specific time-varying characteristics, state-specific shocks that impact SSH's, and time shocks specific to SSH's. My results from these models confess with my main results.

Another concern is that the distribution of sexual minorities may not be random, and anti-discrimination laws acts may be more likely to be passed in states with a greater proportion of sexual minorities. Alternatively, the passage of an ADL may be associated with sexual minorities migrating into newly treated states to enjoy the additional protection offered in these states. Nonetheless, results presented in Appendix Table A1 demonstrate that ADLs cannot be predicted by the proportion of people that are a sexual minority in a state, nor is there a significant change in the number of SSH's in a state following the passage of an ADL. I deal with further concerns regarding the robustness and sensitivity of my results in Section 7.

6. Main Results

Below, I present a collage of evidence on the effects of ADLs on the mental health of people in same-sex households. I begin by presenting raw trends in the mental health of people in SSH's by treatment status, accompanied by descriptive statistics. I then show results from difference-in-differences models that compare changes in mental health for people in same-sex households that live in states that introduced an ADL to the changes in the mental health of people in same-sex households that live in states that did not introduce an ADL. These are accompanied by synonymous specifications

[15] The only exception to this is parenthood. People in control states are significantly more likely to be parents than those in treatment states.

for people in DSH's, which serve as falsification tests. Additionally, I provide results from event study models that allows me to identify dynamic effects of ADLs.

6.1. Descriptive Statistics and Trends

The mean number of poor mental health days for people in same-sex households in treatment states (Column 1) and control states (Column 2) are presented in Table 3. Results for men and women are presented in Panels A and B, respectively. Table 3 shows that the average number of poor mental health days is significantly larger in states that do not introduce an ADL than those that do. Among men in SSH's the average number of days with poor mental health in the past 30 days is 4.156 in control states, while in treatment states it is 3.865. For women, the average number of days is 5.718 in control states, and 5.137 in treatment states.

Figure 4 presents raw trends in the number of poor mental health days among people in SSH's by treatment status, for men in Panel A and women in Panel B. Two clear patterns are apparent. First, among both men and women in SSH's the difference in the average number of poor mental health days is similar in earlier years, i.e., years where fewer states had introduced anti-discrimination laws. Second, in more recent years (i.e., years where a larger proportion of treatment states had implemented ADLs) there is a gap between the mental health of people in SSH's in treatment states (black) and controls states (blue). These gaps emerge in the early to mid 2000's which is in line with the increasing take up of ADLs during this period (see Figure 1). Broadly, the patterns documented in Figure 4 indicate that people that live in states that introduce ADLs during the sample period report better mental health outcomes than people that live in states that do not introduce ADLs during the sample period. However, people in SSHs report similar levels of poor mental health in the beginning of the sample period. That is, this gap emerges later in the sample period, and around the same time as there was increased coverage of ADLs in treated states.

6.2. Effect of Anti-Discrimination Laws on Mental Health of people in SSH's.

Table 4 presents results from my difference-in-differences model (Equation 1). Each column and panel represent a different regression specification. Column 1 documents results from a difference-in-differences model that does not include covariates, instead including only year and state fixed effects. In Colum 2 I additionally include individual covariates which account for observable demographic differences, such as race, age, education, marital status, and parental status. In Column

3 I additionally include state level controls which cover both economic factors and state level LGBTQ+ policies. In Column 4 I re-estimate the specification in Column 3 using the static Sun and Abraham (2021) approach. Results for men in SSH's are presented in Panel A, while corresponding results for men in DSH's, women in SSH's, and women in DSH's are presented in Panels B, C, and D, respectively.

The results presented in Table 4 demonstrate that the passage of anti-discrimination laws is associated with a significant reduction in the number of reported poor mental health days among people in same-sex households, principally driven by men in same-sex households. Results that do not consider covariates demonstrate that the passage of ADLs is associated with around a half a day reduction in poor mental health days among men in same-sex households and around a third of a day reduction among women in same-sex households, while coefficients for both men and women in DSH's are statistically insignificant. Given the baseline means this corresponds to around an 11% reduction in the number of poor mental health days for men in same-sex households and around a 6% reduction in the number of poor mental health days for women in same-sex households.

Including individual level covariates is associated with a reduction in the magnitude of the effect for both men and women in same-sex households. After taking consideration of individual covariates the effect of ADLs on the mental health of people in same-sex households is estimated to be around two fifths of a day (or around 9.5%) and around one quarter of a day (or around 5%) for men and women respectively. In the fully saturated model (Column 3), which additionally includes state level covariates, I estimate that ADLs reduce poor mental health by around 11% among men in same-sex households, but do not significantly impact women in SSH's. Estimates from Sun and Abraham (2021) models that account for the biases present in traditional TWFE models are extremely similar. For men in same-sex households I estimate that ADLs reduce poor mental health by around 11%, while for women in same-sex households I estimate statistically insignificant effects. Given the difference in poor mental health between men in SSH's and DSH's in the pre-policy period (1.703 days), my estimates indicate that the passage of an ADL reduced the mental health disparity between men in SSH's and men in DSH's by around 28%. In all four columns, falsification tests from men and women in DSH's return coefficients that are statistically indistinguishable from zero.

Figure 5 presents the corresponding Sun & Abraham event study models that include both individual and state level controls, following Equation (2)[16]. Panel A of Figure 5 presents the results for men in same-sex households, while the corresponding results for women in same-sex households are provided in Panel B.

The results for men (Panel A) provide evidence that anti-discrimination laws led to reductions in the number of reported poor mental health days among men in same-sex households. In the pre-policy period coefficients are statistically indistinguishable from zero (including at the 10% level), while in the post-policy period coefficients are negative and statistically significant. One notable concern is that the coefficient at t-2 is negative and has a large confidence interval, raising concerns regarding the parallel trends assumption required for identification. In Section 7.2 I provide results from several additional tests to confirm that my results do not violate the parallel trends assumption.

Results for women in same-sex households are presented in Panel B. Coefficients in the pre policy period are statistically indistinguishable from zero and therefore offer support for the parallel trends assumption. In the post policy period coefficients remain statistically insignificant, for all periods. This is line with the results documented in Table 4, which show that for women the effect of ADLs on poor mental health is statistically indistinguishable from zero.

Broadly, my main results demonstrate that ADLs substantially reduce the number of poor mental health days among men in same-sex households. However, for women, I document statistically insignificant results; ADLs do not significantly impact the number of poor mental health days among women in same-sex households. For men, I document that ADLs reduce the number of poor mental health days by around 11% or reduced the mental health disparity between men in SSH's and men in DSH's by around 28%.

To put these findings in perspective, one could compare my effect sizes to the effects of other LGBTQ+ policies on mental health. Chen and Ours (2022) for example find that same-sex marriage legalization led to around a 50% reduction in the sexual minority depression disparity and an 87% reduction in the disparity in anxiety between heterosexuals and sexual minorities. Relatedly,

[16] Appendix Table A3 documents coefficient estimates. Corresponding event study models for two-way fixed effects models are presented in Appendix Figure A1.

Hatzenbuehler, Keyes, and Hasin (2009) document that same-sex marriage bans were associated with a 36.6% increase in mood disorders among sexual minorities, while Raifman et al. (2018) identify that laws that permit the denial of services for sexual minorities are associated with a 46% increase in the incidence of mental distress among sexual minorities.

While the size of these effects from other studies imply that the effect of ADLs is much smaller than that of other LGBTQ+ policies it should be noted that these studies used data that identify either legally partnered sexual minorities or used data on sexual identity. As such, their data do not suffer from the bias towards zero present in my analysis, due to my approach to identify sexual minorities (see Section 4.2). To make comparisons more appropriate one can scale up the effect sizes of ADLs that I identify using the descriptive statistics provided in Table 1 from the BRFSS data covering the period 2014 to 2019, where respondents were asked their sexual identity (in a similar vein to Bullinger, 2019). Table 1 demonstrates that 33.51% of men in SSH's identify as non-heterosexual. As such, one can estimate the treatment-on-the-treated (TOT) by scaling the effect based on this value – i.e., multiplying the effect by 2.984 (1/0.3351)[17]. In this case my results indicate that ADLs reduced the incidence of poor mental health on the treated (i.e., non heterosexuals) by around 33% (or 1.370 days). This places my effect sizes as being much closer in size to the effect of other LGBTQ+ policies. Additionally, this implies that the effect of ADLs on mental health is close to the effect of other labor market policies on mental health. For example, Evans and Garthwaite (2014) document that EITC expansions reduce the number of poor mental health days among mothers by around 19%, while Bilgrami, Sinha, and Cutler (2020) estimate that paid parental leave leads to a 14% to 18.5% reduction in depression among mothers. While my point estimates are lower, and my scaled-up effects are larger, my standard errors encompass these effect sizes[18].

[17] It should however be noted that this may be an overestimate. As discussed in Section 4.2. one of the advantages of using SSH's to identify sexual minorities is that I do not rely on people having to self-identify as a sexual minority to interviewers, and prior work has demonstrated that survey mode may impact the likelihood of self-identification, which suggests that given the survey mode, a portion of people that do identify as a sexual minority will not report as such to BRFSS interviewers (for example, Dahlhamer, Galinsky, and Joestl (2019) highlights that there is substantial differences in the disclosure of sexual identity across survey mode in their review of the literature). It is for this reason that I focus largely on the reduced form estimates that are bias towards zero.

[18] It should be noted that my standard errors are large. However, my estimates are statistically significant, even at the 1% level, giving confidence that I can reject the null hypothesis. Furthermore, even if one focuses on the lower bound of the confidence interval (0.127) results suggest that ADLs improve the mental health of men in SSHs by at least 3.1%, which re-scaled, equates to at least a 9.25% reduction in the number of poor mental health days among male sexual minorities.

7. Robustness

As is usual with difference-in-differences analysis there are numerous potential threats to my identification strategy. In what follows I describe the results of several companion analyses designed to address the robustness of my core findings presented in Table 4 to confirm the credibility of these findings.

7.1. Model Choice

Given that the outcome variable in Table 4 (number of poor mental health days) is in fact a count variable the use of linear probability modelling to estimate the difference-in-differences equation may be inappropriate. As such, in Table 5 I present results from a series of analyses that use alternative, more suitable approaches to estimate the TWFE specification.

In Column 1 I use a Poisson model rather than OLS to estimate the effect of ADLs on the mental health of people in same sex households, given that Poisson models are more appropriate when using outcome variables that are count data. These results are presented for men in same-sex households in Panel A and women in same-sex households in Panel B. The results demonstrate that ADLs led to around a 10% reduction in the number of poor mental health days reported by men in same-sex households. The effect on women in same-sex households is negative, but statistically indistinguishable from zero. In Column 2 I use a zero inflated negative binomial model. While Poisson models deal better than OLS with count data, zero inflated negative binomial models, unlike Poisson models, account for an overdispersion at zero, which is indeed the case with the mental health data. Results from a zero-inflated negative binomial model are presented in Column 2 and are consistent with the results reported in Table 4 and Column 1 of Table 5: ADLs lead to around an 11% reduction in poor mental health days among men in same-sex households.

An additional model choice that may be of concern is the use of Sun and Abraham's (2021) approach to deal with the staggered timing of the policy. While results from this model are robust to time heterogeneity and negative weighting problems, results may still be biased when effects are heterogeneous across space. I provide results from two alternative estimators: de Chaisemartin and D'Haultfœuille's (2020) multiperiod estimator and Gardner's (2021) two-stage estimator in Figure 6. Both are robust to heterogeneity across space and over time. Event study estimates from these models are presented in Figure 6 in blue (de Chaisemartin and D'Haultfœuille, 2020) and orange (Gardner 2021) and accompanied by the Sun and Abraham (2021) estimates in black.

Broadly, results from both de Chaisemartin and D'Haultfœuille (2020) and Gardner's (2021) approaches follow extremely similar patterns to the results from the Sun & Abraham (2021) approach. For men, in the pre-period coefficients are statistically indistinguishable from zero. In the post-period coefficients become negative and statistically significant in several periods. For women, I document that coefficient's are statistically indistinguishable from zero throughout[19]. Broadly, these results confirm that my main findings are not driven by the biases present in TWFE models, nor are they driven by the choice of a Sun and Abraham (2021) model as opposed to other models that account for the biases in TWFE models.

7.2. Violations of the Parallel Trends Assumption

Results from event study models documented in Figures 5 and 6 indicate that the coefficient for the period t-2 is negative, with a large confidence interval, raising concerns regarding violations of the parallel trends assumption. It should be noted that these coefficients are statistically indistinguishable from zero, including at the 10% level, however, the pattern indicates a slight, albeit statistically insignificant negative pre-trend. To explore this in more detail and alleviate concerns regarding violation of the parallel trends assumption I report several additional estimations.

First, I test whether pre-policy coefficients are jointly significant following Borusyak, Jaravel, and Spiess (2021); F-tests of joint significance from TWFE event study models indicate that pre-policy coefficients are also jointly statistically indistinguishable from zero, and this remains true when both t-1 and t-2 are omitted, and the remaining pre-event dummies are tested for joint significance (Appendix Table A4).

Second, alternative modelling choices also lead to statistically insignificant pre-policy indicators. Following Freyaldenhoven, Hansen, and Shapiro (2019) I re-estimate the Sun & Abraham (2021) model, excluding t-1 and t-2 rather than t-1 and t-6+, coefficients for the remaining pre-period

[19] The only exception is a negative and statistically significant coefficient in period t+2 in Gardner's (2021) model.

indicators remain statistically indistinguishable from zero, even at the 10% level (Appendix Figure A2).

Finally, I apply Rambachan and Roth's (2022) "honest" differences-in-differences approach which relaxes the parallel trends assumption and provides sensitivity analyses of event study estimates. Their approach involves constructing confidence intervals that allow deviations from linearity, and in doing so estimates the amount of non-linearity that is allowable, while still rejecting the null hypothesis (this is referred to as the "breakdown" value of M). I present sensitivity estimates that allow for pre-policy deviations from a linear trend between the values $0 \geq M \leq 1$ in Appendix Figure A3. When ($M = 0$) only linear violations of parallel trends are allowed, while increasing values of M relate to greater deviations from linearity. These sensitivity estimates provide evidence that imposing linear parallel trends yields an estimate that is negative and statistically significant ($M = 0$) and this remains the case with increasing non-linear violations. My results indicate that the treatment effect for men in same-sex households is robust to non-linearity of differential trends equal to $M = 0.40$, which is equal to around half of the average change in slope in the pre-treatment period[20]. An alternative interpretation of these results is that the "breakdown" value is equivalent to allowing violations that are equivalent to around 80% of the standard error of the coefficient of interest (0.480). Broadly, similar patterns are observed when I impose that non-linear trends be positive or negative (Appendix Figure A4) and in fact these impositions lead to larger allowable deviations from a linear trend.

While the slight negative pre-trend documented at t-2 in the pre-policy period in Figures 5 and 6 may introduce concerns regarding violations of the parallel trends assumption, it should once again be reiterated that these coefficients are statistically indistinguishable from zero, even at the 10% level. Furthermore, the additional sensitivity estimates provided in this section provide evidence that the slight negative trend in the pre-policy period documented in Figures 5 and 6 is unlikely a sizeable threat to a causal interpretation of the effect of ADLs on the mental health of men in same-sex households. Furthermore, even if it was, one can still observe large negative associational changes.

[20] I calculate that the average change in slope in the pre-treatment period is 0.744.

7.3. Distributional Analysis

While results from Table 4 document the effect of ADLs on the average number of poor mental health days among people in SSH's, these results are unable to give any indication as to where in the distribution of mental health these reduction in poor mental health days occur, i.e., are these reductions at the mean driven by fewer people moving from some poor mental health days to zero poor mental health days, or a reduction in the quantity of poor mental health days among people with a larger quantity of poor mental health days prior to the policy enactment. To explore this in more detail I re-estimate my primary Sun and Abraham (2021) specification but use a series of indicator variables that take the value 1 if the individual reports having more poor mental health days than several thresholds. These results are provided in Table 6.

In practice, I use 6 different thresholds: over zero poor mental health days (Column 1), 7 or more poor mental health days (Column 2), 14 or more poor mental health days (Column 3), 21 or more poor mental health days (Column 4), 28 or more poor mental health days (Column 5), and 30 poor mental health days (Column 6). The results presented in Table 6 demonstrate that there is a significant reduction in the quantity of poor mental health days, rather than a shift from having any poor mental health days to zero poor mental health days. Results demonstrate that the reductions documented for men in SSH's in Table 4 are driven by reductions at the bottom and middle portions of the distribution. There is around a 2.5 percentage point reduction in the proportion of men in SSH's reporting 1 week or more poor mental health days following the enactment of an ADL or reporting 2 weeks or more poor mental health days following the enactment of an ADL, and around a 1.5 percentage point reduction in the proportion of men in SSH's reporting 3 week or more poor mental health days following the enactment of an ADL. This translates to around a 14% reduction in the proportion of men in SSH's that have at least 7 days of poor mental health and around an 18% and 20% reduction in the proportion of men in SSH's that have at least 14 or 21 days of poor mental health, respectively.

Importantly, the CDC (2004) defines frequent mental distress as having poor mental health for 14 in the past 30 days. Prior work has demonstrated that frequent mental distress (according to this definition) is associated with adverse health outcomes, including diagnoses of mental disorders (Cree et al. 2020; Slabaugh et al. 2017), and indeed, 14 or more days of self-reported poor mental health is used as a threshold to diagnose mental health disorders such as depression (NIH 2022)[21]. The results therefore indicate that ADLs substantially reduce the incidence of frequent mental distress (which can be used to proxy depression) among men in SSH's. In fact, according to this threshold, ADLs reduce the incidence of frequent mental distress by around 18% for men in SSH's. In line with my main findings results for women in SSH's are statistically indistinguishable from zero.

7.4. Difference-in-Difference-in-Differences Model

Next, I provide results from a triple-difference specification in Table 7. A triple difference model allows me to account for all unobservable factors at the state by year, state by same-sex household, and year by same-sex household level. I estimate models that compare the incidence of poor mental health days among people in SSH's to the incidence of poor mental health days among people in DSH's, in states that pass an ADL compared to those that do not, following the passage of an ADL. Following Olden and Møen (2022) I estimate a triple difference model within a difference-in-differences framework facilitated by replacing the outcome variable and all covariates with the differential between people in SSHs and DSHs,[22] allowing me to presents triple difference results from a Sun and Abraham (2021) model. These results are presented in Table 7 for men in Panel A and women in Panel B. Results are consistent with the main results documented in Table 4, though the magnitude of the coefficients is slightly smaller. In triple difference models I document that ADLs led to around a third of a day reduction in the number of poor mental health days reported by men in same-sex households. Given the pre-policy difference in poor mental health between men in SSH's and men in DSH's of 1.703 days, this equates to ADL reducing the mental health disparity between men in SSH's and men in DSH's by around 18%. For women, coefficients are statistically indistinguishable from zero.

7.5. Other Sensitivity Tests

Finally, my results are robust to several additional robustness and sensitivity tests. As aforementioned, my results pass standard covariance balance tests (Table 2). Furthermore, Appendix Table A1 demonstrates that the policy cannot be predicted by the proportion of people in SSH's at the state by

[21] The ICD–10 states that patients must have several symptoms (including depressed mood) for at least two weeks to make a diagnosis (unless symptoms are particularly severe). The DSM requires that patients have several symptoms including at least one of the following: depressed mood or loss of interest, for at least two weeks to make a diagnosis (NICE Clinical Guidelines 2017).

[22] See Olden and Møen (2022) for a more extensive discussion of this approach.

year level, nor is there a change in the proportion of people in SSH's following the passage of an ADL, ruling out migration effects. Appendix Figures A5 and A6 report compositional balance tests that estimate the effect of ADLs on a number of demographic characteristics for men and women in SSH's, respectively. These results, paired with the evidence of a null effect of ADLs on the proportion of people in a SSH presented in Table A1, indicate that it is unlikely that my results are driven by compositional changes[23].

In addition, results presented in Table 8 demonstrate that my TWFE results are robust to the use of wild bootstrapped standard errors (Column 1), and to permutation testing (Column 2). Estimates from Sun and Abraham (2021) models are robust to the inclusion of region specific linear time trends (Column 3), or state-specific linear pre-trends (Column 4).

As aforementioned, the mental health question is included in every year in the BRFSS, however, in the 2002 wave of the BRFSS the mental health question was administered as part of the optional module rather than being included in the core questionnaire, as it was in all other years. As such, several states (29) did not administer the mental health question to respondents in 2002. Results presented in Column (5) of Table 8 indicate that excluding 2002 from my analysis does not significantly change my point estimate (or it's associated statistical significance). In fact, this exclusion increases the magnitude of my point estimates.

Finally, my results are fairly homogenous across treatment states. In Appendix Tables A5 and A6 I present results where I drop one of the treated states at a time, for men and women, correspondingly; coefficients remain stable and consistent across these specifications. Relatedly, Burn (2018) demonstrates that there are heterogeneous effects of ADLs depending on the strength of the law. Burn (2018) finds that laws with compensatory damage provisions had a greater impact on the earnings of gay men than those without compensatory damages, but laws that additionally included punitive damages reduced the magnitude of the effect of ADLs on wages. In results presented in Appendix

[23] Coefficients are statistically insignificant at the 5% level in all cases in Table A1 and Figures A1 and A2. The only exception to this is that there is a small reduction of the proportion of women in SSH's that are white following the passage of an ADL.

Table A7 I demonstrate that there is no evidence of differential effects dependent on damages covered by ADLs[24].

8. Mechanisms

In this section I provide evidence from three plausible mechanisms that could explain the effect of ADLs on the mental health of men in same-sex households. More explicitly, I test the effect of ADLs on the labor market outcomes of sexual minorities, the effect on the health insurance coverage of sexual minorities, and the effect of ADLs on the incidence of prejudice and discrimination. My results indicate that the effect of ADLs on mental health are likely driven by changes in prejudice and discrimination rather than changes in health insurance coverage or labor market outcomes.

8.1. Labor Market Outcomes

Anti-discrimination laws aim to reduce discrimination in the workplace by prohibiting discriminatory practices. As such, it may be the case that the improvements in mental health that I document in the main analysis are driven by changes in employer discriminatory practices which lead to improvements in the labor market outcomes for sexual minorities, given the extensive work linking labor market outcomes and income to mental health and wellbeing (Clark et al. 2008; Ettner et al. 1997; Gardner and Oswald 2007; Lindqvist et al. 2020; Theodossiou 1998).

In Figure 7 I estimate the effect of ADLs on the labor market outcomes of people in SSHs, using Sun and Abraham (2021) dynamic models. Panel A reports results for employment and Panel B reports results for household income. Results for men in SSH's are reported in Black, while results for women in SSH's are reported in Blue.

Results presented in Figure 7 demonstrate that ADLs do not significantly impact the labor market outcomes of people in SSH's. For both men and women coefficients are statistically insignificant in both the pre- and post-policy period, for both employment and household income[25]. Unlike my

[24] The coefficient for ADLs in this regression remains negative but loses statistical significance. This is likely driven by a loss of power due to the inclusion of additional interactions across policy coverage. Nonetheless, the main coefficient remains negative and of similar magnitude to my main results in Table 4.

[25] In additional results presented in Appendix Figure A7 I also explore whether ADLs change job mobility using data on same-sex couples from the Current Population Survey (CPS) (1995 to 2019) and responses to questions regarding whether the individual has the same employer as last month and the number of employers an individual had in the

results, prior work has documented that ADLs have small positive impacts on male sexual minorities and small negative effects on female sexual minorities (see *inter alia*: Martell, 2013; Burn, 2018; Delhommer, 2020). However, the way that I identify sexual minorities leads to a bias towards zero, as aforementioned. As a result, these small changes that others have documented are not visible in my zero-biased results.

Prior work estimates that ADLs increase employment by around 1 percentage point and increase wages by around 2-3% among men in SSC's (Delhommer 2020). However, using the scaling procedure discussed earlier highlights that it is unlikely that I would be able to detect effects of this size (for example, multiplying the employment change (0.014) estimated by Delhommer (2020) by the proportion of men in SSHs that identify as non-heterosexual (33.51%) would imply a coefficient size of 0.005 following ADLs to be detectable in my regressions. Furthermore, given the magnitude of previous estimates it is unlikely that these results could explain the large improvements in mental health; focusing on the employment change this would mean that the 11% change in mental health would reflect around a 0.5 percentage point change in employment when scaled down to reflect the bias present in my data. Put differently, taking the literatures estimates of the effect of ADLs on the labor market outcomes of sexual minorities and given my scaled-up estimates, this would imply that a 2-3% change in earnings and a 1 percentage point increase in employment equate to around 30% improvement in mental health. It is therefore unlikely that labor market changes following ADLs can fully explain the gains in mental health that I document.

8.2. Access to Healthcare

Alternatively, the impact of ADLs on mental health may be driven by increased access to health care and medicine due to these policies requiring that employers provide the same benefits to sexual minorities as they do to other workers. If employers offer these benefits at differential rates pre-ADLs, then these policies may help to reduce the sexual orientation based health insurance gap (Ash and Badgett 2006; Gonzales and Henning-Smith 2017; Gonzales et al. 2021), and in turn this may be driving my core findings.

preceding year. Results from these models demonstrate that there is no change in the job mobility of people in same-sex couples following the passage of an ADL.

Figure 8 reports Sun and Abraham (2021) event study models that analyze the effect of ADLs on the health insurance coverage of people in SSH's. Coefficients for men are reported in black, while the female coefficients are reported in blue. Broadly, these results indicate that I cannot reject the null hypothesis regarding whether ADLs effect the health insurance coverage of people in SSH's; coefficients are small and statistically insignificant for both men and women in both the pre- and post-policy period.

8.3. Changes in Discrimination

Finally, prior work has demonstrated that other LGBTQ+ policies led to significant reductions in discriminatory attitudes and behaviors (Aksoy et al. 2020; Blasco et al. 2021; Nikolaou 2022; Sansone 2019) and indeed, some papers have found evidence that anti-discrimination laws improve attitudes towards sexual minorities (Deal 2022; Delhommer 2020). Even if ADLs do not directly change labor market outcomes or benefits for sexual minorities, they may have an impact on discriminatory attitudes and behaviors. If this is the case it is likely that this will have a large impact on mental health, and therefore may be a key mechanism in explaining my core findings. Indeed, prior work has demonstrated that experiences of discrimination and animosity can have serious mental health consequences (Meyer, 2003; Johnston and Lordan, 2012; Perales and Todd, 2018; Hole and Ratcliffe, 2020; Armijos Bravo and Vall Castelló, 2021). Furthermore, Meyer (2003) posits in minority stress theory that the mental health disparity between heterosexuals and sexual minorities is likely driven by experiences of and expectations of discrimination among sexual minorities. More precisely, minority stress theory proposes that the sexual minority mental health disparity arises due to homophobia, which results in harassment, discrimination and victimization, as well as expectations of these. As such, reductions in discrimination may improve the mental health of sexual minorities, and over time reduce expectations of discrimination, in turn, having positive long run effects on mental health.

In what follows I present a collage of evidence that the rollout of state ADLs led to reductions in discrimination towards sexual minorities. ADLs aim to reduce discrimination and improve the workplace environment for sexual minorities, as such, I first estimate the effect of ADLs on workplace climate using data from the Human Rights Campaign's Corporate Equality Index (CEI). Second, I estimate the effect of ADLs on hate crimes towards sexual minorities using data from the NIBRS of the FBI's Uniform Crime Reporting program. Finally, I make use of data from google trends to demonstrate that ADLs reduced sexual orientation based animosity, primarily driven by reductions in animosity towards gay men.

8.3.1. Workplace Climate

In Figure 9 I estimate the effect of ADLs on firm level CEI scores in a Sun and Abraham (2021) event study model where I compare the CEI scores of firms that are headquartered in states that have an ADL to the CEI scores of firms headquartered in states that do not have an ADL, before and after the passage of an ADL.

Figure 9 demonstrates that ADLs significantly improve the coverage of LGBTQ+ supportive firm level policies and practices. Pre-policy coefficients are statistically indistinguishable from zero. In the post-policy period coefficients are positive and statistically significant. To put the magnitude of these effects into perspective, firms headquartered in states that have an ADLs have CEI scores that are around 5 points higher in the first and second year after the passage of an ADL compared to years prior to the policy. This is equal to the score associated with introducing LGBTQ+ diversity training or introducing a workforce protection policy (5 points each). As such, the effect of ADLs on workplace policies and practices may also have positive spillover effects. If ADLs increase the coverage of diversity training, for example, then ADLs may have downstream effects whereby the passage of an ADL increases awareness of LGBTQ+ diversity and increases LGBTQ+ visibility in the workplace, which in turn may spillover to everyday life practices, including changing the behavior of employees both in and out of the workplace.

8.3.2. Hate Crimes

Next, I use data from the NIBRS of the FBI's Uniform Crime Reporting program to estimate the effect of the passage of an ADL on the incidence of LGBTQ+ motivated hate crimes. In Figure 10 I present event study models that estimate the effect of ADLs on the hate crime rate. Results presented in Figure 10 demonstrate that ADLs led to significant reductions in hate crimes motivated by LGBTQ+ status[26]. Prior to the introduction of an ADL coefficients are statistically indistinguishable from zero. However, following the passage of an ADL there are significant reductions in the incidence of LGBTQ+ motivated hate crimes, with negative and statistically significant coefficients detected.

[26] Unfortunately, I am unable to distinguish across LGBTQ+ identities given that bias motivations are often listed as LGBTQ+ rather than a specific sexual or gender identity.

Coefficients indicate that in the first year there is around a 0.230 per 100,000 people reduction in the number of LGBTQ+ motivated hate crimes. Given the baseline mean of 0.632 per 100,000 people this is equivalent to around a 36% reduction in LGBTQ+ motivated hate crimes in the year that the policy was introduced.

Additionally, results presented in Appendix Table A8 demonstrate that these changes in hate crimes are not experienced by other minorities, i.e., there are no significant changes in the incidence of gender, racial, ethnicity, or religiously motivated hate crimes following the passage of sexual orientation based ADLs. Broadly, these results indicate that anti-discrimination laws led to substantial reductions in violence towards LGBTQ+ persons.

8.3.3. Google Trends

Finally, I exploit google trends data to estimate the effect of ADLs on animosity towards sexual minorities. Event study results that plot the dynamic effects of ADLs on google search intensity are reported in Figure 11. Results are reported for search intensity for the term Faggot in Panel A, the term Dyke in Panel B, the term Sodomy in Panel C, and the term Leviticus in Panel D.

Given that Faggot and Dyke are the two terms that are derogatory slurs the comparison of these two affords an analysis of whether ADLs differentially changes animosity towards gay men and lesbian women: the results presented in Figure 10 suggest that this is the case. In Panel A coefficients for the search term faggot are statistically indistinguishable from zero in the pre-policy period. However, in the post policy period, coefficients are negative and statistically significant. Furthermore, they remain negative and statistically significant as long as five years after the passage of the policy, indicating long-run rather than transitory changes in the search intensity for the word Faggot. This is not the case for animosity towards lesbian and bisexual women, captured by search intensity for the word Dyke, in Panel B. In both pre- and post-policy periods coefficients are close to zero and statistically insignificant. This suggests that ADLs do not have a significant impact on animosity towards lesbian women and bisexual women.

Panels C and D report results for search intensity for the terms Sodomy and Leviticus, correspondingly. Results presented in Panels C and D indicate that there is no significant effect of

ADLs on the search intensity of these terms[27]. However, it may be the case that one does not observe changes in these terms as these terms continue to be searched for, for non-animosity-based reasons – i.e., due to their religious, rather than homophobic connotations. It may therefore be that most searches for these two words (in both the pre- and post-policy period) do not reflect animosity, and therefore I am unable to capture changes in those searches that were animus related, for example, within the 27 chapters in Leviticus only one discusses sexual behavior, thus many searches may be unrelated to sexual orientation or behavior. On the other hand, searches for the slurs Faggot or Dyke, are unlikely to be non-animosity related.

Broadly, the results presented in this section provide evidence that changes in discrimination may explain the improvements in mental health among men in same-sex households related to the passage of ADLs documented in Table 4. First, results suggest that ADLs spur organizations to improve workplace climates, increasing their coverage of workplace policies and practices, and therefore improving the workplace environment for sexual minorities, and increasing LGBTQ+ visibility. Second, I demonstrate that there is a substantial reduction in the incidence of LGBTQ+ motivated hate crimes following the passage of an ADLs which, given the large literature related to violence, discrimination, and mental health, is likely to have a positive impact on the mental health of sexual minorities. Finally, my results indicate that there is a substantial reduction in animosity towards sexual minorities, as proxied by google searches. These findings accompanied with prior work that has documented that ADLs improve attitudes towards sexual minorities (Deal 2022; Delhommer 2020) imply that ADLs substantially reduce prejudice, discrimination, and violence towards sexual minorities. Additionally, I provide suggestive evidence that these reductions in animosity are driven by reductions towards gay and bisexual men rather than lesbian and bisexual women. While I am unable to rule out a lack of power for the statistically insignificant results documented in Table 4 for women in SSH's (see the weaker identification of sexual minority women when using this approach in Table 1), it may be the case that these statistically insignificant effects are in fact drive by ADLs having differential effects on animosity towards gay men and lesbian women.

[27] Results for sodomy are however negatively trending and the coefficient for t+4 is statistically significant.

9. Conclusions

This paper exploits geographic and temporal variation in the coverage of anti-discrimination laws for sexual minorities to document the causal effect of ADLs on the mental health of sexual minorities for the first time. My results demonstrate that the passage of an ADL leads to around an 11% reduction in the number of poor mental health days among men in same-sex households, while for women in same-sex households I find that effects are statistically indistinguishable from zero. Broadly, these results demonstrate that employment protection policies can help to reduce the mental health disparity for sexual minorities and that recent federal rulings such as *Bostock vs Clayton County* may be key in reducing the mental health disparity. Furthermore, my results imply large positive economic effects of anti-discrimination laws, given the substantial direct and productivity costs associated with poor mental health.

In addition to studying the effect of ADLs on mental health I explore three potential mechanisms. First, I show that the effect of ADLs on labor market outcomes and health insurance coverage are unlikely able to explain the core finding that ADLs improve mental health. Second, I document the effect of anti-discrimination laws on sexual minority based hate crimes, firm-level climate, and animosity. Using firm-level data I demonstrate that ADLs increase firm-level coverage of LGBTQ+ policies and practices. Using data on hate crimes I demonstrate that anti-discrimination laws significantly reduce the incidence of sexual orientation motivated hate crimes. Using google trends data I demonstrate that anti-discrimination laws led to a substantial reduction in animosity towards gay men, but that ADLs do not significantly impact animosity towards lesbians. These results imply that ADLs substantially reduce discrimination and violence towards sexual minorities.

Broadly, my results indicate that equality policies, especially anti-discrimination laws can have positive mental health effects, and I use data on sexual minorities to document the magnitude of these effects. This is the first analysis of the causal effect of anti-discrimination laws on mental health. While prior work has explored the effect of ADLs on unemployment spells and wage disparities these analyses fail to consider the nuanced forms that discrimination may take. My results build on this work by documenting that ADLs are effective in reducing discrimination and in turn this leads to substantial improvements in mental health. Future work that can this further, through explore for example alternative outcomes (such as more objective health measures) as well as alternative impacted groups (such as racial minorities or women) will be fruitful additions to the literature and will be helpful to policy makers looking to understand the impact of equality policies on the people that these laws protect. Indeed, the generalizability of these findings to other minority groups is likely questionable given the mechanisms that best explain my results, and the weak effects that these laws have on the labor market outcomes of sexual minorities. However, similar laws for other minority groups have been demonstrated to have much larger labor market effects, for example, Bailey et al. (2022) finds that gender based equal pay laws increase the wages of women by 4 to 12%. It may therefore be the case that these laws have an even larger effect on the health outcomes of women given their much larger direct effects, which are not apparent in the case of sexual minorities.

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Tables

Table 1: Household Structure and Sexual Identity

	(1)	(2)
	SSH	DSH
Panel A: Male		
Non-Heterosexual	33.51%	1.18%
Gay man	29.95%	0.31%
Bisexual or Other	3.56%	0.87%
Panel B: Females		
Non-Heterosexual	16.67%	1.47%
Lesbian	12.78%	0.12%
Bisexual or Other	3.88%	1.35%

Notes: Raw means and percentages. Source: BRFSS (2014-2019). This sample includes the full sample of people aged 25-64 from states that included the SOGI optional module at least once during the period 2014 and 2019. Non-heterosexual includes people that responded as lesbian, gay, bisexual, or other.

Table 2: Covariance Balance Test

	(1)	(2)
	Treatment	Control
Panel A: Males		
Bad Mental Health Days	3.020	2.989
Age	37.743	37.348
Non-White	0.061	0.043
Married	0.078	0.118
Has Children	0.082	0.139**
BA or +	0.367	0.310
Some College	0.245	0.254
High School	0.306	0.321
Panel B: Females		
Bad Mental Health Days	4.345	4.415
Age	41.594	41.775
Non-White	0.044	0.042
Married	0.046	0.055
Has Children	0.349	0.428***
BA or +	0.285	0.254
Some College	0.315	0.292
High School	0.319	0.309

Notes: Raw means. Source: BRFSS (1993) Sample includes all landline respondents in a same-sex household between the ages 25 and 64. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Summary Statistics

	(1)	(2)
	Ever introduced a state ADL	Never introduced a state ADL
Panel A: Males		
Bad Mental Health Days	3.865	4.156***
Observations	*26,107*	*19,746*
Panel B: Females		
Bad Mental Health Days	5.137	5.718***
Observations	*53,320*	*49,218*

Notes: Raw means. Source: BRFSS (1993-2019) Sample includes all landline respondents in a same-sex household between the ages 25 and 64. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Anti-Discrimination Laws and Poor Mental Health

	(1) No Controls	(2) + Individual Controls	(3) + State Controls	(4) Sun & Abraham (2021)
Panel A: SSH Males				
ADL	-0.440**	-0.374**	-0.474**	-0.459***
	(0.165)	(0.167)	(0.210)	(0.164)
Demographic Controls	X	✓	✓	✓
State Level Controls	X	X	✓	✓
Pre-Policy Mean	4.095	4.095	4.095	4.095
%Δ	10.74%	9.13%	11.57%	11.21%
Observations	*45,853*	*45,853*	*45,853*	1,131
Panel B: DSH Males				
ADL	-0.123	-0.098	-0.046	-0.042
	(0.085)	(0.084)	(0.064)	(0.063)
Demographic Controls	X	✓	✓	✓
State Level Controls	X	X	✓	✓
Pre-Policy Mean	2.392	2.392	2.392	2.392
%Δ	5.14%	4.10%	1.92%	1.76%
Observations	940,909	940,909	940,909	1,131
Panel C: SSH Females				
ADL	-0.352***	-0.267**	-0.154	-0.081
	(0.124)	(0.118)	(0.152)	(0.262)
Demographic Controls	X	✓	✓	✓
State Level Controls	X	X	✓	✓
Pre-Policy Mean	5.641	5.641	5.641	5.641
%Δ	6.24%	4.73%	2.73%	1.44%
Observations	*102,538*	*102,538*	*102,538*	1,131
Panel D: DSH Females				
ADL	-0.085	-0.057	0.115	-0.045
	(0.113)	(0.112)	(0.081)	(0.094)
Demographic Controls	X	✓	✓	✓
State Level Controls	X	X	✓	✓
Pre-Policy Mean	3.570	3.570	3.570	3.570
%Δ	2.38%	1.60%	3.22%	1.26%
Observations	1,316,785	1,316,785	1,316,785	1,131

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010. Demographic controls: education, age, race, ethnicity, marital status, and parenthood. State level controls: same-sex marriage legalization, domestic partnerships laws, civil union laws, LGBT hate crime laws, LGBT health non-discrimination laws, sodomy laws, log population, labor force participation rate, and unemployment rate. Columns 1, 2, and 3 are based on TWFE models. Column 4 presents results from a Sun & Abraham (2021) model. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Anti-Discrimination Laws and Poor Mental Health: TWFE Other Models

	(1)	(2)
	Poisson	ZINB
Panel A: SSH Males		
ADL	-0.098*	-0.108*
	(0.055)	(0.059)
Observations	*45,853*	*45,853*
Panel B: SSH Females		
ADL	-0.026	-0.051
	(0.030)	(0.037)
Observations	*102,538*	*102,538*

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Column 1 reports results from a TWFE Poisson model. Column 2 reports results from a TWFE zero-inflated negative binomial model. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Anti-Discrimination Laws Acts and Poor Mental Health: Distributional Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
	Any	1 Week +	2 Weeks +	3 Weeks +	4 Weeks +	30 Days
Panel A: SSH Males						
ADL	-0.010	-0.024***	-0.023***	-0.016**	-0.008	-0.007
	(0.009)	(0.008)	(0.007)	(0.006)	(0.006)	(0.006)
Pre-Policy Mean	0.347	0.168	0.124	0.078	0.069	0.066
%Δ	2.88%	14.29%	18.55%	20.51%	11.59%	10.61%
Observations	1,131	1,131	1,131	1,131	1,131	1,131
Panel B: SSH Females						
ADL	0.018	-0.006	-0.004	-0.004	-0.004	-0.004
	(0.016)	(0.013)	(0.012)	(0.007)	(0.007)	(0.007)
Pre-Policy Mean	0.451	0.237	0.170	0.102	0.092	0.088
%Δ	3.99%	2.53%	2.35%	3.92%	4.35%	4.54%
Observations	1,131	1,131	1,131	1,131	1,131	1,131

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Each Panel and Column reports results from a Sun & Abraham (2021) model. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Anti-Discrimination Laws Acts and Poor Mental Health: Triple Difference Model

	(1)
	Number of Bad Mental Health Days
Panel A: Males	
ADL x SSH	-0.313*
	(0.158)
Pre-Policy Difference	1.703
%Δ	18.38%
Observations	*1,131*
Panel B: Females	
ADL x SSH	-0.148
	(0.091)
Pre-Policy Difference	2.071
%Δ	7.15%
Observations	*1,131*

Notes: All specifications include state and year fixed effects. Outcome variables and all controls equal the difference between people in DSH's and SSH's, following Olden & Møen (2022). Each Panel and Column reports results from a Sun & Abraham (2021) model. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

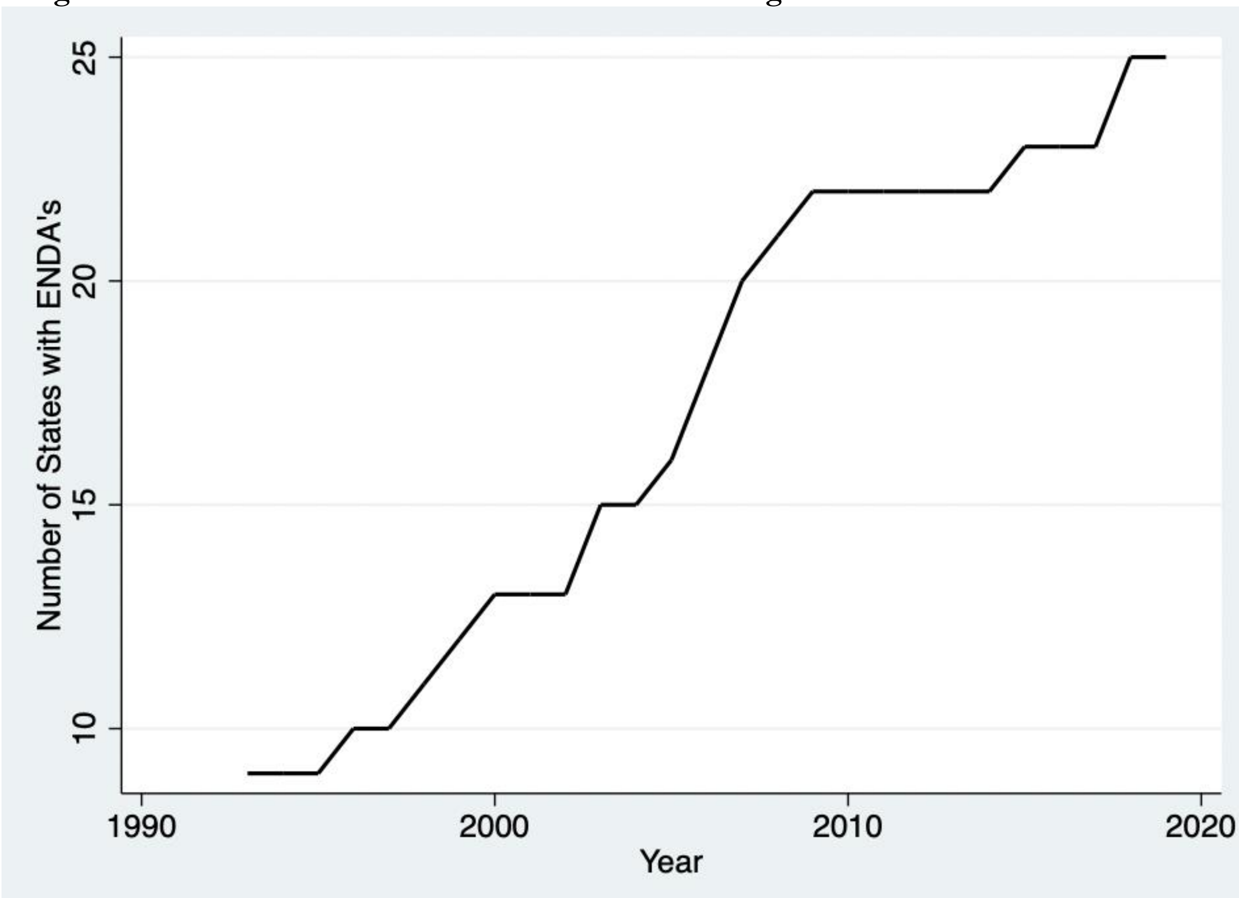
Table 8: Anti-Discrimination Laws and Poor Mental Health: Other Sensitivity Tests

	(1)	(2)	(3)	(4)	(5)
	Wild Bootstrap	Randomization Inference	Region Specific Linear Time Trends	State Specific Linear Pre-Trends	Excluding 2002
Panel A: SSH Males					
ADL	-0.474*	-0.474*	-0.358**	-0.493***	-0.544***
	(-1.039, 0.005)	(0.076)	(0.166)	(0.163)	(0.160)
Panel B: SSH Females					
ADL	-0.425	-0.154	-0.179	-0.063	-0.207
	(-0.482, 0.256)	(0.466)	(0.230)	(0.262)	(0.264)

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Columns 1 and 2 are based on TWFE models. Columns 3, 4, and 5 are based on Sun & Abraham (2021) models. Column 1 presents confidence intervals from wild bootstrapped standard errors in parantheses. Column 2 presents permutation adjusted p-values in parathesis. In all other columns paratheses denote clustered robust standard errors $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

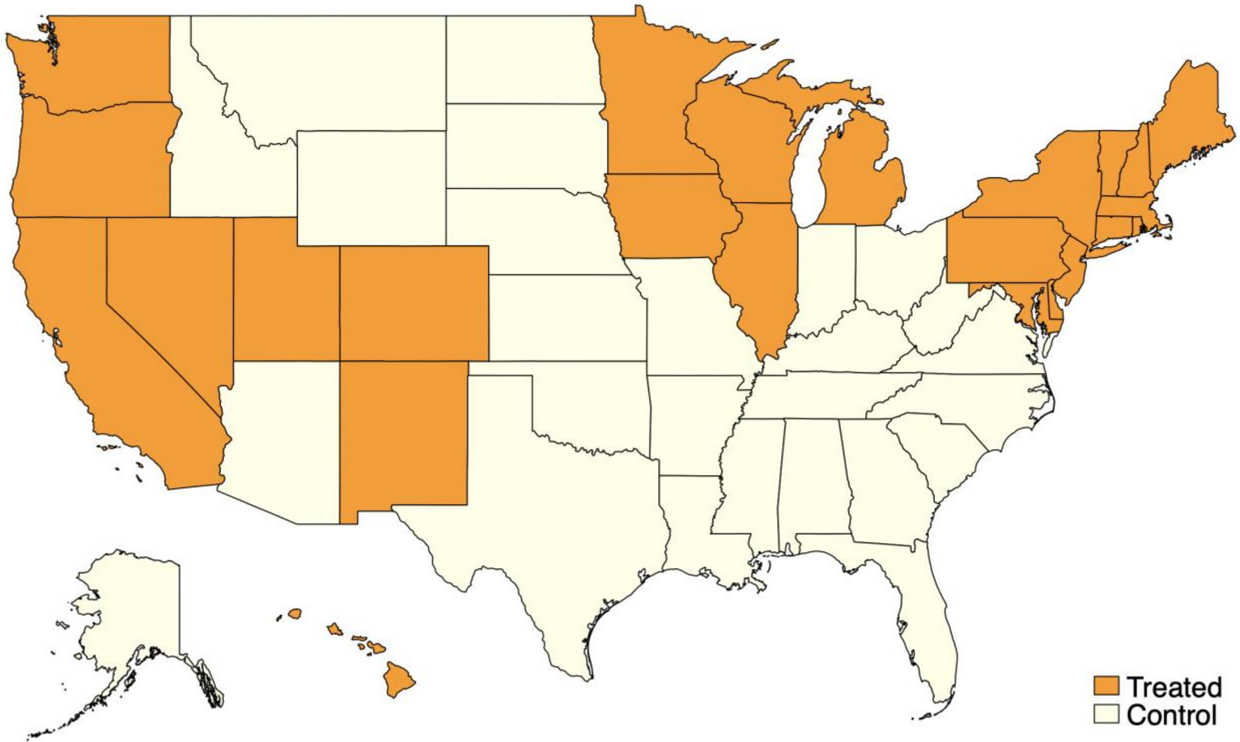
Figures

Figure 1: The Evolution of Anti-Discrimination Coverage in The United States 1993-2019.



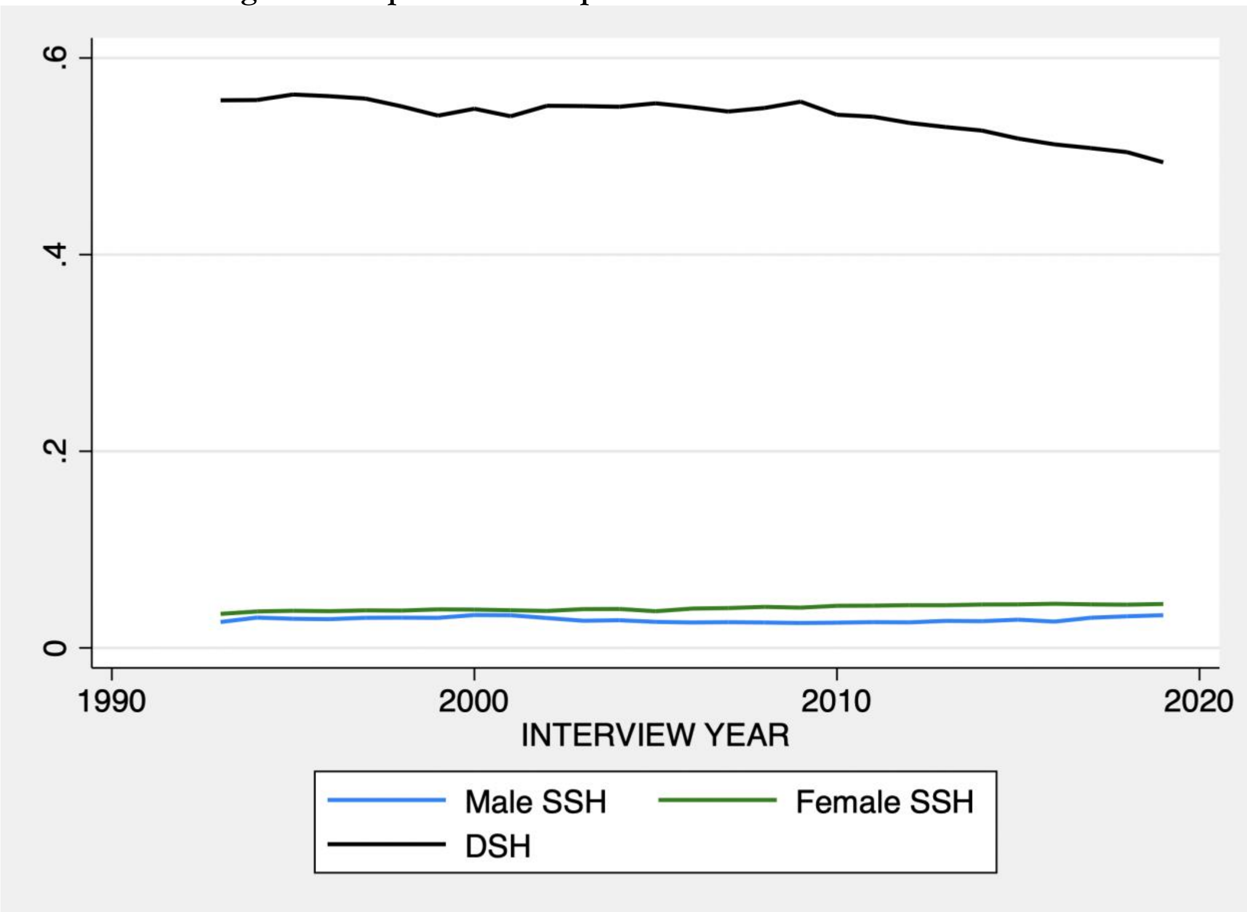
Notes: Counts of states with ADLs. Source: Movement Advancement Project

Figure 2: Geographic Coverage of ADLs 1993-2019.



Notes: Source: Movement Advancement Project

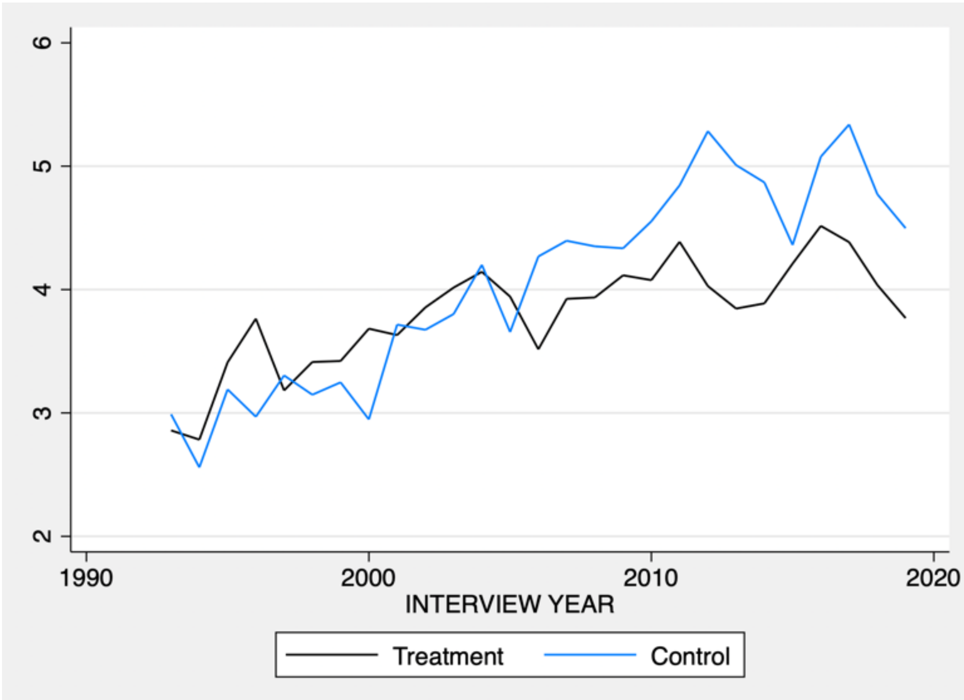
Figure 3: Proportion of People in SSH's and DSH's over time



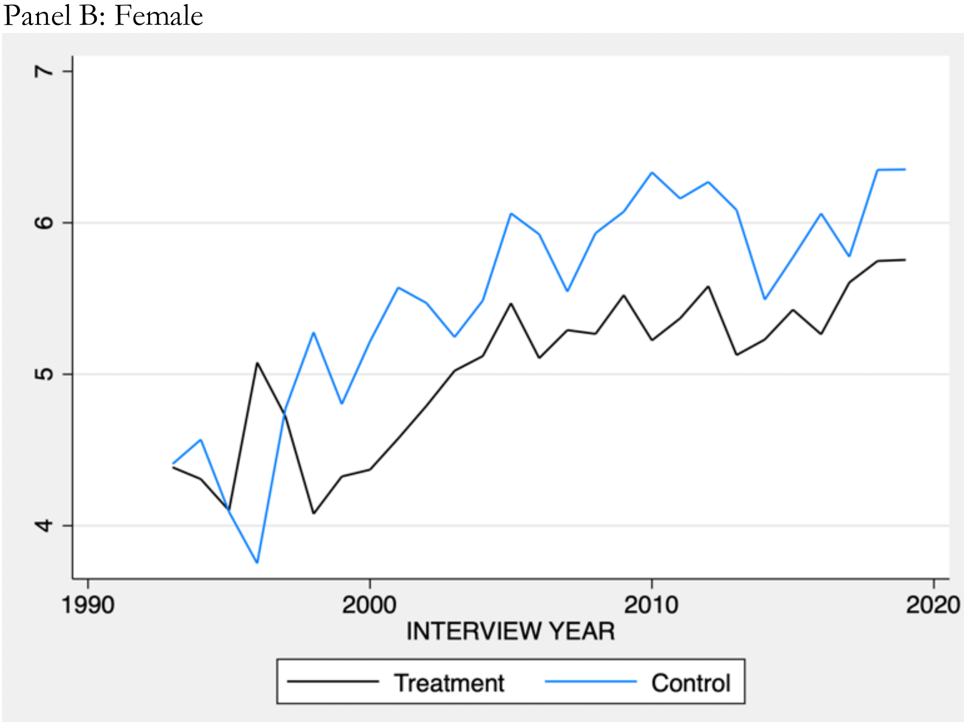
Notes: Raw Percentages. Sample includes all respondents aged 25 to 64. Source: BRFSS (1993-2019)

Figure 4: Anti-Discrimination Laws and Poor Mental Health: Descriptive Trends:

Panel A: Male



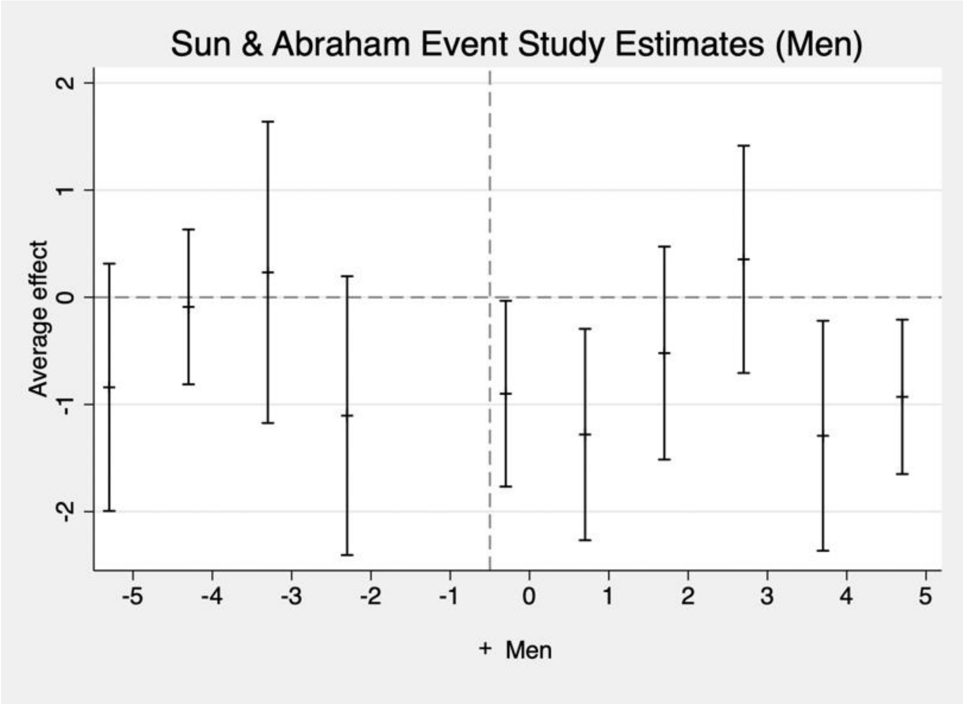
Panel B: Female



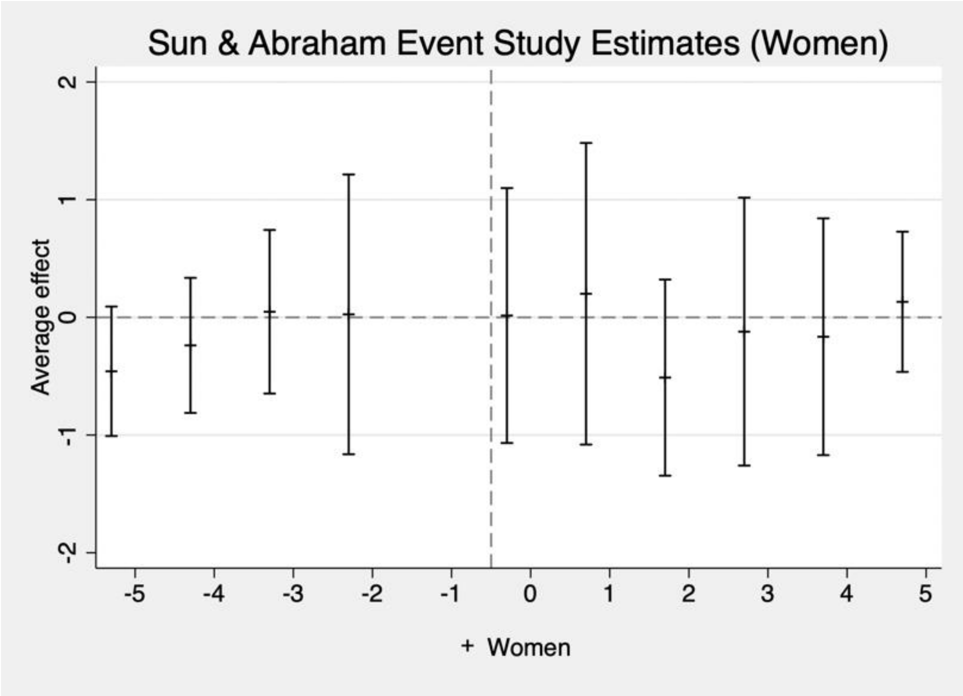
Notes: Raw means. Sample includes all respondents in SSHs aged 25-64. Source: BRFSS (1993-2019).

Figure 5: Anti-Discrimination Laws and Poor Mental Health: Event Studies:

Panel A: Males



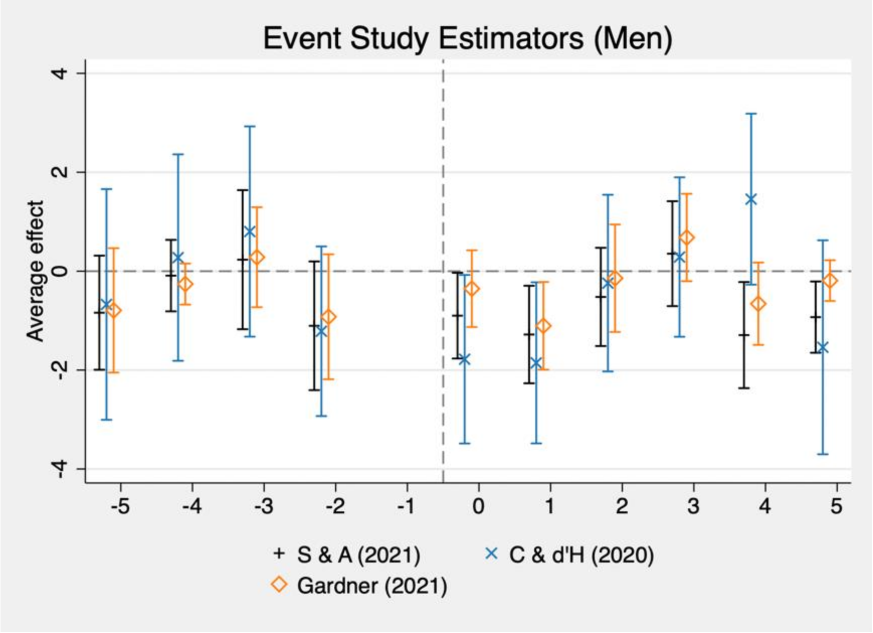
Panel B: Females



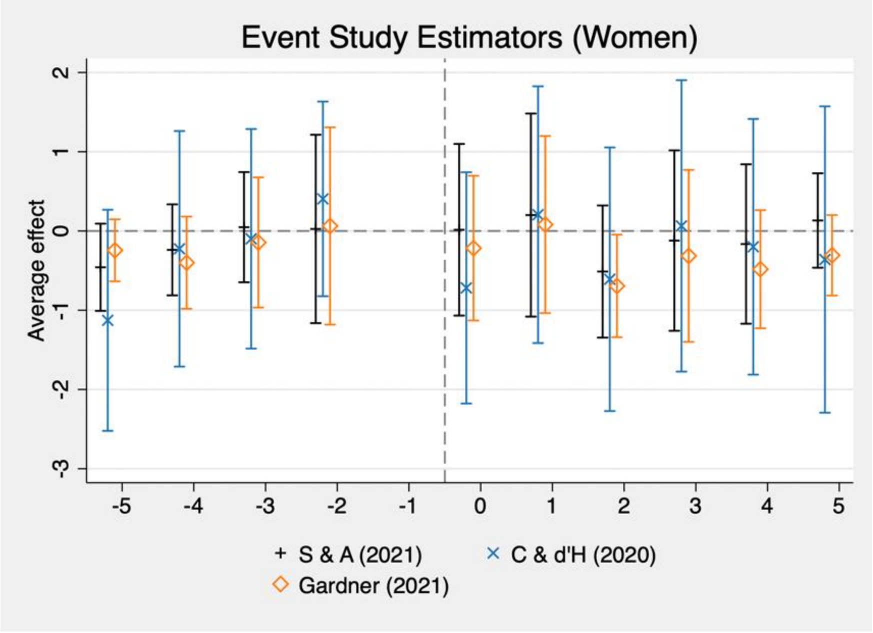
Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Bars represent 5% confidence intervals.

Figure 6: Anti-Discrimination Laws and Poor Mental Health: Other Models

Panel A: Males



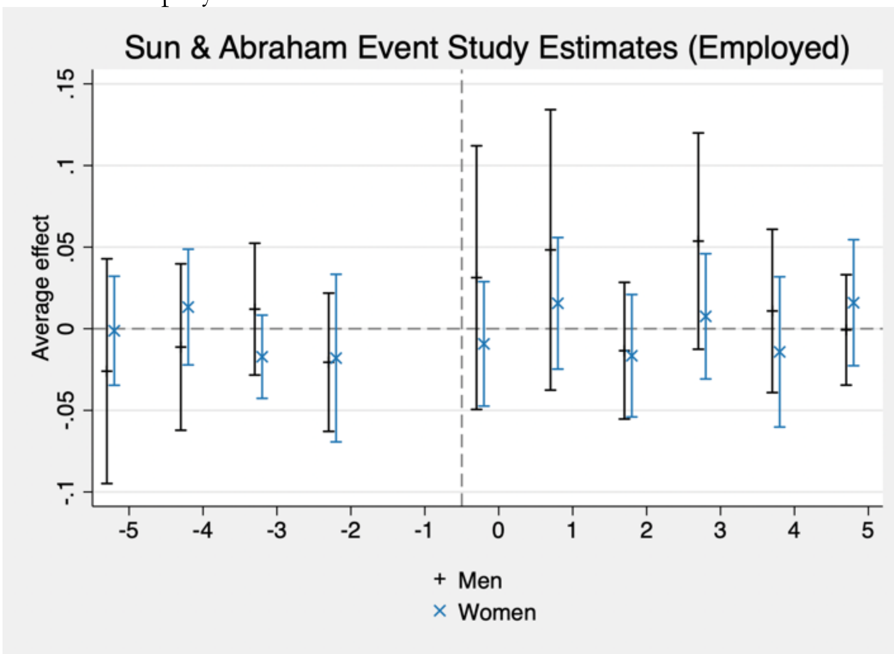
Panel B: Females



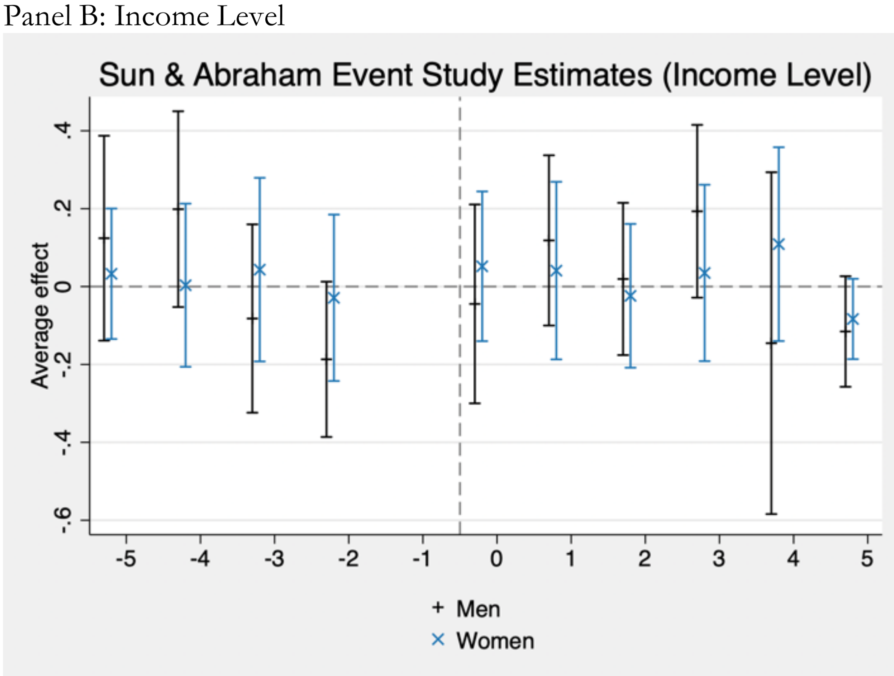
Notes: These figures overlays the event-study plots constructed using three different estimators: Sun & Abraham (2021) IW estimator (in black), De Chaisemartin and d'Haultfoeuille (2020) multiperiod estimator (in blue) and Gardner's (2021) two-stage estimator (in orange). All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Bars represent 5% confidence intervals.

Figure 7: Anti-Discrimination Laws and Labor Market Outcomes

Panel A: Employed

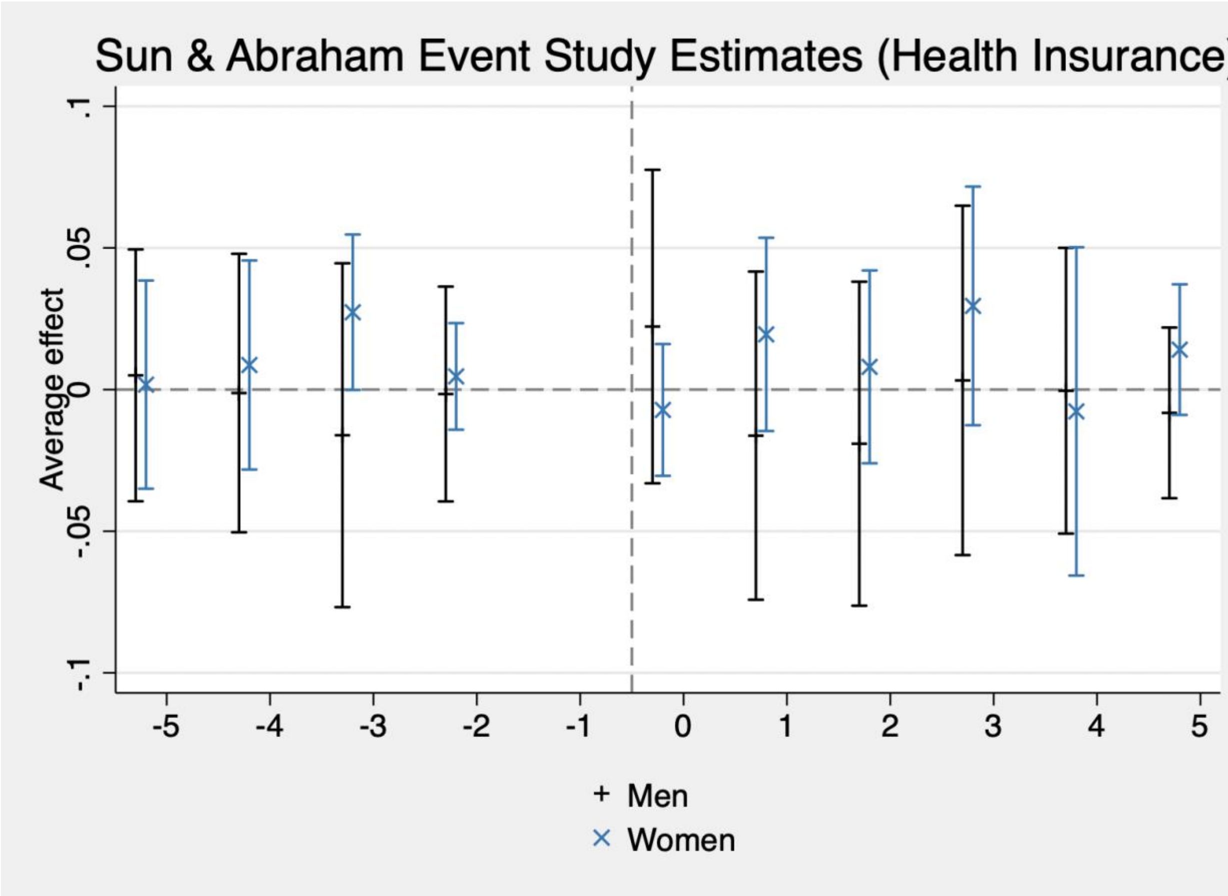


Panel B: Income Level



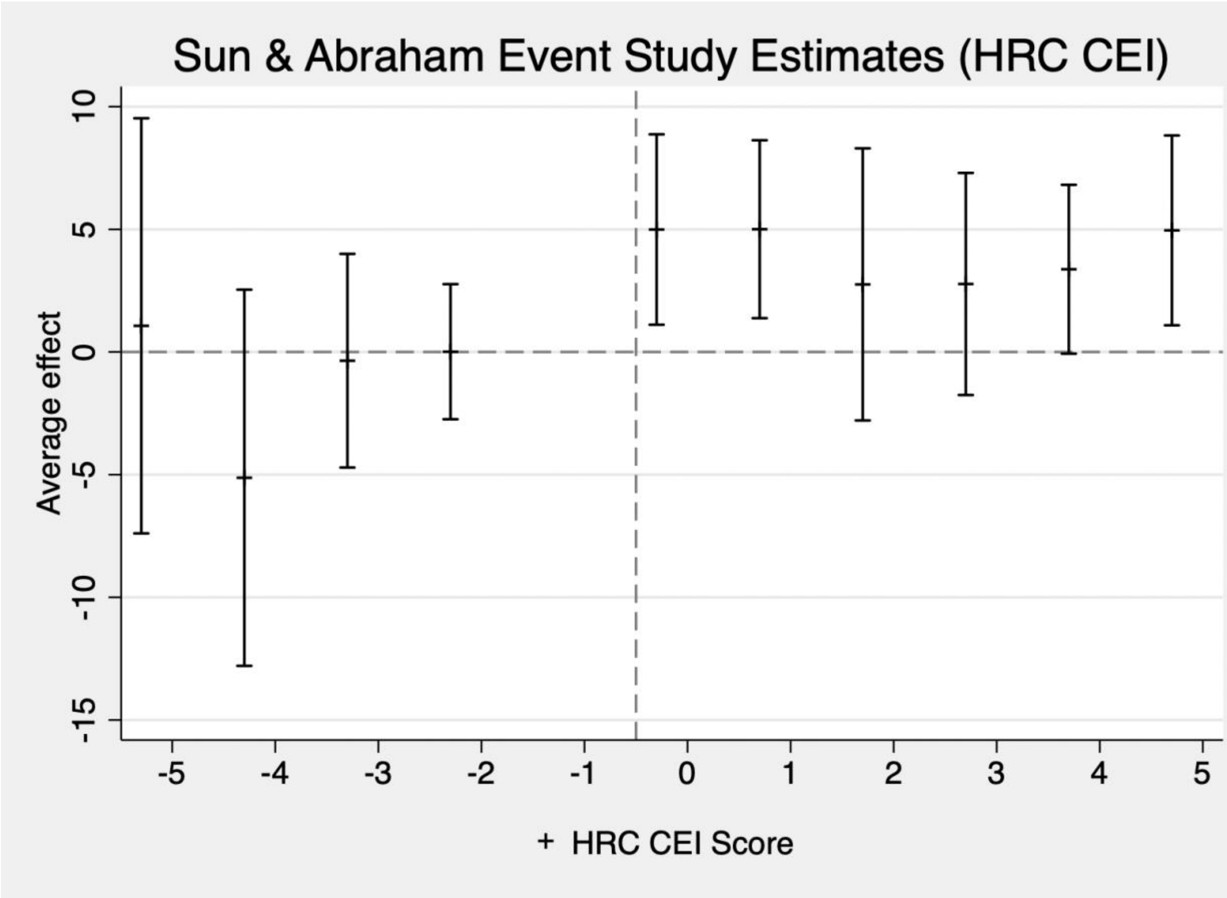
Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Models are estimated following Sun & Abraham (2021). Data source: BRFSS (1993-2019). Bars represent 5% confidence intervals.

Figure 8: Anti-Discrimination Laws and Health Insurance



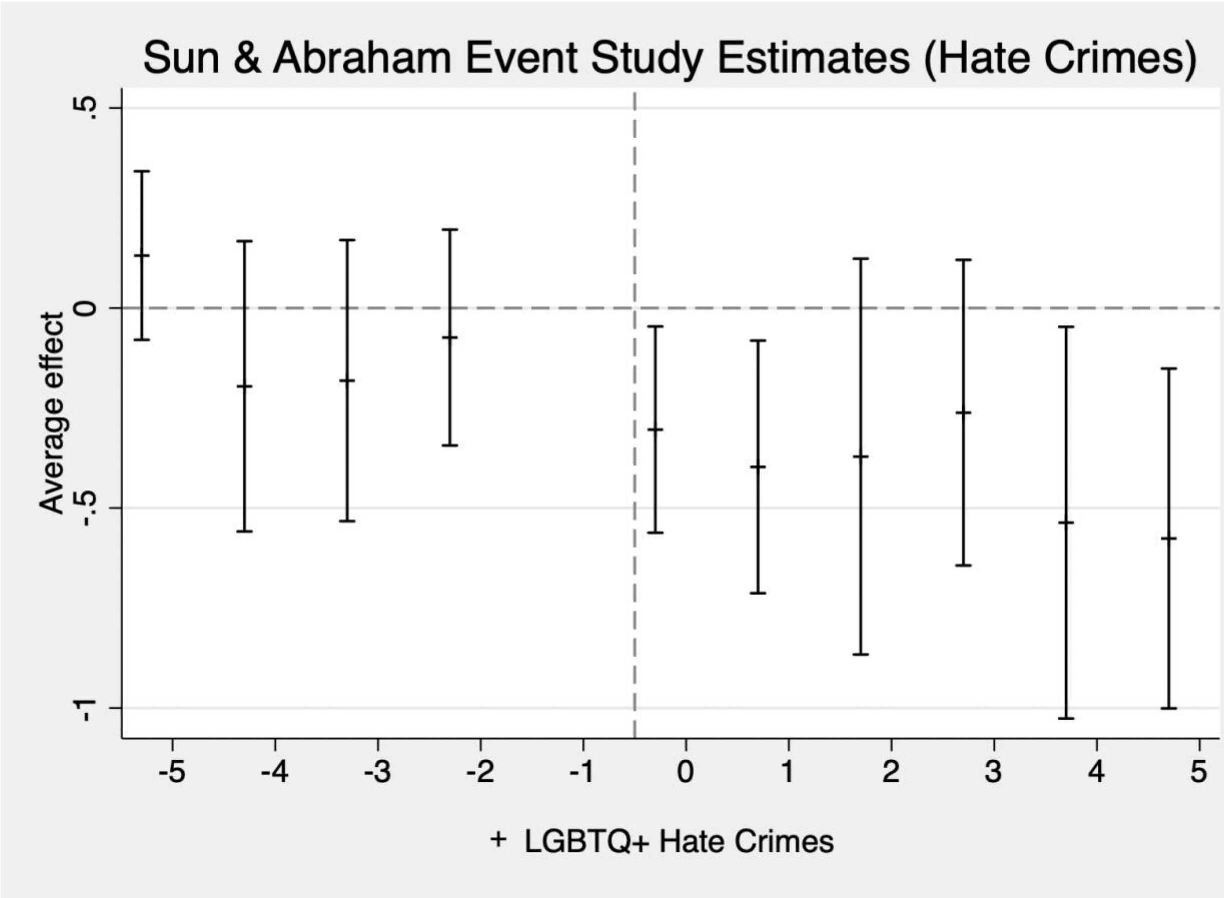
Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Bars represent 5% confidence intervals.

Figure 9: Anti-Discrimination Laws and HRC CEI Index Score



Notes: Specification includes state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: HRC CEI Index (2002-2019). Bars represent 5% confidence intervals.

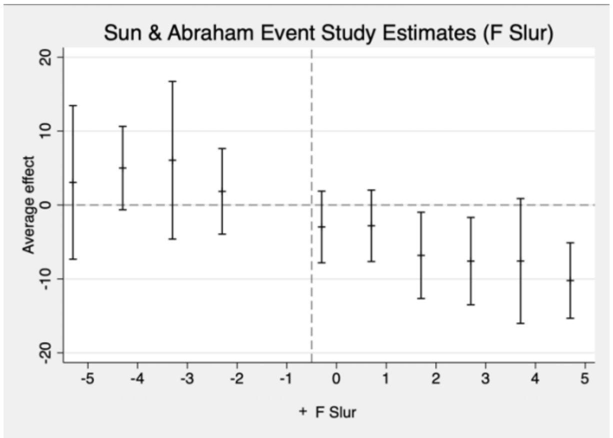
Figure 10: Anti-Discrimination Laws and Hate Crime Incidence



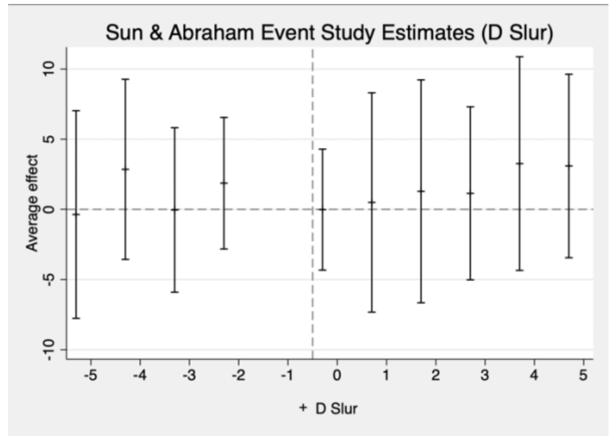
Notes: Specification includes state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: FBI UCR NIBRS (1998-2019). Bars represent 5% confidence intervals.

Figure 11: Anti-Discrimination Laws and Google Trends Searches

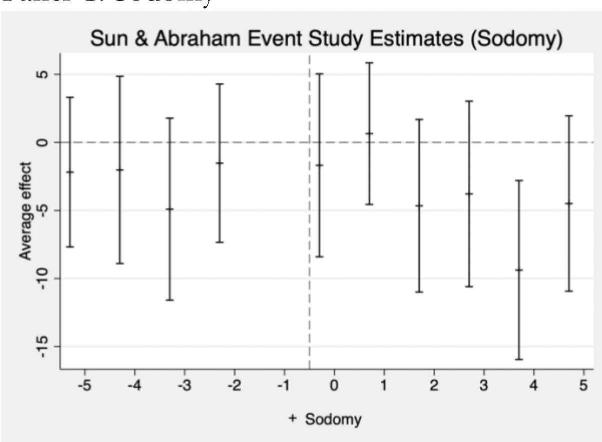
Panel A: F Slur



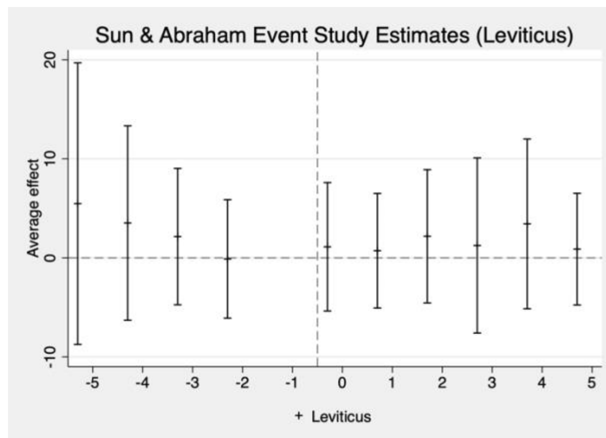
Panel B: D Slur



Panel C: Sodomy



Panel D: Leviticus



Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: Google Trends (2004-2019). Bars represent 5% confidence intervals.

Appendices

Appendix Table A1: Does the policy predict SSH or SSH predict the policy?

	(1)	(2)	(3)	(4)
	Male	Female	Male	Female
ADL	-0.0004 (0.002)	0.001 (0.002)		
SSH			-0.001 (0.002)	0.001 (0.003)

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A2: Goodman-Bacon Decomposition

	(1)	(2)
	Weight	Average DD Estimate
Panel A: SSH Males		
Earlier Treated vs Later Control	0.076	-0.085
Later Treated vs Earlier Control	0.075	0.041
Treated vs Never Treated	0.631	-0.628
Treated vs Already Treated	0.218	-0.203
Panel B: SSH Females		
Earlier Treated vs Later Control	0.076	-0.271
Later Treated vs Earlier Control	0.075	0.097
Treated vs Never Treated	0.631	0.364
Treated vs Already Treated	0.218	-0.399

Notes: Source: BRFSS (1993-2019)

Appendix Table A3: Sun & Abraham Event Study Estimates

	(1)	(2)
	SSH Males	SSH Females
t-5	-0.840	-0.458
	(0.589)	(0.281)
t-4	-0.090	-0.238
	(0.369)	(0.293)
t-3	0.233	0.048
	(0.717)	(0.355)
t-2	-1.104	0.026
	(0.664)	(0.606)
t	-0.900**	0.015
	(0.442)	(0.553)
t+1	-1.281**	0.201
	(0.503)	(0.654)
t+2	-0.521	-0.512
	(0.507)	(0.425)
t+3	0.354	-0.121
	(0.541)	(0.581)
t+4	-1.292**	-0.165
	(0.547)	(0.513)
t+5	-0.929**	0.132
	(0.368)	(0.304)

Notes: Specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Each Column reports event study estimates from a Sun & Abraham (2021) model. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A4: Joint Significance F-Tests

	(1)
	F-Stat
Panel A: t-2 to t-5	
Pre-Trend F	1.87
Pre-Trend P	0.134
Panel B: t-3 to t-6	
Pre-Trend F	0.62
Pre-Trend P	0.604

Notes: F-Statistics and their associated p values are estimated post-estimation of TWFE event study models presented in Appendix Figure A1. Panel A provides joint significance pre-trends F-tests for the periods t-2 to t-5. Panel B provides joint significance pre-trends F-tests for the periods t-3 to t-5 in a model that omits both t-1 and t-2.

Appendix Table A5: Are Results Driven by a Specific State? (Male)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	CO	DE	IL	IA	ME	MD	MI	MN	NV
ADL	-0.528***	-0.469***	-0.361**	-0.490***	-0.498***	-0.468**	-0.403**	-0.546***	-0.488**
	(0.180)	(0.165)	(0.160)	(0.178)	(0.161)	(0.177)	(0.176)	(0.188)	(0.200)
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	
	NH	NM	NY	OR	PA	RI	UT	WA	
ADL	-0.531***	-0.491***	-0.371*	-0.371**	-0.323*	-0.470***	-0.456***	-0.444**	
	(0.172)	(0.154)	(0.191)	(0.158)	(0.190)	(0.171)	(0.166)	(0.168)	

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Models are estimated following Sun & Abraham, (2021). Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A6: Are Results Driven by a Specific State? (Female)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	CO	DE	IL	IA	ME	MD	MI	MN	NV
ADL	-0.124 (0.297)	-0.085 (0.266)	-0.143 (0.188)	-0.083 (0.271)	-0.068 (0.264)	-0.050 (0.283)	-0.087 (0.272)	-0.129 (0.309)	-0.266 (0.335)
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	
	NH	NM	NY	OR	PA	RI	UT	WA	
ADL	-0.055 (0.268)	-0.025 (0.262)	-0.123 (0.293)	-0.046 (0.204)	-0.131 (0.277)	-0.129 (0.286)	-0.073 (0.262)	-0.088 (0.250)	

Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Models are estimated following Sun & Abraham (2021). Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A7: Law Heterogeneity and Mental Health

	(1)	(2)
	Men in SSH	Women in SSH
ADL	-0.523	-0.091
	(0.369)	(0.347)
ADL x Compensatory Damages	-0.054	-0.090
	(0.063)	(0.066)
ADL x Punitive Damages	0.005	-0.003
	(0.046)	(0.056)

Notes: Raw means. Source: BRFSS (1993-2019) Sample includes all landline respondents in a same-sex household between the ages 25 and 64. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

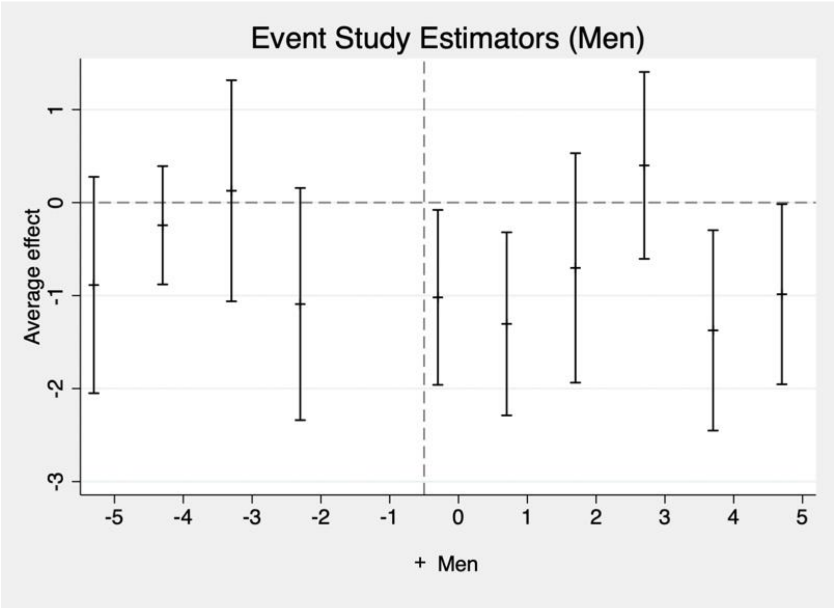
Appendix Table A8: Anti-Discrimination Laws and Non-LGBTQ+ Hate Crimes

	(1)	(2)	(3)	(4)
	Racial Hate Crimes	Ethnicity Based Hate Crimes	Gender Based Hate Crimes	Religious Based Hate Crimes
ADL	-0.339 (0.274)	-0.020 (0.081)	-0.007 (0.006)	-0.101 (0.289)

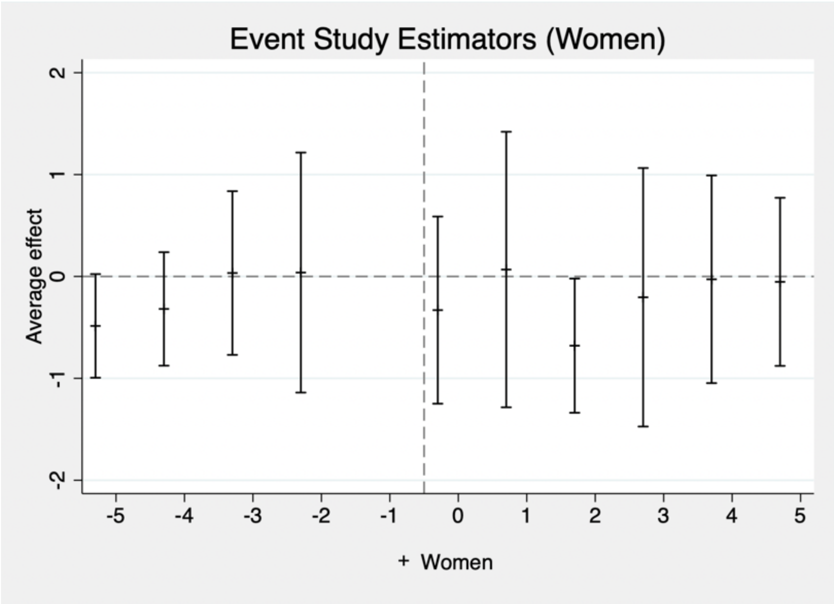
Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Models are estimated following Sun & Abraham (2021). Data source: FBI NIBRS (1998-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Anti-Discrimination Laws and Poor Mental Health: TWFE Event Studies:

Panel A: Males

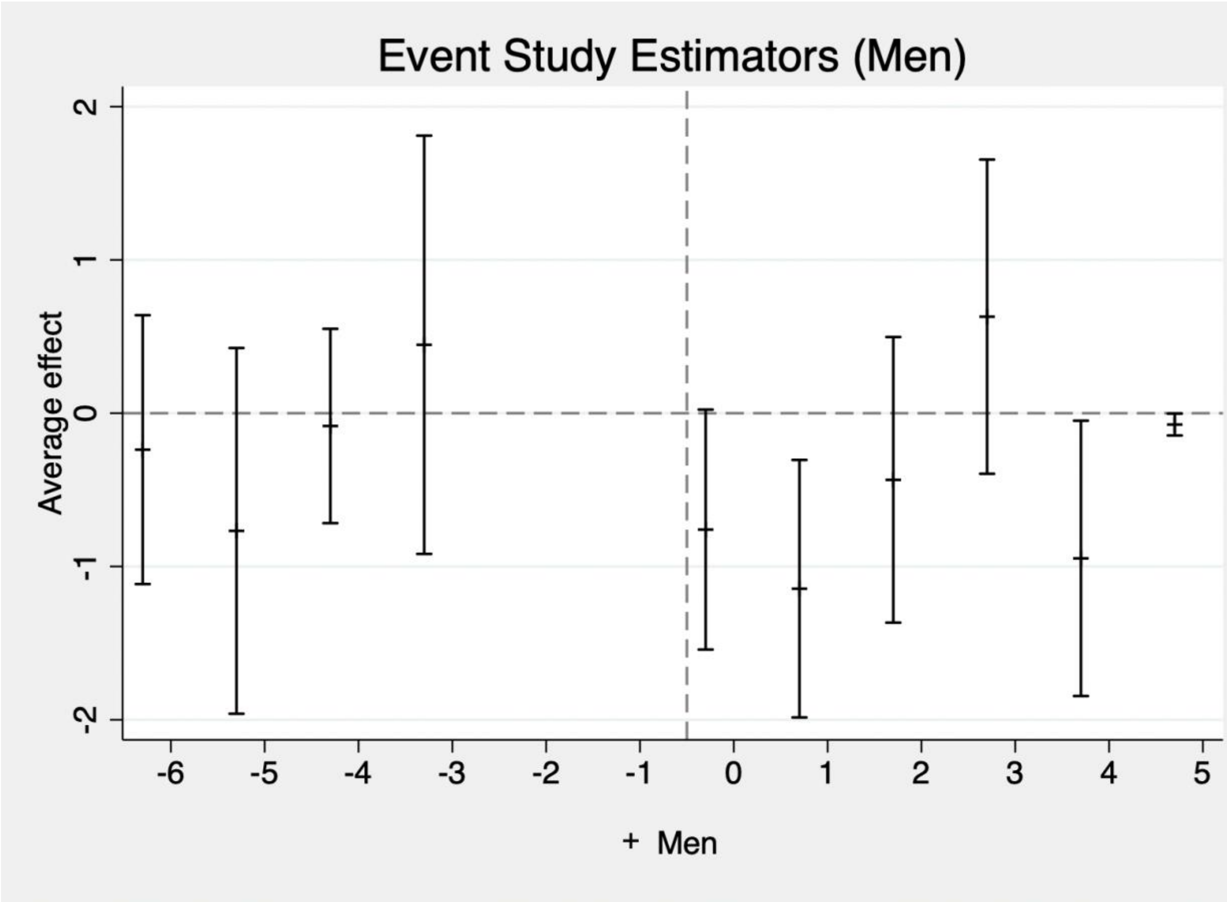


Panel B: Females



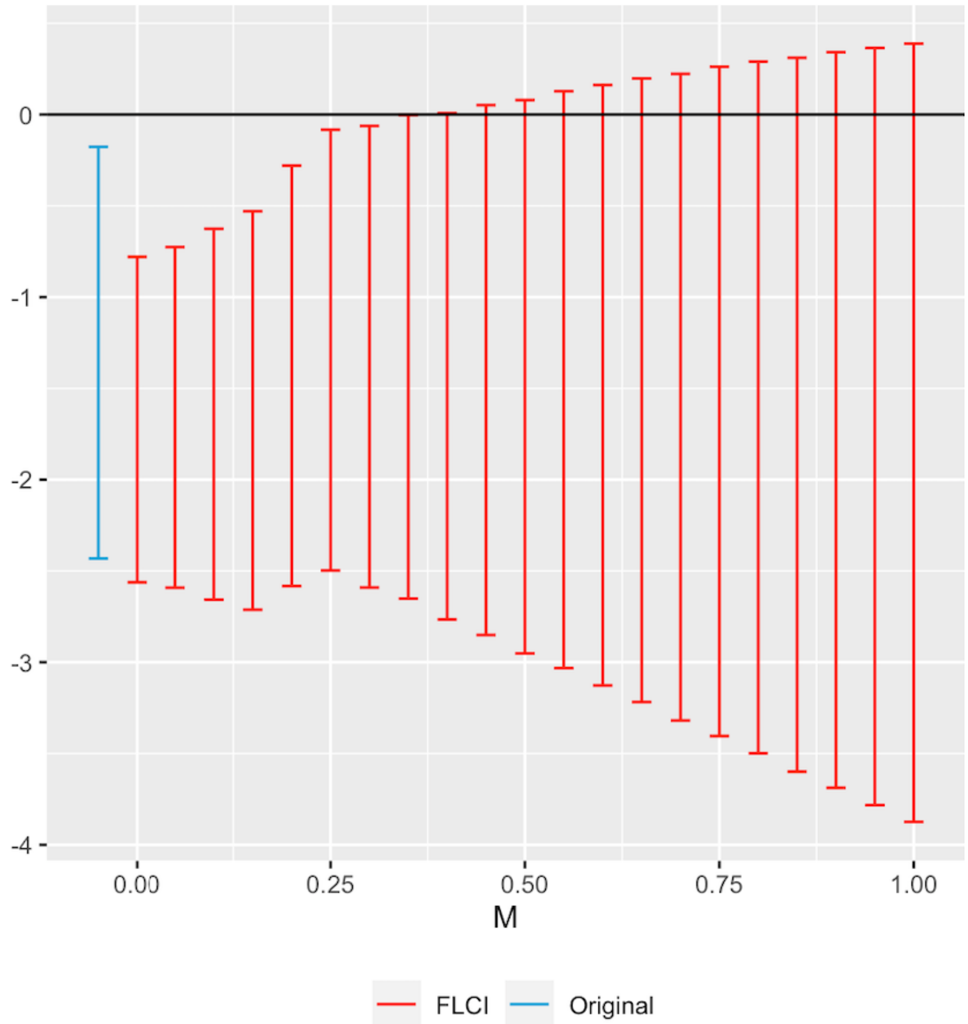
Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Bars represent 5% confidence intervals.

Figure A2: Sun & Abraham Event Study with Alternative Reference Periods.



Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Bars represent 5% confidence intervals.

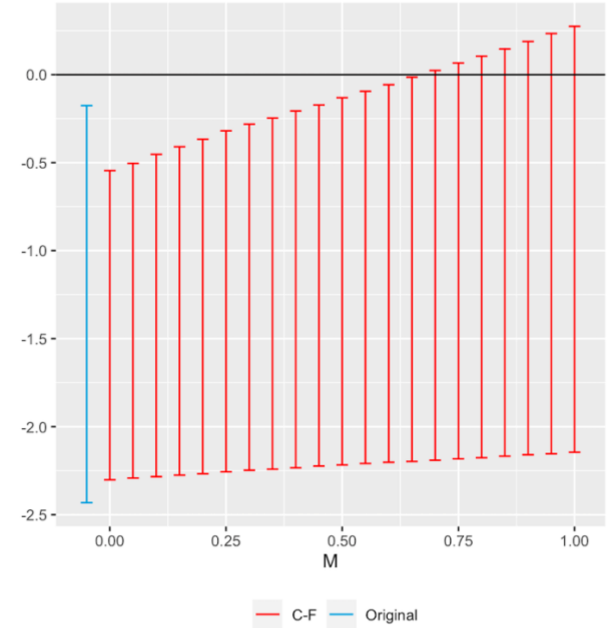
Figure A3: Pre-Trend Sensitivity Analysis



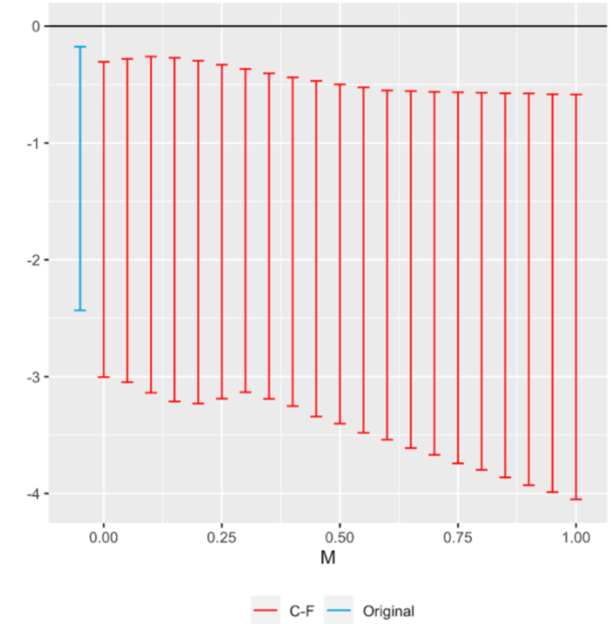
Notes: This figure shows sensitivity analysis of estimated effects of the TWFE event study analysis. The blue bar represents the 95% confidence interval of the DD estimate for relative time t = 1. The red bars represent corresponding 90% confidence intervals when allowing for per-period violations of parallel trends of up to M. M refers to the largest allowed slope violation of an underlying trend between two consecutive periods. Note that a treatment group specific linear trend (M = 0) still allows for linear violations of the parallel trends assumption. Results show the sensitivity of my main results under increasing non-linearities. All inference follows Rambachan & Roth's (2022) Fixed Length Confidence Interval Procedure.

Figure A4: Pre-Trend Sensitivity Analysis

Panel A: Negative Non-Linear Trends

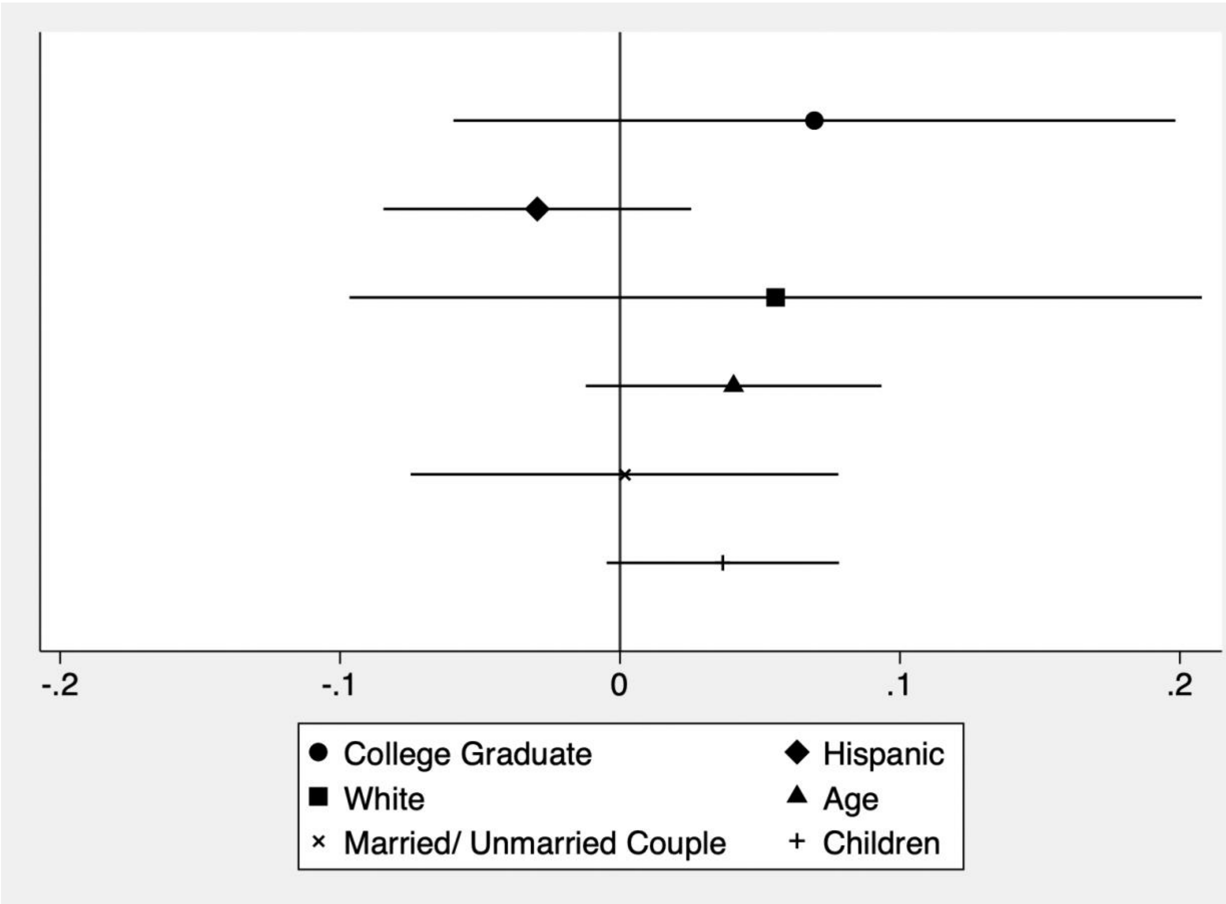


Panel B: Positive Non-Linear Trends



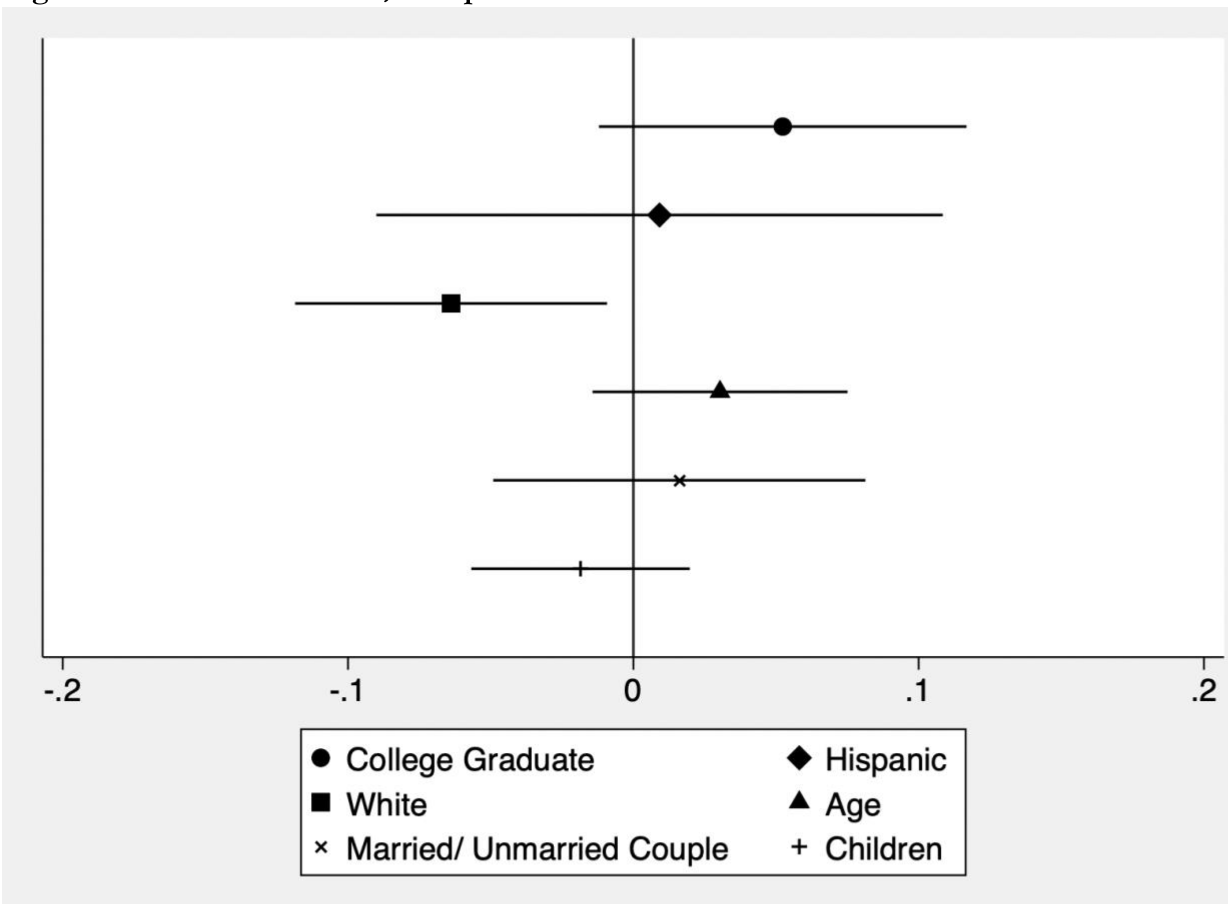
Notes: See Figure A3 notes. Here, similar approaches are used, but I separately consider only positive or negative violations. In Panel A I impose that violations of parallel trends are allowed to vary by M negative units, in Panel B I impose that violations of parallel trends are allowed to vary by M positive units.

Figure A5: Men in SSH's, Compositional Balance:



Notes: Outcome variables are standardized to have a mean of zero and a standard deviation of one to aid comparability. Each plot is a separate specficiation and each specification includes both state and year fixed effects. Bars denote 95% confidence intervals. Source: BRFSS (1993-2019).

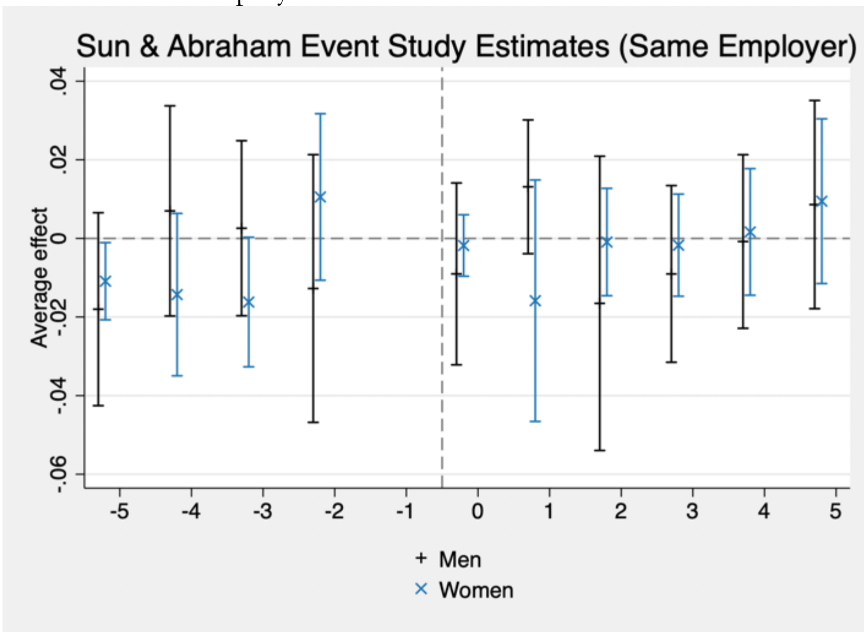
Figure A6: Women in SSH's, Compositional Balance:



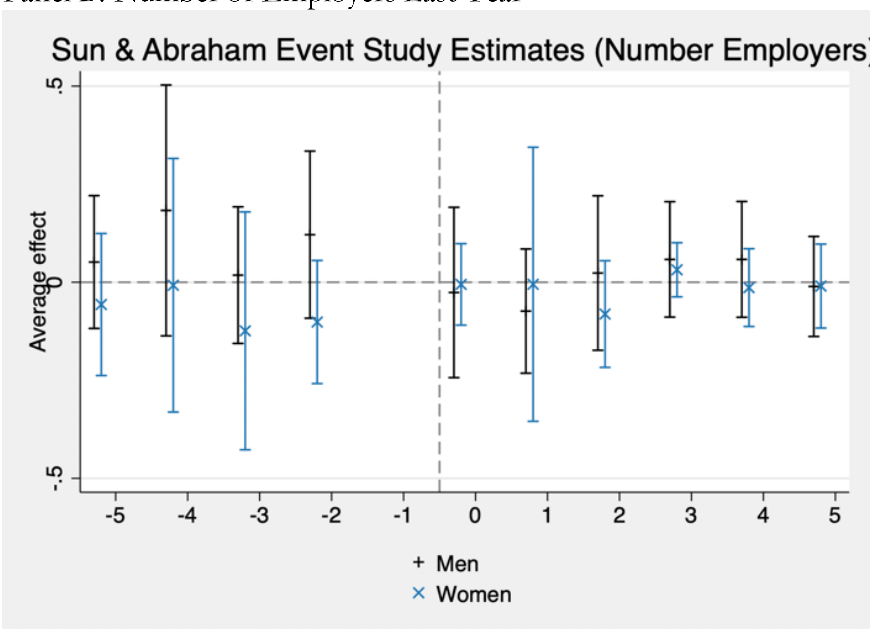
Notes: Outcome variables are standardized to have a mean of zero and a standard deviation of one to aid comparability. Each plot is a separate specficiation and each specification includes both state and year fixed effects. Bars denote 95% confidence intervals. Source: BRFSS (1993-2019).

Figure A7: Job Mobility, Current Population Survey

Panel A: Same Employer as Last Month



Panel B: Number of Employers Last Year



Notes: All specifications include state and year fixed effects, demographic controls, and state level controls. Data Source: CPS (1995-2019) – Panel A uses data from the monthly CPS and responses to the question, "do you have the same employer that you had last month?" Panel B uses data from the Annual Social and Economic Supplement of the CPS and responses to the question "how many employers did you have last year"; this question is conditional on being employed, i.e., responses take the value of 1 or more; values are top coded at 3. SSC's are identified by identifying relationships between individuals in a household ad liking this to the sex of household members. Models are estimated following Sun & Abraham (2021). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.