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

Samuel Mann

Online Appendix

Appendix A: Additional Details Regarding Sexual Orientation Employment Based Anti-Discrimination Laws

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. However, the bill died. State level coverage has grown rapidly since. In 1982 Wisconsin became the first state to implement a sexual orientation based ADL, following the District of Columbia's introduction of an ADL in 1977. This inspired a series of proposed legislation at the state level to prevent such laws; several laws were proposed to prevent gay and lesbian workers from working in specific environments[1] and some states introduced laws that prevented the passage of local level non-discrimination acts.[2]

Despite legislative backlash, ADLs gained progress during the 90's and 2000's and by 2019 25 states had passed ADLs. This 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). For the most part, these policies preceded other LGBTQ+ progressive policies such as same-sex marriage legalization, for example, the first state to legalize same-sex marriage was Massachusetts in 2004, 15 years after it passed an ADL.

The fight for employment protection for sexual minorities ended in 2020 when the Supreme Court introduced a federal ADL for sexual minorities, ruling in favor of *Bostock* in the *Bostock vs*

[1] see for example Proposition 6 which would have barred sexual minorities from working in schools.
[2] Arkansas, North Carolina, and Tennessee have all introduced laws preventing the passage of local ADLs.

Clayton County case (6-3 decision). The court ruled on the 15th of June 2020 that discrimination in employment 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.

Appendix Table A1 provides the dates of the passage of sexual orientation employment based anti-discrimination laws at the state level, as well as dates of the passage of other LGBTQ+ policies.

Appendix Table A1: Dates of LGBTQ+ Policies, by State

State	Anti-Discrimination Law	SSM	Domestic Partnership	Civil Unions	Sodomy Law Repeal	LGB Hate Crime Law	LGBT Hate Crime Law	Health Non-Discrimination Law
Alabama		2015			2003			
Alaska		2014			1980			
Arizona		2014			2001	2003		
Arkansas		2015			2002			
California	September, 1992	2008-2010; 2013-	2000	2005	1976	1984	1999	2005
Colorado	*May, 2007*	2014	2009	2012	1972	2005	2005	2013
Connecticut	April, 1991	2008		2005	1971	1987	2004	
Delaware	*July, 2009*	2013		2012	1973	1997	2013	
District of Columbia	December, 1977	2010	2002	2008	1994	1989	1989	1986
Florida		2015			2003	1991	1991	
Georgia		2015			1998	2000-2004	2000-2004	
Hawaii	1991*	2013	1997	2012	1973	2003	2003	2016
Idaho		2014			2003			
Illinois	*January, 2006*	2014		2011	1962	2001	2016	2006
Indiana		2014			1977	2002		
Iowa	*April, 2006*	2009			1978			

Kansas		2015			2003			
Kentucky		2015			1992	2001		
Louisiana		2015			2003	1997		
Maine	*November, 2005*	2012	2004		1975	2002		2019
Maryland	*October, 2001*	2013	2008		1999	2005	2005	
Massachusetts	October, 1989	2004			2002	1996	2012	
Michigan	*June, 2018*	2015			2003			
Minnesota	*April, 1993*	2013			2001	1989	1993	1993
Mississippi		2015			2003			
Missouri		2015			2003	1999	1999	
Montana		2014			1997			2016
Nebraska		2015			1978	2002		
Nevada	*May, 1999*	2014		2009	1993	2001	2013	2009
New Hampshire	*January, 1998*	2010		2008	1975	1991	2019	
New Jersey	1992*	2013	2004	2007	1979	2002	2008	
New Mexico	*May, 2003*	2013			1975	2003	2003	
New York	*January, 2003*	2011			1980	2000	2019	2018
North Carolina		2014			2003			
North Dakota		2015			1975			
Ohio		2015			1974			
Oklahoma		2014			2003			
Oregon	*January, 2008*	2014		2008	1972	2001	2008	2016
Pennsylvania	*August, 2018*	2014			1980	2002-2008	2002-2008	
Rhode Island	*May, 1995*	2013		2011	1998	1998	2012	2015
South Carolina		2014			2003			
South Dakota		2015			1977			
Tennessee		2015			1996	2001		
Texas		2015			2003	2001		
Utah	*March, 2015*	2014			2003	2019	2019	
Vermont	April, 1992	2009		2000	1977	1990	1999	1992
Virginia		2014			2003			
Washington	*January, 2006*	2012	2007	2009	1976	1993	2009	2006
West Virginia		2014			1976			
Wisconsin	1982*	2014	2009		1983	2002		
Wyoming		2014			1977			

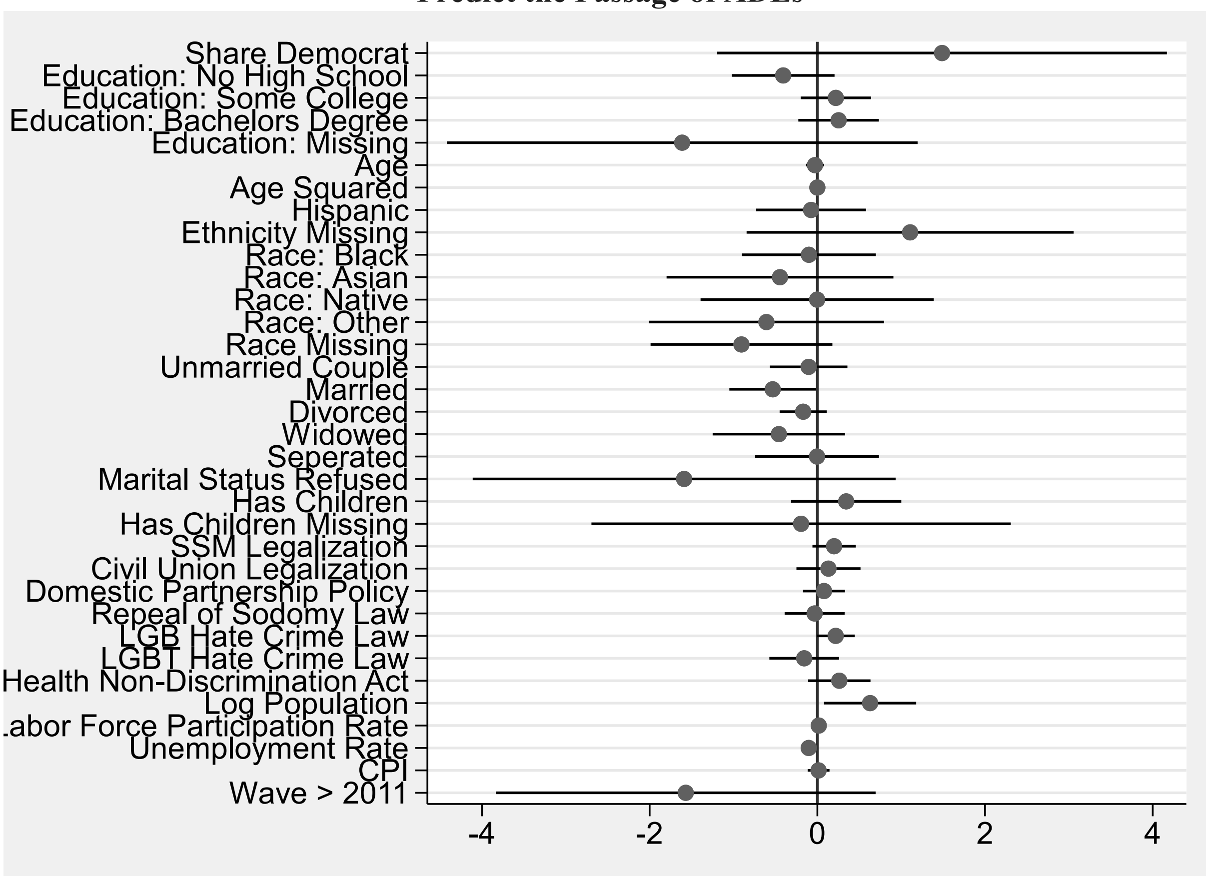
Notes: * Month is unclear but date is prior to the study period. Author identified dates from a range of sources, including data shared by Dario Sansone, the National Centre for Lesbian Rights, the Movement Advancement Project, the Human Rights Council, Sansone (2019), and reading case text. *Italicized* dates are those laws that drive the underlying variation in difference-in-difference and event study models.

Appendix B: Policy Endogeneity and Threats to Internal Validity

This appendix deals with concerns regarding the endogeneity of ADLs and potential threats to internal validity.

First, one may be concerned that my results are driven by broader changes that drive the adoption of ADLs. That is, one may be concerned that other LGBTQ+ policies as well as political (e.g. share of democratic voters), demographic (e.g. racial and marital composition), and economic (e.g. CPI) factors at the state level predict the passage of ADLs. In Appendix Figure B1 I estimate whether economic, political, policy, and demographic factors at the state level predict the passage of ADLs. Results presented in Appendix Figure B1 show that the passage of ADLs is not predicted by broader policy, demographic, economic, or political factors at the state level.

Appendix Figure B1: Do Economic, Demographic, Policy, or Political Characteristics Predict the Passage of ADLs



Notes: Specifications include state and year fixed effects. Data sources: BRFSS (1993-2019); MIT Election Data and Science Lab; St Louis Fed; and BLS. Sample is restricted to the 17 states that passed an ADL during the sample period.

Further, covariance balance tests presented in Appendix Table B1 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.[3]

Appendix Table B1: 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Relatedly, sexual minorities may not be randomly distributed, and ADLs 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 protections offered in these states. Results presented in Appendix Table B2 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. Further, results presented in Appendix Figure B2 demonstrate that ADLs do not lead to changes in the composition of sexual minorities.

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

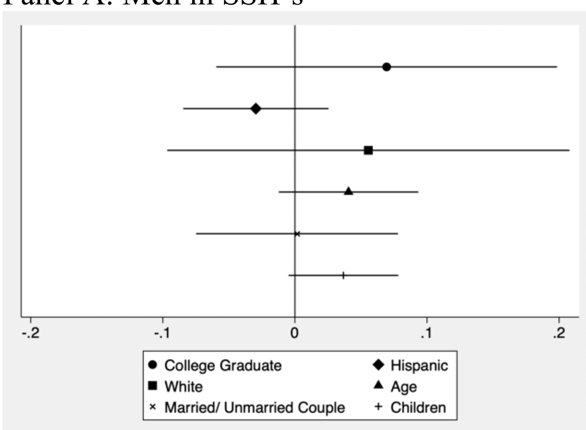
Appendix Table B2: Does the policy predict SSH or SSH predict the policy?

	(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)

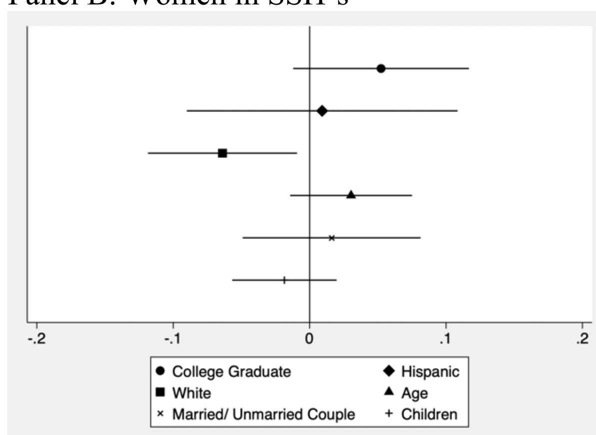
Notes: All specifications include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p 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

Appendix Figure B2: Compositional Balance

Panel A: Men in SSH's



Panel B: Women in SSH's



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).

Additionally, Appendix Table B3 presents results from a triple difference model. Difference-in-Difference-in-Differences models control for all unobservable factors at the state by year, state by same-sex household, and year by same-sex household level. In comparison to my baseline difference-in-difference model, the triple difference model additionally controls for the average mental health of both SSHs and DSHs in each state (through the state by SSH fixed effects), trends or shocks in mental health outcomes for SSHs compared to DSHs (through the inclusion of SSH by year fixed effects) and shocks or trends in mental health at the state level that affect SSH's and DSH's equally (through the inclusion of state by year fixed effects).

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 for each state by year cell,[4] allowing me to present triple difference results from a Sun and Abraham (2021) model.

Appendix Table B3: 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 are the value of the difference between people in DSH's and SSH's, following Olden & Møen (2022); models include both state level and demographic controls. Each Panel and Column reports results from a Sun & Abraham (2021) model which is estimated by compressing event time to two periods. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Results are consistent with the main results, though coefficient magnitudes are slightly smaller. In triple difference models I document that ADLs result in around 0.3 fewer days poor mental health days reported among men in SSHs. 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 ADLs reducing the mental health disparity by around 18%. For women, coefficients are statistically indistinguishable from zero.

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

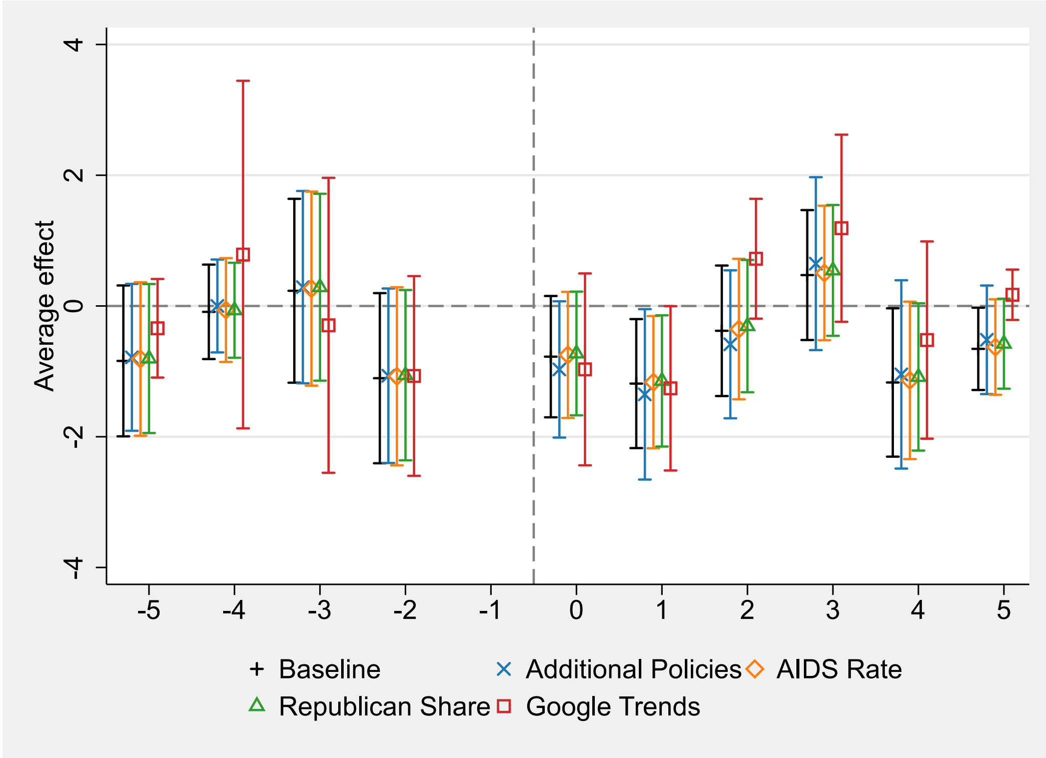
However, the possibility that state level shocks and policy changes specific to SSHs could bias results remains, as these are not captured in the triple difference model. As such, one may be concerned that my results are driven by other factors, that coincide with the introduction of employment based anti-discrimination laws that protect sexual minorities, and that impact the mental health of sexual minorities, but not those in DSHs. For example, one may be concerned that the main findings are driven by the introduction of anti-discrimination laws in other (non-employment areas), for example, in housing. This may be the case if there exists correlation in the timing of the passage of employment-based ADLs and the passage of other non-employment based protections. While it is true that my baseline results include controls for protections in health care (i.e. health non-discrimination laws), it also remains true that sexual minorities are protected in other areas of their lives.

Alternatively, one may be concerned that the passage of ADLs coincides with changes in HIV/AIDS mortality, and that my underlying results are driven by these changes. For example, prior studies have demonstrated that attitudes towards sexual minorities significantly improved in 1992 which was largely driven by pre-1992 changes in AIDS mortality (Fernández, Parsa and Viarengo, 2024). While others have demonstrated that the HIV epidemic led to a substantial improvement in the democratic vote share (Mansour and Reeves, 2022). Broadly, these studies demonstrate the important role that the HIV epidemic played in shaping social attitudes and attitudes towards sexual minorities, thus one may be concerned that the underlying findings of the effect of ADLs on the mental health of sexual minorities may in fact be driven by differences in exposure to the HIV epidemic.

Additionally, states that adopt ADLs are generally more liberal states. As such, a key threat to a causal interpretation is that my identification approach could be unintentionally capturing the association between changing liberalness and mental health rather than changes in mental health driven from the introduction of an ADL. Relatedly, prior studies have studied whether LGBTQ+ policies are predicted by or predict the passage of LGBTQ+ policies. Studies indicate that the passage of (pro-) LGBTQ+ policies improve attitudes towards sexual minorities (Aksoy *et al.*, 2020; Delhommer, 2020; Deal, 2022) and that improvements in attitudes towards sexual minorities increases the political representation of pro-LGBTQ+ individuals (Reynolds, 2013), which could in turn increase the likelihood of policy adoption. That is, one may be concerned that the underlying findings are driven by improvements in attitudes which predict the adoption of ADLs.

To explore these concerns, results presented in Appendix Figure B3 compare my baseline Sun & Abraham event study estimates for men in SSHs to several Sun & Abraham event study estimates that include additional controls for other non-discrimination laws, namely, credit non-discrimination acts, housing non-discrimination acts, and public accommodation non-discrimination acts, as well as controls for the aids mortality rate (from the CDC compressed mortality and underlying cause of death files), controls for liberalness (proxied by the Republican vote share at the state by year level from the MIT Election Data and Science Lab), and controls for animosity towards LGBTQ+ people (derived from google search intensity for Faggot, Leviticus, and Sodomy). In all cases coefficients are extremely similar to the baseline models, indicating that it is likely the case that the mental health effects documented in my baseline models are indeed driven by the passage of employment based anti-discrimination laws, rather than other, non-discrimination laws, changes in AIDS mortality, or changes in underlying liberalness or animosity towards sexual minorities.

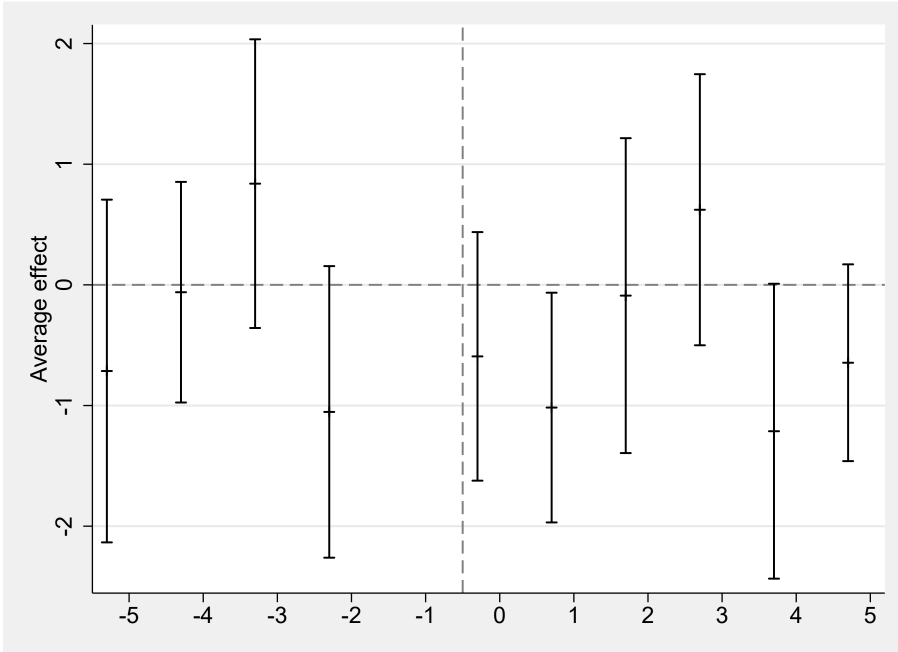
Appendix Figure B3: ADLs and Mental Health with Additional Controls



Notes: Specifications are estimated following Sun & Abraham (2021) and 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 sources: BRFSS (1993-2019); MIT Election Data and Science Lab; Google Trends, and CDC compressed mortality and underlying cause of death files. The sample that includes controls for google trends is restricted to the period 2004 to 2019 given that the data from google trends do not precede 2004. Omitted periods are period t-1 and t-6+. Bars represent 95% confidence intervals.

Finally, as can be seen in Appendix Table A1, several states passed ADLs close to the beginning or end of the sample window. In turn, this impacts the number of periods that they can exist in within the event study models. For example, a state that passes an ADL in 2017 would exist in all pre-periods, but would not exist in all post-periods, as the sample ends in 2019. As such, results from a balanced panel (that restricts the sample to those states that have a full pre- and post-period coverage) for men in SSHs are presented in Appendix Figure B4.

Appendix Figure B4: Anti-Discrimination Laws and Poor Mental Health: Balanced Panel



Notes: Specifications are estimated following Sun & Abraham (2021) and 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. Omitted periods are period t-1 and t-6+. Data source: BRFSS (1993-2019); Sample is restricted to states that did not pass an ADL and states that passed an ADL between 1999 and 2014. Bars represent 95% confidence intervals. The aggregate estimate is 0.489 (SE: 0.359).

Appendix C: Violations of the Parallel Trends Assumption
This appendix provides additional evidence regarding satisfaction of the parallel trends assumption.

Results from event study models documented in Figure 1 indicate that the coefficient for the period t-2 is negative, with a large confidence interval, which seems to trend negatively and may raise 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, as highlighted in Appendix Table C1, however, the pattern indicates a slight, albeit statistically insignificant negative pre-trend.

Appendix Table C1: 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 (2024); 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 C2).

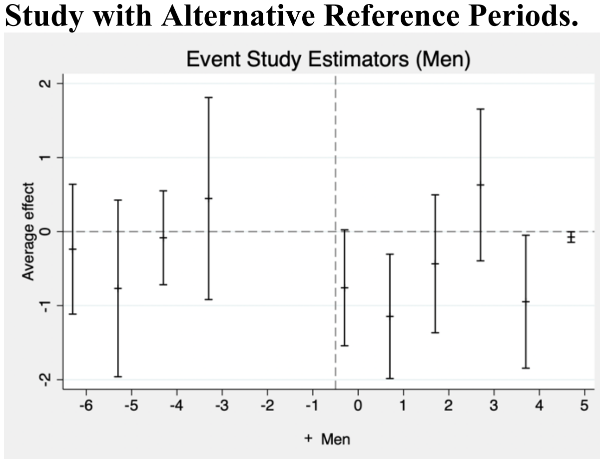
Appendix Table C2: Joint Significance F-Tests

	(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. 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.

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 pre-policy indicators remain statistically indistinguishable from zero, even at the 10% level (Appendix Figure C1).

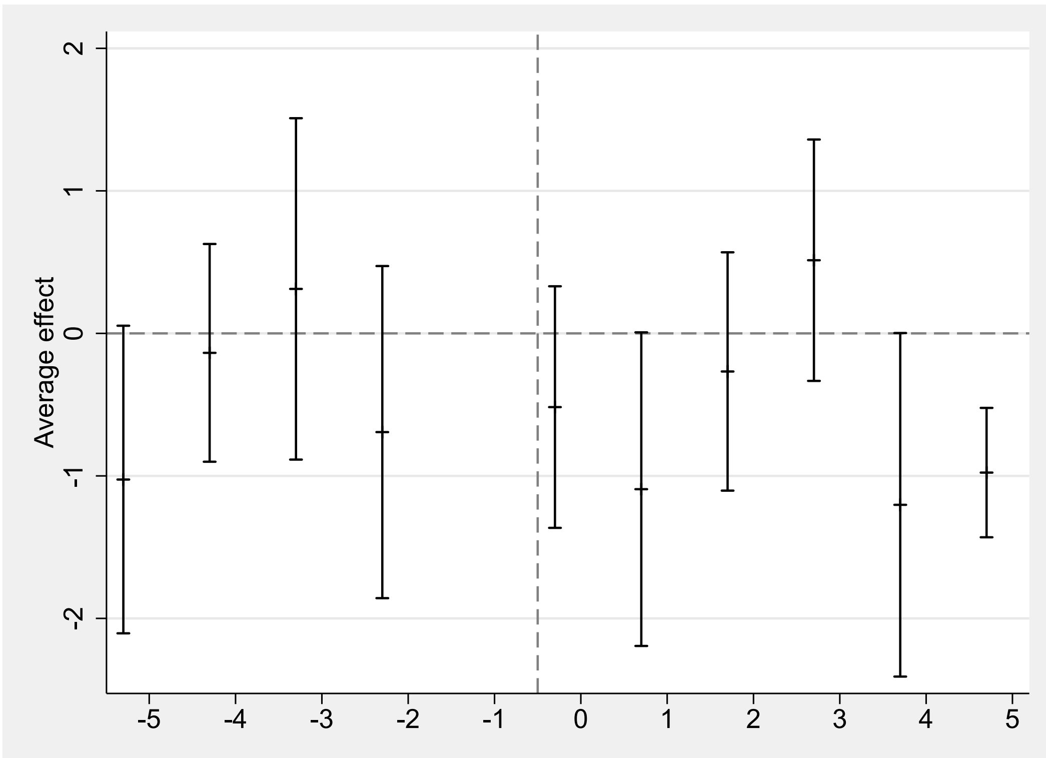
Appendix Figure C1: Sun & Abraham Event Study with Alternative Reference Periods.



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.

Next, satisfaction of the parallel trends assumption with controls are somewhat different to satisfaction of the parallel trends assumption when estimating models that do not include controls – i.e. a conditional or unconditional parallel trends assumption. Therefore, below I re-estimated the Sun & Abraham (2021) event study model for men in SSHs without including controls to explore whether similar coefficients were identified, which indeed they are. These models are reported in Appendix Figure C2.

Appendix Figure C2: Anti-Discrimination Laws and Poor Mental Health: No Controls



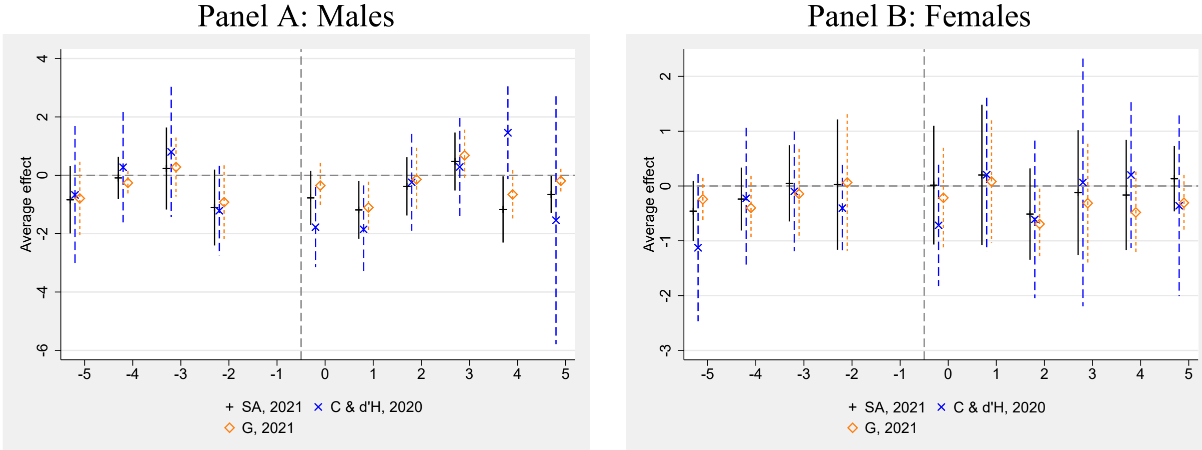
Notes: Black solid bars refer to estimates for men in SSHs, blue dashed bars refer to estimates for women in SSHs. Specifications are estimated following Sun & Abraham (2021) and include state and year fixed effects as well as an indicator equal to one if survey was completed after 2010, demographic controls, and state level controls. Omitted periods are period t-1 and t-6+. Data source: BRFSS (1993-2019). Bars represent 95% confidence intervals.

Relatedly, one may be concerned about the choice of a Sun and Abraham (2021) model as opposed to other proposed approaches to dealing with heterogeneity and negative weighting problems that arise from the use of TWFE models. While results from Sun & Abraham (2021) models are indeed 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 (2020) multiperiod estimator and Gardner's (2021) two-stage estimator in Appendix Figure C3. Both are robust to heterogeneity across space and over time. Event study estimates from these models are presented in Figure E1 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[5]. 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.

Appendix Figure C3: Anti-Discrimination Laws and Poor Mental Health: Other Models

Panel A: Males Panel B: Females

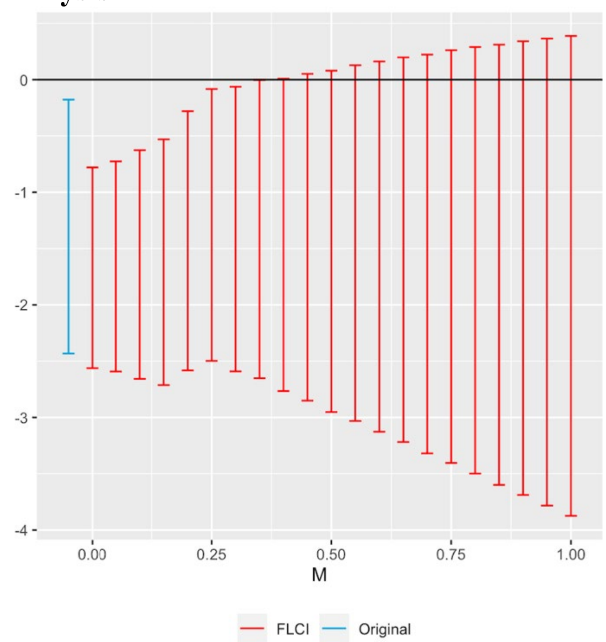


Notes: These figures overlay the event-study plots constructed using three different estimators: Sun & Abraham (2021) IW estimator (in black solid line), De Chaisemartin and d'Haultfoeuille (2020) multiperiod estimator (in blue dashed line) and Gardner's (2021) two-stage estimator (in orange short dashed line). 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 95% confidence intervals.

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

Finally, I apply (Rambachan and Roth, 2023)"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 C4.

Appendix Figure C4: Pre-Trend Sensitivity Analysis



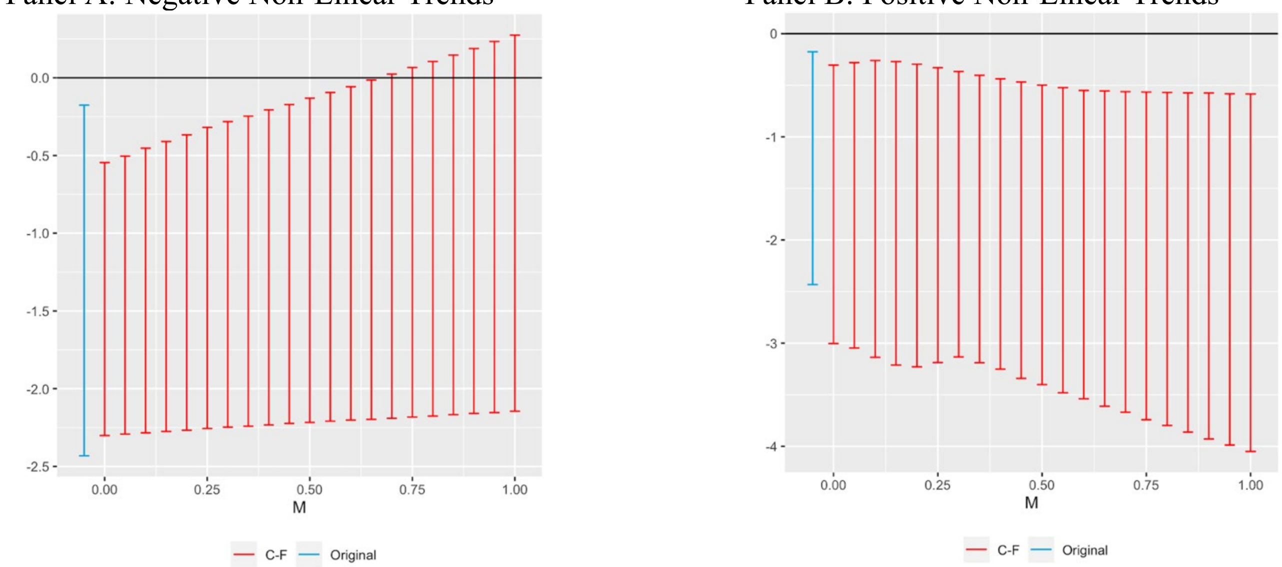
Notes: This figure shows sensitivity analysis of estimated effects of the TWFE event study analysis. The blue bar represents the 95% confidence interval of the DD estimate for relative time t = 1. The red bars represent corresponding 90% confidence intervals when allowing for per-period violations of 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.

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[6]. 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 C5) and in fact these impositions lead to larger allowable deviations from a linear trend.

Appendix Figure C5: Pre-Trend Sensitivity Analysis

Panel A: Negative Non-Linear Trends

Panel B: Positive Non-Linear Trends



Notes: See Appendix Figure C4 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.

While the slight negative pre-trend documented at t-2 in the pre-policy period in Figure 5 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 appendix provide evidence that the slight negative trend in the pre-policy period documented in Figures 5 is unlikely a sizeable

[6] The average change in slope in the pre-treatment period is 0.744.

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.

Appendix D: Spillovers

This appendix explores whether there are spillover effects from the improvements in mental health observed for men in SSHs to physical health, self-rated health, and risky health behaviors. Appendix Table D1 provides estimates from Sun & Abraham (2021) models for each of these outcomes for men in SSHs.

Appendix Table D1: Spillovers

	Coefficient
Panel A: Poor Physical Health Days	
ADL	-0.371
	(0.345)
Panel B: Fair or Poor Self Rated Health	
ADL	-0.040***
	(0.014)
Panel C: Smoker	
ADL	-0.025
	(0.022)
Panel D: Drinker	
ADL	0.007
	(0.024)
Panel D: Binge Drinker	
ADL	0.006
	(0.026)

Notes: All specifications include state and year fixed effects, an indicator equal to one if survey was completed after 2010, and state and demographic controls. Each panel is a separate regression. Specifications follow Sun & Abraham (2021) static estimator. Data source: BRFSS (1993-2019). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix E: Additional Robustness and Sensitivity Tests

This appendix reports on a host of additional robustness and sensitivity tests.

First, 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 Table E1 I demonstrate that there is no evidence of differential effects dependent on damages covered by ADLs.

Appendix Table E1: 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: OLS regression specifications that 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 are estimated separately for Columns 1 and 2. Data source: BRFSS (1993-2019. Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, given that the outcome variable in Table 3 (number of poor mental health days) is a count variable the use of linear probability modelling to estimate difference-in-differences equations may be inappropriate. In Appendix Table E1 I present results from alternative approaches to estimate models with count data. These results are presented for men in same-sex households in Panel A and women in same-sex households in Panel B. Results from a Poisson model are presented in Column 1 and results from a zero-inflated negative binomial model[7] are presented in Column 2. The results demonstrate that ADLs led to around a 10-11% reduction in the number of poor mental

[7] 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.

health days reported by men in same-sex households, but do not significantly impact the mental health of women in same-sex households.

Appendix Table E2: Anti-Discrimination Laws and Poor Mental Health: Alternative 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, results presented in Appendix Table E3 demonstrate that 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).

Appendix Table E3: 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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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, resulting in several states (29) not administering the mental health question to respondents in 2002. Results presented in Column (5) of Appendix Table E3 indicate that excluding 2002 from my analysis does not significantly change my point estimate (or it's associated statistical significance).

Appendix F: Robustness of Mechanisms

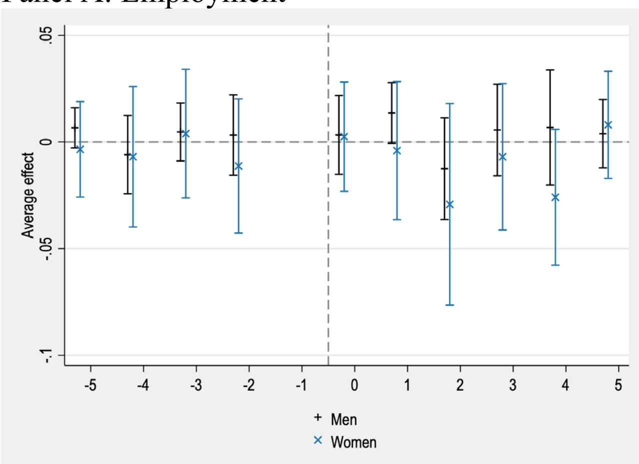
This appendix deals with concerns regarding the robustness of the mechanisms results.

Labor Market Outcomes

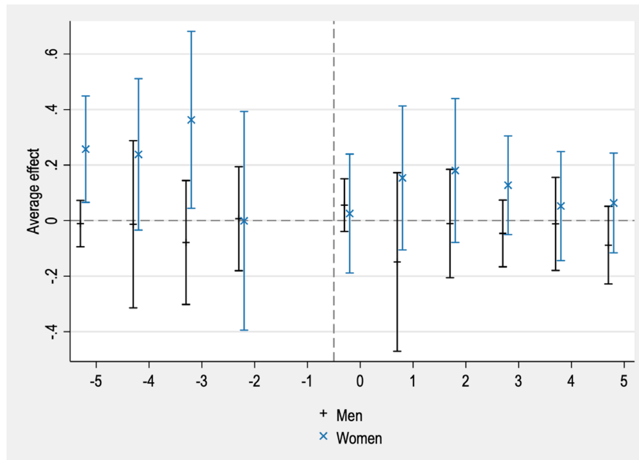
First, I deal with the results regarding the labor market mechanisms. One concern with these findings is that the BRFSS is, by nature, a health survey. That is, the labor market information in the survey is much weaker compared to available information in other datasets such as the Current Population Survey. As such, in Appendix Figure F1 I re-estimate the results for the labor market outcomes (employment and earnings) using data from the monthly CPS (1995 to 2019), rather than the BRFSS. Importantly, with the CPS I can use same-sex couple rather than same-sex households to identify sexual minorities. Notably, like the results reported in the main analysis, I find no evidence of an effect of ADLs on the labor market outcomes of men or women in SSHs.

Appendix Figure F1: Labor Market Outcomes - CPS

Panel A: Employment



Panel B: Income

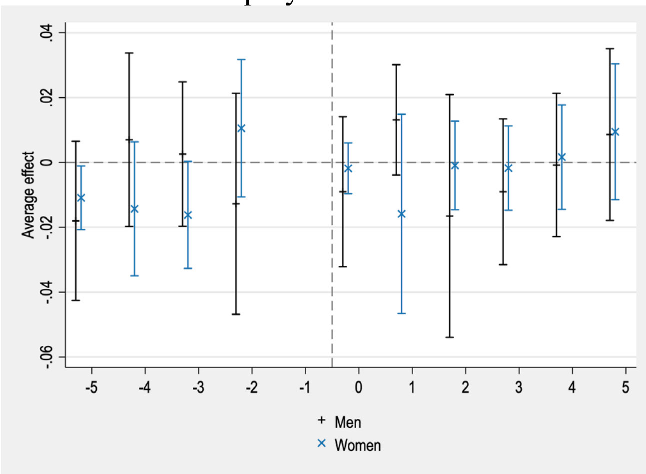


Notes: All specifications include state and year fixed effects, demographic controls, and state level controls. SSC's are identified by identifying relationships between individuals in a household and liking this to the sex of household members. Panel A reports results with the outcome equaling 1 if an individual is employed and 0 otherwise. Panel B reports results for the log of individual income, conditional on income being non-zero. Models are estimated following Sun & Abraham (2021). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

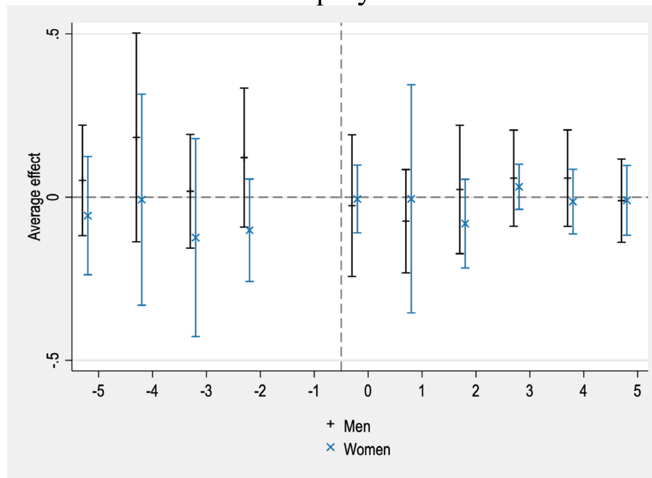
Next, I explore whether ADLs change job mobility using data on same-sex couples from the Current Population Survey and responses to questions regarding whether the individual has the same employer as last month and the number of employers an individual had in the preceding year. Results from these models (Appendix Figure F2) demonstrate that there is no change in the job mobility of people in same-sex couples following the passage of an ADL.

Figure F2: Job Mobility, Current Population Survey

Panel A: Same Employer as Last Month



Panel B: Number of Employers Last Year

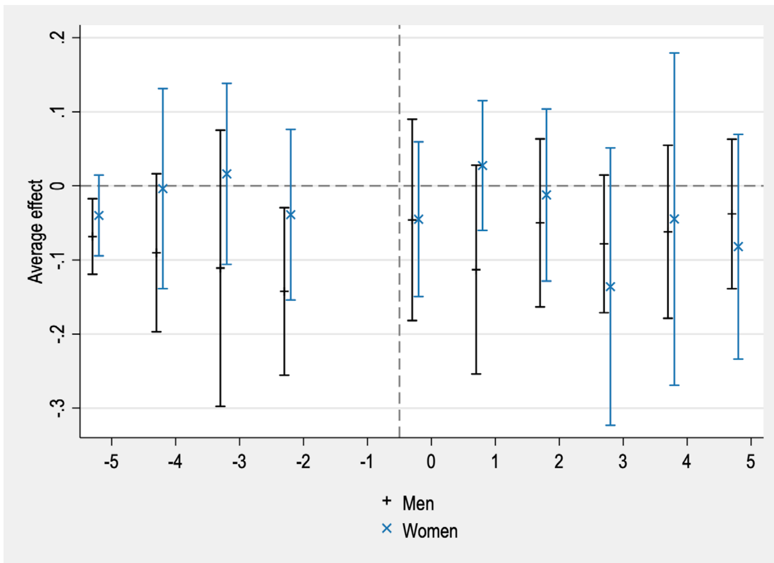


Notes: All specifications include state and year fixed effects, demographic controls, and state level controls. Data Source: CPS (1995-2019) – Panel A uses data from the monthly CPS and responses to the question, "do you have the same employer that you had last month?" Panel B uses data from the Annual 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 and liking this to the sex of household members. Models are estimated following Sun & Abraham (2021). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Health Insurance Coverage

Next, one may be concerned with the health insurance results given that the BRFSS only contains information regarding health insurance coverage and does not include information regarding the source of coverage. As such, in Appendix Figure F3 I re-estimate the effect of ADLs on health insurance, focusing on employers sponsored health insurance in the monthly CPS. Like the main results, I find no evidence of changes in health insurance coverage following the passage of ADLs.

Appendix Figure F3: Health Insurance Coverage - CPS



Notes: All specifications include state and year fixed effects, demographic controls, and state level controls. SSC's are identified by identifying relationships between individuals in a household and liking this to the sex of household members. The outcome takes the value 1 if the individual has employer sponsored insurance coverage and zero otherwise. Models are estimated following Sun & Abraham (2021). Clustered robust standard errors in parenthesis * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Social and Workplace Climate

Additionally, one may be concerned that the changes in social climates towards LGBTQ+ people may reflect broader changes in prejudice, rather than changes specific towards LGBTQ+ people. To explore this, I test whether sexual orientation-based ADLs resulted in changes in the incidence of hate crimes towards other marginalized populations. These results are presented in Appendix Table F1. In all cases, coefficients are statistically indistinguishable from zero.

Appendix Table F1: Anti-Discrimination Laws and Non-LGBTQ+ Hate Crimes

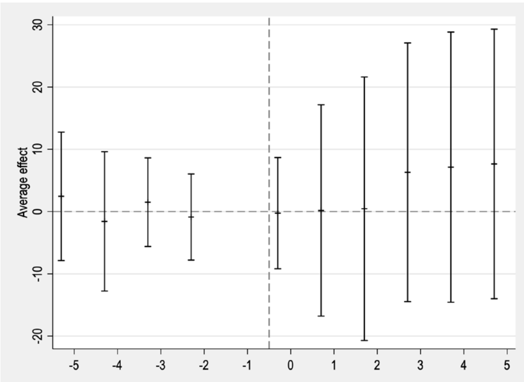
	(1)	(2)	(3)	(4)
	Racial Hate Crimes	Ethnicity Based Hate Crimes	Gender Based Hate Crimes	Religious Based Hate Crimes
ADL	-0.339 (0.274)	-0.020 (0.081)	-0.007 (0.006)	-0.101 (0.289)

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 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

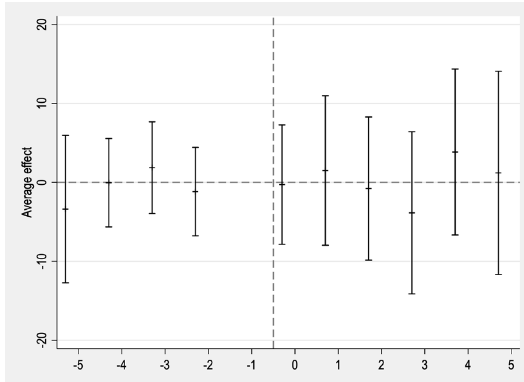
Next, I utilize data on words that have previously been used to proxy race- and gender-based animosity within Google trends data (Stephens-Davidowitz, 2014, 2017; Corbi and Picchetti, 2020) to estimate whether sexual orientation based ADLs are associated with changes in animosity towards other populations according to the google trends data. These results are presented in Appendix Figure F4. I find no evidence of ADLs being associated with changes in race- or gender-based animosity.

Appendix Figure F4: Changes in Animosity Towards Other Populations

Panel A: Gender Based



Panel B: Race Based



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. Omitted periods are period t-1 and t-6+. Data source: Google Trends (2004-2019).

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