Legal Differences in Non-Discrimination Laws and the Effect of Employment Protections for Gay Men

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Abstract

This paper studies the impact of legal differences in state-level employment nondiscrimination acts (ENDAs) for gay men on their labor market outcomes. I focus on what effect differences have on cohabitating gay men. Using a differences-in-differences approach, the results indicate that ENDAs increased the wages and employment of gay men when enacted. ENDAs increase hourly wages by 4.2% and employment by 2%. Controlling for the differences in these laws results in larger increases in the wages of gay men covered by ENDAs, but has no effect on employment. Stronger damages, statute of limitations, and attorney's fees increase the effect of ENDAs. The legal differences mattered more in the 2000s than in the 1990s. The age of the law helps explain why the employment effects were lower in the 1990s than in the 2000s, but not the wage effects. Possible reasons for this difference include changing returns to education and stronger enforcement. The results of this work suggest that ENDAs work along both the intensive and extensive margins, but strength of the law does not matter as much along the extensive margin.

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1 Introduction

Employment discrimination affects an estimated 15% to 43% of LGBT employees (Badgett, Lau, Sears and Ho (2007); Sears and Mallory (2011)). Over the last thirty years, the LGBT rights movement has lobbied to extend employment protections to cover gay men and lesbian women. These laws add sexual orientation to the list of reasons an employer may not discriminate against a worker. Despite the increasing number of states and localities passing laws and efforts to pass a federal law, there is little research on how effective these policies have been. This paper makes three important contributions to the literature. First, I provide improved estimates of the causal effect of employment nondiscrimination acts for gay men. Previous work either relied on cross-section analysis (Klawitter and Flatt (1998); Klawitter (2011)) or used very small samples of gay men (Martell (2013)). These papers found small positive effects of the law that were often not significant. This paper uses a larger sample of the gay population and the number of states with laws by using data across multiple years. Because of the longer time frame and increased sample size, the policy variation is richer. This allows me take into account how state laws differ on important policy questions. The size and scope of the data enables me to explore potential mechanisms behind the increase in wages and employment for gay men.

Second, I present evidence of the effect of individual provisions in the laws. Stronger laws may result in larger declines in discrimination towards gay men. Other papers treated all state nondiscrimination laws as the same (Klawitter and Flatt (1998); Klawitter (2011); Martell (2013)). This may have biased the results towards zero if strong laws and weak laws had offsetting effects. This paper looks at how differences in damages, employer size minima, statute of limitations, and attorney's fees lead to different effects of the laws. Work on the effect of legal provisions is normally limited by the federal minimum for discrimination cases (Neumark and Song (2013)). The benefit of studying nondiscrimination laws

for gay men is the lack of any federal law. States with provisions that would normally be superseded by the federal law are binding on employers in these cases.

Finally, by observing how the wage effects of ENDAs vary across the population of cohabitating gay men, this paper explores what one may expect from the passage of other LGBT employment nondiscrimination act. Discussions of whether to pass these laws occur at all levels of government. The results of this research provide guidelines for crafting these laws. Information on how provisions shape the effect of the laws is especially useful in this context.

This paper focuses on cohabitating gay men because they experience labor market outcomes that are relatively worse than cohabitating heterosexual men. Lesbian women are a less ideal case study because compared to cohabitating heterosexual women, they experience more advantageous labor market outcomes (Antecol, Jong and Steinberger (2008); Badgett (1995)) [1]. Baseline results show that employment nondiscrimination acts result in increased wages and employment for cohabitating gay men. Results also indicate the strength of the law matters if the goal of the law is to reduce labor market disparities. Stronger laws result in larger increases in wages. The provisions driving these differences are the statute of limitations and the damages available.

The rest of the paper proceeds as follows. Section 2 discusses previous research on discrimination towards gay men. Section 3 provides a brief overview of employment nonidscrimination acts for gay men, with Section A1 in the appendix containing a more detailed overview of the laws. Sections 4 and 5 discuss the data and methodology used. Section 6 reports the results, while Section 7 discusses the robustness checks used. I conclude in Section 8 by discussing the effect of ENDAs on subgroups of the gay population and what this may mean for a federal law.

[1] Lesbian women are also covered by ENDAs, but because of their gender they were previously a protected class. Therefore, any estimate of the effect of an ENDA for lesbian women is an estimate of the effect of increased protection, not the effect of new coverage.

2 Literature Review

Through the use of survey data and national representative samples, researchers have found strong evidence that gay men are discriminated against in the workplace, while the evidence is mixed for lesbians. Gay men experience both negative wage differentials relative to heterosexual men and negative reported experiences with discrimination (Sears and Mallory (2011)).

Survey results of LGBT individuals find consistent evidence of perceived discrimination against LGBT employees (Badgett et al. (2007)). Convenience samples find higher rates of perceived discrimination than surveys using random samples (Badgett et al. (2007)). Using the population representative General Social Survey, Sears and Mallory (2011) found that 27.1% of LGBT have suffered some form of discrimination at work and 7.1% report having lost a job because of their sexual orientation in the past five years. The majority of these cases in the General Social Survey are individuals who are out at work. Of gays and lesbians in the survey who are out at work, 37.7% have experienced some form of discrimination. This is compared to only 10.4% of the gays or lesbians who are not out. When looking at administrative data on discrimination complaints, gays and lesbians file formal complaints against employers at similar rates as women and racial minorities (Badgett et al. (2007); Government Accountability Office (2013)).

The literature in economics has focused on the wage differential between homosexuals and heterosexuals. Research has found large and persistent wage penalties for gay men and small wage premiums for lesbians (Antecol et al. (2008); Badgett (1995); Badgett et al. (2007); Black, Makar, Sanders and Taylor (2003); Blandford (2003); Carpenter (2004); Clain and Leppel (2001); Elmslie and Tebaldi (2007)). These differences are present for both the self-identified sexual orientation and the behaviorally populations of gay men and lesbian women (Blandford (2003); Carpenter (2004)). Returns to co-habitation and mar-

riage for gay men are not large enough to explain the negative wage differential (Allegretto and Arthur (2001); Burn and Jackson (2014); Zavodny (2007)). Occupational sorting and educational attainment also fail to explain the wage penalties and premiums observed in U.S. Census data (Antecol et al. (2008)).

Correspondence study exploring discrimination in hiring found strong evidence of discrimination against gay men (Drydakis (2009); Tilcsik (2011)). Tilcsik (2011) covered seven states in different parts of the United States. Resumes were indicated to be gay by involvement in a LBGT equality political organization. The control group was given membership in an environmental organization. There was large regional variation in the differences in callbacks. Industries that valued masculine traits were less likely to ask a gay applicant to interview.

Historically, the most common policy implemented in the U.S. to combat employment discrimination based on age, race, or gender has been an employment nondiscrimination act. Past evidence of the effect of these laws has shown them to increase the wages of those who are covered by the laws (Collins (2003); Donohue and Heckman (1991); Neumark and Stock (2006); Landes (1968)). Research on the effect of stronger laws has shown that in the case of age discrimination, stronger laws led to higher employment of older workers (Neumark and Song (2013)). This suggests that stronger laws may deter discrimination.

Research into the effects of LGBT nondiscrimination acts has found mixed evidence for the effectiveness of employment protections at the state-level. Klawitter and Flatt (1998) found that there was no effect of employment protections on the wage or employment differences between cohabitating gays and lesbians and heterosexuals using the 1990 Census. While average earnings for gay men and lesbians were higher in states and localities with employment protections, these higher earnings were due to worker and area characteristics. Klawitter (2011) revisited the question using 2000 Census data. She found that ENDAs did decrease the earnings differentials between cohabitating gay men and heterosexual men

by increasing hours worked. The laws had no effect on cohabitating lesbians. The result is that in 2000 cohabitating gay men in states with antidiscrimination policies earned 8% more than gay men in states that lacked employment protections. White men working for private employers in the upper half of the earnings distribution were the main beneficiaries of this wage increase (Klawitter (2011)).

Martell (2013) studies the effect employment protections on behaviorally gay men in the General Social Survey. He includes both single and cohabitating gay men. Using the 1994 to 2010 General Social Surveys, he found that employment nondiscrimination laws decrease the wage differential between behaviorally gay men and heterosexual men between 2% and 15% each year that the law has been enacted. When decomposing the wage differential, Martell (2013) finds that the differences in predicted wages of gay men and heterosexual men are only significant in states without employment nondiscrimination acts. The main cause of the increase in wages for gay men is the higher return to potential experience for gay men in states with employment protections.

3 Overview of Employment Nondiscrimination Legalization

While local municipalities began passing employment protections for LGBT individuals in the late 1970s, it was not until 1982 that Wisconsin passed employment protections for gay men and lesbian women. Figure 1 shows that by 1990 only 2 states and Washington, D.C. had employment protections for LGBT individuals. In 2000, the number of states had grown to 12. Currently 21 states and Washington, D.C. have employment nondiscrimination acts. This has led to increasingly larger portions of the LGBT population to be covered. In 6.1% of the cohabitating gay men in the 1990 Census were protected by a state-level employment nondiscrimination act. By 2012, that number had grown to 52% of

cohabitating gay men. There are two mini-booms in the early 1990s and the middle 2000s, with longer lulls in the mid-1980s and early 2010s (Figure 1).

The differences in attitudes towards gays and lesbians at the state-level may contribute to the pattern in the distribution of these protections (Figure 2). State laws are concentrated in the Northeast, Midwest, and the West. The geographic dispersion of local laws is broader. There are fewer states with no employment protections at any level. The South and the Southwest contain few LGBT employment protections. These areas feature higher levels of prejudice towards gays and lesbians (Figure 3).

States and localities with large LGBT communities or those that have politically active communities are more likely to have these protections (Colvin and Riccucci (2002); Wald, Button and Rienzo (1996)). The LGBT population in 2012 in states with an employment nondiscrimination law was 32% larger than in states with no law (Gates, Gary J. and Newton, Frank (2013)). In the states that passed laws, politicians do not appear to be reacting to observable measures of discrimination. Using the wage differential in the preceding Census, I compare the wage differential with the year passing a law. While the average wage differential is lower for states that passed a law in the 2000s than in the 1990s, there no significant correlation between the year the law was passed and the wage differential in the preceding Census (Figure 4). Just focusing on the wage differential in 1990 results in a similar conclusion. There is no relationship between the wage differential in a state and the timing of passing a law.

Given the large geographic dispersion of the law, it is unsurprising that policy differences exist on a number important points. When comparing state laws, the differences appear on three important issues: who is in the protected class under this law, how a complaint is resolved, and what damages and remedies are available for plaintiffs. Within these three groups there are thirteen provisions over which states differ. Table A8 details the areas where state laws diverge. Information about provisions comes from state laws and

from reports compiled by the Williams Institute and the Government Accountability Office (Sears, Hunter and Mallory (2009) and Government Accountability Office (2013)). For a more extensive discussion of the laws, please see the Legal Appendix in Section A1[2].

While it is not feasible to tease out the effect of every single difference, there are many provisions that can be studied. In this paper, I focus on damage availability, employer size minima, attorney's fees, and the statute of limitations.

There are three categories of damages: equitable relief, compensatory damages, and punitive damages. All states allow for equitable relief, which consists of remedies such as backed pay or being reinstated. Compensatory damages are used to replace lost earnings and compensate for pain and suffering. There are 18 states that provide for these in their laws. Punitive damages are designed to punish egregious violations of the employment nondiscrimination laws and are determined by the seriousness of the violation, not the damage done to the plaintiff. There are 13 states that provide for punitive damage in their statutes. Damage awards may be capped at a dollar amount, with the level varying by state. Some states cap the damages based on the size of the employer, while others cap the awards at a set amount. In 18 states, it is possible for a successful plaintiff to recoup attorney's fees as part of the damage awards.

The statute of limitations determines how long employees have to file their complaints. The average statute of limitations in states with an ENDA is 241 days (8 months). States range from 120 days to 365 days. There are 14 states with statutes of limitations of 6 months or shorter. The employer size minima determine how large a firm must be before they have to comply with the law. The minima range from 1 employee to 15 employees. In 9 states, all firms are covered. There are 4 states with minima of 15 employees (equal to the federal minimum).

[2]Table A10 details the differences in protected classes across states. The damages available in each state are detailed in depth in Table A12 and Table A11. Table A13 highlights differences in how discrimination complaints are handled by each state's enforcement agency.

These legal differences are important because they determine the expected cost of discriminating for employers. Damage provisions and attorneys' fees provisions will increase the expected cost of discriminating by increasing the cost of being found guilty. Provisions that lower the cost of filing a complaint increase the probability that an employer is sued. Laws with wider protected classes will increase the number of employees able to file a suit. This may cause stronger laws to result in a stronger response from employers because the expected cost of discriminating is higher.

4 Data

The data used come from the 2012 American Community Survey (ACS) 1-Year Sample and the 1990 and 2000 U.S. Census 5% Samples (Ruggles, Alexander, Gendadek, Goeken, Schroeder and Sobek (2010)).

To identify gay men in the U.S. Census, the Census collects information on householders and the relationships of everyone in the household to the householder. A gay couple is identified when the gender of the householder and the gender of the unmarried partner (or spouse) of the householder are both male. This means that there is no information on single gay men, only cohabitating gay men. Also missing from the sample are gay men in a household where one of the partners is not the household head (such as living with one's parents). Therefore, the sample in the analysis is restricted to comparisons between different groups of cohabitating couples.

The sample used in the analysis starts with all men older than 18 who claim to be the householder, spouse, or unmarried partner. Cohabitating gay men are identified in the sample if they are cohabitating with an individual of the same gender[3]. Once the gay

[3]Cohabitating is defined as either being married or in an unmarried partnership. Unmarried partnerships are defined as relationships where the unmarried partner shares a close personal relationship with the reference person.

cohabitating couples have been identified, the sample is restricted to those of prime working age 22 and 65. The age of 22 to avoid school-aged individuals and 65 is selected to avoid retired age individuals. To simplify the analysis a 25% sample of the full set is taken for heterosexual, while preserving all homosexuals.

An important issue to be aware of is heterosexual couples miscoding the sex of one of the individuals. Researchers have found little need to worry about miscoding in the sample of gay couples who claim to be unmarried partners (O'Connell and Golding (2006)). The miscoding is concentrated in the portion of same-sex couples that had their marital status reallocated by the Census Bureau according to their designated procedures (O'Connell and Loftquist (2009); O'Connell and Loftquist (2009)). Work on the wording of relationship questions in the ACS resulted in a large decline in the number of miscodings in the 2008 and subsequent surveys. Because of this miscoding, the sample of cohabitating gay men is restricted to unmarried partner households or those gay couples that reported being married using CAPI/CATI response modes. This is a very conservative cut of the cohabitating gay population, but it reduces the measurement error and allows for better causal estimates of the effect of ENDAs on cohabitating gay men[4].

Restricting the gay sample in this way results in a final sample of 1,359,287 heterosexual men and 29,183 cohabitating gay men. In 1990, there are 459 cohabitating gay men living in states with an employment nondiscrimination act. By 2000, there are 9,516 cohabitating gay men covered by the laws. In 2012, the number of cohabitating gay men in states with a law is 2,667.

To understand how the demographics of the sample of cohabitating men has changed over the past two decades, Table A2 reports the descriptive statistics for gay men in 1990 and 2012. Table A3 contains the same information for heterosexual men. The average years of schooling for a gay man has increased from 14.51 years to 15.59. Heterosexual

[4]For a more detailed discussion of the issue of miscoding, please see Section A2 in the Appendix.

men saw an increase in their number of years of schooling by 0.77 years. There was also a corresponding increase in the percent of men having degrees of higher education. The 2012 sample of gay men is less African-American and slightly less Hispanic than the 1990. The heterosexual male sample has seen an increase the percent that are African-American or Hispanic. Hours of work has declined, while weeks worked has increased. These changes were larger for gay men than they were for heterosexual men. Fewer gay men and fewer heterosexual men reported having children in the household in 2012 than they did in 1990. The average age of the sample has also increased from 1990 to 2012 for both groups. In 1990, gay men earned $18.94 per hour (in constant 1999 dollars). This increased to $25.01 in 2012, an increase of 32%. In the same time frame, heterosexual men only saw their incomes increase a dollar, from $21.01 per hour to $22.67, an increase of 8%.

Data on the prejudice of U.S. Census divisions towards gays and lesbians comes from the General Social Survey taken between 1990 and 2010. Prejudice is measured using responses from the General Social Survey. Using results from questions about respondents' views of homosexual sexual relationships, a person was coded as prejudiced if they viewed homosexual sexual relationships as wrong. They were considered not prejudiced if they viewed it as never wrong or expressed no opinion on the matter. The percent of prejudiced individuals in a given census division in the year of the survey was then calculated.

A longitudinal database of all state laws was created using information from The Williams Institute on Sexual Orientation and Gender Identity Law and Public Policy, the Government Accountability Office, and state laws (Sears et al. (2009); Sears and Mallory (2011); Government Accountability Office (2013)).

5 Estimation Strategy

The goal of this paper is to estimate the impact of employment nondiscrimination acts on the employment outcomes of gay men. I focus on the relationship between strength of the law and the observed changes. Because the laws in question vary by state over time, I use the state and year variations to estimate a differences-in-differences-in-difference model. I exploit differences across state, over time, and sexual orientation. I use the comparison of gay men and their married heterosexual counterparts to isolate the effect these laws have on the labor market outcomes of cohabitating gay men. To do this, I estimate the following differences-in-differences-in-differences specification for individual i:

$$\begin{aligned} Y_{ist} = \beta_0 + \beta_1 Gay_{ist} + \beta_2(Gay_{ist} \times ENDA_{st}) + \mathbf{X}_{ist}\delta + I_s\gamma_s \\ + I_t\gamma_t + (I_s \times I_t)\gamma_{st} + (Gay_{ist} \times I_s)\theta_{G,s} + (Gay_{ist} \times I_t)\theta_{G,t} + \epsilon_{ist} \end{aligned} \tag{1}$$

Y is the dependent variable of interest. In this paper, I look at log hourly wages, full-time employment, and being employed. G is a dummy for being a cohabitating gay man. *ENDA* is a dummy for state s having an employment nondiscrimination act that protected gay men in year t. The vector $\mathbf{X}$ contains controls for demographic, occupation, and geographic variables that may affect log wages, hours worked, or employment, and ϵ is a mean-zero error term[5]. Also included in the regression are state, year, and state-by-year fixed effects (α_s, γ_t, and θ_{st}). To account for differences between cohabitating gay men and heterosexual men, state and year fixed effects are interacted with the dummy for cohabitating gay couples ($\theta_{G,s}$ and $\theta_{G,t}$). Due to individual preferences potentially being correlated within a state and treatment occurring at the state-level, the standard errors are clustered at the state-level.

[5]X contains controls for demographic characteristics like schooling, potential experience, number and age of children, and race. The occupation controls are dummies for groupings of occupations in the ACS. The full list of controls can be found in Table A1.

For the analysis of employment status, the sample of all cohabitating men is used. In the analysis of full-time status, the sample of currently employed workers is used (defined as being actively employer more than 25 weeks in the previous year and working more than 30 hours a week on average). For the analysis of wages the sample was restricted to men working full-time. Individuals were also dropped from the analysis if they did not earn the majority of their income in wages.

The differential effect for being a cohabitating gay man in the United States across all years is β_1. How this differential changes over time is captured by $\theta_{G,t}$. The state fixed effects for the cohabitating gay couples ($\theta_{G,s}$) captures how this wage differential varies by state. The main parameter of interest is β_2, which is interpreted as the effect of the law on cohabitating gay men in a state with employment protections after legalization. It will capture how the differential between gay men and heterosexual men in those states with ENDAs has changed due to the change in the law. In order to interpret β_2 as being the effect of enacting employment protections for gay men, it must be the case that there are no other factors related to any changes in the gay-heterosexual wage differential in states that do and do not have LGBT employment protections.

The differences-in-differences methodology rests on the assumptions that the composition of the sample is not changing and that the unobservables are uncorrelated with the treatment. Error terms of the wage equation may not be parallel if the level of discrimination is changing faster in states with employment protections than in those states without employment protections. Using responses from the General Social Survey, it is possible to calculate the percent of individuals in a Census division that view homosexuality as always, almost always, or mostly wrong. The results of this calculation are shown in Figure 3. There has been a large decline in the level of prejudice towards gays and lesbians in all parts of the United States. The majority of Census divisions follow the same time-trend, with only one of the nine being significantly different at the 5% level. New England has a

region specific time trend that is decreasing faster than the rest of the country [6].

The composition of the sample of gay men has not changed between 1990 and 2012. We can test this by regressing being in a cohabitating same-sex couple on being in a state with an ENDA, state fixed effects, year fixed effects, state-by-year fixed effects, and state linear time trends. I find that while the coefficient on the ENDA variable is negative, it is not statistically significant. Men are no more likely to claim to be cohabitating with another man in states that have passed a law. The composition of cohabitating heterosexual men has changed over the time period. Men are less likely to be cohabitating in 2012 than in 1990. The rate of cohabitation has declined, but the rates have not changed in states with ENDAs after passage of a law.

In order to test the reasonableness of the estimates, I test how robust the results are to confounding factors. Results of the robustness checks are reported in Section 7. It may be the case that the results are biased since heterosexual households only have one respondent in the sample, but gay households have two (Klawitter (2011)). This could be due to intra-household allocation of resources in gay couples in response to the changes in the labor market after the passage of employment protections. To address this concern, I restrict the analysis to be between the householders and the partners.

I test how sensitive the results are to changes in marriage availability for same-sex couples. Previous research has found a marriage premium for gay men that is positive, but not statistically significant ((Burn and Jackson 2014); (Zavodny 2007)). The amount of overlap between states with same-sex marriage and employment protections is large enough to warrant concern. Every state with same-sex marriage has LGBT employment protections. To test the effect of same-sex marriage on the results, I control for same-sex couples live in a state where same-sex marriage is legal. I also report results where the

[6]The presence of ENDAs results in people becoming less prejudiced. The possibility of a change in prejudice induced by the change in the law is important in this context. The last state in New England passed its law in 2005. Before 2005, New England did not have a trend that was different from the rest of the nation. Their trend diverges only after the last state has legalized passed their law.

2008 data is used instead of the 2012 data with Massachusetts excluded to test whether same-sex marriage is biasing the results.

The geographic distribution of ENDAs means that much of the American South has no employment protections. Figure 3 shows these parts of the country also have the largest amount of prejudice towards gays and lesbians. So as a robustness checks, these states are dropped from the sample. This tests whether a large cluster of states not being treated biases the results.

In states with no state-level protections, there are some cities and counties that provide employment protection for private employees. Using information from the 2012 Municipality Equality Index (Human Rights Campaign (2012)), I control for living in a city with employment protections in states with no state-level protections.

6 Main Results

I begin by looking at how has the wage differential between gay men and heterosexual men evolved over the past 22 years. Table 2 reports the results for Equation 1 without $Gay \times ENDA$. The full list of controls can be found in Table A1. Across all years, gay men earned 29% less than a comparable heterosexual married man. States that enact ENDAs by 2012 had a smaller wage gap than those that do not pass a law. The wage differential between cohabitating gay men and married heterosexual men fell by 6.5% between 1990 and 2000 and by 1.9% from 2000 to 2012. The decline was larger in states with a law than in states without a law. It fell by 0.8% in the 1990s and by 1.3% more in the 2000s. This would imply that the coefficient of β_2 is positive.

6.1 Baseline Results

Table 3 contains the results from the baseline specification, Equation 1. The first three columns report the effect of ENDAs on log hourly wages. Gay men working full time earn 23.3% less than heterosexual workers between 1990 and 2000. The wage differential between cohabitating gay men and heterosexual men increased in the 2000-2012 sample to 23.6% less. The effect of having an ENDA was positive for both periods. From 1990 to 2000, the effect was 3% and not statistically significant. Between 2000 and 2012, the effect was 5.6% and significant at the 5% level. When I combine these two samples and look at the full period of 1990 to 2012, the effect of an ENDA was 4.2% and significant at the 5% level[7]. The next three columns in the top panel report the effect of ENDAs on full-time employment. In all periods, cohabitating gay men are less likely to work full-time than heterosexual men. There is no effect of ENDAs on full-time work status for cohabitating gay men. The last three columns contain the results for being employed. Cohabitating gay men are less likely to be employed than heterosexual men. This disparity was worse in the 2000-2012 sample than the 1990-2000 sample. ENDAs had a positive effect on employment for cohabitating gay men. From 1990-2000 cohabitating gay men in states with ENDAs were 1.8% likely to be employed. Between 2000-2012, they were 3% more likely.

The effects of the laws may not occur immediately after passage. There may be a learning curve for the agencies enforcing the law, employers, employees, and the lawyers. To test what effect the age of the law has on the effect, I modify Equation 1 by adding interactions between the presence of a law for gay men and the age of the law[8].

[7]This combined sample has data from the 1990 Census, the 2000 Census, and the 2012 ACS. The ACS has data on cohabitating gay men going back to 2008, so it is possible to increase the time frame observed in the sample. In this much broader sample, the effect of an ENDA is smaller. The laws increase wages by 2.6% and this effect is significant at the 5% level.

[8]This specification uses a linear term for the age of the law. I do a similar specification with cubic terms for age and found similar patterns for the labor market outcomes. These results for cubic terms of age are shown in Table A4. A graphical comparison of the effect of the laws on wages and employment is shown in Figure A2.

$$Y_{ist} = \beta_0 + \beta_1 G_{ist} + \beta_2 (G_{ist} \times ENDA_{st}) + \beta_3 (G_{ist} \times ENDA_{st} \times Age_{s,t}) + \mathbf{X}_{ist}\delta + I_s\gamma_s + I_t\gamma_t + (I_s \times I_t)\gamma_{st} + (G_{ist} \times I_s)\theta_{G,s} + (G_{ist} \times I_t)\theta_{G,t} + \epsilon_{ist} \quad (2)$$

The second panel of Table 3 shows the effects of controlling for the age of the law. The age of the law has a positive effect on the change in log hourly wages. In the 1990 to 2000 sample, the age of the law helps explain why there was no statistically significant effect in the baseline estimation. After controlling for age, there is no immediate effect of the law, but each year the law is in effect results in a 0.5% increase in wages. The age of the law has no effect on the probability of being employed full-time for cohabitating gay men. The effect of ENDAs on employment is larger after controlling for the age of the law, but the effect declines as the laws age by 0.2% per year. These results suggest that the wage effects of employment nondiscrimation laws are felt fairly shortly after passage. The employment gains that are caused by the laws are larger in the beginning, but decline as the laws age.

6.2 Effects of Legal Provisions

To test how the heterogeneity of the law affects the wage gains of cohabitating gay men, I add a term in Equation 1 to capture policy differences in the state law. I estimate the following equation:

$$Y_{ist} = \beta_0 + \beta_1 G_{ist} + \beta_2 (G_{ist} \times ENDA_{st}) + \beta_3 (G_{ist} \times ENDA_{st} \times P_{s,t}) + \mathbf{X}_{ist}\delta + I_s\gamma_s + I_t\gamma_t + (I_s \times I_t)\gamma_{st} + (G_{ist} \times I_s)\theta_{G,s} + (G_{ist} \times I_t)\theta_{G,t} + \epsilon_{ist} \quad (3)$$

where $P_{s,t}$ is the policy variable. States rarely change their nondiscrimination laws, but there have been amendments or court cases changing the law. The policy variables used

are the availability of damages, statute of limitations, employer size minima, and recouping attorney's fees. Table 1 shows how this policy variable varies by year and policy.

After controlling for differences in the provisions, the strength of the provisions matters for the results. This result occurs primarily for log hourly wages, but not much for full-time work status or employment. Table 4 uses the 1990-2012 sample to test what effect controlling for policy differences has on the effect of ENDAs[9].

The top panel in Table 4 shows the effect of damage availability. The first column reports the results for log hourly wages. All of the gains in wages from 1990 to 2012 are due to the presence of compensatory damages. Just passing an ENDA decreases wages by 1.2%. This decrease in wages is not statistically significant. Providing for compensatory damages in the law leads to a 6.3% increase in wages for cohabitating gay men. Adding punitive damages on top of the compensatory damages reduces wages by 2.9%, but this effect is not statistically significant[10]. After controlling for damage availability, the effect of ENDAs is a decline full-time work for cohabitating gay men by a not statistically significant 2.0%. Compensatory and punitive damages increase full-time work, but neither of these effects is statistically significant. After controlling for differences in the damages available, the effect of ENDAs on employment is now a significant 2.8% increase.

The second panel reports the results for the statute of limitations of complaints. Cohabitating gay men earn 3.9% more after the passage of an ENDA and this increase is significant at the 5% level. For every month the complaint period is lengthened above the 8-month average, gay men experience a 1.1% increase in wages in that state. This effect is

[9]While this table reports the results separately, Table A7 reports the results for the regression where all the policy variables were included. In that case, it is the damages provisions that are the main driver of the effect. The effect of the provisions on wages does vary differently over time (Table A5). The effect of damages on wages declines from the 1990-2000 sample to the 2000-2012 sample, while all other provisions increase in effect and significance in the 2000-2012 sample.

[10] Including controls for caps on the amount of damages rewarded results in estimates that are slightly different (Table A6). After controlling for caps, the effect of having punitive damages is now positive. Caps on compensatory damages have a positive effect on wages and caps on punitive damages have a negative effect. Caps on compensatory damages are associated with a 9.3% increase in wages and caps on punitive damages a not significant -2.5% decrease.

statistically significant at the 1% level. This means that a 12-month statute of limitation increases wages by 8.3%. The second and third columns report the results for full-time work status and employment. There is no effect of statute of limitations on full-time work or employment.

The third panel in Table 4 shows that the employer size minimum has no direct effect on wages for gay men. Controlling for differences in the size minima increases the effect of ENDAs. After adding the policy interaction for employer size minima, the effect of ENDAs on the wages of gay men rises to 6.8%. The second column considers full-time work. For every worker added to the minimum, the effect on full-time work status of cohabitating gay men increases by 0.2%. A state-law with an employer size minima equal to the federal minima of 15 employees results in a 3.0% increase in the odds of working full-time for cohabitating gay men. Employer size minima have no effect on employment for cohabitating gay men.

The last panel reports the effect of attorney's fees. Allowing for attorney's fees results in a 4.7% increase in wages. It also indicates that the effect in the baseline for full-time work status is masking a positive increase in full-time work for cohabitating gay men in states with laws that allow for attorney's fees to be recouped and a 3.2% decline in states that do not. After controlling for the availability of attorney's fees, the effect of passing a law on employment has increased to a 4.1% increase and is significant at the 1% level.

Taken as a whole, the results point to the fact that not all laws are created equal. The content of a law matters and can lead to very different outcomes depending on how weak or strong a law is. The results provide a framework for policy makers seeking to create a federal ENDA. The strength of the law matters when considering if the laws reduce the wage differential or increase employment. The effects of the law may not be immediate and take some time to develop. For policy researchers, this is evidence that taking into account differences in laws can change the conclusions one draws from their research. Even if the

provisions do not increase wages directly, controlling for the differences in the laws results in larger and often significant increases in wages for gay men in states with ENDAs.

7 Robustness Checks

A possible confounding factor to the above results is the introduction of same-sex marriage in some states. In 2012, 7 states had marriage equality[11]. If gay men receive a marriage premium similar to the one received by heterosexual men, then potentially we could be mistaking an increase in wages from the marriage premium as an increase in wages due to employment protections. The first column of Table 5 reports these results. It appears that this is a valid concern because being gay, claiming to be married, and living in a state with marriage equality results in a positive wage increase of 5.7% (significant at the 1% level). The effect of an ENDA with these controls has fallen to 3.1%, but is still significant at the 10% level.

Given the geographic distribution of ENDAs it is may be the case that having some of the most LGBT unfriendly labor markets with no ENDA protections biases the results. To address this concern, I replicate the above analysis and drop Census Divisions where all the states do not have an ENDA. This excludes the two divisions that cover much of the South, the West South Central and the East South Central Divisions. The second column of Table 5 reports these results. The results show that baseline results for gay men are robust to this exclusion. The effect of ENDAs has risen to 5.1% and is still significant at the 1% level.

Cohabitating gay men are both present in the sample for the analysis. This may bias results if they do not make their labor market choices independently. To test the effect of only using a single gay man, I restrict the sample to only use householders. This will

[11] Connecticut, Delaware, D.C., Massachusetts, New Hampshire, New York, and Vermont (Human Rights Campaign (2011))

provide an estimate of the effect of employment protections on the head of households. The third column of the bottom panel of Table 5 shows the effect of ENDAs has fallen to 3.9%, but is still statistically significant at the 10% level.

In many of these states with no employment protections, cities and counties have stepped in to provide employment protection for LGBT workers. Using information on cities and counties with laws from HRC's Municipality Equality Index (Human Rights Campaign (2012)), I add a specification where I control for city-level ENDAs in any city with more than 100,000 residents in a state with no law in 2011. This results in 2,629 gay men in 48 cities being added to the treatment group. Table 6 reports the effect of a local ENDA in states with no state law. The effect of a city-level ENDA is close to zero and not statistically significant[12]. This suggests that not controlling for them in the baseline estimation was not biasing the results.

8 Discussion

To this point, the effects reported were for the average cohabitating gay man, but the wage increase may vary across the gay population. Table 7 presents results of the wage effects of ENDAs by race, occupation, and location.

ENDAs may behave differently when they are the only protections covering a man than if they are layered on top of Title VII protections. In the first panel of Table 7, this pattern is clear to see. The effect of ENDAs for white gay men is a 4.4% increase in wages, while the effect for African-Americans and other minorities is close to zero.

Cohabitating gay men work in occupations that are on average 52.4% male, cohabitating heterosexual men work in occupations that are 64.4% male. This gender-nonconformity may lead different effects for ENDAs in different occupations. The second panel panel of

[12]The local laws are often weak compared to the state laws. The damages are often fines and there is no opportunity to take the case to court.

Table 7 shows the effect of ENDAs is larger in occupations that are more male. For every 10% increase in the proportion male in the occupation, the effect of ENDAs increases by 3%.

In the third panel of Table 7, the results show that passing a law benefits those gay men living in areas with smaller gay communities[13]. They receive a 6.9% wage increase, which is significant at the 5% level. Gay men in areas where the gay population is larger than average see no effect of a law. The groups that benefit the most from these policies are gay men who were not covered by the laws previously and who live or work in places where one might expect the wage differential to be largest. The benefits are concentrated in occupations and PUMAs that are more male-dominated and with fewer gay men.

How these laws increase wages and employment is an important question to consider. ENDAs may work by changing the returns to schooling or experience. There is evidence from Martell (2013) that ENDAs work by changing the returns to experience for behavioral gay men. In the first panel of Table 8, I interact the years of schooling with *(Gay* $\times$ *ENDA)* to test whether the passage of the law results in a change in the returns to schooling. After the passage of an ENDA, the returns to school increase for cohabitating gay men. Each year of school yield a 1% larger increase in wages than before the passage of the law. An extra year of school increases the odds of being employed by 0.7%.

The second panel reports the results for experience. There is an increase in the returns to experience. Each additional year yields a larger increase in wages than before the law. This difference is significant at the 5% level. The levels of potential experience for gay men in states with a law are not different from those in states without a law. Therefore, changing returns to experience would result in similar effects of the laws.

The change in the return to education and experience was no larger in the 2000s than

[13]The size of the gay population and the size of the cohabitating gay population are highly correlated. In 2012, the size of the LGBT population in each state and the number of cohabitating same-sex couples in a state had a correlation coefficient of 0.87 (Gates, Gary J. and Newton, Frank (2013); Ruggles et al. (2010))

in the 1990s. So this shift does not explain why the effect of laws is stronger from 2000 to 2012 than it was from 1990 to 2000 (Table 3 and Table A5). There is some evidence from government reports that the enforcement was less in the 1990s, with very few complaints going to court (Government Accountability Office (1997)). Those cases that did go to court in the 1990s were mainly focused on legal questions about the law. If the enforcement agency or employees are unlikely to file a complaint, the threat decreases and so does the effectiveness of the law. Despite the low numbers of suits in the 1990s, the number of complaints filed increased by 209% from 1996 to 2012 (Government Accountability Office (1997); Government Accountability Office (2013)). States that passed their laws before 1997 averaged 45.8 complaints filed per year in 1996. The average number of complaints in these states increased to 141.4 in 2012. This large increase in the number of complaints could have caused larger declines in discrimination in the second half of the sample. More complaints may lead to greater awareness of the law and the consequences of discriminating.

Taking the evidence as a whole, it is possible to predict the most likely effect of a federal law. Based on the results presented here, it is likely that a federal law would have larger effects as these state laws did. The take-away from the results presented in this section is that laws are most effective in areas here the wage differential is largest. Gay men in states without ENDAs are more white, work in more male dominated industries, and live in areas with fewer gay men. This tends to suggest that the laws will have larger effects than those that were passed by the states. The population of cohabitating gay men in states with no ENDA is less educated than those in states with a law. Therefore, a federal law may result in smaller effects if the changing return to schooling dominates other effects.

The most important determinant of the effect of a federal ENDA is the strength of the provisions in the law. The current draft of the federal law contains provisions similar to many state laws. The available damages and damage caps are similar to many state laws. The one difference is that the federal law would cover fewer firms because of its higher

employer size minimum. If we consider all of the estimates shown in this paper, it is clear that a federal law would reduce wage differentials and increase the employment of gay men. Whether the effect is larger than observed in states that passed their own laws depends on the construction of the bill. It may also be the case that a federal law will have positive spillover effects on gay men in states that have laws if it provides increased enforcement or is stronger than the state law.

9 Conclusion

The success of employment nondiscrimination laws at reducing the wage differential and increasing employment of cohabitating gay men is impressive. The effects reported in Table 3 are on similar in magnitude to the effects observed for African-American men under Civil Rights laws (Collins (2003)). In 2012, the wage differential in states with a law has fallen from 14.8% in 1990 to 4.7% in 2012. ENDAs were responsible for 42% of this decline (4.2% of 10.1%).

The results also allow us to consider what effect a federal employment nondiscrimination act may have. If a federal law was as strong as the average state law currently passed, then we would expect the labor market outcomes of cohabitating gay men to improve after the passage. ENDAs increase wages and employment of gay men. They also may increase the percent of gay men working full time. The biggest determinant of the success of the law would be whether or not the damages are similar to Title VII race cases or to age and gender cases. As shown in Table 4, when only liquidated damages are available the effect of ENDAs is much smaller than when some form of compensatory and punitive damages are available.

Taking into account what the law contains and how strong it was designed to be is important for accurately measuring the effect of ENDAs on labor market outcomes. When

researchers fail to take into account that weak laws may be unlikely to change employer behavior, results will underestimate the impact of employment protections. This is important for both economists and policy makers to consider as a federal ENDA is debated. The results from this analysis provide important insight into how such a law should be designed. Policy makers should consider each provision and whether they are strong enough to change employer behavior.

There are many questions left about how these employment protections work. This paper avoided years of data during the Great Recession, but understanding how the employment protections perform in recessions is important for policy makers to consider. While these laws may work in normal labor markets, they may not be able to combat hiring discrimination when there is a large number of unemployed.

This paper only addressed cohabitating gay men. Lesbian women present an interesting example of the effect of intersecting employment protections and provide interesting avenues for research. Other questions raised by this paper that are worthy of further thought include why there is no effect of city laws and what role did increased enforcement play in the increase in ENDA effectiveness.

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Figure 1: Timing of ENDA Enactment

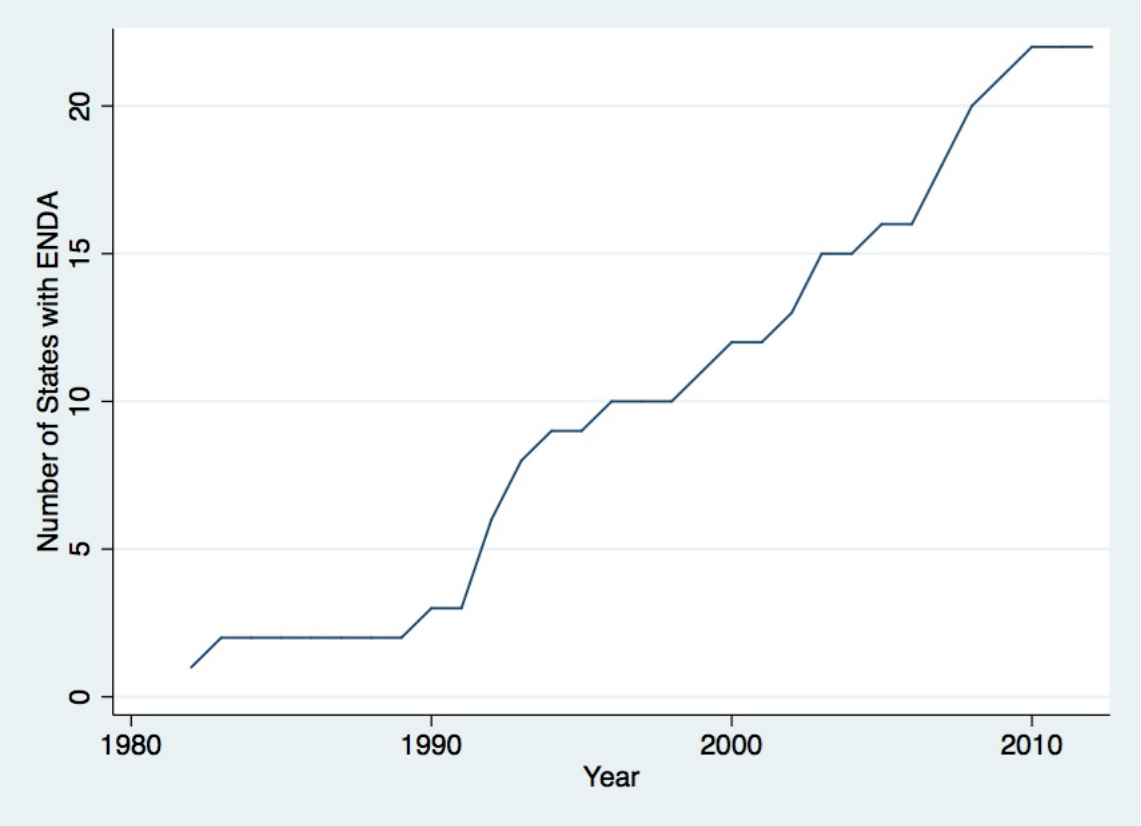


Figure 2: Map of ENDA Laws in 2011

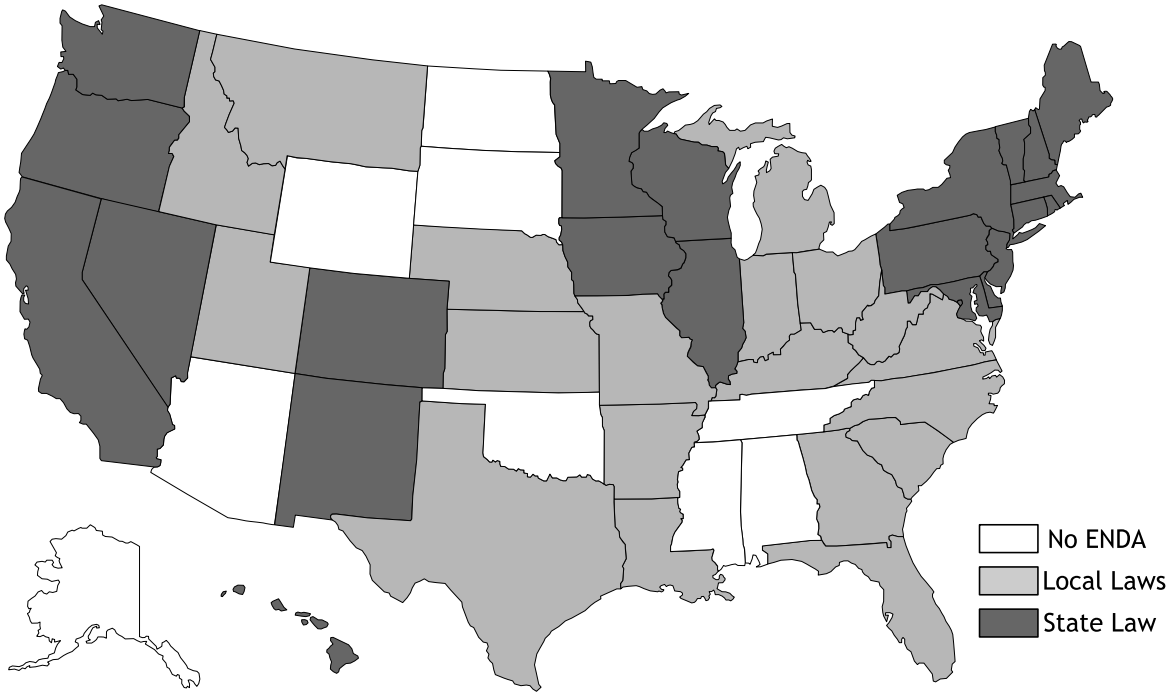
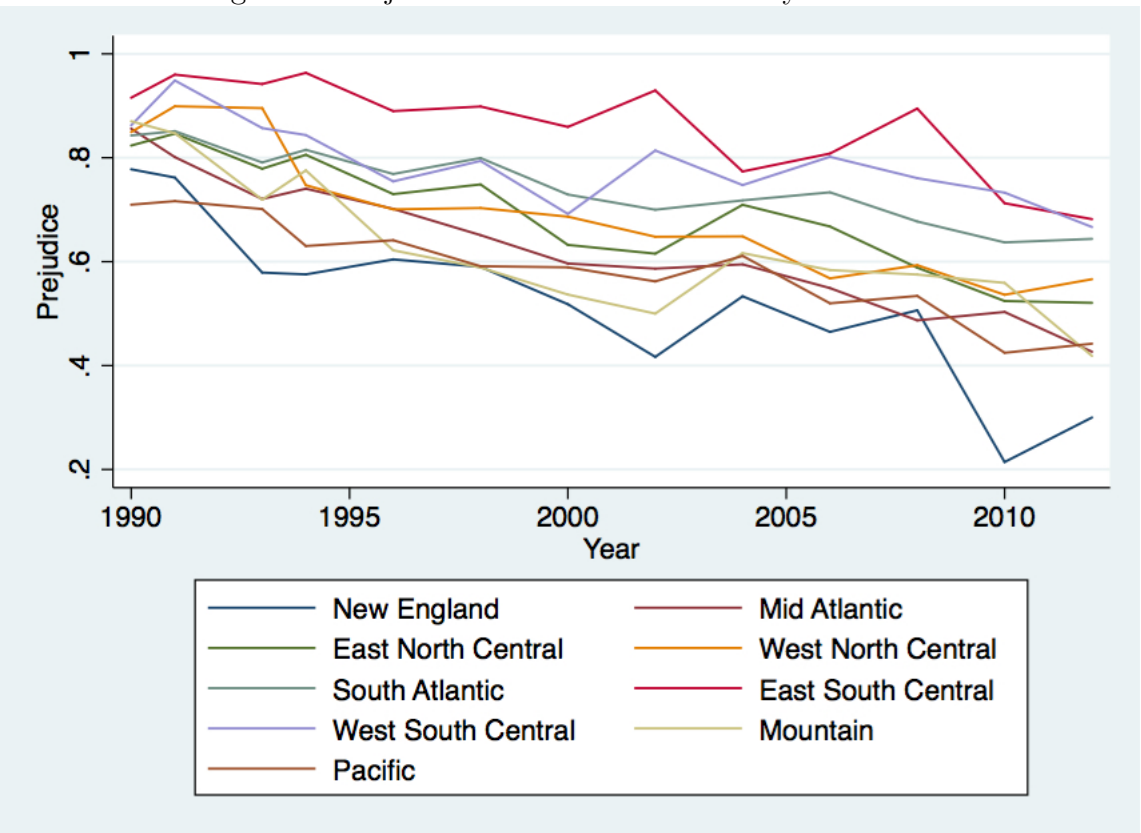
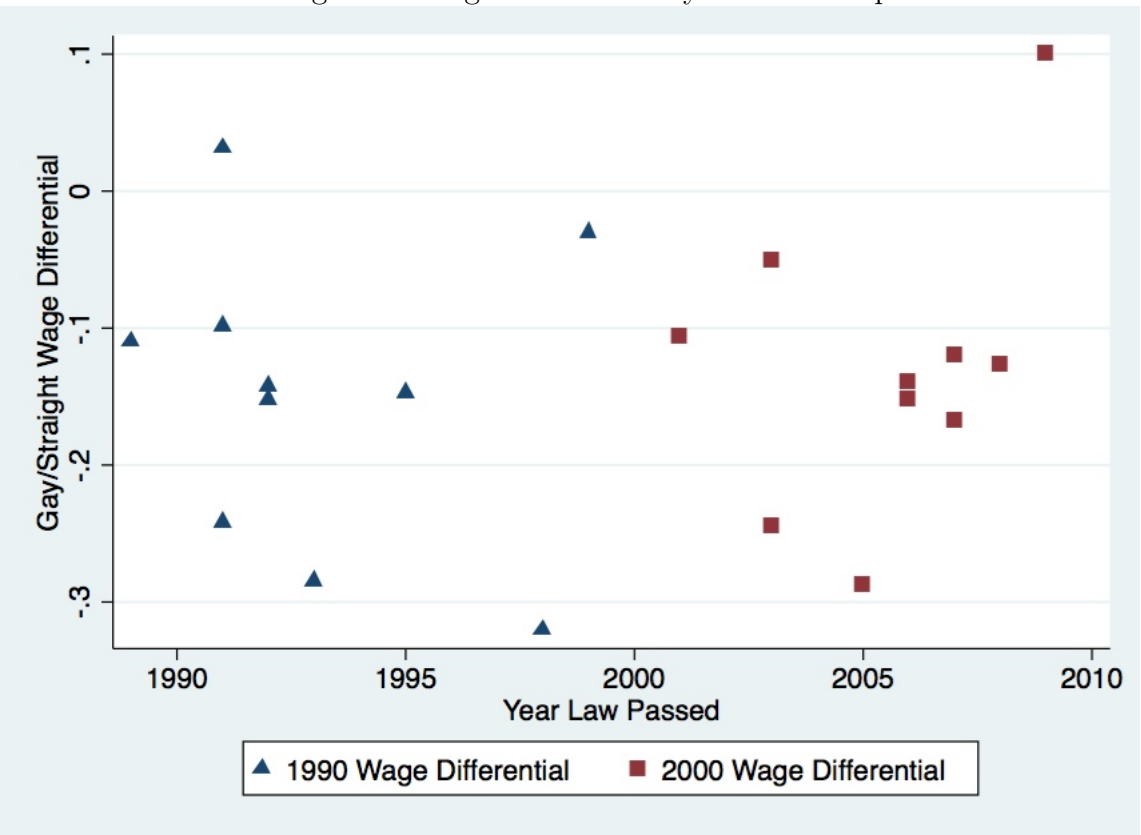


Figure 3: Prejudice over Time in the US by Census Division



Notes: This is graph shows how prejudice towards homosexuals has evolved in the General Social Survey. Prejudice is calculated as the percent of individuals in a Census division that view sexual relations between consenting adults of the same gender as always wrong, almost always wrong, or sometimes wrong. Only New England has a trend that is different from the other regions. Since 2008, New England is becoming less prejudiced at a faster rate than other regions.

Figure 4: Wage Differential by Year of Adoption



Notes: The wage differential for a state adopting a law from 1989 to 1999 is calculated using the 1990 Census data. Any state adopting a law after 2000 is calculated using the 2000 Census.

Table 1: Legal Provisions by Number of States

Law	2000	2012
ENDA	12	22
No ENDA	40	30
Damage Awards	2000	2012
Equitable Relief	12	22
Compensatory Damages	11	20
Punitive Damages	9	14
Attorney's Fees	10	19
Statute of Limitations	2000	2012
120 Days	0	1
180 Days	7	12
300 Days	3	4
365 Days	3	5
Employer Size Minima	2000	2012
1 Employee	6	9
3 Employees	1	1
4 Employees	1	4
5 Employees	1	1
6 Employees	2	2
8 Employees	0	1
15 Employees	1	4

Notes: Information on state laws comes from Sears et al. (2009), the Government Accountability Office (Government Accountability Office (2013)), and information from state laws collected by the author.

Table 2: Homosexual-Married Heterosexual Wage Differentials Over Time

	Dependent Variable: Log Wage		
	All States	ENDA	No ENDA
Gay	-0.290***	-0.148***	-0.275***
	(0.011)	(0.015)	(0.123)
Gay × 2000	0.065***	0.073***	0.051***
	(0.014)	(0.021)	(0.015)
Gay × 2012	0.084***	0.101***	0.059***
	(0.015)	(0.022)	(0.017)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. Weights in estimation are ACS and Census individual sample weights. Regressions include demographic, occupation, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. In this table, ENDA means a state has adopted a law at some point between 1990 and 2012. Standard errors clustered at the state level are in parentheses.

Table 3: Effects of ENDAs on Labor Market Outcomes of Cohabitating Gay Men

	Baseline								
	Log Hourly Wages			Full-Time Employed			Employed		
	1990-2000	2000-2012	1990-2012	1990-2000	2000-2012	1990-2012	1990-2000	2000-2012	1990-2012
Gay	-0.233***	-0.236***	-0.251***	-0.041***	-0.046***	-0.056***	-0.040***	-0.100***	-0.105***
	(0.010)	(0.008)	(0.009)	(0.004)	(0.004)	(0.005)	(0.003)	(0.006)	(0.007)
Gay × ENDA	0.038	0.056**	0.042**	0.001	0.014	0.005	0.018***	0.030*	0.020
	(0.023)	(0.022)	(0.016)	(0.006)	(0.009)	(0.007)	(0.005)	(0.018)	(0.013)
	Age of Law								
	Log Hourly Wages			Full-Time Employed			Employed		
	1990-2000	2000-2012	1990-2012	1990-2000	2000-2012	1990-2012	1990-2000	2000-2012	1990-2012
Gay	-0.230***	-0.237***	-0.275***	-0.041***	-0.046***	-0.057***	-0.042***	-0.103***	-0.109***
	(0.011)	(0.008)	(0.013)	(0.004)	(0.005)	(0.005)	(0.004)	(0.008)	(0.008)
Gay × ENDA	-0.000	0.058**	0.041	-0.003	0.014*	0.006	0.045***	0.034*	0.033**
	(0.024)	(0.027)	(0.025)	(0.015)	(0.008)	(0.007)	(0.005)	(0.018)	(0.014)
Gay × ENDA × Age of Law	0.005**	-0.001	0.000	-0.000	0.000	0.001	-0.003***	-0.001	-0.002*
	(0.002)	(0.003)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The coefficient on *Gay* × *ENDA* corresponds to β_2 in the baseline specification, Equation 1. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic and occupation controls, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses.

Table 4: Effects of ENDA Provisions on Cohabitating Gay Men:1990-2012

	Damages		
	Log Hourly Wages	Full-Time Employment	Employed
Gay	-0.273***	-0.056***	-0.104***
	(0.009)	(0.005)	(0.007)
Gay × ENDA	-0.012	-0.020	0.028*
	(0.019)	(0.017)	(0.015)
Gay × ENDA × Compensatory	0.063***	0.025	-0.004
	(0.031)	(0.018)	(0.020)
Gay × ENDA × Punitive	-0.029	0.010	-0.024
	(0.043)	(0.015)	(0.020)
	Statute of Limitations		
	Log Hourly Wages	Full-Time Employment	Employed
Gay	-0.272***	-0.056***	-0.104***
	(0.010)	(0.005)	(0.007)
Gay × ENDA	0.039**	0.005	0.020
	(0.014)	(0.007)	(0.012)
Gay × ENDA × Statute of Limitation	0.013***	0.001	0.002
	(0.003)	(0.002)	(0.004)
	Employer Size Minima		
	Log Hourly Wages	Full-Time Employment	Employed
Gay	-0.273***	-0.057***	-0.104***
	(0.012)	(0.005)	(0.007)
Gay × ENDA	0.068***	-0.009	0.019
	(0.025)	(0.855)	(0.019)
Gay × ENDA × Size Minimum	-0.004	0.002*	0.000
	(0.003)	(0.001)	(0.001)
	Attorney's Fees		
	Log Hourly Wages	Full-Time Employment	Employed
Gay	-0.274***	-0.056***	-0.105***
	(0.009)	(0.005)	(0.007)
Gay × ENDA	-0.003	-0.032**	0.041***
	(0.021)	(0.013)	(0.011)
Gay × ENDA × Fees	0.047**	0.039***	-0.022
	(0.022)	(0.014)	(0.015)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The coefficient on *Gay* × *ENDA* corresponds to β_2 in the baseline specification, Equation 1. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic and occupation controls, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses.

Table 5: Robustness Checks: 1990-2012

	Same-Sex Marriage Controls	No Southern States	Householders
Gay	-0.274***	-0.287***	-0.231***
	(0.011)	(0.013)	(0.018)
Gay × ENDA	0.031*	0.051***	0.039*
	(0.015)	(0.015)	(0.022)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The coefficient on *Gay* × *ENDA* corresponds to β_2 in the baseline specification, Equation 1. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic and occupation controls, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses.

Table 6: Effect of Local Laws on Wages of Cohabitating Gay Men

	No State Law		
	1990-2000	2000-2012	1990-2012
Gay	-0.245***	-0.252***	-0.289***
	(0.010)	(0.012)	(0.017)
Gay × Local ENDA	-0.007	0.018	0.006
	(0.019)	(0.017)	(0.016)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The coefficient on *Gay* × *Local ENDA* corresponds to β_2 in the baseline specification, Equation 1. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic and occupation controls, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses. Inforation on local laws comes from Human Rights Campaign (2012)

Table 7: Effects of ENDA By Subgroup

	Race/Ethnicity		
	White	African-American	Other
Gay	-0.313***	-0.163***	0.165**
	(0.012)	(0.046)	(0.067)
Gay × ENDA	0.044**	-0.003	0.001
	(0.020)	(0.114)	(0.063)
	Percent Male in Occupation		
Gay	-0.275***		
	(0.011)		
Gay × ENDA	0.045***		
	(0.016)		
Gay × ENDA × Percent Male	0.003***		
	(0.001)		
	Gay Population		
	Below Average	Above Average	
Gay	-0.253***	-0.458***	
	(0.012)	(0.036)	
Gay × ENDA	0.062**	0.007	
	(0.027)	(0.040)	

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The coefficient on *Gay* × *ENDA* corresponds to β_2 in the baseline specification, Equation 1. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic and occupation controls, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses.

Table 8: Drivers of Wage Effect: 1990-2012

	Change in Returns to Schooling	
	Log Hourly Wage	Employment
Gay	-0.274***	-0.104***
	(0.011)	(0.007)
Gay × ENDA	0.033*	0.015
	(0.017)	(0.013)
Gay × ENDA × Years of School	0.010**	0.007***
	(0.004)	(0.002)
	Change in Returns to Experience	
	Log Hourly Wage	Employment
Gay	-0.275***	-0.106***
	(0.011)	(0.008)
Gay × ENDA	0.028*	0.019
	(0.015)	(0.013)
Gay × ENDA × Experience	0.013**	-0.001
	(0.005)	(0.003)
Gay × ENDA × Experience^2	-0.0003***	-0.0000
	(0.0001)	(0.0001)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The coefficient on *Gay* × *ENDA* corresponds to β_2 in the baseline specification, Equation 1. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic and occupation controls, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses. Schooling and potential experience have been demeaned to allow for easier intepretation of the coefficients.

Table A1: List of Variables

Variable Definition	*Acronym*
Dependent Variable:	
Natural logarithm of hourly earnings (= total annual salary earnings divided by total number of hours worked per year) in previous year, in constant 1999 USD	LnWage
Usual hours worked per week	Uhrswork
Employed	Emp
Full-time employee (=1 if working more than 30 hours a week and 25 weeks a year)	FT
Independent Variables:	
Relationship Status	
Sexual Orientation (=1 if Homosexual, =0 if Heterosexual)	Gay
Marital Status (=1 if Married, =0 if Otherwise)	Married
Demographics	
Experience (Potential, =Age - Schooling - 5)	Exp
Experience Squared	ExpSq
Black	Black
Hispanic	Hispanic
Non-Native English Speaker (=1 if True, =0 if False)	English
Children (=1 if True, =0 if False)	Kids
Children Under 5 Years Old (=1 if True, =0 if False)	Youngkids
Education (=1 if True, =0 if False)	
Schooling	Schooling
Schooling Squared	SchoolingSq
High School Graduate	HSG
Associate's Degree	AA
Bachelor's Degree	BA
Master's Degree	MA
Ph.D. or Professional Degree	PhD
Occupation (=1 if True, =0 if False)	
Service	Service
Manager	Manager
Professional	Professional
Health Care Professional	Health
Teacher or Education	Teach
Technical Profession	Tech
Sales	Sales
Administrative	Admin
Finance	Finance
Agriculture or Farming	Farm
Craft or Repair	Craft
Laborer	Labor
Transportation	Transport
Military	Military
Location (=1 if True, =0 if False)	
Urban	Urb
State (=1 if True, =0 if False)	
Non-Wage Benefits Variables	
Treatment Variables	
Employment Non-Discrimination Act Protections for Homosexuals in State (=1 if True, =0 if False)	ENDA
Years Since ENDA Enactment (Enactment Year = 1)	AgeLaw
Strength of Provisions(=1 if True, =0 if False)	
Compensatory Damages	Comp
Punitive Damages	Pun
Compensatory Damages Cap	CompCap
Punitive Damages Cap	PunCap
Statute of Limitations	SoL
Employer Size Minimum	Size
Attorney's Fees	Fees

Notes: Sources of all variables is the 2012 American Community Survey 1-Year Sample, the 1990 to 2012 General Social Surveys, and Sears et al. (2009).

Table A2: Summary Statistics: Homosexual Men

	1990		2012	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Hourly Wage	18.94	24.24	25.01	39.83
Years of Schooling	14.511	3.69	15.59	3.11
Years of Schooling Squared	224.43	94.07	252.79	89.58
AA	0.06	0.25	0.08	0.28
BA	0.23	0.42	0.28	0.45
MA	0.10	0.30	0.13	0.34
PhD	0.05	0.21	0.07	0.26
Experience	15.93	10.05	24.01	11.53
Experience Squared	390.49	433.17	710.01	562.79
African American	0.09	0.28	0.06	0.24
Hispanic	0.13	0.33	0.12	0.33
Poor English	0.04	0.20	0.02	0.14
Service	0.12	0.32	0.10	0.31
Manager	0.16	0.37	0.20	0.40
Professional	0.10	0.30	0.12	0.32
Health	0.02	0.14	0.05	0.21
Teacher	0.04	0.19	0.06	0.23
Technical	0.04	0.20	0.05	0.21
Sales	0.06	0.23	0.05	0.22
Admin	0.13	0.34	0.11	0.31
Finance	0.02	0.16	0.03	0.17
Farm	0.02	0.14	0.01	0.09
Craft	0.07	0.26	0.04	0.20
Laborer	0.05	0.20	0.02	0.14
Transportation	0.07	0.24	0.04	0.21
Military	0.00	0.05	0.00	0.04
Children	0.09	0.27	0.07	0.25
Young Children	0.03	0.15	0.02	0.14
Weeks worked per Year	45.55	11.32	47.62	9.63
Usual Hours Worked per Week	37.96	15.53	34.67	18.30
Observations	7356		5205	

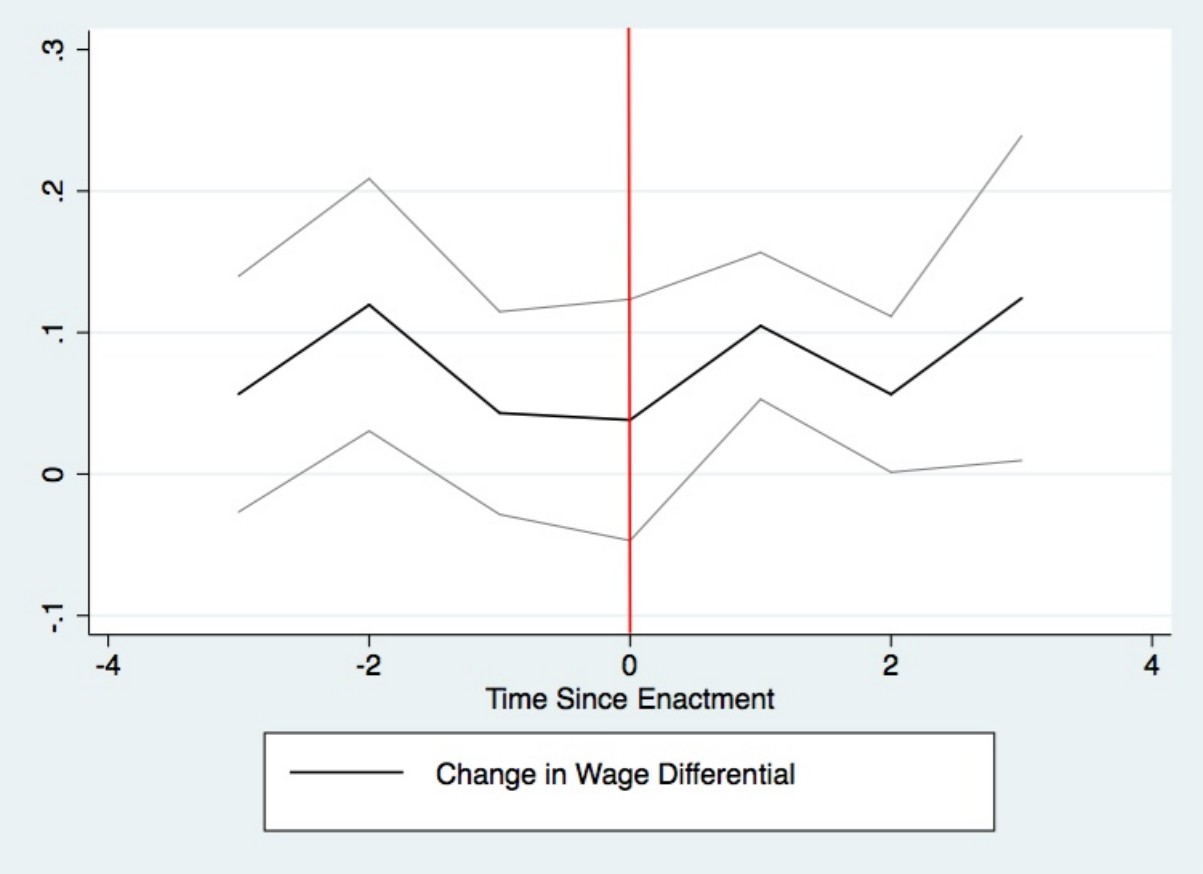
Notes: Author's calculations based on data from the 2012 American Community Survey 1-Year Sample and 1990 Census. Columns contain variable means and standard deviations in parentheses. Variable means are weighted by corresponding census and ACS individual sample weights. All variables are as defined in text or Table A1.

Table A3: Summary Statistics: Heterosexual Men

	1990		2012	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Hourly Wage	21.00	27.81	22.67	36.27
Years of Schooling	13.78	3.35	14.55	3.14
Years of Schooling Squared	201.18	85.62	221.57	85.72
AA	0.07	0.25	0.08	0.27
BA	0.15	0.35	0.20	0.40
MA	0.06	0.24	0.09	0.28
PhD	0.03	0.17	0.04	0.20
Experience	22.92	12.62	26.51	12.22
Experience Squared	684.79	572.04	852.55	642.38
African American	0.07	0.26	0.08	0.27
Hispanic	0.08	0.26	0.13	0.33
Poor English	0.03	0.16	0.04	0.20
Service	0.08	0.26	0.10	0.30
Manager	0.13	0.33	0.14	0.34
Professional	0.07	0.26	0.09	0.28
Health	0.01	0.10	0.02	0.13
Teacher	0.03	0.17	0.03	0.18
Technical	0.04	0.19	0.04	0.19
Sales	0.05	0.21	0.05	0.21
Admin	0.06	0.24	0.06	0.25
Finance	0.02	0.14	0.02	0.13
Farm	0.03	0.15	0.02	0.15
Craft	0.20	0.40	0.16	0.37
Laborer	0.09	0.28	0.06	0.23
Transportation	0.13	0.33	0.11	0.32
Military	0.02	0.13	0.01	0.10
Children	0.63	0.48	0.55	0.49
Young Children	0.22	0.42	0.16	0.37
Weeks Worked per Year	47.89	9.21	47.87	9.18
Usual Hours Worked per Week	39.93	15.84	37.24	18.02
Observations	434,347		494,608	

Notes: Author's calculations based on data from the 2012 American Community Survey 1-Year Sample and 1990 Census. Columns contain variable means and standard deviations in parentheses. Variable means are weighted by corresponding census and ACS individual sample weights. All variables are as defined in text or Table A1.

Figure A1: Change in Wage Differential Over Time



Notes: This is the change in the wage differential in states that passed an ENDA between 2005 and 2008. The timing has been normalized so that 0 is the year before the law was passed and 1 is the first year post legalization. Becuase of the severe miscoding in the pre-2008 data, the individuals in this sample are those individuals who did not have their marital status reallocated.

Table A4: Age of Law as a Cubic: 1990-2012

	Log Hourly Wage	Employment
Gay	-0.273***	-0.109***
	(0.013)	(0.008)
Gay × ENDA	-0.021	-0.025
	(0.049)	(0.045)
Gay × ENDA × Age of Law	0.016	0.011
	(0.010)	(0.008)
Gay × ENDA × Age of Law^2	-0.0011*	-0.0008*
	(0.0005)	(0.0004)
Gay × ENDA × Age of Law^3	0.0002**	0.0000*
	(0.0001)	(0.000)

*** p<0.01, ** p<0.05, * p<0.1

Notes:Author's calculations based on data from the 2012 American Community Survey 1% PUMS, the 1990 Decennial Census 5% PUMS, and the 2000 Decennial Census 5% PUMS. The wage differential used is the absolute value of the wage differential in the preceding Census before the law was passed. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic, occupation, state fixed effects, year fixed effects, and state-by-year fixed effects. For a detailed list of controls, see Table A1. Standard errors clustered at the state level are in parentheses.

Figure A2: Change in Log Hourly Wage and Employment as Law Ages

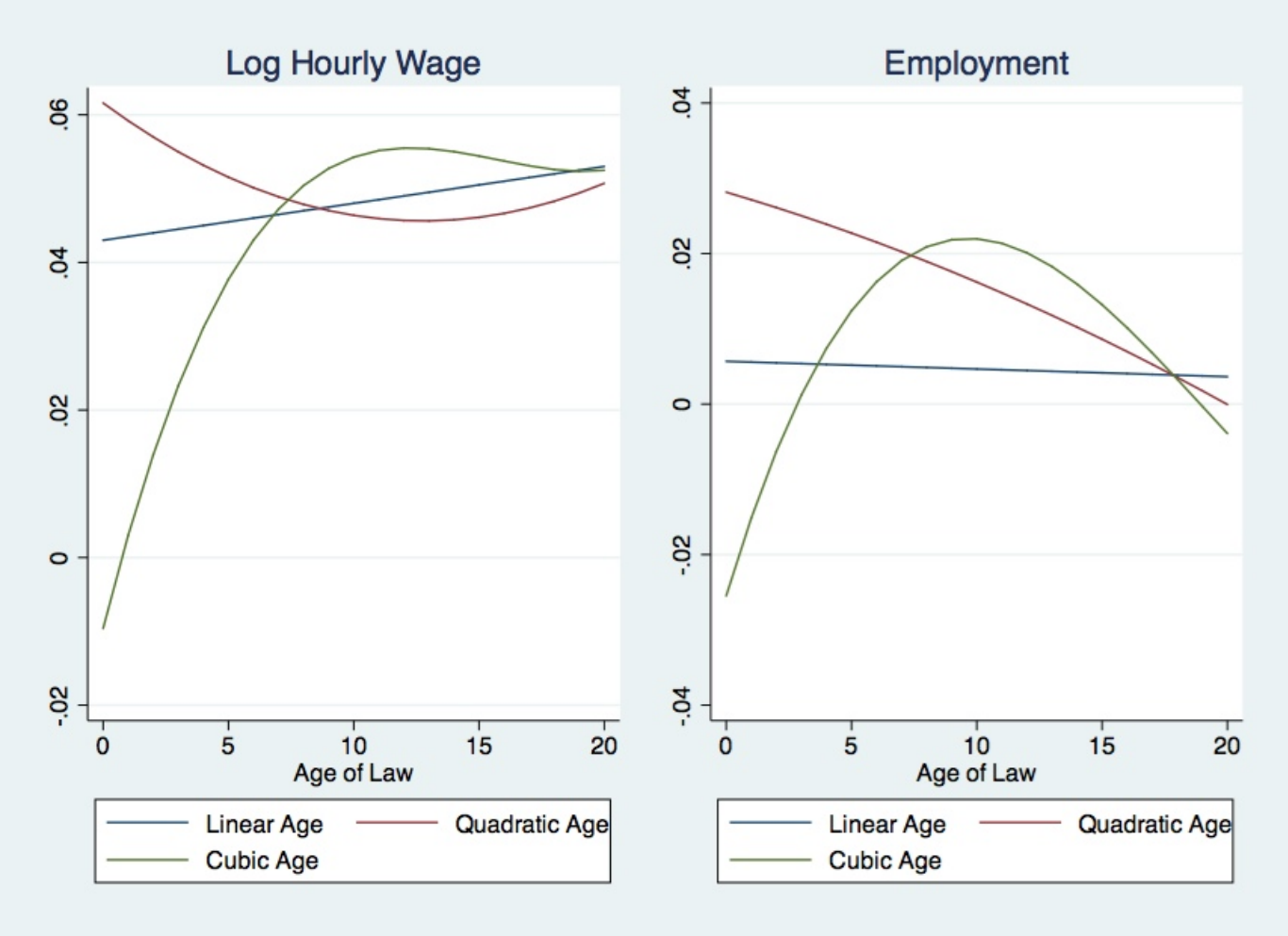


Table A5: Effects of ENDA Provisions on Wages of Gay Men

	Damages		
	1990-2000	2000-2012	1990-2012
Gay	-0.233***	-0.236***	-0.274***
	(0.010)	(0.008)	(0.011)
Gay × ENDA	-0.061	-0.012	-0.012
	(0.039)	(0.024)	(0.019)
Gay × ENDA × Compensatory	0.118***	0.079***	0.063***
	(0.036)	(0.023)	(0.021)
Gay × ENDA × Punitive	-0.078	-0.015	-0.029
	(0.048)	(0.064)	(0.044)
	Statue of Limitations		
	1990-2000	2000-2012	1990-2012
Gay	-0.233***	-0.208***	-0.272***
	(0.010)	(0.004)	(0.012)
Gay × ENDA	0.029	0.056**	0.039***
	(0.021)	(0.022)	(0.013)
Gay × ENDA × Statute of Limitation	0.012	0.011**	0.013***
	(0.010)	(0.005)	(0.003)
	Attorney's Fees		
	1990-2000	2000-2012	1990-2012
Gay	-0.233***	-0.236***	-0.274***
	(0.010)	(0.008)	(0.011)
Gay × ENDA	-0.071	-0.018	-0.003
	(0.085)	(0.015)	(0.021)
Gay × ENDA × Fees	0.112	0.081***	0.047**
	(0.086)	(0.017)	(0.022)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Gay × Post × ENDA is the DDD estimate. The additional variables estimate the additional effect of the provisions. Author's calculations based on data from the 2012 American Community Surveys and the 1990 US Decennial Census PUMS. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic, occupation, state fixed effects, year fixed effects, state-by-year fixed effects, and regional prejudice. For a detailed list of controls, see Table A1. Regional prejudice controls are the percent of individuals in a region who believe sexual relations between individuals of the same gender is wrong. Standard errors clustered at the state level are in parentheses.

Table A6: Effect of Caps on Damages:1990-2012

	1990-2012
Gay	-0.274***
	(0.013)
Gay × ENDA	-0.081*
	(0.042)
Gay × ENDA × Compensatory	0.059**
	(0.022)
Gay × ENDA × Compensatory Caps	0.093**
	(0.037)
Gay × ENDA × Punitive	0.014
	(0.044)
Gay × ENDA × Punitive Caps	-0.0259
	(0.034)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Gay × Post × ENDA is the DDD estimate. The additional variables estimate the additional effect of the provisions. Author's calculations based on data from the 2012 American Community Surveys and the 1990 US Decennial Census PUMS. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic, occupation, state fixed effects, year fixed effects, state-by-year fixed effects, and regional prejudice. For a detailed list of controls, see Table A1. Regional prejudice controls are the percent of individuals in a region who believe sexual relations between individuals of the same gender is wrong. Standard errors clustered at the state level are in parentheses.

Table A7: All Provisions

	1990-2012
Gay	-0.273***
	(0.013)
Gay × ENDA	-0.009
	(0.035)
Gay × ENDA × Compensatory	0.059*
	(0.030)
Gay × ENDA × Compensatory Caps	0.065***
	(0.021)
Gay × ENDA × Punitive	0.008
	(0.043)
Gay × ENDA × Punitive Caps	-0.036*
	(0.021)
Gay × ENDA × Statute of Limitations	0.006
	(0.005)
Gay × ENDA × Size Minimum	-0.004
	(0.003)
Gay × ENDA × Fees	-0.022
	(0.022)

*** p<0.01, ** p<0.05, * p<0.1

Notes: Gay × Post × ENDA is the DDD estimate. The additional variables estimate the additional effect of the provisions. Author's calculations based on data from the 2012 American Community Surveys and the 1990 US Decennial Census PUMS. Cohabitating gay men are those who are coded as unmarried same-sex partners. Weights in estimation are ACS and Census individual sample weights. All regressions include demographic, occupation, state fixed effects, year fixed effects, state-by-year fixed effects, and regional prejudice. For a detailed list of controls, see Table A1. Regional prejudice controls are the percent of individuals in a region who believe sexual relations between individuals of the same gender is wrong. Standard errors clustered at the state level are in parentheses.

A1 Legal Appendix

When a state passes an employment nondiscrimination act they are attempting to force employers to pay minority workers more by making it costly to discriminate. The goal is to set the law so the expected cost of discriminating is high enough to deter discrimination. The problem is that this would require different damage caps for each minority group since the amount of discrimination varies widely by minority group. There is some evidence of policy makers attempting to do this at the federal level by having the value of damages capped at the same levels for all discrimination complaints, but with different types of damages available [14]. The effect of these protections depends on the expected cost of discriminating for employers. The expected cost of discriminating is the amount an employer can expect to lose in a court if they lose the case. The expected cost will depend on the legal remedies available, the probability of being sued, and the number of people who can sue. Increasing these values will result in larger incentives not to discriminate.

These laws were written in the 1960s and 1970s to address race and gender discrimination in the workplace. The laws are rarely amended, often the only change is in which classes of workers are covered (as is the case in LGBT ENDAs) [15]. In some cases this means the legal protections may be higher than needed to combat discrimination. In others, it may mean the protections do not lead to a high enough expected cost to employers. This leads to a large variance in the effect of employment protections due to the heterogeneity in their design. We can exploit these differences in legal regimes to understand what employment protections contribute to the observed change in employer decisions.

The actual number of lawsuits filed does not determine the effect of these laws. The number of complaints filed with state agencies each year is very small. There are an estimated 1,338,164 gays and lesbians in California, but each year only 750 formal complaints of sexual orientation discrimination are filed with the California Department of Fair Employment and Housing (Gates, Gary J. and Newton, Frank (2013); Government Accountability Office (2013)). In smaller states, less than 10 complaints are filed each year[16]. It is very unlikely that this small number of complaints, many of which never go to trial, is leading to large changes in behavior for employers of gay or lesbian workers. Instead research has indicated that the knowledge that employment protections exist in a state is enough to cause individuals to change their behavior (Tilcsik (2011)).

Policy differences exist on a large number of questions, some as basic as how to define

[14]Under federal guidelines plaintiffs in gender and race discrimination cases are entitled to compensatory and punitive damages. Age cases are only eligible for liquidated damages. The availability of punitive damages for age and race complaints suggests a willingness to punish discrimination compared to compensating its effects.

[15]MA increased the statute of limitations from 300 days to 12 months. MN increased their damages cap to be in line with the federal Title VII caps

[16]Between 2009 and 2011, 3.7% of all employment discrimination complaints filed with state agencies were from LGBT individuals (Government Accountability Office (2013)). The proportion is as low as 1.8% in Hawaii and as high as 6.3% in New Mexico. It is important to note that this is a count of all complaints that include sexual orientation discrimination. Cases that include sexual orientation discrimination, but where it is not the primary charge are still counted.

sexual orientation. From looking at the existing state laws, the differences appear on three important issues: who is in the protected class under this law, how a complaint is resolved, and what damages and remedies are available for plaintiffs. Within these three large groups there are as many as fourteen provisions and issues over which states differ significantly. Table A8 details the areas where state laws diverge.

Information on the provisions comes from state laws and information compiled by the Williams Institute and the Government Accountability Office (Sears et al. (2009) and Government Accountability Office (2013)). Table A10 details the differences in protected classes across states. The damages available in each state are detailed in depth in Table A12 and Table A11. Table A13 highlights differences in how discrimination complaints are handled by each state's enforcement agency.

The provisions that define the protected class are the firm size minima, perceived sexuality, and gender identity. Firm size minima determine how many employees a firm must have before it must comply with the law. Larger minima mean the laws protect fewer employees. Table A10 details the differences in protected classes across states. The average firm size minimum was 5 employees. Eight states have laws that apply to all employers regardless of size, while 4 states have laws that cover only firms with 15 or more employees.

Successful cases may result in damage awards to plaintiffs. There are three broad categories of damages: equitable relief, compensatory damages, and punitive damages. The damages available in each state are detailed in depth in Table A12 and Table A11. All states call for equitable relief, which consists of remedies such as backed pay or being reinstated. Compensatory damages replace lost earnings and compensate for pain and suffering. Punitive damages are designed to punish egregious violations of the employment nondiscrimination laws and are determined by the seriousness of the violation, not the damage done to the plaintiff. These damages are meant to deter other firms from engaging in similar behavior.

Compensatory damages are the most common form of damages. Table A11 details the compensatory damages available in each state. All but three states have compensatory damages for private sector employees. Many states place restriction upon these damage payouts. Connecticut, Massachusetts, Maine, and Wisconsin allow for compensatory damages, but only if the complaint was filed in court. Damages in these states are not available to complaints that are settled during the administrative process[17]. In terms of caps on damages, 9 states cap compensatory damages and 10 do not. Of the 9 states that do, California, Minnesota, and Washington feature caps that are lower than federal minimum (Sears et al. (2009)).

Punitive damages are less common than compensatory damages. Details on punitive damages can been seen in Table A12. Thirteen states allow for punitive damages and 8 states do not. Of the states with punitive damages available, only 5 allow them in administrative cases. The remaining states require civil suits to be filed in order to receive them. Most states cap the damages available through punitive damages. Many caps are

[17]Some states require any complaint to proceed through an administrative hearing before moving to civil court.

much lower than federal caps in Title VII cases(Sears et al. (2009)).

There are variations in the way states handle these cases. Laws differ in the statute of limitations, how the agency in charge of such cases enforces their decisions, the process required for a suit, and how much of one's attorneys fees can be recouped. Table A13 shows that statute of limitations in these cases ranges from 120 days to 365 days. The average statute of limitation for complaints is 238 days. The federal statute of limitations is 180 days. Five states don't allow fees to be recouped if the plaintiff wins. In the states that do provide for fees to be recouped, six of them allow for it in only cases heard in court.

The agencies in charge of enforcing employment nondiscrimination acts have a wide range of powers. In 18 states, the agency in charge of overseeing the law may operate on behalf of the agency itself and any employees that seek its help. This includes the power to file complaints or lawsuits and litigate on the behalf of employees and the agency itself. Four states restrict the agency to merely responding to complaints file, not allowing the agency to take any action on its own initiative (Colorado, Iowa, Nevada, and New Hampshire). In Massachusetts, the agency is allowed to intervene on behalf of an employee who requests it, but it may not file suits or complaints on its own (Sears et al. (2009)). In all but 6 states, employees seeking remedies or damages must first file an administrative complaint before proceeding to civil court to file a suit. This process is different from other types of discrimination because the Equal Employment Opportunity Commission provides a common procedure for all federal complaints.

All of the state laws have the same goal and go about it in very similar manners. Yet, there are also very stark differences between states. What that means for gay or lesbian employees is that the protections for LGBT workers can vary. These legal differences are important because they determine the expected cost of discriminating for employers. Damage provisions and attorneys' fees provisions will increase the expected cost by increasing the cost of being sued. Provisions that lower the cost of filing a complaint increase the probability that an employer is sued. Laws with wider protected classes will increase the number of employees able to file a suit. This may cause stronger laws to provoke a stronger response from employers because the expected cost of discriminating is higher.

In order to gauge the strength of these laws, we can look at it two different ways. The first way was to take each a law as a whole and label them strong or weak based on the combination of provisions. Delaware, D.C., Hawaii, New Jersey, Oregon, and Rhode Island have strong laws. They are characterized by low employer size minima, long statute of limitations, a full set of damages with no caps, and agencies with the ability to enforce decisions. The laws that are noticeably weaker than other laws are those in Colorado, Nevada, and Washington. They contain higher employer size minima, fewer people covered, shorter statutes of limitation, limited damages, and lower damage caps.

The other approach will focus on specific provisions to explore what effect the differences have. It is not feasible to tease out the effect of every single difference, but we can look at some of the most important provisions. In this paper we will focus on damages, employer size minima, and the statute of limitations. These provisions are areas where the differences between states are large. The effects of these provisions are most likely to enter prominently

into the decision making process of employers and employees.

An example of a weak damage provision is Nevada. It has no compensatory damages and no punitive damages. In contrast, New Jersey has strong damage provisions. It has both punitive and compensatory damages, no caps on the amounts, and allows for compensatory damages to be obtained in administrative complaints. In the following analysis, we focus on the effect of a state having only equitable relief, a state having equitable relief and compensatory damages, and a state all forms of damages.

Colorado has weak process provisions. It has a statute of limitation of 180 days, individuals are required to exhaust administrative complaints before proceeding to civil suits, and attorneys' fees cannot be recouped if the suit is won. The agency in charge of complaints is limited in what actions it may take. Minnesota has strong process provisions. The statute of limitations is 180 days. Individuals may proceed directly to civil suits without administrative hearings. Attorneys' fees are recoupable for both civil suits and administrative hearings. The agency in Minnesota has the ability to operate on behalf of itself and any employee who seeks its help. We will focus on the statute of limitations since it will effect the decision of employees to sue.

With regards to coverage provisions, Washington has weak provisions. Perceived sexual orientation is not covered, neither is gender identity. Domestic workers and workers employed by family members may not sue. Firms employing 8 or more individuals are subject to the law. Oregon has strong protection provisions. Perceived sexual orientation is covered, along with gender identity. All firms in Oregon are required to comply with the law.

Table A8: Differences in State-Level Employment Nondiscrimination Acts

Coverage
Firm Size Minimum
Perceived Sexuality
Damages
Compensatory Damages
Punitive Damages
Damage Caps
Damages through Administrative Filings
Process
Exhaust Administrative Process
Statute of Limitations
Recoup Attorney's Fees in Civil Suit
Agency Enforces Rulings

Notes: Information on state laws comes from Sears et al. (2009), the Government Accountability Office (Government Accountability Office (2013)), and state laws.

Table A9: Employment Non-Discrimination Acts

State	Effective Date	Law	Enforcement Agency	Strength
California	1992	CAL. GOV. CODE 12940	California Department of Fair Employment and Housing	Average
Colorado	2007	COLO. REV. STAT. 24-34-401(7.5)	Colorado Civil Rights Commission	Weak
Connecticut	1991	CONN. GEN. STAT. 46a-81a	Connecticut Commission on Human Rights and Opportunities	Weak
Delaware	2009	DEL. CODE ANN. tit. 19 710	Delaware Department of Labor	Strong
District of Columbia	1997	D.C. Code 1-2512	Washington, D.C. Commission on human Rights	Strong
Hawaii	1991	HAW. REV. STAT. 378-1	Hawaii Civil Rights Commission	Strong
Illinois	2006	775 ILCS 5/1-102(O-1)	Illinois Department of Human Rights	Average
Iowa	2007	IOWA CODE 216.2(14)	Iowa Civil Rights Commission	Strong
Maine	2005	ME. REV. STAT. ANN. tit. 5 4553(9-C)	Maine Human Rights Commission	Average
Maryland	2001	MD Code, State Government, 20-606	Maryland Human Rights Commission	Average
Massachusetts	1989	MASS. GEN. LAWS ch. 151B, 3(6)	Massachusetts Commission Against Discrimination	Average
Minnesota	1993	MINN. STAT. 363A.03 subd. 44	Minnesota Department of Human Rights	Average
Nevada	1999	NEV. REV. STAT. 613.310(6)	Nevada Equal Rights Commission	Weak
New Hampshire	1998	N.H. REV. STAT. Ann. 354-A:2(XIV-c)	The New Hampshire State Commission on Human Rights	Average
New Jersey	1992	N.J. STAT. 10:5-5(hh)	The New Jersey Division of Civil Rights	Strong
New Mexico	2003	N.M. STAT. 28-1-2(P)	The New Mexico Human Rights Division	Average
New York	2003	N.Y. EXEC. LAW 292(27)	New York Division of Human Rights	Average
Oregon	2008	OR. REV. STAT. 174.100(6)	Oregon Bureau of Labor and Industry	Strong
Rhode Island	1995	R.I. GEN. LAWS 28-5-6(7)	The Rhode Island Commission for Human Rights	Strong
Vermont	1991	1 VT. STAT. ANN. 143	The Vermont Human Rights Commission	Average
Washington	2006	WASH. REV. CODE 49.60.040(15)	The Washington State Human Rights Commission	Weak
Wisconsin	1982	WIS. STAT. 111.32(13m)	The Wisconsin Department of Workforce Development	Average

Table A10: Coverage Provisions

State	Effective Date	Domestic Workers	Family Members	Perceived Orientation	Employer Size	Complaint Period
California	2001	No	No	Yes	5	300
California	1992	No	No	No	5	300
Colorado	2007	No	No	Yes	1	180
Connecticut	1991	No	Yes	Yes	3	180
Delaware	2009	Yes	Yes	No	4	120
District of Columbia	1977	No	No	Yes	1	365
Hawaii	1991	Yes	Yes	Yes	1	180
Illinois	2006	No	Yes	Yes	15	180
Iowa	2007	No	Yes	Yes	4	180
Maine	2005	Yes	No	Yes	1	180
Maryland	2001	Yes	Yes	Yes	15	180
Massachusetts	2002	No	No	Yes	6	300
Massachusetts	1989	No	No	Yes	6	180
Minnesota	1993	No	No	Yes	1	180
Nevada	1999	Yes	Yes	Yes	15	180
New Hampshire	1998	No	No	Yes	6	180
New Jersey	1992	No	Yes	Yes	1	180
New Mexico	2003	Yes	Yes	Yes	15	300
New York	2003	No	No	Yes	4	365
Oregon	2008	No	No	Yes	1	365
Rhode Island	1995	No	No	Yes	4	365
Vermont	1991	Yes	Yes	No	1	365
Washington	2006	No	No	No	8	180
Wisconsin	1982	Yes	No	Yes	1	300

Table A11: Compensatory Damages

State	Effective Date	Back Pay	Compensatory Damages	Administrative Compensatory Damages	Damages Cap	Cap Amount
California	1992	Yes	Yes	No	Yes	150,000
Colorado	2007	Yes	No	No		
Connecticut	2002	Yes	Yes	No	No	
Connecticut	1991	Yes	No	No		
Delaware	2009	Yes	Yes	Yes	Yes	Graduated Scale
District of Columbia	1977	Yes	Yes	Yes	No	
Hawaii	1991	Yes	Yes	Yes	No	
Illinois	2006	Yes	Yes	Yes	No	
Iowa	2007	Yes	Yes	Yes	No	
Maine	2005	Yes	Yes	No	Yes	Graduated Scale
Maryland	2001	Yes	Yes	Yes	Yes	Graduated Scale
Massachusetts	1989	Yes	Yes	No	Yes	3x the damage
Minnesota	2006	Yes	Yes	Yes	Yes	3x the damage
Nevada	1999	Yes	No	No		
New Hampshire	1998	Yes	Yes	Yes	No	
New Jersey	1992	Yes	Yes	Yes	No	
New Mexico	2003	Yes	Yes	Yes	No	
New York	2003	Yes	Yes	Yes	No	
Oregon	2008	Yes	Yes	Yes	No	
Rhode Island	1995	Yes	Yes	Yes	No	
Vermont	1991	Yes	No	No		
Washington	2006	Yes	Yes	Yes	Yes	20,000
Wisconsin	1982	Yes	Yes	No	Yes	Graduated Scale

Table A12: Punitive Damages

State	Effective Date	Punitive Damages	Administrative Punitive Damages	Damages Cap	Cap Amount
California	1992	No	No		
Colorado	2007	No	No		
Connecticut	2002	Yes	No	No	
Connecticut	1991	No	No		
Delaware	2009	Yes	Yes	Yes	Graduated Scale
District of Columbia	1977	Yes	Yes	Yes	Graduated Scale
Hawaii	1991	Yes	Yes	No	
Illinois	2006	No	No		
Iowa	2007	No	No	No	
Maine	2005	Yes	Yes	Yes	Graduated Scale
Maryland	2001	Yes	No	Yes	Graduated Scale
Massachusetts	1989	Yes	No	Yes	3x the damage
Minnesota	2006	Yes	Yes	Yes	25,000
Minnesota	1993	Yes	Yes	Yes	8500
Nevada	1999	No	No		
New Hampshire	1998	Yes	Yes	Yes	Graduated Scale
New Jersey	1992	Yes	No	No	
New Mexico	2003	No	No		
New York	2003	No	No		
Oregon	2008	Yes	No	No	
Rhode Island	1995	Yes	No	No	
Vermont	1991	Yes	No	No	
Washington	2006	No	No		
Wisconsin	1982	Yes	No	Yes	Graduated Scale

Table A13: Process Provisions

State	Year	Requirement to Exhaust Administrative Remedies Before Civil Suit	Recoup Attorney's Fees	Recoup Attorney's Fees for Administrative Processes	Enforcement Agency Can Act On Its Own
California	1992	Yes	Yes	No	Yes
Colorado	2007	Yes	No	No	No
Connecticut	1991	Yes	Yes	No	Yes
Delaware	2009	Yes	Yes	Yes	Yes
District of Columbia	1977	Yes	Yes	Yes	Yes
Hawaii	1991	Yes	Yes	Yes	Yes
Illinois	2006	Yes	Yes	Yes	Yes
Iowa	2007	Yes	Yes	Yes	No
Maine	2005	Yes	Yes	Yes	Yes
Maryland	2001	Yes	Yes	Yes	Yes
Massachusetts	1989	Yes	Yes	Yes	No
Minnesota	1993	No	Yes	Yes	Yes
Nevada	1999	Yes	No	No	No
New Hampshire	1998	Yes	No	No	No
New Jersey	1992	No	Yes	Yes	Yes
New Mexico	2003	Yes	Yes	Yes	Yes
New York	2003	No	No	No	Yes
Oregon	2008	No	Yes	No	Yes
Rhode Island	1995	Yes	Yes	Yes	Yes
Vermont	1991	No	Yes	No	Yes
Washington	2006	No	Yes	No	Yes
Wisconsin	1982	Yes	Yes	No	Yes

A2 Miscoding of Same-Sex Couples

The Census Bureau has instituted a series of changes over the years to address the issue of miscoded same-sex couples. In 1990, any same-sex couple reporting to be married had the gender of the spouse recoded to the opposite gender. This resulted in all same-sex couples reported to be married to be recoded as opposite-sex couples. The only same-sex cohabitating couples in the 1990 Census are same-sex unmarried partners. In 2000, this process was changed. Instead of recoding the gender of the individual labeled as the spouse, the Census recoded the relationship from married to unmarried partners. The result of this change was that if a man was filling out the American Community Survey for his household and accidently indicated that his wife was a male then they would be counted as a same-sex unmarried partners.

The change in 2000 led to a large number of miscoded same-sex couples. The miscoding was concentrated heavily in the sample of same-sex couples that claim to be spouses. O'Connell and Golding (2006) found that there was only 4% miscoding from unmarried partner heterosexuals. They estimated this using the first names of respondents. An individual was counted as miscoded if their name is almost exclusively given to members of the opposite gender. The cutoff used was 990 times out of 1000 in the 2000 U.S. Census in their state of residence. Lower cutoffs could be used and would lead to higher rates of miscoding. O'Connell and Golding (2006) found that the miscoding was as much as 40% of the same-sex couples with reallocated marital status. In the 2010 Census, DeMaio, Bates and O'Connell (2013) found that the amount of potential miscoding was less. Using the cutoff of names from their earlier work, they found that at most 28% of the sample of same-sex couples was miscoded heterosexuals.

The evidence of miscosing is that it is is most common on paper forms (DeMaio et al. (2013); O'Connell and Golding (2006), Black, Makar, Sanders and Taylor (2007), and Gates and Steinberger (2009))[18]. Estimates range widely for the same-sex partnerships miscoding and there is much disagreement as to the exact amount. To estimate the miscoding error, researchers use a DiNardo, Fortin, and Lemeiux semi-parametric reweighting procedure. Black et al. (2007) suggest that almost 40% of same-sex couples in the 2000 Census were miscoded, while Gates and Steinberger (2009) find the rate of potential miscoding in the 2005-2007 ACS is closer to 20%.

[18]Gender miscoding of spouses cannot occur on in-person or phone interviews since the interviewer is prompted to have the respondent confirm the gender of their spouse.