Earnings differences between homosexuals and heterosexuals and the effects of anti-discriminatory laws: equal but still unmarried

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Abstract

Anti-discrimination laws on the basis of sexual orientation have been adopted by many states to counteract perceived discrimination in the labor market. We find that relative to married heterosexual men, homosexual men earn less and anti-discriminatory laws, over time, partially lessen this gap. This gap is statistically non-existent relative to unmarried heterosexual men. Homosexual women, on the other hand, experience higher earnings than their heterosexual female counterparts, and the law shrinks this gap over time. Our results suggest that although the earnings differential may be due to the marriage premium, anti-discriminatory laws do help reduce labor market differences between homosexuals and heterosexuals. We conjecture that allowing homosexuals to marry could reduce the earnings inequality without creating potentially significant labor market distortions.

JEL Classification: J12, J16, J70, J78
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1 Introduction

Gay and lesbian rights advocates have pursued passage of legislation that explicitly prohibits workplace discrimination based on sexual orientation since the 1970s (Rimmerman et al., eds, 2000). They do so on the grounds that this group is the target of discriminatory practices with respect to wages and employment outcomes. Indeed, several studies have found evidence that on average, homosexual men do earn lower wages than their heterosexual counterparts, presumably in part due to labor discrimination. While this result has not been found with respect to homosexual women, popular support for laws protecting homosexuals remains.[1] These anti-discriminatory laws have the potential to affect the estimated 4 million gays and lesbians currently residing in the United States (Gates, 2011); consequently, the effect of these laws is of notable importance.

Currently the efforts of advocates have resulted in significant successes at the state and local level, but a federal anti-discriminatory law remains elusive.[2] Twenty-one states and the District of Columbia prohibit labor market discrimination against gays and lesbians by privately owned companies (HRC, 2009). Additionally, many localities, cities, and in some cases counties have independently passed their own version of the law.[3] Similar to racial, gender and disability anti-discrimination provisions, these laws attempt to establish equal access and opportunity to employment to those with different sexual orientations. Research on the effect of these laws has not provided conclusive results regarding the impact it has on the group in question (Acemoglu and Angrist, 2001; Collins, 2003; Beegle and Stock, 2003).

[1] See Klawitter (2011) for a comprehensive overview.

[2] There is currently a proposed bill, "The Employment Non Discrimination Act", being considered in the U.S. Congress which would legally prohibit discrimination on the basis of sexual orientation at the federal level.

[3] Data gathered from Klawitter (2011) estimate that in 2010 approximately 195 localities protect gays and lesbians from labor market discrimination at private and state agencies, while 137 do so for government entities only.

In this paper we consider the issue of labor market discrimination against gays and lesbians and examine the effect of state-wide anti-discriminatory laws. Recent studies using Census data compare the wages of gays and lesbians to their married heterosexual counterparts and the difference in wages is interpreted as evidence of discrimination.[4] However, previous findings by Allegretto and Arthur (2001) revealed that the presence of a wage gap between homosexual and heterosexual men is partly explained by a marriage premium. Our up-to-date data confirm that the earnings of homosexual men are not statistically different from those of cohabitating unmarried heterosexual men, but are different from the earnings of heterosexual married men.

The presence of a strong marriage premium highlights the potentially significant social and economics distortionary effects of the Defense of Marriage Act, suggesting reexamination and reevaluation of the current approach to deal with discrimination based on sexual orientation.[5] To date, most U.S. states trying to correct for this type of discrimination have opted to implement anti-discrimination laws covering a wide range of subjects, including housing, accommodation, and wages. We analyze the effect of anti-discriminatory laws on relative earnings using a difference-in-difference-in-difference approach (DDD). This approach allows us to capture the relative differences in earnings in states that have passed a law compared to states that have not. This method has well regarded advantages when considering the analysis of the effect of a law on particular groups. Most importantly, it allows us to control for the established differences in earnings differentials of homosexuals and heterosexuals that exist between states over time, independent of any law that was passed. If, for example, a state that had a relatively high wage gap adopted the law and the earnings differential between homosexuals and heterosexuals decreased, but not to the level of states that did not

[4]Our data also comes from the Decennial Census. The Census allows individuals to classify their relationship to head of household as an unmarried partner. Using this information, sexual orientation is inferred from the gender identification of both the head of household and the unmarried partner. In doing so, our data on gays and lesbians are limited to same-sex male and same-sex female couples. However, Black et al. (2007a) find that these couples are representative of the general gay and lesbian population.

[5]The Defense of Marriage Act (DOMA) states that the federal government only recognizes marriages that occur between one man and one woman.

pass the law, a simple cross-sectional analysis would suggest that the law did not have any impact. A DDD approach is preferred because it controls for state specific time trends that are independent of the passage of an anti-discriminatory law.

To our knowledge, this is the first study to employ a DDD approach to analyze the effect of anti-discriminatory laws on wages of homosexuals and heterosexuals.[6] In addition to the methodological approach, our analysis differs from previous research with respect to the classification of the data. Previous research links individuals' earnings to their place of residence while we link wages to place of work. Even though most people work in the same state where they reside, many live in either MSAs that cross state borders, or in border counties. This can result in a person residing in a state with no law, but working in a state with one, affecting the estimated law impacts. While the share of individuals working in a state different from the one where they reside is small, and may not yield significant differences, we believe that this adjustment is still important in terms of precision. Also, since we follow Klawitter (2011) in allowing for the influence of laws passed at the local city level, these adjustments seem pertinent given the degree of inter-city commuting in the United States.

The results of our analysis suggest that when we control for labor market trends in each state, anti-discrimination laws do have an effect on wages as time since passage of the law increases. Yet, we do not find evidence of any contemporaneous statistical difference between the relative earnings of homosexuals that work in a state with a law and those that work in a state without one. We also find little evidence of an impact of the law on employment of those in same-sex relationships, both at the intensive and extensive margins. Furthermore, the effect of the law through time appears to also affect heterosexual men and

[6]Beegle and Stock (2003) used this approach to analyze the effect of disability laws. The DDD methodology is possible given that the Census now allows for the identification of households with same-sex couples for a number of years. Our estimates rely on IPUMS micro data from the 1990 and 2000 U.S. Censuses and the 2009 American Community Survey (ACS). For computational ease, and in order to match the density of the ACS sample (1 percent), the Census data extracts are re-weighted samples from the 5 percent sample of the United States population. This allows us to exploit the timing of the state and local laws passed over a period of almost two decades.

women, potentially and unintendedly helping to close the overall gap created by the marriage premium.

In section 2 we provide a general background and summarize the previous literature. In section 2 we also discuss the theory and previous empirical findings. Section 3 describes the data and the empirical method. In section 4 we present the results, and in section 5 we conclude.

2 Background

2.1 On Labor Market Discrimination

Extensive research exists on the differences in employment outcomes between homosexuals and heterosexuals. Although the research has found wage differentials between these groups, it has not been able to provide conclusive evidence for or against the existence of discrimination. In general, studies using various types of survey data have found that homosexual men tend to earn lower wages than their heterosexual counterparts (Badgett, 1995; Klawitter and Flatt, 1998; Clain and Leppel, 2001; Berg and Lien, 2002; Black et al., 2003; Blanford, 2003; Plugg and Berkhout, 2004; Frank, 2006; Carpenter, 2007; Elmslie and Tebaldi, 2007; Ahmed and Hammarstedt, 2010). In contrast to the findings for men, similar studies done for women only have found that homosexual females typically earn the same or more than their heterosexual counterparts (Klawitter and Flatt, 1998; Clain and Leppel, 2001; Berg and Lien, 2002; Black et al., 2003; Blanford, 2003; Plugg and Berkhout, 2004; Arabsheibani et al., 2005; Elmslie and Tebaldi, 2007; Ahmed and Hammarstedt, 2010), although Badgett (1995); Carpenter (2004) did find that lesbians earn less than heterosexual women.[7]

[7]Black et al. (2007b) show descriptive statistics using Census data for 2000 that supports the idea that gay men earn less than their heterosexual counterparts (married or unmarried). Similarly, they show that lesbian women earn more than their heterosexual counterparts (married or unmarried). Their analysis, however, pools married and unmarried heterosexuals into one group, while we separately compare each group to gays and lesbians. Allegretto and Arthur (2001) use data from the 1990 Census only, and find that unmarried heterosexual men still enjoy slightly higher earnings than men in same-sex relationships.

Perceived discrimination in the labor market is also well documented in the literature. Most recently, Tilcsik (2011) experiments using fictitious resumes that highlight previous participation in a gay campus organization in the U.S., and finds that openly gay men receive less calls from potential employers. Herek (2009) uses survey responses from a nationally representative sample of gays and lesbians to determine if they are the victims of various types of discrimination. He finds that one in ten surveyed have experienced discrimination in the housing or labor market. Previously Badgett et al. (2007) provided a comprehensive overview of many of the surveys that have been conducted of homosexuals regarding discrimination. In their analysis they find that in studies from the mid-1980's to mid-1990's, 16% to 68% of gay, lesbian, and bisexual respondents reported experiencing employment discrimination at some point in their lives, while in the 15 studies conducted since the mid-1990's 15% to 43% of respondents reported experiencing discrimination in the workplace.

Despite evidence of perceived discrimination from survey data analysis, the documented earnings differentials between gays or lesbians and their heterosexual counterparts cannot be taken as conclusive proof of actual discrimination. Becker (1971) developed a household specialization model which predicts that in heterosexual relationships females invest in obtaining fewer labor market skills as they expect to be coupled with a high earning male in the future, while men tend to invest in more labor market skills because of their belief in being coupled with a low earning female in the future.[8]

Applying this model to same-sex relationships would predict that gays invest in obtaining less labor market skills relative to a heterosexual male (since they plan on being partnered with a male in the future), while lesbians invest in obtaining more labor market skills than heterosexual females (since they plan on being partnered with a female in the future).[9] To the extent that these decisions are unobservable, they may be driving the observed

[8]In the presence of potential discrimination, Becker (1971) predicts that market forces will eliminate wage differentials due to discriminatory practices. His model predicts that discriminatory firms will be driven out of the industry since non-discriminatory ones will benefit in terms of increased profits. However, Rosén (2003) argues that this prediction does not hold under markets with friction and wage bargaining.

[9]See also Becker (1991) for more on household specialization.

earnings differences often found in the literature. Therefore, lower earnings for homosexual males cannot be considered definitive evidence of discrimination against gays, while the higher earnings enjoyed by homosexual females cannot be considered definitive evidence that lesbians are not discriminated against.

Black et al. (2007b) considers this hypothesis by examining the choice of college major of heterosexuals and homosexuals. They combine Census data for individuals in same-sex relationships with data from the National Survey of College Graduates and statistically show that homosexual men are more likely to graduate with a "typically female major" relative to heterosexual men. The opposite is true for homosexual women. Under the assumption that "typically male" majors yield higher returns, that suggests that wage differentials can be partly explained by education choice, rather than actual discrimination. [10]

In an attempt to overcome these difficulties, some researchers have employed labor market experiments to test for the presence of discrimination. Weichselbaumer (2003) conducted a national Swedish experiment in the hiring process, randomly assigning sexual orientation to different job applicants. She finds that gays experience discrimination in male-dominated industries and lesbians experience discrimination in female-dominated industries. Drydakis (2011) follows a similar procedure for a United States based experiment for gays, signaling sexual orientation through participation in a gay organization appearing on the resume. He also finds evidence for discrimination against gay men. However, these results concern openly gay men who may be different in unobserved ways from the general gay population. Both experiments test for discrimination in the hiring process rather than wages or earnings. [11] These results have, in part, motivated the adoption of anti-discriminatory laws and it is useful to analyze whether these laws did have the outcome intended.

[10] See Brown and Corcoran (1997) for detailed descriptions of "typical female/male majors."

[11] Note that in this analysis, like in some others looking at the differences in earnings, we employ Census data rather than survey data to allow for a more nuanced approach.

2.2 On the History of Anti-Discriminatory Laws

Although establishing the existence of discrimination against gays and lesbians is complicated, the perception of discrimination has motivated law makers to adopt anti-discriminatory legislation on the basis of sexual orientation. There is not currently a federal anti-discriminatory law but a few states have had such legislation in place for decades. In 1975, Pennsylvania adopted a law that prohibited discrimination on the basis of sexual orientation at all state agencies. Wisconsin was the first state to implement a law that covered the private sector in 1982 (although the District of Columbia adopted a law that covered employment in the private sector in 1973). By 1990, the first year in our sample, only Wisconsin and Massachusetts (1989) had adopted a law that applied to all private companies.

Table 1 summarizes the development of the state laws adopted to date. We are particularly interested in laws that cover employment in the private sector. As table 1 shows, there are some states in which discrimination is explicitly prohibited in state agencies, but not in private companies. In other states, neither state nor private employers are required by the law to provide equal opportunities to those with different sexual preferences. Additionally, state provisions differ from each other in regard to the inclusion of gender identity. For example, the Maryland Annexed Code art. 49B §5 of 2001 does prohibit discrimination on the basis of sexual orientation but does not explicitly address the issue of gender identity (e.g. transgender).

[INSERT TABLE 1 HERE]

These state laws have largely been designed after other stipulations that prohibit employment discrimination based on race, gender, and to some extent disability (Klawitter, 2011). In California, for instance, the term 'sexual orientation' was added to a broader legislation that includes race, religious creed, color, national origin, ancestry, physical and mental disability, medical condition, marital status, sex, and age. It explicitly prohibits discrimination in hiring, firing, compensation package, and employment conditions. It prohibits discrimination against the participation in labor organizations such as unions or other

training programs, and also forbids sexual harassment. Many of these laws are not limited to employment and also include housing and other public accommodations. Interestingly, the equal employment opportunity self-identification form does not include a category for sexual orientation. This implies that the identification of homosexuals in the workplace depends on their degree of openness, either by word or behavior. In many cases, independently of whether there exists a law or not, individuals may be reluctant to reveal their sexual orientation for personal reasons unrelated to the workplace. This in turn may have consequences for the analysis of such laws that are discussed below.

2.3 On the Theoretical Effects of Anti-Discriminatory Laws

Logic dictates that anti-discrimination laws should help dissipate the differences in employment outcomes due to discriminatory behavior. For the case of race and sex, this has been partly true. The laws have reduced earning gaps, but not eliminated them.[12] However, theory suggests that the effects may not be straightforward. Similar to Beegle and Stock (2003), we consider the theoretical model laid out by Acemoglu and Angrist (2001) and apply it to gays and lesbians in particular. Due to the similarities, and for brevity, we summarize the predictions without presenting the mathematical derivations of the model. In general, the model predicts that employment outcomes for gays and lesbians are affected by the passage of anti-discrimination laws through two main channels.

The first channel of effect occurs because the passage of the law allows individuals to sue the employer over the belief that failure to hire (or fire for those already employed) was due to their sexual orientation. This increases the firms' costs by adding the risk of potentially going to court over such a lawsuit. The higher the potential cost to the firm, the higher probability of a member of that group being hired (or not fired). However, the cost of not hiring and/or firing homosexuals may not be the same, yielding ambiguous predictions for the demand of homosexual workers. Indeed, the costs in either case depend on the firm's perceived

[12]See Klawitter (2011) for a discussion of studies.

probabilities of potential lawsuits over not hiring or firing a homosexual. Conditional on the probability of firms being sued on discrimination grounds, they will weigh the relative probabilities of a lawsuit. If the expected probability of a lawsuit for firing homosexuals is greater than the expected probability of a lawsuit from not hiring homosexuals, firms will choose to decrease the demand for homosexuals. On the other hand, if the fear (or risk) from lawsuits stemming from not hiring homosexuals is bigger than that for firing them once they are employees, the overall demand for this group will increase. Rubenstein (2001) finds that population adjusted complaint rates based on sexual orientation discrimination are similar to the population adjusted complaint rates based on race and gender differences. Rubenstein (2001) provides evidence that firms have a reason to fear a costly lawsuit from gays and lesbians and that law has an impact on firms hiring decisions. Yet, there exists no conclusive evidence on which probability of lawsuit is higher.

The second channel of effect acts through an equal pay provision that is typically included in anti-discriminatory laws. The equal pay provision aims at reducing wage inequalities between homosexual workers and heterosexual workers. Homosexuals might receive lower wages for various reasons. Some employers might have a "taste for discriminating" against minority groups, implying that firms with such taste will hire a homosexual only at a lower wage than a heterosexual worker with similar characteristics (Black, 1995). It also might be the case that the taste for discrimination simply reduces the demand for homosexuals, relative to heterosexuals, driving down wages. Another possibility is that there exist systematic unobserved productivity differences between homosexuals and heterosexuals which explain the earnings gap. However, it is likely that other than sexual orientation, homosexuals are not systematically different from heterosexuals on average. Thus, in the presence of an employer's distaste for homosexuals, we expect to observe a lower wage for homosexual workers relative to the equilibrium wage for heterosexual workers.[13] This implies that at

[13]Note that distaste for homosexuality could also come from other employees. If other workers feel uncomfortable working around homosexuals, work morale and productivity may be affected. As a result, employers may choose not to hire homosexuals, or simply hire them at reduced wages to offset the loss of other workers' productivity.

the equilibrium point, an equal pay provision will act as a price floor in the market for homosexual workers, raising the wage but reducing net employment.

Given these two effects, the passage of the anti-discrimination law may result in several different employment outcomes for gays and lesbians. Keeping supply constant, we would expect wages to increase through an increase in demand if the probability of law suits from failure to hire is greater than the probability of law suits for firing a homosexual. This is commonly interpreted as a "hiring subsidy". The opposite occurs if the probability of a lawsuit from firing a homosexual is relatively higher (a "hiring cost"). In the presence of an equal pay provision, if the hiring subsidy dominates but wages are still less than those for the heterosexual counterparts, then wages increase further. The overall effect of the law in this case is an increase in wages. The effect on employment is ambiguous given that the increase that arises from increased demand is offset by decreases in quantity demanded from the equal pay provision. On the other hand, if wages for homosexuals are indeed lower than those for heterosexual and 'hiring costs' do indeed decrease demand, then net employment decreases further (both through in equilibrium and due to the price floor). Yet, the overall wages of homosexuals increase from the equal pay provision. Even if there is no evidence of wage disparities, often interpreted as discrimination, we should expect to observe changes in wages, or employment, or both, due to the possibility of lawsuits resulting from the implementation of the law. For instance, if the fear/risk of lawsuits from not hiring homosexuals is smaller than that of firing them, one should expect a downward shift in the demand for homosexuals, lowering equilibrium wages and employment. However, the equal pay provision would keep wages at the initial level, but decrease employment even further.

2.4 On the Empirical Findings of Anti-Discriminatory Legislation Based on Sexual Orientation

To our knowledge there have been three previous papers using data from the 1990 and 2000 U.S. Census to analyze the effect of state anti-discrimination laws on gay and lesbian labor

market outcomes. Klawitter and Flatt (1998) collect data on both state and local public and and private employment laws and find no evidence that the existence of these laws has an effect on the wages of gays or lesbians. However, this study was performed using 1990 data and most of the anti-discrimination laws that were in place at that time had only been around for a short time period. Therefore, the insignificant effects of the laws may have been due to the laws not having a long enough time to be fully implemented and their effects to be observable. Gates (2009) performs a similar analysis using data from the 2000 U.S. Census, but only uses state laws because they allow for an exact geographic match of same-sex couples and state-level policies provide a more consistent standard of application and enforcement than local laws.

Gates (2009) finds that the presence of an anti-discrimination law increases the relative earnings of gays by 3.0% and by 0.3% for each year the law is in effect. For lesbians, he finds an increase in relative earnings of lesbians of 2.0% in states with an anti-discrimination law and an increase of 0.3% for each year the law is an effect. More recently, Klawitter (2011) also uses 2000 U.S. Census data but includes local as well as state laws. She employs a multi-level cross-sectional approach and finds evidence that anti-discrimination laws decrease the earnings penalty of gay men, primarily by increasing hours worked per week, but finds no evidence that anti-discrimination laws are associated with differences in the earnings of lesbians.

Our paper differs from these previous studies of sexual orientation anti-discrimination laws in three ways. First, we identify the potential impact of a law based on the place of work rather than the place of residence. Second we use a difference-in-difference-in-difference approach to account for trends in local labor markets. We extend the work of Gates (2009) by also including laws adopted at the local level. Unlike Klawitter (2011), however, in our estimation we use local laws indicators only when state laws have not been adopted. As in Gates (2009), we assume that once a state-wide law is passed, individuals will be more likely to use state courts, rather than the local government, in case of a legal dispute. This

is partly reaffirmed by Klawitter (2011), as she finds no strong evidence of labor market effects in places with both laws. Finally, our analysis follows **?** by using two separate estimations. One measuring the differences between individuals in same-sex couples with respect to married heterosexuals, and another with respect to unmarried heterosexual.

All three of these studies use a pooled sample composed of married heterosexuals, unmarried cohabitating heterosexuals, and same-sex cohabitating couples. Since married and unmarried heterosexuals may differ from individuals in same-sex couples in different ways, we prefer estimating them separately, allowing us to take full advantage of the difference-in-difference-in-difference approach. Theoretically, the treatment and the control or reference group should only differ in that one is subject to the law, while the other is not. Due to some of the social constraints encountered by homosexual couples in the United States, it is not obvious who should be chosen as the correct control group: married or unmarried heterosexuals. Hence, we find it necessary to estimate the effect of the law with respect to both groups separately.[14]

3 Empirical Approach

3.1 Difference-in-Difference-in-Difference

Recent studies which analyze the effect of race and gender anti-discriminatory laws have estimated regressions with pooled data from different time periods using the difference-in-difference-in-difference (DDD) methodology. Neumark and Stock (2001) employs the DDD methodology to analyze the effect of the passage of sex and race anti-discrimination laws at the state level. They find that race anti-discrimination laws generally increase the relative earnings and employment of blacks and that gender anti-discrimination laws increase relative

[14] Allegretto and Arthur (2001) also adopt this approach in their analysis of earnings differentials, finding that the difference between homosexual and heterosexual men lies between an upper and a lower bound that is determined by the estimates using the two reference groups separately. Their difference is partly explained by the marriage premium.

earnings and decrease relative employment for women. Collins (2003) applies a similar DDD framework to study the effect of race anti-discrimination laws and also finds, at least for the 1940's, that the passage of race anti-discrimination laws improved labor market outcomes for blacks. The DDD methodology is also used by Beegle and Stock (2003) to analyze the impact that the passage of disability anti-discrimination laws at the state level have on wages, employment, and labor market participation of the disabled. Their findings suggest that disability anti-discrimination laws generally lead to lower relative earnings and lower relative labor force participation for the disabled without influencing their relative employment.

The difference-in-difference-in-difference approach allows us to to identify the net effect of the laws on the employment outcomes for individuals in same-sex relationships. We follow Beegle and Stock (2003) in controlling for differences in employment outcomes across states and time. We allow for interactions between state and year, controlling for differences in employment outcomes across states for each specific year. Additional interactions include those between time and state with an indicator for homosexual. This controls for the presence of individual shocks in the labor market for homosexuals in different states, as well as their time trends. However, using these interactions precludes us from estimating the coefficient on the general effect of the law across all observations, given the generation of a common intercept for all individuals in a state and year. The model is characterized as follows

$$\begin{aligned} Y_{ist} =& \alpha + (SL_{st} * SS_{ist})\varphi + SS_{ist}\gamma + (SS_{ist} * State_s)\delta_s + (SS_{ist} * Year_t)\delta_t \\ &+ State_s\eta_s + Year_t\eta_t + (State_s * Year_t)\eta_{st} + X_{ist}\beta + LL_{ist}\rho \\ &+ (LL_{ist} * SS_{ist})\theta + Duration_{st}\varphi + (Duration_{st} * SS_{ist})\varsigma + \varepsilon_{ist} \end{aligned} \tag{1}$$

where Y_{ist} is the employment outcome of interest for an individual i in state s and Census year t. The vector denoted by X includes information about race, gender, age, age squared, a dummy to indicate if the person lives with children, dummies for educational attaintment, disability status, English proficiency, urban or rural residency, non-earned income, and aver-

age work hours per week. SL corresponds to a binary indicator for the existence of the law in a state in a given year, and SS is an indicator defining whether the person is part of a same-sex relationship. Similarly, LL identifies if the individual lives in a city or town where there exists an anti-discrimination law in that particular year, but no law has been passed at the state level. Finally, *Duration* measures the time since the state law has been passed at the time of measurement.

Previous studies have reported results for subsets of the population (Gates, 2009; Klawitter, 2011). Beegle and Stock (2003), for instance, estimate the impact of disability laws for groups characterized by age, race, and sex. The purpose of dividing the sample is to account for the passage of other anti-discrimination laws. Like most studies of homosexuals, we are able to estimate the model for men and women separately but not for race or age. Our limitations come from the fact that we use state instead of regional dummies. The combination of state and year interactions, and the relative low number of observations in same-sex relationships when we cut the sample into smaller subgroups presents problems of perfect predictability and collinearity. As such, the best we can do is to control for these characteristics in the main regression.

The data and specification do allow us to estimate the results using two subgroups. Due to the living arrangements of the individuals selected in our sample, it is possible that even unmarried partners make joint decisions in the labor market. Hence, we follow Klawitter (2011) by using a randomly selected person from each household.[15]

It is recognized in the literature that the majority of laws are the result of predisposed conditions, namely political and economic. This implies that the passage of laws is not completely exogenous and quasi-experimental approaches like the difference-in-difference methodology need to account for this possibility. Besley and Case (2000) propose two meth-

[15]We also test our results using a subgroup of only those that identified themselves as heads of household. Restricting our sample to heads of household only serves as a means of checking for the robustness of the previous results, but must be used carefully as heads of households in the census data is nothing but the person who fills up the questionnaire. As such, it does not necessarily mean that it is the person with higher income, or the person that specializes in labor-market-intensive activities.

ods. One is an instrumental variable approach where the laws can be instrumented with political variables, and second is the use of a third difference adding a valid comparison group (making this a DDD approach). Though not a perfect solution, we opt for using the DDD and control for these preconditions by using state and year dummies. However, by choosing a triple difference approach, we must choose an appropriate comparison group. This choice is complicated by the fact that homosexuals face social constraints not experienced by heterosexuals.

A valid control group must have the same characteristics as the treatment group except for the access to the treatment. The census data allow us to restrict our sample to only those that are in committed relationships, whether by marriage or cohabitation. However, homosexual marriage is not recognized by the federal government so it is not clear whether married or cohabitating unmarried heterosexuals is the appropriate reference group.[16] As pointed out by Allegretto and Arthur (2001), using a subsample where heterosexual individuals in married relationships are the control group, we implicitly assume that all homosexuals would get married if allowed to do so. The opposite assumption is made if unmarried but cohabitating heterosexuals are the control group. In the absence of a broad marriage recognition for gays and lesbians, it is unclear who would choose to be legally married. This presents challenges when studying both the difference in earnings as well as the effect of anti-discriminatory laws.

Since the earnings differentials can be partly due to the marriage premium, and the marriage premium could be partly explained by the signaling hypothesis, it is possible for the law to unintendedly affect the non-treatment groups. Under the assumption that a nontrivial portion of homosexuals may not be completely open about their orientation at the moment of hiring, it is plausible that the employer cannot distinguish between an unmarried heterosexual and a homosexual. In this case, if employers fear the cost of lawsuits from not

[16] Although a few states have ruled in favor of same-sex marriage in the last decade, some of the basic federal benefits granted to married couples are still unavailable to these individuals, regardless of state recognition.

hiring homosexuals, they may err on the side of caution, increasing the demand for both homosexuals and heterosexuals, which can in turn raise wages for both groups. Hence, the law could affect unmarried heterosexuals as well, while not the married ones. Yet, under budget constraints and the fear of lawsuits due to unequal treatment, employers may also reduce the supply of married heterosexuals, with the law negatively affecting that group. We must note, however, that the marriage premium may exist because of specialization (i.e. human capital) or ability bias. Although we cannot resolve the issue of the ability bias, the census data do allow us to compare the occupations of homosexuals and heterosexuals, helping us narrow the similarities between individuals in same-sex couples and those in heterosexual marriages, as well as those in heterosexual cohabitation.

Finally, as noted by Beegle and Stock (2003) and Bertrand et al. (2004), there is also the potential for serial correlation. Our specification controls for the time since the law was adopted. Although this does not eliminate this concern, it does greatly reduce the problem. Additionally, we consider only three well spaced time periods. Following Beegle and Stock (2003), we argue that this further reduces the threat of serial correlation, which would persist if we used annual data from sources such as the American Community Survey (ACS). As a practical matter, the passage of the laws can increase the incentive to self-identify as a homosexual, leading to compositional bias. We are not overly concerned with this problem since the data show that percent changes of self-identified homosexuals is small and very similar in states with and without laws.

3.2 Data

In order to allow for sufficient time for the laws to be fully implemented, and avoid the potential for serial correlation mentioned above, we use Census data in 10-year intervals. The data on gays and lesbians come from the 5% samples of the U.S. Census for 1990 and 2000 and the 1% sample of the 2009 American Community Survey (Ruggles et al., 2009).[17]

[17]This data is publicly available from www.ipums.org. At the point of measurement, data for the Census 2010 was not publicly available, leading us to choose the ACS 2009 as the latest comparable sample. For

The Census Bureau does not ask individuals directly about their sexual orientation, but beginning with the 1990 Census it has been possible to identify same-sex couples living in cohabitation in the same household. This is inferred from the answer to "relationship to household head" question on the Census.[18] A major concern about this way of identifying gay and lesbian couples has been raised by Black et al. (2007a) and Gates and Steinberger (2010). Carpenter (2004) finds that most of these Census couples are indeed gays and lesbians. However, the Census also recoded those that appeared to be same-sex married couples as same-sex unmarried couples. However, Black et al. (2007a) find that many of these same-sex married couples are actually different-sex married couples that had misclassified themselves. We follow Gates and Steinberger (2010) suggestion of excluding same-sex couples for which at least one of the members of the household had their marital status allocated by the Census to correct for this misclassification.

As mentioned above, we separately consider two comparison groups, married heterosexual individuals and unmarried heterosexuals classified as part of a different-sex cohabitation arrangement. We follow the previous literature and divide each analysis into samples of only men and only women. Since we acknowledge that even in unmarried couples (homosexual or not) there might be some selectivity, we repeat the empirical analysis using a sample of randomly selected individuals within the couple.[19]

Census data presents trade-offs that we acknowledge. While it only allows for the identification of gays and lesbians that are in a committed relationship, it is the only national data set that allows for the identification of their location. The samples selected for this analysis allow us to also identify the state and locality where the respondent resides and works. We exploit this by only using individuals in committed relationships (homosexual or heterosex-

computational ease, we extracted a re-weighted sample of 1% density for the years 1990 and 2000.

[18]Gay and lesbian households are identified from the "relationship to household head" heading on the Census. The categories under this heading are spouse, child, inlaw, unmarried partner, and other non-relative.

[19]Even though the number of women in heterosexual couples that identified themselves as heads of households is reduced, a sample using those identified as 'heads of household' is used as a robustness check which yields consistent results.

ual), while being able to determine location. Also, while the majority of respondents work in the same state where they reside, there is a nontrivial number of respondents that work in another state. We use place of work in order to increase precision. The Census also provides information on race, gender, disability status, education, and age, which are used as control variables.

The data on state anti-discrimination laws are drawn from the Human Rights Campaign (HRC, 2009) and information for local laws is obtained from Klawitter (2011). Our indicator for the presence of a local law takes the value of one (1) only if a state law that prohibits discrimination on the basis of sexual orientation in all private companies has not been adopted and implemented. This is done for every one of the three periods in the sample. In other words, workers in Los Angeles, CA are subject to the local law in 1990 but no longer in 2000, since the state-wide law was adopted in 1992. We create this variable to control for the fact that workers in Los Angeles had the opportunity to file complaints with the local authorities in 1990, while others in other California towns could not. Local laws often cover cities, counties, and other smaller towns. Using the Census, we are able to first allocate those laws to workers in each specific city and county. Towns for which we cannot find a match in the Census data, we follow Klawitter (2011) by using PUMAs, for which the law covers at least 40% of the PUMA.[20]

The sample is restricted to include only those of working age (16 to 64 years of age), and choosing subgroups depending upon the outcome of interest.[21] We only analyze the earnings of those that are working "full time" for most of the year (i.e. 30 or more hours per week and 27 or more weeks per year). Like in Beegle and Stock (2003), we do not account for the probability that employed homosexuals are likely to be a selected group of workers, perhaps with better opportunities. These opportunities are not available in our data, and as such,

[20]See Klawitter (2011) for the use of PUMAs for the allocation of local laws.

[21]Most studies restrict the sample to exclude those of ages 16 and 17. Legally, these individuals are allowed to work, and we observe that a few identify themselves as living with a partner of the same sex. For comparability, we also restricted the sample to include only those between the ages of 18 and 64. The results are nearly identical and are available upon request.

our analysis estimates the effects of the law conditioned on being employed. Furthermore, the Census does not report wages. We use annual earnings and control for weeks worked during the previous year, as well as the usual number of hours worked per week. Since weeks and hours worked are reported in ranges we use midpoints. We also exclude observations with missing data and those with real hourly wages equivalent to $1.00 or less.

Table 2 is a general description of the data. The share of individuals identifying themselves as homosexual (i.e. in same-sex partnership) is measured as a percentage of total number of couples, including both married and unmarried heterosexuals. When looking at the number of same-sex couples as a percentage of all unmarried couples in the labor force, we find that this number oscillates between 2.5 (for 1990) and 8 percent (for 2009), independent of whether or not a law exists.[22] Table 2 shows that there are slightly more same-sex couples in states with a law in any given year, but the differences are not significant ant are usually less than 1 percent.

[INSERT TABLE 2 HERE]

Overall we do not observe major abnormalities in labor outcomes. Labor force participation is slightly less for those that are married. It is likely that more individuals in this group choose to opt out of the labor force as part of the household decision. At the same time, the data suggests that married persons are more likely to be employed given their participation in the labor force. Interestingly, the percentage of employed individuals in same-sex relationships is higher than that of unmarried heterosexuals. This is true for all years whether they locate in a state with an anti-discrimination law or not. Additionally, the percentage of same-sex partners employed is higher in states with a law than in states without a law. Although this is also true for all years, the difference is consistently small, especially at a level of employment around 95 percent.

There are not noticeable differences in other employment outcomes such as weeks of

[22]Our sample contains approximately 868,996 individuals in cohabitating couples in the labor force in 1990, and 1,041,551 in 2009.

employment or earnings.[23] Persons in same-sex relationships tend to be more educated and more likely to live in metro areas. Additionally, those in same-sex relationships tend to be younger than married individuals, but older than persons in unmarried different-sex relationships. This is of particular importance for determining the appropriate reference group. Theoretically, even though we control for these characteristics in the estimation, we should should choose a control group that reflects similar ages, levels of education, location, and English proficiency. Yet, the statistics provided in table 2 use the the pooled sample of men and women. We conduct similar analyses by gender, and run statistical tests for the differences in proportions and means. Since the law may influence location as well as other personal decisions, we compare the means using the states that never passed the law. Although not reported, we find statistically different means and proportions for both married and homosexuals, and unmarried and heterosexuals. Yet, we find that for both men and women these differences are smaller between married individuals and homosexuals, providing a slight support for using married individuals as the control group.[24] Further investigation provides support for using married couples as controls, but this support is not particularly strong.

Employment success and earnings are not only determined by education, but also by occupation. The census classifies each individual in our data set into one of 384 occupations.[25] We choose the occupations with highest concentration of men and women in same-sex relationships and compare them to their married and unmarried counterparts. In Figure 1 we display the occupations preferred by male and female homosexuals ranked from left to right in the horizontal axis. The bars show the differences between the proportion of married and unmarried heterosexuals in those same occupations with respect to homosexuals. The figure shows no clear pattern. The difference between the proportion of male homosexuals and

[23]Earnings, hours, and weeks worked are conditional on employment. Earnings are annual real earnings conditioned on "full-time" employment, given that the Census does not report wages.

[24]These results are available from the authors upon request.

[25]Due to the high number of occupations and the widespread distribution of individuals among those, the inclusion of occupation dummies in the empirical analysis becomes problematic due to problems of collinearity and perfect predictability.

married heterosexual men in management is smaller than that between homosexuals and unmarried heterosexuals, but this is not true for sales, managers of food service, or waiters/waitresses. For the case of men, there is a slight support for having smaller differences with married heterosexuals, but the pattern is less clear for case of women. The lack of certainty leads us to present results using both married and unmarried as separate control groups.

[INSERT FIGURE 1 HERE]

4 Results

Table 3 shows the results obtained from the initial difference-in-difference-in-difference estimation. Most of the coefficients have the expected sign and significance. While Black et al. (2007b) suggest that college major may be important in explaining wage differences, calling for the need to control for area of study, we do not have data on major. The Census does identify industry, but there may be many occupations within an industry. Controlling for occupation presents problems of perfect predictability given the wide distribution of homosexuals in all 384 occupations across each state.

We analyze men, women, married, and unmarried separately. Since we do not observe wages, we follow previous studies that utilize Census data and use real earnings per year for those that work full-time for most of the year. Klawitter (2011) found that men that are part of same-sex and different sex unmarried couples earned less than married men. As expected we find that men in same-sex relationships earn less than married men. We confirm prior results finding no statistical difference when we compare men in same-sex couples to coupled-unmarried heterosexual men. Although determining the correct comparison group is difficult, our results confirm that a large part of the difference in annual earnings found in previous studies of homosexual men may be due to the marriage premium.

[INSERT TABLE 3 HERE]

Specific for the purpose of our study, we find that, at a given point in time, men in same-sex partnerships working in states that have passed the anti-discrimination laws are not better off than their counterparts in states without laws. The law, however, does tend to increase the relative earnings of homosexual men over time, as shown by the coefficient of *Time Passed*SS* in Table 3. Table 4 provides a comprehensive summary of the results. To account for the possibility of household specialization, we also estimate the model using a restricted sample of one individual in a same-sex partnership rather than both. The partner chosen is randomly selected to ensure a mixture of individuals designated as head of household and those designated as the partner.

Table 4 also highlights the time and state interactions used in all estimations.[26] On average, an additional year the law is in effect increases the relative wages of men in same-sex relationships by approximately 0.50 to 0.85 percent. While the effect of the law on the target group is consistent regardless of the comparison group, we find that the law also influences the earnings of heterosexuals. Results in the first two columns (using married men as the control group) show that all men in general earn less in states where the law has been in place longer. This is the opposite of what we find when we use unmarried heterosexual men as a comparison group (columns 3 and 4). It may be that the law affects wages not only by increasing the wages of homosexual men, but also by decreasing those of married men. Since the percent of homosexual men is relatively low compared to that of married men, the decrease in overall earnings implies that through time, the law has affected the earnings of married men negatively. On the other hand, we observe that time since enactment increases the overall earnings of all men when unmarried heterosexuals are used as the control group.

[INSERT TABLE 4 HERE]

In theory the law should not affect the control group, much less in the opposite direction. Our finding is subject to three possible interpretations. If married heterosexuals are the

[26]The results on the difference in earnings between heterosexuals and homosexuals are consistent across all samples. Note, however, that the estimate when using those identified as heads of household and unmarried partners as the comparison group in a separate estimation is statistically significant at the 10% level only. These results are available upon request.

appropriate reference group, anti-discriminatory laws have acted to close the earnings gap over time. If unmarried coupled heterosexuals are the appropriate reference group, the law may have acted to unnecessarily increase the wages of homosexual men (unnecessary because all other things equal, there does not seem to be a gap among these groups).

Given that the inability of homosexuals to get married precludes us from determining the correct reference group, we consider a third interpretation. In the absence of earnings differences between homosexual men and unmarried heterosexual men, the law has helped to not only reduce the earnings gap between married heterosexuals and homosexual men through time, but also that between married and unmarried heterosexuals. If sexual orientation is not always disclosed during the hiring process, employers cannot distinguish between unmarried men and individuals in same-sex relationships. Under the fear of lawsuits from potentially not hiring a homosexual, risk averse employers increase demand for both groups. The increase in the relative earnings of homosexual men can be still justified by the fact that a number of homosexuals may purposely disclose their sexual orientation during the process. Due to employer budget constraints, married individuals may be subject to lower demand and lower wages. This combined effect is supported by the fact that the relative earnings of homosexual men increase faster relative to those of married than unmarried men. Note that this is one of many possible explanations and that the true answer requires data which captures the marriage inclinations of men in same-sex relationships. Yet, it provides a step forward toward the understanding the influence of these laws and highlights the need for future research to carefully consider the reference group comparison construction.

For the case of women, our DDD results confirm that women in same-sex relationships do tend earn more than heterosexual women, but the estimates are not statistically significant.[27]

[27]Our robustness check using the group with women that consider themselves heads of household does yield statistical significance. However, we believe the results may be biased due to a small number of women willing to consider themselves as head of households in heterosexual couples, compared to homosexual ones where at least one must meet this requirement. This difference is greater for women in unmarried heterosexual partnerships. This suggests that married women that consider themselves as the head of the family may earn slightly more than those that decide to not get married but also consider themselves as the head of the household. Here, it is important to remember that in the Census, the head of the household is simply the person chosen to answer the questionnaire. As such, we must acknowledge that there might be a random

In terms of the law, the second row in panel B of Table 4 suggests that given the time, place, and state trends, women in same-sex relationships in states with the law do not have earnings that are statistically different than those of their counterparts in states without the law. Like in the case of men, the time for which the law has been in place does have an impact on the overall earnings of women. For the most part, the longer the time the law has been in place, the higher the earnings for all women.

The estimated impact of the law on the overall earnings of heterosexuals could also be driven by unobserved market changes. In the case of heterosexual men, other factors may have caused a downward trend in the earnings of married men, and an upward one for the earnings of unmarried cohabitating men in those states. We examine the earnings trends for men and women and find that both married and unmarried groups follow similar trends. Figure 2 shows the average earnings of married and unmarried heterosexuals, and individuals part of same-sex couples. In order to provide a comprehensive picture, we group them according to their decision to adopt the law or not. First, we show the trends using all fifty states. Second, we use the earnings of only those that have passed the law by the time of measurement (like the one used for the DDD estimate). In order to account for previous trends, we use those that have passed the law at any point in time. This means, for instance, that the 1990 and 2000 averages include the earnings of workers in Maryland even though the state did not pass the law until 2001. Finally, we look at the trends for those states that never passed the law. In all cases, we observe that real earnings spiked in 2000, but had dropped by 2009.

[INSERT FIGURE 2 HERE]

The drop in real earnings for all groups in 2009 can be mostly explained by the 2008 financial crises. However, we do observe that individuals in unmarried heterosexual relationships appeared to be the most affected. Married heterosexuals and homosexuals appeared to have experienced similar declines, which may be partly explained by the higher levels of

component in who is considered the head of household.

education. Despite the steeper fall for unmarried couples, the picture does show that all groups experienced similar trends regardless of whether a law was in place or not. Such similarities lead us to conclude that while other forces may shape the earnings of heterosexuals in states with anti-discriminatory laws, the opposite effects for married versus unmarried heterosexual men may actually be a product of the law rather than unobserved factors.

It also appears that 'the time since law enactment' is acting as a earning equalizer for homosexual women as well, impacting women in the opposite direction, compared to men. In other words, given that homosexual women tend to earn more than their heterosexual counterparts, employers in states with laws have no reason to fear the threat of a lawsuit from this group. Instead, they may be concerned about complaints from heterosexual women and be inclined to raise their wages. Although not statistically significant, we observe that the time since enactment lowers the relative wages of women in same-sex partnerships, partially providing some support to the argument above.[28]

5 Conclusion

The influence, and potential influence, of anti-discriminatory laws on the basis of sexual orientation have been increasingly considered in literature. Though other studies have analyzed the effect of these laws in the labor market, we are the first to use an extended time frame, considering the robustness of the reference group specification, using a difference-in-difference-in-difference estimation approach. Like some of the previous literature, we confirm the evidence that findings of earnings discrimination against homosexual men are sensitive to the choice of reference group. This supports previous findings that suggest that earnings differentials may be largely driven by a marriage premium. Overall our results suggest that, relative to married heterosexual men, gay men earn less and anti-discriminatory laws, over

[28]We also estimated effects of the anti-discrimination law on the probability of being in the labor force, employment, and full-time versus part-time work, capturing both extensive and intensive margins. Our data, however, is limited not allowing us to control for all factors that may affect labor supply, including employment opportunities in other cities, or perhaps other states. These and other concerns are supported by very low pseudo and regular r-squared values. These results are available from the authors upon request.

time, lessen this gap. Relative to unmarried, but coupled heterosexual men, homosexual men experience similar levels of earnings and both groups experience an increase in earnings over time after the passage of an anti-discriminatory law.

Homosexual women experience higher earnings than their unmarried, coupled and married heterosexual female counterparts. The effect of the law is to shrink this gap over time, but in this case that results in lower earnings for homosexual women since they out-earned their heterosexual counterparts. We employ a DDD approach that controls for trends in states and other local labor markets. In addition to allowing for different possible reference groups, we also specify the impact of the laws at the state of work rather than the state of residence as previous research has done.

Our results highlight two important policy issues. Statistically, the law seems to help equalize the earnings of homosexuals with respect to their married counterparts. Yet, our results suggest that this equalization may not be solely due to increases in earnings of those with lower wages (e.g. homosexual men), but also decreases in the earnings of those who start with higher wages (e.g. married heterosexual men). Unfortunately, the lack of information on the attitudes of men and women in same-sex relationships toward marriage preclude us from conclusively understanding what part of the wage differentials is due to discrimination, and what to marriage, and what part of the law could be more effective at addressing pure issues of discrimination. However, our findings represent an increase in the understanding of the effect of anti-discriminatory laws on homosexual employment outcomes. While the law seems to reduce the marriage premium, it raises the question on whether legislation to allow marriage between two men or two women will help to further dissipate the differences in the labor market between homosexuals and heterosexuals. If marriage is acting as a signal, blocking the ability of some groups to signal appropriately may result in labor market distortions.

On the other hand, if we assume that wage gaps are mostly due to household specialization, instead, it is not clear if legal recognition of same-sex marriage would have an effect

on earnings. Indeed, gay and lesbian couples are more likely to split household tasks more equally.[29] Assuming that the allocation of household tasks remains unchanged after same-sex marriage, repealing DOMA may have little impact on specialization and/or the marriage premium. Yet, the ability bias explanation for the existence of a marriage premium may make DOMA have an effect on the relative earnings of homosexuals as marriage may incentivize single (or uncoupled) homosexuals to look for more capable spouses.

In the absence of a comprehensive law that allows marriage between individuals of the same sex, future research focusing on finding ways to determine the correct reference group is necessary in order to understand what policies will help eliminate wage gaps that are due to discrimination, and reduce those due to marriage but unrelated to productivity. Our results also suggest that allowing homosexuals to marry may reduce the earnings differential without the imposition of labor market distortions created by anti-discrimination laws.

[29]See Kurdek (1993) for a discussion on the determinants of household specialization among gay and lesbians couples.

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Figure 1: Difference Between Proportion of Homosexuals and Heterosexuals in Occupations with the Highest Concentration of Homosexual

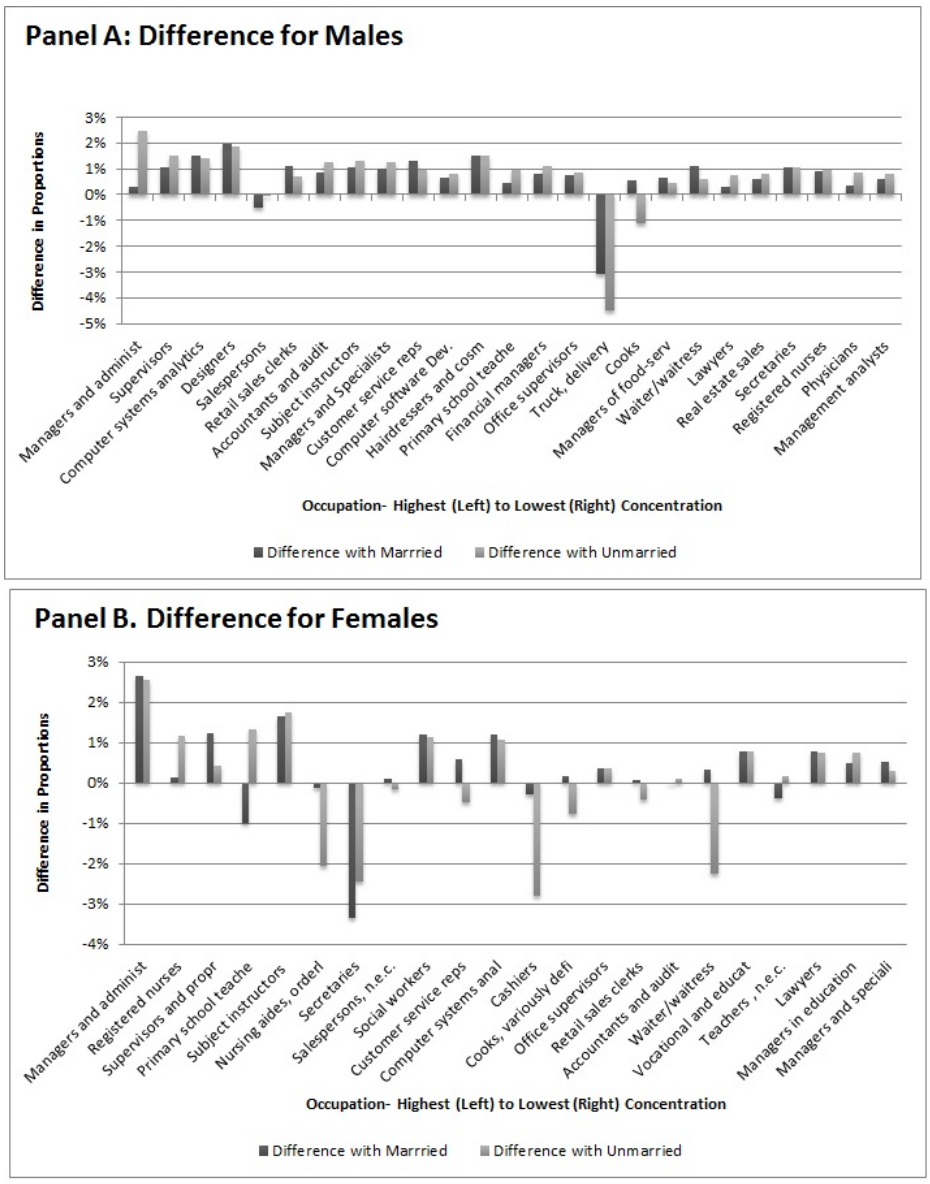

Figure 2: Average Men and Women's Earnings Grouped by the Passage of Anti-Discrimination Law on the Basis of Sexual Orientation

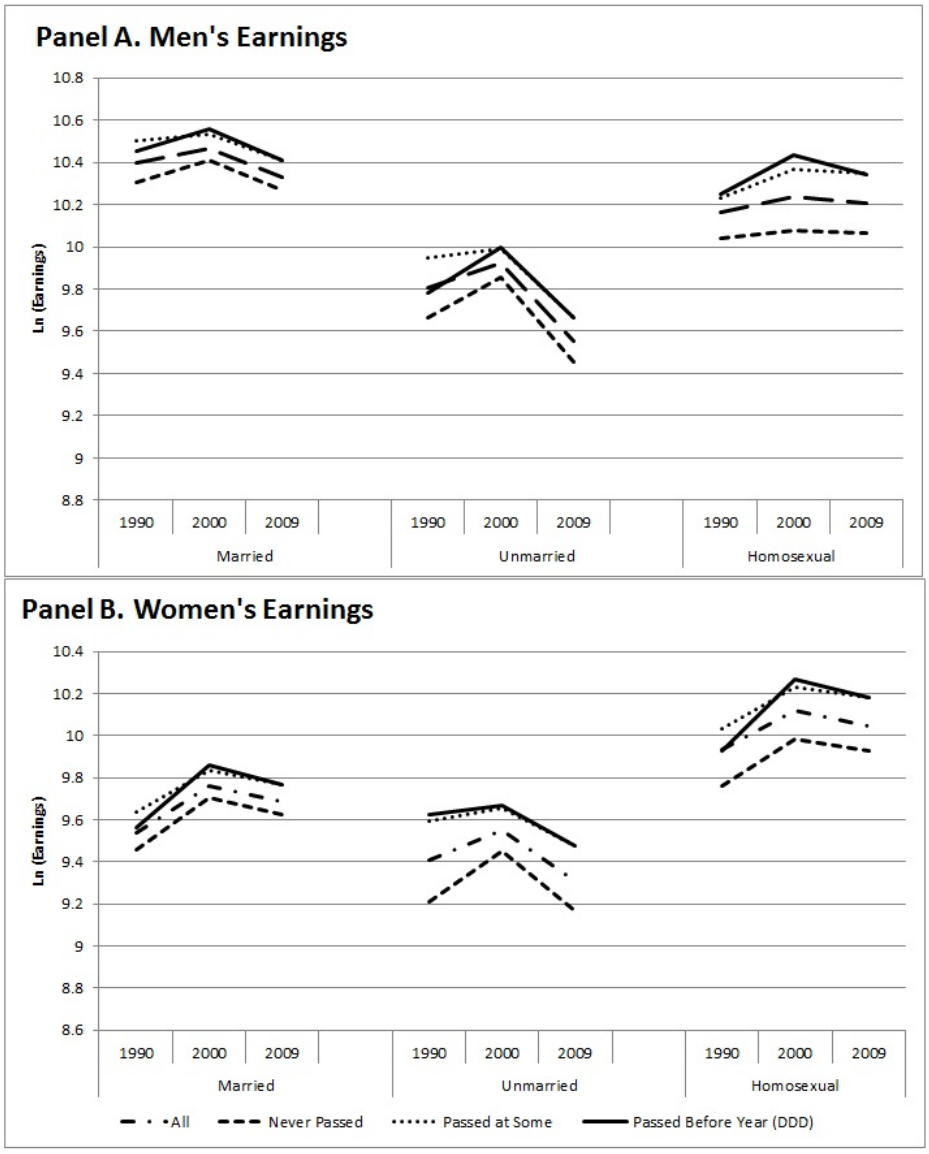

Table 1: States with Sexual Orientation Provisions in Anti-Discrimination Laws

State	Sector	Year	Statute or Law	Source	Other
Alaska (AK)	Public	2002	Admin. Order No. 195	HRC[a]	
Arizona (AZ)	Public	2003	Exec. Order No. 2003-22	HRC	
California (CA)	Public	1992	Gov. Code §121940	HRC	Gender Identity (2003 See below)
	Private	1992	§12920 and Civ. Code §51	Klawitter (2011)	Gender Identity Protected §12926 and §12949 (2003) HRC cites 2001
Colorado (CO)	Public	1990	Exec. Order D0035	Williams Institute	Almost repealed in Amendment 2 (1992)
	Private	2007	C.R.S 24-34-401, 24-34-402	HRC	Gender Identity Included
Connecticut (CT)	Public/Private	1991	Conn. Gen. Stat. §46a - 81c-m	HRC	Gender Identity (2011)
Delaware (DE)	Public	2001	Exec. Order No. 2000-83	Delaware Registrar of Regulations, Vol 4(9)	Gender Identity in Executive branch (2009)
	Private	2009	Senate Bill 121 (2009)	HRC	
Hawaii (HI)	Public/Private	1991	H.R.S. 515-2	HRC	Gender Identity Included
Illinois (IL)	Public	1996	Admin. Order No. 2	Illinois Code Section 302.790	
	Private	2005	775 ILCS 5/1-102	HRC	Gender Identity Included
Indiana (IN)	Public	2001	N/A	Klawitter (2011)	Gender Identity Added (2004)
Iowa (IA)	Public/Private	2007	§216.2(14)	Iowa Code, Chapter 216 (HRC)	Gender Identity Included
Kansas (KS)	Public	2007	Gov. Sebelius Exec. Order	HRC	Gender Identity Included
Kentucky (KY)	Public	2008	Gov. Beshear Exec. Order	The Equality Party[b]	Gender Identity Included First implemented from 2003-2006 and removed by Republican Gov. Fletcher (2006)
Louisiana (LA)	Public	1992-1996 and 2004-2008		Division of Admn. LA	Dropped in 2008 by Republican Gov. Jindal
Maine (ME)	Public	2001	Gov. King Jr. Revision of Code	Service Bulletin 13.4B Bureau of Human Resources	First from 1997-1998
	Private	2005	M.R.S. ANN. tit. 5 §4571-76	HRC	Gender Identity Included
Maryland (MD)	Public/Private	2001	Md. Ann. Code art. 49B §5	HRC	
Massachusetts (MA)	Public/Private	1989	MASS. GEN. LAWS ANN. ch. 151B, §§3-4	HRC	Gender Identity Included
Michigan (MI)	Public	2003	Exec. Order No. 2003-24	Office of the Governor	Gender Identity in 2007
Minnesota (MN)	Public	1991	Exec. Order No. 91-4	MN Legislature. 1991-01-29	
	Private	1993	MINN. STAT. §363A.01 to §363A.41	HRC	Gender Identity Included
Missouri (MO)	Public	2010	Exec. Order 10-24 Art. 1	Office of MO Governor	
Montana (MT)	Public	2000	N/A	Williams Institute[c]	
Nevada (NV)	Public/Private	1999	NV. REV. STAT. 233.010(2); 613.330	HRC	Gender Identity 2011
New Hampshire (NH)	Public/Private	1997	N.H. R.S.A. §§21-I:42, 354-A:2 , 354-A:6	HRC	Effective 1998 Gender Identity by court decision
New Jersey (NJ)	Public/Private	1992	N.J. STAT. ANN. §10:2-1; 10:5-1 - 49	HRC	Gender Identity Included
New Mexico (NM)	Public	1985	New Mexico Exec. Order No. 85-15	HRC	
	Private	2003	N.M. Stat. Ann. §§28-1-2, 7, 9	HRC	Gender Identity Included
New York (NY)	Public	1983	Gov. Cuomo Exec. Order	Office of the Governor	
	Private	2002	NY EXEC. LAW §296, 296-a	HRC	Gender Identity in some cases only[d]
Ohio (OH)	Public	2007	Exec. Order 10S	Gov. Strickland's website	First 1984-1999 Gender Identity removed (2011)
Oregon (OR)	Public/Private	2008	OR Equality Act 100, Oregon SB 2	HRC	Added to Equality Act. Public Sector previously covered 1988-1992 but repealed by voter initiative
Pennsylvania (PA)	Public	1975	Exec. Order 1975-5	Equality Pennsylvania	
Rhode Island (RI)	Public	1985	R.I. Exec. Order No. 11	Governor's Office website	Amended Exec. Order 9
	Private	1995	R.I. Gen. Laws §28-5-7	HRC	Gender Identity added 2001
Vermont (VT)	Public/Private	1992	21 §495; 9 §4503; 8 §10403; 8 §4724; 3 §963	HRC	Gender Identity added 2007
Virginia (VA)	Public	2006	Exec. Order 1	Office of Governor	
Washington (WA)	Public	1985	Exec. Order 85-09	HRC	
	Private	2006	§49.60.130-175, §356-09-020	HRC	Gender Identity Included
Wisconsin (WI)	Public/Private	1982	Relevant: §36.12, §106.50 §106.52 §111.31 §230.18 §224.77.	HRC	Gender Identity not Included

[a] Human Rights Campaign: State Laws and Legislation http://www.hrc.org/laws-and-legislation/state.

[b] Internet Blog site that seeks equal constitutional liberties and freedom for all people regardless of sexual orientation or gender identity.

[c] Memorandums created by The William Institute of UCLA's Law School.

[d] A court decision has mandated that discrimination based on Gender Identity may be pursued in some cases under the 'sex' category.

Table 2: Descriptive Statistics for Married and Unmarried Couples According to the U.S. Census

	1990						2000						2009					
	Same-Sex[a]		Opp.-Sex[b]		Married[c]		Same-Sex		Opp.-Sex		Married		Same-Sex		Opp.-Sex		Married	
	Law	No Law	Law	No Law	Law	No Law	Law	No Law	Law	No Law	Law	No Law	Law	No Law	Law	No Law	Law	No Law
In labor force	0.897	0.897	0.900	0.839	0.821	0.762	0.848	0.846	0.817	0.809	0.756	0.749	0.871	0.865	0.853	0.833	0.800	0.776
Privately Employed																		
Total	0.885	0.844	0.817	0.756	0.790	0.721	0.814	0.805	0.752	0.744	0.724	0.720	0.814	0.804	0.753	0.723	0.746	0.722
If in labor force	0.986	0.941	0.908	0.901	0.961	0.946	0.960	0.951	0.921	0.919	0.958	0.962	0.935	0.929	0.883	0.868	0.932	0.931
Ln(earnigns)[d]	10.202	10.290	10.033	10.031	10.306	10.326	10.542	10.313	10.189	10.024	10.492	10.354	10.581	10.351	10.144	9.951	10.539	10.372
Weeks Worked	48.486	46.884	48.486	44.499	45.535	46.894	47.772	47.659	47.772	45.697	45.765	47.795	47.142	47.001	44.866	44.152	44.866	47.033
Hours Worked																		
If employed	42.986	40.818	40.872	40.198	40.684	40.744	41.964	41.927	40.589	41.003	41.071	41.552	41.132	40.768	38.469	38.371	39.821	40.400
If 'Full-Time' Worker	44.952	42.960	43.008	42.720	43.480	43.417	43.925	43.386	43.110	43.194	43.838	44.033	43.699	43.343	41.998	42.032	43.206	43.411
Percent with Full Employment	0.900	0.873	0.853	0.823	0.845	0.850	0.887	0.905	0.845	0.851	0.858	0.870	0.876	0.873	0.817	0.807	0.847	0.861
Age	35.414	35.088	32.658	32.705	40.678	40.400	39.500	37.329	34.515	33.928	42.728	42.216	42.173	41.477	36.297	35.430	45.442	44.806
Percent Male	0.700	0.558	0.518	0.529	0.548	0.567	0.476	0.484	0.518	0.518	0.546	0.549	0.504	0.464	0.511	0.514	0.535	0.537
Percent White	0.914	0.885	0.824	0.808	0.917	0.875	0.829	0.873	0.738	0.781	0.775	0.857	0.860	0.897	0.777	0.802	0.806	0.858
HS Degree Only	0.114	0.138	0.364	0.339	0.378	0.316	0.114	0.171	0.275	0.348	0.230	0.295	0.117	0.170	0.270	0.334	0.211	0.261
Some College	0.186	0.257	0.226	0.236	0.178	0.213	0.211	0.242	0.260	0.252	0.227	0.236	0.206	0.250	0.249	0.273	0.210	0.226
Bachelor's Degree	0.286	0.273	0.107	0.111	0.157	0.158	0.379	0.338	0.246	0.192	0.299	0.261	0.381	0.371	0.285	0.222	0.337	0.315
Higher Degree	0.286	0.187	0.069	0.043	0.104	0.092	0.237	0.181	0.065	0.041	0.132	0.105	0.265	0.183	0.078	0.047	0.170	0.130
Percent with Disability	0.071	0.047	0.038	0.042	0.034	0.038	0.064	0.064	0.100	0.108	0.093	0.091	0.045	0.058	0.045	0.057	0.041	0.053
Percent in Metro	0.857	0.867	0.658	0.703	0.592	0.641	0.879	0.837	0.803	0.685	0.803	0.658	0.872	0.856	0.779	0.703	0.781	0.683
Speaks English	0.986	0.988	0.997	0.976	0.995	0.979	0.984	0.988	0.956	0.981	0.947	0.978	0.995	0.990	0.947	0.970	0.953	0.976
	Law		No Law		Law		No Law		Law		No Law							
Percent in Same-Sex Relationship	0.0037		0.0025		0.0072		0.0049		0.0085		0.0069							

Source: U.S. Census (1990 and 2000), American Community Survey (ACS 2009). Individuals include the head of household and their respective partners. Individuals are grouped according to their living arrangements.

[a] Same-Sex: Those couples in which the head of household and his/her unmarried partner are coded with the same gender.

[b] Opp.-Sex: Individuals in cohabitating couples of different sex

[c] Married: Head of household and partner are of different-sex and legally married.

[d] For *Ln(Earnings), weeks, and Hours Worked* we follow Beegle and Stock (2003) conditioning them on employment.

Table 3: Overall DDD Estimates of the Impact of Antidiscrimination Laws on $Ln(AnnualEarnings)$

	Married Individuals as Reference				Different-Sex Unmarried Cohabitants as Reference			
	Men	**t-stat**	**Women**	**t-stat**	**Men**	**t-stat**	**Women**	**t-stat**
Hours Worked	0.0136	27.70	0.0207	28.66	0.0159	35.83	0.0203	27.26
Weeks Worked	0.0327	82.58	0.0339	83.17	0.0330	71.75	0.0340	55.28
Age	0.0674	66.41	0.0485	46.79	0.0659	33.13	0.0643	34.95
Age Squared	-0.0007	-58.34	-0.0005	-42.39	-0.0007	-26.92	-0.0007	-28.47
Head of Household	0.0758	24.61	0.0289	9.80	0.1239	24.06	0.0561	10.75
Presence of Children	0.0371	11.56	-0.0292	-7.68	-0.0258	-3.83	-0.0683	-9.58
High-School Degree	0.1188	13.59	0.0663	4.56	0.1161	18.42	0.1116	8.83
Some College	0.2270	20.42	0.2062	13.10	0.1897	19.92	0.2279	16.07
College Degree	0.4907	33.48	0.4931	26.26	0.4239	29.34	0.4950	29.32
Post-College Degree	0.7424	31.44	0.7595	32.73	0.6873	24.73	0.7349	29.97
Disabled	-0.1012	-15.40	-0.0767	-11.07	-0.0908	-7.98	-0.0908	-8.32
Speaks English	0.3627	21.02	0.3501	16.38	0.2692	11.19	0.3492	11.98
White	0.1798	28.98	0.0745	15.89	0.1381	20.97	0.0718	10.54
Resides in Metro Area	0.1334	21.99	0.1590	24.07	0.0934	11.47	0.1389	16.91
ADL*SS	0.0084	0.28	-0.0145	-0.49	0.0327	1.04	0.0038	0.11
Unearned Real Income	0.0143	25.31	0.0101	18.57	0.0111	9.25	0.0042	4.57
Time since Passed	-0.0013	-17.00	0.0032	34.36	0.0050	24.70	0.0024	9.63
Time since Passed*SS	0.0078	4.00	-0.0039	-1.45	0.0050	2.27	-0.0046	-1.59
Local ADL	0.0300	1.91	0.0839	5.92	0.0457	2.05	0.0757	4.07
Local ADL*SS	0.0031	0.14	-0.0026	-0.08	0.0032	0.14	0.0094	0.31
In Same Sex Relationsihip (SS)	-0.2577	-2.24	0.0067	0.12	-0.1050	-0.89	0.0903	1.12
R-Squared	0.3738		0.3915		0.4029		0.4513	
No. Observations	808,603		548,931		74,963		65,569	

t-statistics are calculated based on standard errors that are corrected for clustering of observations by state and year. Additional controls include age, age-squared, race, gender, education (in which no-high-school degree is the reference group), metropolitan area, and English proficiency. Number of weeks worked, and usual hours worked per week are also included. All specifications use samples with individuals that worked 27 weeks or more, and at least 30 hours per week only. Earnings are adjusted for inflation where 1990 is the base year.

Table 4: DDD Estimates of the Impact of Antidiscrimination Laws on $Ln(AnnualEarnings)$

	Married Individuals as Reference		Different-Sex Unmarried Cohabitants as Reference	
	All	**Randomly Selected**	**All**	**Randomly Selected**
Panel A: Men				
In Same-Sex Relationship (SS)	-0.2577	-0.2541	-0.1050	-0.0776
	-2.24	**-1.54**	**-0.89**	**-0.43**
ADL*SS	0.0084	0.0318	0.0327	0.0572
	0.28	**0.83**	**1.04**	**1.57**
Time since Law Passed	-0.0013	-0.0015	0.0050	0.0054
	-17.00	**-17.11**	**24.70**	**25.15**
Time since Law Passed * SS	0.0078	0.0085	0.0050	0.0053
	4.00	**2.51**	**2.27**	**1.61**
R-Squared	**0.3738**	**0.3729**	**0.4029**	**0.3969**
Panel B: Women				
In Same-Sex Relationship (SS)	0.0067	-0.0407	0.0903	0.0348
	0.12	**-0.58**	**1.12**	**0.39**
ADL*SS	-0.0145	-0.0689	0.0038	-0.0502
	-0.49	**-1.80**	**0.11**	**-1.22**
Time since Law Passed	0.0032	0.0029	0.0024	0.0025
	34.36	**27.17**	**9.63**	**9.85**
Time since Law Passed * SS	-0.0039	-0.0098	-0.0046	-0.0101
	-1.45	**-2.36**	**-1.59**	**-2.36**
R-Squared	**0.3915**	**0.3941**	**0.4513**	**0.4460**

t-statistics are reported below each coefficient, and are calculated based on standard errors that are corrected for clustering of observations by state and year. Additional controls include age, age-squared, race, gender, education (in which no-high-school degree is the reference group), metropolitan area, English proficiency, existence of a local law, state and year dummies, interaction between same-sex and state and year dummies, as well as state and year interactions. Number of weeks worked, and usual hours worked per week are also included. All specifications use samples with individuals that worked 27 weeks or more, and at least 30 hours per week only. Earnings are adjusted for inflation where 1990 is the base year.