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Revisiting Evidence of Labor Market Discrimination against Homosexuals and the Effects of Anti-Discriminatory Laws

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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. However, we find the evidence of earnings disparities between homosexual and heterosexual men to be extremely sensitive to the choice of reference group. Relative to married heterosexual men, gay men earn less, and, over time, anti-discriminatory laws lessen this gap. Relative to unmarried, coupled heterosexual men, however, gay men experience similar levels of earnings. The choice of reference group leads to opposite conclusions regarding the effectiveness and necessity of an anti-discriminatory law for homosexual men, which highlights the need to construct reference groups with care. We also find that homosexual women experience similar earnings to their heterosexual female counterparts, and the law has no effect on these relative wages.

Keywords: earnings, employment, sexual orientation, policy

JEL Codes: J16, J70, J78

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., 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, gays do earn lower wages than their heterosexual counterparts.¹ Thus, 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

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¹ See Klawitter (2011) for a comprehensive overview.

² "The Employment Non Discrimination Act," is a bill that is currently being considered by the U.S. Congress to legally prohibit discrimination on the basis of sexual orientation at the federal level.

have independently passed their own version of the law.³ Similar to other anti-discrimination provisions (i.e., racial, gender, and disability), these laws attempt to establish equal access and opportunity to employment to those with different sexual orientations. Research on the effect of the other anti-discrimination provisions has not provided conclusive results of the impact they have on the group in question (Acemoglu and Angrist, 2001; Beegle and Stock, 2003; Collins, 2003).

This paper presents a re-evaluation of labor market outcomes for gays and lesbians and the effect of statewide anti-discriminatory laws. Recent studies using U.S. Census data compare the wages of gays and lesbians to their married heterosexual counterparts, and the difference in wages is sometimes interpreted as evidence of discrimination.⁴

We analyze the effect of anti-discriminatory laws on employment outcomes using a difference-in-difference-in-difference approach (DDD). This approach allows us to capture the relative differences in wages in states that have passed a law compared to states that have not. This method has two 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 wage differentials of homosexuals and heterosexuals that exist among states over time, independent of any law that was passed. If, for example, a state that had a relatively high earnings differential adopted the law and the earnings differential between homosexuals and heterosexuals decreased, but not to the level of states that did not pass the law, a simple cross-sectional analysis would suggest that the law did not have any impact. Secondly, the DDD 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.⁵ In addition to the approach, our analysis also differs from previous research with respect to data observation classification. Previous research links an individual's wages to their place of residence; we link wages to their place of work. Even though most people work in their state of residence, many also do not, as they live in MSAs that cross state borders. Thus a person can reside in a jurisdiction with no law, but work in a place *with* one. Data suggest that this may not be as important at the state level since many of the states with anti-discrimination laws (ADLs) based on sexual orientation are clustered in the northeast. Yet, since we follow Klawitter (2011) in allowing for the influence of laws passed at the local level, the use of place of work rather than place of residence is necessary.

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.

⁴ Our data on the classification of an observation as a gay or lesbian individual also come from the decennial U.S. Census. The Census allows individuals to classify their relationship to head of household as a same-sex partner, which is taken as an individual who is in a same-sex relationship. In doing so, our data is limited to same-sex male and same-sex female couples. Although Black et al. (2007a) find that these couples are representative of the general gay and lesbian population, this paper is limited to an analysis of the earning differentials between homosexual and heterosexual (married and unmarried) cohabitating couples.

⁵ Beegle and Stock (2003) used this approach to analyze the effect of disability laws. The DDD methodology is possible given that the U.S. 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 2010 American Community Survey (ACS). For computational ease, and in order to match the density of the ACS sample (one percent), the Census data extracts are re-weighted samples from the five 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.

The results of our analysis suggest that when we control for labor market trends in each state, anti-discrimination laws only have an effect on relative earnings as time since passage of the law increases. Conditional on employment, we find no statistical evidence of contemporaneous earning differences between the relative earnings of gays and lesbians 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 gays and lesbians. These results, however, may reflect two phenomena: a) homosexuals may be reluctant to reveal their sexual orientation in states without the law, making it hard for employers to discriminate in the first place, and b) even in states that adopt the law, homosexuals may not reveal their sexual orientation immediately, resulting in delayed effects.

Overall, however, we find mixed evidence of the existence of earnings differentials between homosexual and heterosexual men. Conditional on an urban location, where the majority of same-sex couples live and work, gay men do appear to have lower earnings, although the results are not statistically significant. Furthermore, the results do seem sensitive to the choice of comparison group (married versus unmarried men). Like Allegretto and Arthur (2001) and Carpenter (2005), we find that some of these economic differences are partly driven by the difference between individuals in same-sex couples and those in conventional heterosexual marriages. These results suggest that in order to better identify the source of these differences and be able to better understand the effect of ADLs, extending the current policies that allow same-sex marriage in some states may be necessary.

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 Discrimination and Labor Market Differences

Extensive research exists on the differences in employment outcomes between gays and heterosexual males and between lesbians and heterosexual females. This research has not been able to provide conclusive evidence for or against the existence of discrimination against gays or lesbians. In general, studies using various types of survey data have found that gays tend to earn lower wages than heterosexual males (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 these findings for gays, similar studies have found that lesbians typically earn the same or more than heterosexual females (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; Carpenter, 2007; Elmslie and Tebaldie, 2007; Ahmed and Hammarstedt, 2010), although Badgett (1995) did find that lesbians earn less than heterosexual women.

Discrimination in the labor market is well documented in the literature. 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 experience discrimination in the housing or labor market. Badgett et al. (2007) provides a comprehensive overview of many of the surveys that have been conducted of gays and lesbians

regarding discrimination. In their analysis they find that in studies conducted from the mid-1980s to mid-1990s, 16 percent to 68 percent 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-1990s 15 percent to 43 percent of respondents reported experiencing workplace discrimination.

Wage differentials between gays or lesbians and their heterosexual counterparts cannot be taken as conclusive proof of labor market discrimination, however. Differences in earnings between individuals in same-sex couples and heterosexuals may be partly explained by specializing households or by the marriage premium. Becker (1971) developed a household specialization model that predicts females in heterosexual relationships invest less in obtaining labor market skills as they expect to couple with a high earning male in the future. Meanwhile men tend to invest more in labor market skills because they believe they must compensate for the low earnings of the female they will couple with in the future.⁶

Becker's model predicts that discriminatory firms will be driven out of the industry since non-discriminatory ones will benefit from higher profits. Rosen (2003), however, argues that this prediction does not hold under markets with friction and wage bargaining. Applying this household specialization model to same-sex relationships, Becker (1991) predicts that gays invest less in obtaining labor market skills relative to heterosexual males (since they plan on partnering with a male), while lesbians invest more in obtaining labor market skills than heterosexual females (since they plan on partnering with a female). These predictions may seem plausible under the assumption that homosexuals are widely accepted, allowing them to plan their future investments without concern for social norms. Until recently, however, society's acceptance of homosexuality has been limited, making these investment decisions less clear for gays and lesbians. In fact, data show that both gays and lesbians in the workforce invest more in education than their heterosexual equivalents (Carpenter, 2005). This provides little support for Becker's household specialization theory for the case of gays and lesbians, invalidating the notion that the observed earnings differences often found in the literature are in fact due to under-investment by gays and over-investment by lesbians.⁷

In the absence of household specialization within same-sex couples, differences in earnings may be the result of a marriage premium (Allegretto and Arthur, 2001; Carpenter, 2005). Married men are often believed to be more productive, which is reflected in their wages. Married women, on the other hand, may be a relative liability to some employers given that they may be more likely to interrupt their work because of children. If and only if employers use marriage as a signal of future productivity, the inability of homosexuals to marry their partners, which remains prevalent in many states, may play an important role. Indeed, unmarried women in same-sex partnerships may benefit in the hiring process if employers believe their family commitments will not interfere with their future productivity. On the other hand, unmarried men in same-sex partnerships may be perceived by employers as less productive. If the earnings

⁶ In the presence of potential discrimination in a competitive market, Becker (1971) predicts that market forces will eliminate wage differentials due to employer tastes for discrimination.

⁷ An alternative measure of education is considered by Black et al. (2007b). They combine U.S. Census data for individuals in same-sex relationships with data from the National Survey of College Graduates and show statistics in which gay men are more likely to graduate with a "typically female major" than other men. The opposite is true for lesbians. Under the assumption that "typically male" majors yield higher returns, this suggests that wage differentials can be partly explained by education choice. See Brown and Corcoran (1997) for detailed descriptions of "typical female/male majors."

differentials are only due to observed productivity differences, and marriage is simply an outcome that characterizes productive men, any observed differences in earnings between gay men and married heterosexuals could be due to our inability to distinguish between gays that would and would not marry if allowed. Even after accounting for some measure of productivity, the potential for marriage as a signal cannot be ruled out as the source of earnings differentials. Given the small number of same-sex married couples today, employers who dislike homosexuals may assume all (or most) married men are heterosexual, thus increasing demand for married men (Carpenter, 2005).

In an attempt to overcome difficulties in measuring discrimination, some researchers have employed labor market experiments to test for its existence. Ahmed et al. (2013) conducted a national Swedish experiment in the hiring process, randomly assigning sexual orientation to different job applicants. They find that gays experience discrimination in male-dominated industries and that lesbians experience discrimination in female-dominated industries. Using a similar procedure in the United States, Tilcsik (2011) performed an experiment for gays in which the signal for sexual orientation was identified on subject's resumes—participation in a gay organization. He also finds evidence for discrimination against gay men. These results, however, pertain to openly gay men, who may be different in unobserved ways from the gay population in general. Both experiments test for discrimination in the hiring process rather than wages.

As seen, the state of the literature on labor market discrimination based on sexual-orientation highlights many of the difficulties in measuring the causes of differences in labor outcomes for homosexuals and heterosexuals. Although some studies find evidence of direct discrimination, other studies find that it may be a function of the social environment, including the ability to marry. Even in the absence of support for marriage, some outcomes can depend on how societies perceive homosexuality. These perceptions may not only shape gays' investments in education, but it may also affect their location decisions. Gays and lesbians may not only locate in places where homosexuality is widely accepted, but they also may locate strictly based on employers' attitudes. If true, actual potential discrimination may be understated by the data, since we should find fewer "revealed" homosexuals in areas where they are not accepted. Given that all studies depend on individuals' willingness to disclose their orientations, results to date—including ours—must be interpreted carefully.

2.2 History of Anti-Discriminatory Laws

Although measuring discrimination against gays and lesbians through rigorous statistical analysis is complicated, the documented cases 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: States with Sexual Orientation Provisions in Anti-Discrimination Law

State	Sector	Year	Statute or Law	Source
AK	Public	2002	Admin. Order No. 195	HRC ^a
AZ	Public	2003	Exec. Order No. 2003-22	HRC
CA	Public	1992	Gov. Code §121940	HRC
	Private	1992	§12920 and Civ. Code §51	Klawitter (2011)
CO	Public	1990	Exec. Order D0035	Williams Institute
	Private	2007	C.R.S 24-34-401, 24-34-402	HRC
CT	Public/Private	1991	Conn. Gen. Stat. §46a - 81c-m	HRC
DE	Public	2001	Exec. Order No. 2000-83	DE Registrar of Regulations
	Private	2009	Senate Bill 121 (2009)	HRC
HI	Public/Private	1991	H.R.S. 515-2	HRC
IL	Public	1996	Admin. Order No. 2	Illinois Code Section 302.790
	Private	2005	775 ILCS 5/1-102	HRC
IN	Public	2001	N/A	Klawitter (2011)
IA	Public/Private	2007	§216.2(14)	IA Code, Ch. 216 (HRC)
KS	Public	2007	Gov. Sebelius Exec. Order	HRC
KY	Public	2008	Gov. Beshear Exec. Order	The Equality Party ^b
LA	Public	2004	N/A	Division of Admn. LA ^c
ME	Public	2001	Gov. King Jr. Revision of Code	Service Bulletin 13.4B
	Private	2005	M.R.S. ANN. tit. 5 §4571-76	HRC
MD	Public/Private	2001	Md. Ann. Code art. 49B §5	HRC
MA	Public/Private	1989	MASS. GEN. LAWS ANN. ch. 151B, §§3-4	HRC
MI	Public	2003	Exec. Order No. 2003-24	Office of the Governor
MN	Public	1991	Exec. Order No. 91-4	MN Legislature. 1991-01-29
	Private	1993	MINN. STAT. §363A.01 to §363A.41	HRC
MO	Public	2010	Exec. Order 10-24 Art. 1	Office of MO Governor
MT	Public	2000	N/A	Williams Institute ^d
NV	Public/Private	1999	NV. REV. STAT. 233.010(2); 613.330	HRC
NH	Public/Private	1997	N.H. R.S.A. §§21-I:42, 354-A:2, 354-A:6	HRC
NJ	Public/Private	1992	N.J. STAT. ANN. §10:2-1; 10:5-1 - 49	HRC
NM	Public	1985	New Mexico Exec. Order No. 85-15	HRC
	Private	2003	N.M. Stat. Ann. §§28-1-2, 7, 9	HRC
NY	Public	1983	Gov. Cuomo Exec. Order	Office of the Governor
	Private	2002	NY EXEC. LAW §296, 296-a	HRC
OH	Public	2007	Exec. Order 10S	Gov. Strickland's website
OR	Public/Private	2008	OR Equality Act 100, Oregon SB 2	HRC
PA	Public	1975	Exec. Order 1975-5	Equality Pennsylvania
RI	Public	1985	R.I. Exec. Order No. 11	Governor's Office website
	Private	1995	R.I. Gen. Laws §28-5-7	HRC
VT	Public/Private	1992	21 §495; 9 §4503; 8 §10403; 8 §4724; 3 §963 Exec. Order 1	HRC
VA	Public	2006	Exec. Order 1	Office of Governor
WA	Public	1985	Exec. Order 85-09	HRC
	Private	2006	§49.60.130-175, §356-09-020	HRC
WI	Public/Private	1982	Relevant: §36.12, §106.50 §106.52 §111.31 §230.18 §224.77.	HRC

^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 Repealed in 2008.

^d Memorandums created by The William Institute of UCLA's Law School.

Note: Gender identity is currently included in private ADLs in CA, CO, CT, HI, IL, IA, ME, MA, MN, NV, NH, NY (in some cases), RI, VT, and WA.

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.⁸ 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 orientation. 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.

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). California, for instance, has added the term “sexual orientation” to 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.

2.3 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.⁹ 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. 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 increases firms’ costs by adding the risk of being sued over not hiring and/or firing a homosexual worker. The higher the cost to the firm, the increased probability of a member of that group being hired. This is often characterized as a “hiring subsidy” for the target group and may result in firms increasing their demand for gay and lesbian workers to avoid incurring the high cost of a potential lawsuit. On the other hand, firms also want to avoid the costs of lawsuits that arise from firing these workers. This raises the cost of hiring gays and lesbians if this is seen as a potential outcome, thereby providing an incentive for firms to decrease their demand for gay and lesbian workers. To the extent that the costs of both firing and not hiring gays and lesbians induce opposing shifts the demand curve, the overall effect of the law on wages and employment levels depends on the relative probabilities of a lawsuit over not hiring versus that of a lawsuit over firing individuals in the protected group. Within this context, the impact of a sexual orientation anti-discrimination law depends on firms’ fears of potential lawsuits. 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

⁸ We also run additional specifications including controls for the existence of laws covering public employees only. States with ADLs covering employment in the private sector usually have additional laws that cover the public sector. However, some states may have laws that apply to public employees but not those in the private sector. These additional results are qualitatively similar and are available upon request.

⁹ See Klawitter (2011) for a discussion of studies.

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.

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 the earnings gap between gay and lesbian workers relative to heterosexual workers. Gays and lesbians might receive lower wages for various reasons. Some employers might have a "taste for discriminating" with respect to minority groups (gays and lesbians in this case), implying that firms without such taste will hire a gay or lesbian but at a wage lower than a heterosexual worker with similar characteristics (Black, 1995). It also might be the case that the hiring cost, described above, is larger than the hiring subsidy, driving down wages. Another possibility is that there exist systematic unobserved productivity differences between homosexuals and heterosexuals which explain the earnings gap. However, there is no reason to believe that (beyond sexual orientation) homosexuals are systematically different from heterosexuals. Instead, discrimination against gays and lesbians is more likely due to "differences in constraints that systematically alter the incentives and behavior of heterosexuals" (Black et al., 2007b). Regardless of the reason, we expect to observe a lower equilibrium wage in the market for gay and lesbian workers, relative to the equilibrium wage for heterosexual workers. This implies that an equal-pay provision will act as a price floor in the market for gay and lesbian workers, reducing employment levels but raising the wage.

Given these two effects, the passage of an anti-discrimination law may result in several different employment outcomes for gays and lesbians. We would expect wages to increase if the hiring subsidy is greater than the hiring cost. The opposite occurs if the cost of hiring (or probability of a lawsuit over firing) is relatively higher. In the presence of an equal pay provision, if the hiring subsidy dominates and wages are still less than those for the heterosexual counterparts, then wages increase further and employment decreases. 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 from the equal pay provision. On the other hand, if the hiring costs dominate and wages for homosexuals are indeed lower than those for heterosexuals, then employment decreases further but wages increase from the equal pay provision. Even if there is no evidence of discrimination, we should expect to observe changes in wages and the employment level due to the possibility of lawsuits resulting from the implementation of the law.

2.4 Empirical Findings of Anti-Discriminatory Legislation

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 private employment laws and find no evidence that such laws affect wages of gays or lesbians. However, this study was performed using 1990 data, and most of the anti-discrimination laws in place had only been around for a short 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. He examines only state laws because they allow for an exact geographic match of same-sex couples. State-level policies also provide a more consistent standard of application and enforcement than do local laws.

Gates (2009) finds that the presence of an anti-discrimination law increases the relative earnings of gays by 3.0 percent and by 0.3 percent for each year the law is in effect. For lesbians, he finds an increase in relative earnings of lesbians of 2.0 percent in states with an anti-discrimination law and an increase of 0.3 percent for each year the law is in effect. More recently, Klawitter (2011) also uses 2000 U.S. Census data but includes local as well as state laws and uses a multilevel analysis. She 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 previous studies of sexual orientation anti-discrimination laws in two ways. First, given the potential for working in one state and living in another, we identify the impact of a law using the place of work rather than the place of residence; and 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 only use local laws indicators when state laws have not been adopted. As in Gates (2009), we assume that once a statewide 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.

3. EMPIRICAL RESEARCH

3.1 Difference-in-Difference-in-Difference

Recent studies that analyze the effect of race and sex anti-discriminatory laws have estimated regressions with pooled data from different periods using the difference-in-difference-in-difference method. Neumark and Stock (2001) employ the DDD approach 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 sex anti-discrimination laws increase relative 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 1940s, that the passage of race anti-discrimination laws improved labor market outcomes for blacks. The DDD approach 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, and also do not have an effect on their relative employment.

The difference-in-difference-in-difference approach allows us to identify the net effect of the laws on the employment outcomes for individuals in same-sex relationships. We follow Beegle and Stock (2003) by controlling for differences in employment outcomes across states and time. Like them, we also include control for interaction effects between state and year, thereby controlling for differences in employment outcomes across states for each specific year. Additional interactions include those between time and state with the indicator for “gay/lesbian.” 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,

$$(1) \mathbf{y}_{ist} = \alpha + (\mathbf{sl}_{st} * \mathbf{ss}_{ist})\phi + \mathbf{ss}_{ist}\gamma + (\mathbf{ss}_{ist} * \mathbf{state}_s)\delta_s + (\mathbf{ss}_{ist} * \mathbf{year}_t)\delta_t + \mathbf{state}_s\eta_s + \mathbf{year}_t\eta_t + (\mathbf{state}_s * \mathbf{year}_t)\eta_{st} + \mathbf{X}_{ist}\beta + \mathbf{ll}_{ist}\rho + (\mathbf{ll}_{ist} * \mathbf{ss}_{ist})\theta + \mathbf{duration}_{st}\phi + (\mathbf{duration}_{st} * \mathbf{ss}_{ist})\zeta + \varepsilon_{ist}$$

where $\mathbf{y}_{ist}$ is the employment outcome of interest for an individual i in state s and Census year t . The matrix denoted by $\mathbf{X}$ includes information about race, gender, age, age squared, a dummy variable to indicate if the person lives with children, dummy variables for educational attainment, disability status, English proficiency, urban or rural residency, and unearned income. $\mathbf{sl}$ corresponds to a binary indicator for the existence of the law in a state in a given year, and $\mathbf{ss}$ is an indicator defining whether the person is part of a same-sex relationship. Similarly, $\mathbf{ll}$ 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. This is particularly important since there might be regional differences within the states due to the fact that labor markets may be very localized. Finally, **duration** measures the amount of time since the state law has been passed for the year of measurement.

Previous studies, including Gates (2009) and Klawitter (2011), have reported results for subsets of the population. 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 gays and lesbians, we estimate the model for men and women separately. 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 (i.e., by race or age) presents problems of perfect predictability and collinearity, precluding us from obtaining separate estimations by race. We, however, control for these characteristics in the main regression.

Finally, 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 exogenous and quasi-experimental approaches like the difference-in-difference method need to account for this possibility. Besley and Case (2000) propose two approaches. One is an instrumental variable approach in which the laws are instrumented with political variables, and second uses a third difference by adding a valid comparison group (making this a DDD approach). Though no perfect solution, we opt for the DDD and control for these preconditions by using state and year dummies.

As noted by Beegle and Stock (2003) and Bertrand et al. (2004), there is also the potential that a serial correlation problem exists. Our specification controls for time since the law was adopted. Although this does not eliminate this concern, it does greatly reduce the problem. Additionally, we consider only three periods. Following Beegle and Stock (2003), we argue that this further reduces the threat of serial correlation. As a practical matter, the passage of the laws can increase the incentive to self-identify as a gay or lesbian, making the effect of the law harder to interpret. Although a simple look at the data, described below, suggests that the percentage of people who self-identify as gays or lesbians is small and very similar in states with and without laws, we discuss the implications surrounding the possibility of increased self-identification in states with ADLs in the results section.

3.2 Data

The data on gays and lesbians come from the five percent samples of the U.S. Census for 1990 and 2000 and the 1 percent sample of the 2010 American Community Survey (Ruggles et al., 2009).¹⁰ These data sets supplied by the U.S. Census are the only U.S. national data sources that enable researchers to identify individual members of same-sex couples as well as their states and localities of residence and of work. While most respondents work in the same state where they live, there is a nontrivial number of respondents that work in another state. The Census also provides information on race, gender, disability status, education, and age for which we can control.

The U.S. Census Bureau does not ask individuals directly about their sexual orientation. Rather, beginning with the 1990 Census, it has been possible to identify same-sex couples that cohabitate with the nature of the relationship inferred from the answer to “relationship to household head” question on the Census.¹¹ 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). The Census recoded those responses that appeared to be same-sex married couples as same-sex unmarried couples. Black et al. (2007a) find that most same-sex married couples are actually different-sex married couples that misclassified themselves. Gates and Steinberger (2010) suggest 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, which would otherwise overestimate the count of same-sex couples.

To control for labor market conditions, the difference-in-difference-in-difference approach calls for the use of a comparison group. This group must be as similar as possible to the treatment group except that the law does not affect them directly. Since homosexuals could not marry during the period of evaluation, one cannot tell with certainty if those in same-sex partnerships are more similar to married or unmarried heterosexuals. Thus, we first estimate the model using both married and unmarried heterosexuals as the comparison group, and later compare them to the two groups separately and see if the findings vary. Both studies by Gates (2009) and Klawitter (2011) use married couples as the comparison group, but also include different-sex unmarried couples in the sample. Which comparison group is most appropriate is a debated issue that remains unanswered. In part it depends on the type and number of people in same-sex partnerships that would marry if they could. Given the data limitations, we leave this issue for future research as more states legalize same-sex marriage and data on the topic become more available.

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). As noted above, our indicator for the presence of a local law takes the value of 1.0 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 each of the three years in the sample. In other words, workers in Los Angeles, California, are subject to the local law in 1990 but no longer in 2000, since the statewide law was adopted in 1992. We create this variable to control for the fact that

¹⁰ These data are publicly available from www.ipums.org. For computational ease, we extracted a re-weighted sample of one percent density for the years 1990 and 2000.

¹¹ Gay and lesbian households are identified from the “relationship to household head” heading on the U.S. Census. The categories under this heading are spouse, child, in-law, unmarried partner, and other nonrelative.

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 U.S. Census, we are able to first allocate those laws to workers in each specific city and county. For towns for which we cannot find a match in the Census data, we follow Klawitter (2011) by using PUMAs, although we raise the coverage threshold of the local law to 40 percent of the PUMA.¹²

The sample is initially restricted to include all people of working age (16 to 64 years).¹³ In an effort to exclude college students, who are likely to have roommates with the same gender, and also those in the process of retiring, we also examine a sample of individuals between the ages of 25 and 59.¹⁴ In the analysis, employment is conditioned on being in the labor force, while earnings are conditioned on being employed. Furthermore, we only analyze the earnings of those who work full-time for most of the year (i.e., 30 or more hours per week and 27 or more weeks per year). We also exclude observations with missing data and those with real hourly wages equal to \$1.00 or less. Since weeks and hours worked are reported in ranges for 2010 we use midpoints.

Table 2 is a general description of the data. The share of individuals who identify themselves as gay or lesbian (i.e. in same-sex partnership) is measured as a percentage of the total number of couples, including both married and unmarried heterosexuals. When looking at the number of gays and lesbians as a percentage of unmarried couples only, we find that the share oscillates between 4 and 8 percent, independent of whether or not a law exists. Table 2 shows that there are slightly more same-sex couples in states with a law in any given year. But the numbers are not significantly different from a statistical perspective, and the difference is less than a percentage point.

Overall we do not observe any major abnormal labor outcomes. Labor force participation is slightly lower for those who are married. It is likely in certain cases a member of a married couple chooses to opt out of the labor force as part of their household's decisions. At the same time, the data suggest, given they participate in the labor force, that married individuals are more likely to be employed. Interestingly, the employed share of individuals in same-sex relationships is higher than that same share for unmarried heterosexuals. This is true for all years whether they locate in a state with an anti-discrimination law or not. Additionally, the employed share of same-sex partners is higher in states with an ADL than in states without one. Although this is true for all years, the difference is consistently small, especially in years where the level of employment around 95 percent (Privately Employed, If in labor force in Table 2).

There are not noticeable differences in other employment outcomes, such as weeks worked or total earnings received.¹⁵ Persons in same-sex relationships tend to be more highly educated, younger (in some cases), and more likely to live in metro areas. All such factors undoubtedly play a role in employment success.

¹² See Klawitter (2011) for the use of PUMAs for the allocation of local laws.

¹³ 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.

¹⁴ Note that the U.S. Census does identify "roommates" as a category in the "relationship to the household" variable. Thus, for comparability we also used a sample that included all those between the ages of 18 and 64. The results are nearly identical and are available upon request.

¹⁵ Earnings are annualized earnings given that the U.S. decennial Census (1990, 2000) and the ACS (2010) do not report wages.

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

	1990			2000			2010		
	Same Sex	Opp. Sex	Married	Same Sex	Opp. Sex	Married	Same Sex	Opp. Sex	Married
States With Law									
In labor force	0.897	0.900	0.821	0.848	0.817	0.756	0.871	0.853	0.800
Privately Employed									
Total	0.885	0.817	0.790	0.814	0.752	0.724	0.814	0.753	0.746
If in labor force	0.986	0.908	0.961	0.960	0.921	0.958	0.935	0.883	0.932
Ln(earnings)	10.202	10.033	10.306	10.542	10.189	10.492	10.581	10.144	10.539
Weeks Worked	48.486	48.486	45.535	47.772	47.772	45.765	47.142	44.866	44.866
Hours Worked									
If Employed	42.986	40.872	40.684	41.964	40.589	41.071	41.132	38.469	39.821
If 'Full-Time'	44.952	43.008	43.480	43.925	43.110	43.838	43.699	41.998	43.206
Fully Employed	0.900	0.853	0.845	0.887	0.845	0.858	0.876	0.817	0.847
Age	35.414	32.658	40.678	39.500	34.515	42.728	42.173	36.297	45.442
Percent Male	0.700	0.518	0.548	0.476	0.518	0.546	0.504	0.511	0.535
Percent White	0.914	0.824	0.917	0.829	0.738	0.775	0.860	0.777	0.806
HS Degree Only	0.114	0.364	0.378	0.114	0.275	0.230	0.117	0.270	0.211
Some College	0.186	0.226	0.178	0.211	0.260	0.227	0.206	0.249	0.210
Bachelor's Degree	0.286	0.107	0.157	0.379	0.246	0.299	0.381	0.285	0.337
Higher Degree	0.286	0.069	0.104	0.237	0.065	0.132	0.265	0.078	0.170
Percent Disability	0.071	0.038	0.034	0.064	0.100	0.093	0.045	0.045	0.041
Speaks English	0.857	0.658	0.592	0.879	0.803	0.803	0.872	0.779	0.781
States Without Law									
In labor force	0.897	0.839	0.762	0.846	0.809	0.749	0.865	0.833	0.776
Privately Employed									
Total	0.844	0.756	0.721	0.805	0.744	0.720	0.804	0.723	0.722
If in labor force	0.941	0.901	0.946	0.951	0.919	0.962	0.929	0.868	0.931
Ln(earnings)	10.290	10.031	10.326	10.313	10.024	10.354	10.351	9.951	10.372
Weeks Worked	46.884	44.499	46.894	47.659	45.697	47.795	47.001	44.152	47.033
Hours Worked									
If Employed	40.818	40.198	40.744	41.927	41.003	41.552	40.768	38.371	40.400
If 'Full-Time'	42.960	42.720	43.417	43.386	43.194	44.033	43.343	42.032	43.411
Fully Employed	0.873	0.823	0.850	0.905	0.851	0.870	0.873	0.807	0.861
Age	35.088	32.705	40.400	37.329	33.928	42.216	41.477	35.430	44.806
Percent Male	0.558	0.529	0.567	0.484	0.518	0.549	0.464	0.514	0.537
Percent White	0.885	0.808	0.875	0.873	0.781	0.857	0.897	0.802	0.858
HS Degree Only	0.138	0.339	0.316	0.171	0.348	0.295	0.170	0.334	0.261
Some College	0.257	0.236	0.213	0.242	0.252	0.236	0.250	0.273	0.226
Bachelor's Degree	0.273	0.111	0.158	0.338	0.192	0.261	0.371	0.222	0.315
Higher Degree	0.187	0.043	0.092	0.181	0.041	0.105	0.183	0.047	0.130
Percent Disability	0.047	0.042	0.038	0.064	0.108	0.091	0.058	0.057	0.053
Speaks English	0.867	0.703	0.641	0.837	0.685	0.658	0.856	0.703	0.683
	Law	No Law		Law	No Law		Law	No Law	
Percent Same-Sex	0.0037	0.0025		0.0072	0.0049		0.0085	0.0069	

Source: US Census (1990 and 2000) and American Community Survey (ACS 2010). Individuals include the head of household and their respective partners. Individuals are grouped according to their living arrangements as follows. (a) Same Sex: Those couples in which head of household and his/her unmarried partner are coded with the same gender. (b) Opp. Sex: Different-sex individuals in cohabitating households. (c) Married: Head of household and partner are of different sex and legally married.

Note: For Ln(Earnings), weeks, and hours worked we follow Beegle and Stock (2003) conditioning them on employment.

Table 3: DDD Estimates of the Impact of Anti-discrimination Laws on Employment

Variable	Men	Women
<i>Age</i>	0.0519*** 17.76	0.0470*** 17.94
<i>Age Squared</i>	-0.0004*** -13.26	-0.0004*** -12.13
<i>Head of Household</i>	0.1449*** 13.3	0.0176** 2.01
<i>Presence of Children</i>	0.0410*** 5.62	-0.0053 -0.61
<i>High School Degree</i>	0.1240*** 7.37	0.2164*** 14.80
<i>Some College</i>	0.1637*** 8.24	0.3253*** 20.89
<i>College Degree</i>	0.4293*** 18.68	0.5530*** 29.78
<i>Post College Degree</i>	0.5347*** 14.81	0.7000*** 31.75
<i>Disabled</i>	-0.2190*** -8.33	-0.2070*** -5.58
<i>Speaks English</i>	0.0749*** 3.8	0.3481*** 12.79
<i>White</i>	0.2243*** 10.78	0.2246*** 12.97
<i>Resides in Metro Area</i>	0.0095 0.54	0.0403*** 3.39
<i>Unearned Real Income</i>	-0.0531*** -11.23	-0.0633*** -14.80
<i>In Same Sex Relationship (SS)</i>	0.3309* 1.82	0.0954 0.46
<i>ADL*SS</i>	0.0725 0.69	-0.1228 -1.11
<i>Time Since Passed</i>	-0.0037*** -12.66	-0.0065*** -21.09
<i>Time Since Passed *SS</i>	-0.0253*** -3.13	0.0080 0.88
<i>Local ADL</i>	0.1566*** 6.56	0.1191*** 3.32
<i>Local ADL*SS</i>	-0.0655 -0.74	-0.0979 -1.02
Pseudo R-Squared	0.0703	0.0809
No. Observations	1211514	1005408

Notes: Estimates are the raw coefficients of a probit estimation. They do not represent the marginal effects. *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. *State* and *Time* dummies, as well as *State* and *Time* interactions, *State* and *SS*, and *Time* and *SS* interactions are included in order to account for time trends.

Level of significance:

10 percent *(*t*-stat>1.645),
5 percent **(*t*-stat>1.960), and
1 percent *** (*t*-stat>2.576).

4. RESULTS

The estimated effects of ADLs on employment and earnings are shown in Tables 3 and 4. Controlling for education, location, age, race, and other characteristics, we do *not* find evidence to support claims that individuals in same-sex relationships are less likely to be employed, relative to heterosexuals in committed relationships (i.e., married and unmarried but living in cohabitation).

In fact, relative to other heterosexuals, men in same-sex relationships are generally more likely to be employed.¹⁶ Holding everything else equal, including time and place, we find no evidence that, relative to heterosexuals, men and women in same-sex partnerships who locate in states where ADLs have been passed have better employment prospects than those in states without ADLs (*ADL*SS*). Moreover, although not statistically robust, we find that relative employment levels of women in same-sex partnerships are worse in states with ADLs. We find, over time, that state ADLs can have small negative effects on overall employment. Indeed, the evidence suggests that gay men are less likely to be employed in states where the law has been in place longer. Additionally, the evidence suggests that overall employment in areas where ADLs have been adopted at the local level is relatively higher. Interestingly, the data do not support the claim that the probability of employment for men and women in same-sex partnerships is significantly different in states with ADLs.

Table 4 shows the estimates of the impact of the law on annualized earnings. While Black et al. (2007b) suggest that college major may be important in explaining wage differences, we do not have data on individuals' college majors for the years 1990 and 2000. The U.S. Census does identify industry as well as occupations, however. Controlling for occupation presents problems of perfect predictability given the small number of homosexuals in certain occupations in some regions. As in Table 3, we analyze men and women separately. Except for a few variables, the results are mostly comparable to those obtained for the case of employment. Consistent with some previous findings (Klawitter, 2011), we find that men in same-sex relationships do earn less relative to men in heterosexual relationships, while we find no statistical evidence of earnings differentials between women in same-sex partnerships and women in heterosexual ones (married and unmarried).

We find no statistical evidence that the relative earnings are higher for either men or women in same-sex partnerships working in ADL states than their counterparts in states without ADLs. Once again, the *Time Since Passed* appears to negatively influence the overall average earnings, although the effect is economically small. Contrary to findings on employment propensities, the relative earnings of men in same-sex partnerships do tend to rise as time since the implementation of ADLs passes. This might be due to the fact that these men do earn relatively less, but appear to be more likely to be employed. It may be that the law affects earnings not only by increasing the wages of gay men, but also by decreasing those of heterosexual men. Since the percent of gay men is relatively low compared to that of heterosexual men, this could explain the decrease in overall earnings.

¹⁶ The results do not represent the marginal effects, but rather the coefficients from the probit regression, since we are mainly concerned with the signs of these coefficients.

Table 4: DDD Estimates of the Impact of Anti-discrimination Laws on Ln(Earnings) – Conditional on Full-Time Employment

Variable	Men	Women
<i>Hours Worked</i>	0.0139*** 28.70	0.0207*** 29.56
<i>Weeks Worked</i>	0.0331*** 87.70	0.0340*** 85.15
<i>Age</i>	0.0680*** 67.28	0.0535*** 53.42
<i>Age Squared</i>	-0.0007*** -58.62	-0.0006*** -49.21
<i>Head of Household</i>	0.0893*** 26.98	0.0217*** 8.15
<i>Presence of Children</i>	0.0574*** 18.14	-0.0186*** -5.01
<i>High School Degree</i>	0.1200*** 14.70	0.0726*** 5.10
<i>Some College</i>	0.2265*** 21.37	0.2109*** 13.69
<i>College Degree</i>	0.4924*** 34.44	0.5011*** 27.34
<i>Post College Degree</i>	0.7474*** 31.66	0.7689*** 33.34
<i>Disabled</i>	-0.1010*** -15.29	-0.0786*** -11.66
<i>Speaks English</i>	0.3528*** 20.31	0.3475*** 15.97
<i>White</i>	0.1788*** 30.01	0.0760*** 16.28
<i>Resides in Metro Area</i>	0.1295*** 21.27	0.1564*** 23.95
<i>Unearned Real Income</i>	0.0144*** 24.75	0.0090*** 16.33
<i>In Same Sex Relationship (SS)</i>	-0.2246* -1.94	0.0256 0.43
<i>ADL*SS</i>	0.0125 0.42	-0.0133 -0.45
<i>Time Since Passed</i>	-0.0009*** -10.95	0.0029*** 29.08
<i>Time Since Passed *SS</i>	0.0074*** 3.83	-0.0039 -1.46
<i>Local ADL</i>	0.0283* 1.73	0.0804*** 5.42
<i>Local ADL*SS</i>	0.0056 0.26	0.0005 0.02
$\bar{R}^2$	0.3925	0.3993
No. Observations	883566	614500

Notes: Samples include 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. *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. *State* and *Time* dummies, as well as *State* and *Time* interactions, *State* and *SS*, and *Time* and *SS* interactions are included in order to account for time trends.

Level of significance: 10 percent * (*t*-stat>1.645)

5 percent ** (*t*-stat>1.960), and

1 percent *** (*t*-stat>2.576).

The results suggest that while there is insufficient evidence to support the claim that employment and earnings of homosexuals in states with anti-discriminatory laws based on sexual orientation are relatively higher or lower than those in states without the law, there seem to be noticeable differences over time, especially for men. Yet, these results must be interpreted carefully. As mentioned before, individuals in same-sex partnerships may be more likely to reveal their sexual orientation in states where they feel more protected, both economically and socially. This would mean that those in same-sex relationships may be more reluctant to show any signs of differences in sexual orientation in states without ADLs. This would decrease the chances of discrimination given that employers who would be inclined to do so would have a hard time identifying these individuals. A lack of discrimination in states without ADLs could then explain why we find no statistical difference between states with and without ADLs. In terms of policy implications, this would suggest that although the law may not immediately change the relative wages of those in same-sex relationships, it may allow them to be open about their orientation without fear of discrimination.

An additional concern is the location preferences of those in same-sex partnerships. Individuals in same-sex partnerships may be more likely to locate in urban areas where wages are higher. Furthermore, they may be more likely to locate in certain regions of the country. In order to address these and other measurement issues, we check the sensitivity of our results by looking at regional variations and urban living. Additionally, we check the robustness of the results by restricting our sample to only working individuals between the ages of 25 and 59. This allows us to discount college students who may work in low-paying jobs for only a brief period. This also allows us to account for possible measurement error arising from college students living with same-sex roommates.¹⁷

Tables 5 and 6 show the results for three additional specifications. In columns 1 and 2, we present the results when the sample is restricted to individuals of ages 25 to 59. The results in columns 3 and 4 are found by further restricting the sample to those located in urban areas, and the results in columns 5 and 6 regard the inclusion of regional dummies rather than state dummies. Statistically, the results are robust and very similar to those found above. For the case of employment, we observe some differences when using regional dummies. This suggests that although there might be regional differences, there is also variation within the regions, and once we control for state dummies the results obtained generally change. The same appears to be true for earnings. Of importance is the fact that there is no longer statistical support to the claim that men in same-sex partnerships earn less than other men in urban areas. Additionally, the decrease in the relative wages of women in same-sex partnerships overtime, in states with ADLs, is now statistically significant.

Table 7 highlights the importance of the Difference-in-Difference-in-Difference approach. In it we show the results when using each year of data separately. As shown, some of the results vary from year to year. As expected, the attitudes toward homosexuals evolve and labor market conditions have changed over the 20-year period. Failure to account for these changes can lead us to make an inference based on observations for a particular point in time,

¹⁷ Recall that while not immune to coding errors, the U.S. Census does distinguish between roommates and unmarried partners in the "Relationship to the Householder" category.

Table 5: Alternative DDD Estimates of the Impact of Anti-discrimination Laws on Employment

Variable	Age 25-59		Urban Only		Regional Controls	
	Men	Women	Men	Women	Men	Women
<i>Age</i>	0.0361*** 10.49***	0.0314*** 10.02	0.0532*** 15.83	0.0455*** 14.98	0.0514*** 12.70	0.0465*** 11.72
<i>Age Squared</i>	-0.0003*** -6.37	-0.0002*** -6.03	-0.0005*** -12.56	-0.0004*** -10.09	-0.0004*** -9.88	-0.0004*** -7.97
<i>Head of Household</i>	0.1531*** 14.35	0.0183* 1.95	0.1467*** 12.47	0.0201** 2.00	0.1433*** 7.40	0.0175 1.57
<i>Presence of Children</i>	0.0552*** 7.23	0.0197** 2.49	0.0366*** 4.20	-0.0080 -0.80	0.0407*** 3.73	-0.0066 -0.53
<i>High School Degree</i>	0.1453*** 9.07	0.2070*** 13.27	0.1008*** 5.25	0.2132*** 12.65	0.1205*** 4.72	0.2188*** 9.50
<i>Some College</i>	0.1772*** 9.10	0.3016*** 17.73	0.1553*** 7.41	0.3194*** 16.65	0.1589*** 4.45	0.3264*** 13.10
<i>College Degree</i>	0.4395*** 18.88	0.5320*** 27.09	0.4183*** 17.07	0.5337*** 25.33	0.4252*** 10.30	0.5547*** 19.25
<i>Post College Degree</i>	0.5438*** 14.68	0.6874*** 30.10	0.5201*** 13.44	0.6760*** 27.91	0.5303*** 8.03	0.7032*** 19.71
<i>Disabled</i>	-0.2321*** -8.97	-0.2226*** -6.06	-0.2106*** -6.64	-0.1902*** -4.36	-0.2163*** -5.36	-0.2064*** -3.28
<i>Speaks English</i>	0.0868*** 4.14	0.3660*** 13.04	0.0936*** 4.84	0.3627*** 13.41	0.0723*** 3.19	0.3580*** 12.39
<i>White</i>	0.2241*** 10.83	0.2162*** 12.30	0.2123*** 9.30	0.2133*** 11.27	0.2308*** 8.44	0.2280*** 10.32
<i>Resides in Metro Area</i>	0.0151 0.88	0.0387*** 3.27			0.0118 0.68	0.0191 1.06
<i>Unearned Real Income</i>	-0.0531*** -10.88	-0.0632*** -14.24	-0.0528*** -9.52	-0.0643*** -12.68	-0.0534*** -6.71	-0.0629*** -8.80
<i>In Same Sex Relationship (SS)</i>	0.2188 0.73	0.4410 1.49	0.7511*** 4.52	-0.0688 -0.38	0.1017 1.24	-0.0308 -0.24
<i>State ADL</i>					0.0927*** 3.44	0.0440* 1.85
<i>ADL*SS</i>	0.0026 0.02	-0.1990 -1.58	-0.0078 -0.07	-0.0849 -0.76	0.1442* 1.67	-0.0684 -0.67
<i>Time Since Passed</i>	-0.0022*** -7.96	-0.0055*** -19.02	-0.0061*** -23.87	-0.0077*** -31.41	-0.0006 -0.29	0.0019*** 0.61
<i>Time Since Passed*SS</i>	-0.0180** -2.11	0.0085 0.87	-0.0219*** -2.62	0.0105 1.26	-0.0076 -1.51	0.0055 0.64
<i>Local ADL</i>	0.1540*** 6.64	0.1163*** 3.25	0.1377*** 5.12	0.1118*** 3.00	0.1467*** 4.86	0.1100*** 2.77
<i>Local ADL*SS</i>	-0.0905 -0.85	-0.0918 -0.74	-0.0750 -0.88	-0.0785 -0.81	-0.0466 -0.46	-0.1216 -1.07
Pseudo R ²	0.0657	0.0753	0.0726	0.0824	0.0662	0.0783
No. Observations	1088352	898712	843683	689321	1211581	1005540

Notes: Estimates are the raw coefficients of a probit estimation. They do not represent the marginal effects. *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. *State* and *Time* dummies as well as *State* and *Time* interactions, *State* and *SS*, and *Time* and *SS* interactions are included in order to account for time trends.

Level of significance: 10 percent * (*t*-stat>1.645)

5 percent ** (*t*-stat>1.960), and

1 percent * (*t*-stat>2.576).

Table 6: Alternative DDD Estimates of the Impact of Anti-discrimination Laws on $\ln(\text{Earnings})$ – Conditional on Full-time Employment

Variable	Age 25-59		Urban Only		Regional Controls	
	Men	Women	Men	Women	Men	Women
<i>Hours Worked</i>	0.0137*** 28.25	0.0205*** 30.18	0.0148*** 28.18	0.0209*** 27.65	0.0140*** 17.35	0.0207*** 19.73
<i>Weeks Worked</i>	0.0333*** 81.59	0.0343*** 83.83	0.0336*** 76.87	0.0342*** 73.49	0.0331*** 48.29	0.0340*** 54.18
<i>Age</i>	0.0631*** 49.44	0.0438*** 32.71	0.0702*** 61.68	0.0568*** 49.10	0.0683*** 40.88	0.0539*** 43.76
<i>Age Squared</i>	-0.0006*** -40.43	-0.0005*** -32.17	-0.0007*** -54.63	-0.0006*** -45.43	-0.0007*** -37.25	-0.0006*** -44.68
<i>Head of Household</i>	0.0910*** 26.86	0.0246*** 8.93	0.0885*** 22.42	0.0269*** 9.28	0.0884*** 17.37	0.0228*** 6.14
<i>Presence of Children</i>	0.0611*** 18.58	-0.0208*** -5.49	0.0590*** 15.70	-0.0206*** -4.97	0.0588*** 15.30	-0.0188*** -3.19
<i>High School Degree</i>	0.1221*** 14.07	0.0734*** 4.86	0.1113*** 11.17	0.0806*** 4.74	0.1177*** 8.99	0.0697*** 2.88
<i>Some College</i>	0.2324*** 20.88	0.2194*** 13.55	0.2273*** 17.38	0.2230*** 12.32	0.2273*** 14.04	0.2116*** 8.31
<i>College Degree</i>	0.4965*** 32.91	0.5083*** 26.26	0.5024*** 29.83	0.5123*** 23.65	0.4942*** 24.22	0.5031*** 18.49
<i>Post College Degree</i>	0.7471*** 31.02	0.7727*** 32.06	0.7561*** 28.60	0.7739*** 29.26	0.7538*** 21.20	0.7754*** 21.27
<i>Disabled</i>	-0.1035*** -14.99	-0.0790*** -11.31	-0.1032*** -13.88	-0.0828*** -10.56	-0.1019*** -8.50	-0.0787*** -6.57
<i>Speaks English</i>	0.3587*** 20.25	0.3580*** 16.47	0.3580*** 21.16	0.3665*** 17.95	0.3486*** 17.93	0.3388*** 13.57
<i>White</i>	0.1810*** 29.58	0.0797*** 17.00	0.1819*** 27.34	0.0755*** 16.32	0.1688*** 23.87	0.0662*** 8.93
<i>Resides in Metro Area</i>	0.1319*** 21.26	0.1592*** 23.93			0.1403*** 17.35	0.1731*** 27.69
<i>Unearned Real Income</i>	0.0153*** 26.47	0.0096*** 17.01	0.0164*** 28.68	0.0103*** 19.28	0.0146*** 19.26	0.0092*** 10.39
<i>In Same Sex Relationship (SS)</i>	-0.2660* -1.82	0.0793 1.18	-0.1927 -1.34	0.0013 0.01	-0.2573*** -7.12	-0.0494 -1.03
<i>State ADL</i>					0.0641** 2.38	0.0724*** 2.95
<i>ADL*SS</i>	0.0015 0.05	-0.0208 -0.61	0.0191 0.60	-0.0178 -0.60	0.0051 0.11	0.0383* 1.89
<i>Time Since Passed</i>	-0.0001 -1.27	0.0034*** 32.08	-0.0029*** -45.32	0.0019*** 21.56	0.0017 0.71	0.0032 1.39
<i>Time Since Passed*SS</i>	0.0061*** 2.99	-0.0033 -1.04	0.0067*** 2.89	-0.0065** -2.41	0.0024 0.72	-0.0036*** -2.88
<i>Local ADL</i>	0.0297* 1.77	0.0818*** 5.25	0.0212 1.26	0.0767*** 5.08	0.0339* 1.88	0.0755*** 4.61
<i>Local ADL*SS</i>	-0.0063 -0.25	-0.0107 -0.33	0.0092 0.40	-0.0032 -0.11	-0.0086 -0.30	0.0107 0.65
R^2	0.3759	0.3817	0.4011	0.3900	0.3871	0.3935
No. Observations	798506	549734	621172	429344	883566	614500

Notes: Samples include individuals that worker 27 weeks or more, and at least 30 hours per week only. Earnings are adjusted for inflation where 1990 is the base year. *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. *State* and *Time* dummies, as well as *State* and *Time* interactions, *State* and *SS*, and *Time* and *SS* interactions are included in order to account for time trends.

Level of significance: 10 percent *(*t*-stat>1.645)

5 percent **(*t*-stat>1.960), and

1 percent ***(*t*-stat>2.576).

Table 7: Inter-decennial Estimates of the Impact of Anti-discrimination Laws on Ln(Earnings) –Conditional on Full-time Employment

Variable	1990 Only		2000 Only		2010 Only	
	Men	Women	Men	Women	Men	Women
<i>Hours Worked</i>	0.0090*** 18.92	0.0147*** 13.51	0.0139*** 57.66	0.0199*** 48.67	0.0183*** 71.25	0.0255*** 60.43
<i>Weeks Worked</i>	0.0330*** 67.69	0.0346*** 59.81	0.0296*** 51.58	0.0310*** 90.03	0.0367*** 74.62	0.0373*** 44.77
<i>Age</i>	0.0763*** 57.43	0.0556*** 30.81	0.0608*** 39.87	0.0519*** 29.09	0.0734*** 54.58	0.0617*** 39.16
<i>Age Squared</i>	-0.0008*** -54.28	-0.0006*** -30.76	-0.0006*** -30.80	-0.0006*** -26.17	-0.0007*** -44.94	-0.0006*** -34.26
<i>Head of Household</i>	0.1016*** 12.09	0.0352*** 7.67	0.1010*** 25.73	0.0271*** 9.62	0.0684*** 17.38	0.0234*** 5.08
<i>Presence of Children</i>	0.0468*** 9.20	-0.0520*** -7.85	0.0498*** 10.80	-0.0206*** -5.72	0.0707*** 12.35	0.0012 0.33
<i>High School Degree</i>	0.0735*** 9.72	-0.0266*** -3.72	0.1663*** 21.49	0.1727*** 17.96	0.1725*** 26.09	0.1852*** 15.38
<i>Some College</i>	0.1615*** 22.28	0.1028*** 9.79	0.2703*** 24.80	0.3151*** 26.45	0.3026*** 29.08	0.3293*** 23.00
<i>College Degree</i>	0.4160*** 31.95	0.3886*** 38.03	0.5106*** 30.15	0.5776*** 37.06	0.5949*** 39.54	0.6444*** 31.98
<i>Post College Degree</i>	0.5647*** 36.32	0.5658*** 36.26	0.7642*** 39.00	0.8327*** 49.59	0.9292*** 55.53	0.9668*** 59.12
<i>Disabled</i>	-0.1776*** -25.89	-0.1222*** -10.52	-0.0568*** -13.05	-0.0383*** -7.03	-0.1529*** -33.31	-0.1376*** -16.42
<i>Speaks English</i>	0.3325*** 16.25	0.2876*** 8.39	0.2709*** 13.36	0.2345*** 7.99	0.3436*** 19.89	0.3464*** 14.76
<i>White</i>	0.1891*** 18.59	0.0627*** 4.87	0.1629*** 21.30	0.0599*** 8.57	0.1683*** 16.51	0.0678*** 6.68
<i>Resides in Metro Area</i>	0.1733*** 8.78	0.1916*** 8.74	0.1526*** 13.11	0.1811*** 15.24	0.1307*** 10.50	0.1743*** 14.23
<i>Unearned Real Income</i>	0.0181*** 29.47	0.0151*** 17.49	0.0166*** 19.05	0.0094*** 15.95	0.0124*** 11.40	0.0063*** 5.28
<i>In Same Sex Relationship (SS)</i>	-0.1386*** -3.71	-0.0204 -0.76	-0.1006*** -5.50	-0.0018 -0.12	-0.1305*** -4.42	0.0316* 1.70
<i>State ADL</i>	-0.2265*** -14.53	-0.3239*** -22.68	0.1280*** 3.31	0.1561*** 2.89	0.0799*** 3.23	0.0956*** 3.83
<i>ADL*SS</i>	-0.0237 -0.56	-0.5349*** -16.84	0.1193 1.60	0.1205*** 3.45	0.0748 1.28	0.0096 0.34
<i>Time Since Passed</i>	0.0248*** 33.99	0.0348*** 26.94	-0.0034 -0.78	-0.0033 -0.47	0.0028 1.21	0.0041 1.52
<i>Time Since Passed*SS</i>	-0.0136*** -16.21	0.0358*** 32.50	-0.0052 -1.48	-0.0153*** -3.46	0.0010 0.24	-0.0033** -2.28
<i>Local ADL</i>	0.0739*** 4.07	0.1255*** 5.79	0.0873** 2.44	0.1195*** 3.23	-0.0054 -0.20	0.0361* 1.92
<i>Local ADL*SS</i>	-0.0421 -0.88	0.0022 0.03	0.0335 0.67	0.0036 0.10	0.0916* 1.96	-0.0051 -0.13
R^2	0.3650	0.3388	0.3672	0.3829	0.4264	0.4281
No. Observations	270823	173712	302641	213234	310102	227554

Notes: Samples include individuals that worker 27 weeks or more, and at least 30 hours per week only. Earnings are adjusted for inflation where 1990 is the base year. *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. *State* and *Time* dummies, as well as *State* and *Time* interactions, *State* and *SS*, and *Time* and *SS* interactions are included in order to account for time trends. Level of significance: 10 percent *(*t*-stat>1.645), 5 percent **(*t*-stat>1.960), and 1 percent ***(*t*-stat>2.576).

with particular social, political, and economic environments. For instance, studies that only account for a single period (1990 or 2000) may conclude that the law enables changes in the relative earnings of women in same-sex partnerships, when in fact the earnings changes may be due to other factors that are captured by the time trends of the DDD approach.¹⁸

In all specifications, ADLs implemented at the local level are associated with increases in overall earnings. Yet, the results show no statistical evidence supporting a difference in the relative earnings of individuals in same-sex partnerships (*Local ADL*SS*). This suggests that either ADLs are implemented in localities where local labor markets are particularly strong, or that there is little evidence of discrimination in localities without laws due to the reluctance of these individuals to reveal their sexual orientation. Overall, there is insufficient evidence to support the hypothesis that the relative earnings of homosexuals (compared to heterosexuals) are lower in ADL places.

Finally, we look at the relative earnings of homosexuals by using two different control groups (married and unmarried heterosexual couples separately). The literature has found wage differentials between married and unmarried heterosexuals. Although we lack information on the potential marriage outcomes for individuals in same-sex partnerships, we analyze whether the results above can be partly driven by differences in marital status.

We find no statistical difference when we compare the earnings of homosexual and heterosexual unmarried men. Although determining the correct comparison group is difficult, our results suggest that part of the difference in annual earnings found in previous studies of gay men may be partly due to the marriage premium (see Table 8). In terms of the ADL, we find that it increases the relative earnings of gay men to both unmarried and married heterosexual men over time. These results suggest two possibilities for gay men in particular. If men in same-sex partnerships are more similar in unobservable characteristics to married heterosexuals, anti-discriminatory laws are helping to close the earnings gap over time. This is desirable if the source of the marriage premium is firms that discriminate by using marriage as a signal of heterosexuality. However, the labor market may be distorted by the ADL if marriage is used as a signal of productivity by firms. In this case, a desirable labor market policy would be to allow homosexuals to marry, which would enable them to send the same productivity signals as heterosexuals.

If unmarried but coupled heterosexuals are the appropriate reference group, the law may have acted to unnecessarily increase the wages of gay men (unnecessary because all other things equal, we do not observe a statistically significant gap between these groups). If instead it is believed that gay men are more similar to married heterosexual men, the passage of an ADL may act as intended. However, if gay men are deemed more similar to unmarried heterosexual men, the law creates distortionary labor market effects. These results highlight the need to identify the true source of income disparities. This implies being able to find out what share of individuals in same-sex partnerships would marry if allowed. There seems to be little difference from enactment of the laws on labor market outcomes between homosexual women and their heterosexual married and unmarried equivalents.

¹⁸ As mentioned before, there are states with ADLs that cover public-sector employees, but none that cover private-sector ones. ADLs still may affect the private sector since gays have an alternative between private and public sector employment when they face discrimination in the one but not the other. We run additional specifications including controls for public-sector ADLs. The results are qualitatively similar and are available from the authors upon request.

Table 8: DDD Group Estimates of the Impact of Anti-discrimination Laws on Ln(*Earnings*) – Conditional on Full-Time Employment

Variable	Married Individuals as Reference		Cohabiting Heterosexuals as Reference	
	Men	Women	Men	Women
<i>Hours Worked</i>	0.0136*** 27.70	0.0207*** 28.66	0.0159*** 35.83	0.0203*** 27.26
<i>Weeks Worked</i>	0.0327*** 82.58	0.0339*** 83.17	0.0330*** 71.75	0.0340*** 55.28
<i>Age</i>	0.0674*** 66.41	0.0485*** 46.79	0.0659*** 33.13	0.0643*** 34.95
<i>Age Squared</i>	-0.0007*** -58.34	-0.0005*** -42.39	-0.0007*** -26.92	-0.0007*** -28.47
<i>Head of Household</i>	0.0758*** 24.61	0.0289*** 9.80	0.1239*** 24.06	0.0561*** 10.75
<i>Presence of Children</i>	0.0371*** 11.56	-0.0292*** -7.68	-0.0258*** -3.83	-0.0683*** -9.58
<i>High School Degree</i>	0.1188*** 13.59	0.0663*** 4.56	0.1161*** 18.42	0.1116*** 8.83
<i>Some College</i>	0.2270*** 20.42	0.2062*** 13.10	0.1897*** 19.92	0.2279*** 16.07
<i>College Degree</i>	0.4907*** 33.48	0.4931*** 26.26	0.4239*** 29.34	0.4950*** 29.32
<i>Post College Degree</i>	0.7424*** 31.44	0.7595*** 32.73	0.6873*** 24.73	0.7349*** 29.97
<i>Disabled</i>	-0.1012*** -15.40	-0.0767*** -11.07	-0.0908*** -7.98	-0.0908*** -8.32
<i>Speaks English</i>	0.3627*** 21.02	0.3501*** 16.38	0.2692*** 11.19	0.3492*** 11.98
<i>White</i>	0.1798*** 28.98	0.0745*** 15.89	0.1381*** 20.97	0.0718*** 10.54
<i>Resides in Metro Area</i>	0.1334*** 21.99	0.1590*** 24.07	0.0934*** 11.47	0.1389*** 16.91
<i>Unearned Real Income</i>	0.0143*** 25.31	0.0101*** 18.57	0.0111*** 9.25	0.0042*** 4.57
<i>In Same Sex Relationship (SS)</i>	-0.2577** -2.24	0.0067 0.12	-0.1050 -0.89	0.0903 1.12
<i>ADL*SS</i>	0.0084 0.28	-0.0145 -0.49	0.0327 1.04	0.0038 0.11
<i>Time Since Passed</i>	-0.0013*** -17.00	0.0032*** 34.36	0.0050*** 24.70	0.0024*** 9.63
<i>Time Since Passed*SS</i>	0.0078*** 4.00	-0.0039 -1.45	0.0050** 2.27	-0.0046 -1.59
<i>Local ADL</i>	0.0300* 1.91	0.0839*** 5.92	0.0457** 2.05	0.0757*** 4.07
<i>Local ADL*SS</i>	0.0031 0.14	-0.0026 -0.08	0.0032 0.14	0.0094 0.31
<i>R²</i>	0.3738	0.3915	0.4029	0.4513
<i>No. Observations</i>	808603	548931	74963	65569

Notes: Samples include individuals that worked 27 weeks or more, and at least 30 hours per week only. Earnings are adjusted inflation where 1990 is the base year. *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. *State* and *Time* dummies, as well as *State* and *Time* interactions, *State* and *SS*, and *Time* and *SS* interactions are included in order to account for time trends.

Level of significance: 10 percent * (t-stat>1.645), 5 percent ** (t-stat>1.960), and 1 percent *** (t-stat>2.576).

5. CONCLUSION

The influence, and potential influence, of anti-discriminatory laws (ADLs) on the basis of sexual orientation have a rising consideration in the literature. Though other studies have analyzed the effect of these laws in the labor market, we are the first to apply a difference-in-difference-in-difference approach. We find evidence of relative lower earnings for gay men with evidence that the difference may be partly driven by the marriage premium. Relative to married heterosexual men, gay men earn less and, over time, enacted ADLs appear to reduce this gap. Relative to unmarried but coupled heterosexual men, gay men experience similar levels of earnings. Both groups, gay men and unmarried heterosexual men, experience an increase in wages over time after passage of an ADL.

We find do not find evidence that women in same-sex partnerships experience different earnings than their unmarried, coupled and married heterosexual female counterparts. The effect of the law is to increase overall female earnings over time, but the law does not have a statistically significant impact on the relative wages over time. 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 specify the impact of the laws in the state of work rather than the state of residence as previous research has done.

Our results highlight two important policy issues. Statistically, we find mixed evidence with regards to the efficacy of the law to help equalize the wages of gays and lesbians with respect to their heterosexual counterparts. Although we find no statistical differences in the relative wages of homosexuals in states with and without ADLs, we observe statistical differences over time. The lack of differences, however, may be due to the lack of discrimination in states with ADLs. Our results suggest that the observed equalization of earnings may not be due to increases in earnings of those starting with lower wages. Indeed, total earnings those who start with higher wages decrease as a result of the law. Furthermore, the marriage premium seems to contribute to the wage gap between homosexual and heterosexual men. The data, however, do not allow us to identify the source of this contribution. It may be due to unobserved differences in productivity, signaling of sexual orientation, or signaling of productivity. This raises concerns about the efficacy of ADLs in eliminating differences in labor market outcomes between homosexuals and heterosexuals. Policymakers may want to consider alternative solutions.

The increase number of states legalizing same-sex marriages should help us find out whether the differences observed today are indeed due to discrimination or marriage. If marriage is a productivity signal, the ability of only some groups to signal appropriately may result in labor market adjustments. If, instead, marriage is simply a characteristic of productive individuals, we may not see changes in relative earnings, granting opportunities for future research on this subject.

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