

EMPIRICAL ESSAYS ON WAGE DETERMINATION AND MOBILITY

BY

JOSEPH LARON KIRBY

A Dissertation Submitted to the Graduate School at Middle Tennessee State
University in Partial Fulfillment of the Requirements for the Degree Doctor of
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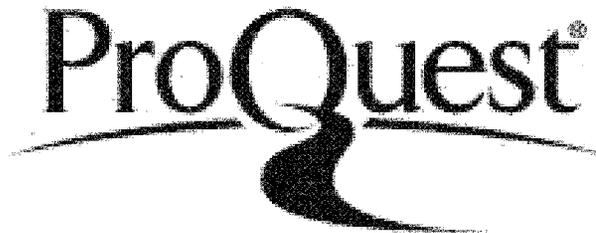


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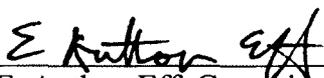
APPROVED BY:



Dr. Adam Rennhoff, Committee Chair



Dr. Charles Baum, Committee Member



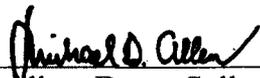
Dr. E. Anthon Eff, Committee Member



Dr. Mark F. Owens, Committee Member



Dr. Charles Baum, Department Chair, Economics and Finance



Dr. Michael Allen, Dean, College of Graduate Studies

DEDICATED TO MY WONDERFUL PARENTS, LARRY AND REBECCA

KIRBY

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ABSTRACT

This dissertation consists of three essays on labor force outcomes that result from implementation of statewide policies. The first essay, “Division of Labor and Marital Institutions: Evidence from Same-Sex Marriage”, tests Becker’s theory on household division of labor and wages with regards to individuals in same-sex partnerships relative to those in heterosexual partnerships. Results indicate that same-sex couples who identify as married have wage differentials similar to those of a heterosexual married couple. Married gay heads of household receive a wage premium relative to unmarried gay heads of household while married gay partners receive a wage penalty relative to unmarried gay partners. Evidence also suggests greater division of labor in married same-sex household compared to unmarried same-sex holds. The second essay, “Legal Protections and Marital Investment”, tests the impact of legal recognition of same-sex partnerships. Results suggest same-sex marriage and civil unions increase the wage differentials of married same-sex partnerships, while domestic partnerships result in wage penalties for married homosexuals. The third essay, “Non-Discrimination Laws, Mobility, and Labor Outcomes”, tests the labor force outcomes for heterosexuals and homosexuals in states with laws prohibiting discrimination based on sexual orientation. Results do not provide significant evidence that gay men and women have better labor force outcomes in states with non-discrimination laws. It appears that laws designed to improve the labor force outcomes of homosexuals do not have much significant impact while laws targeting non-pecuniary aspects of their lives have significant results on wages.

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CHAPTER I

INTRODUCTION

This dissertation consists of three essays that examine the impact of state laws changes that target the homosexual population on labor force outcomes. These law changes include legal recognition of same-sex partnerships and laws that prohibit discrimination in employment based on sexual orientation. Each essay contributes to the existing literature on the labor force outcomes of gay men and women. The first essay examines the wages and division of labor of married same-sex couples relative to unmarried same-sex couples. The second essay explores the effects legal recognition of same-sex partnerships have on the wages of married and unmarried gay households. This essay also controls for interstate mobility as laws providing marriage-like rights and responsibilities to same-sex couples will likely encourage some couples to move. The third essay examines the effects laws prohibiting discrimination against homosexuals in employment have on a variety of labor force outcomes – labor force participation, unemployment, hours worked, and wages. This essay also considers the impacts non-discrimination laws will have on the mobility of homosexual individuals which may confound results if not considered. The effects non-discrimination laws may have on single individuals and heterosexual couples are also considered.

The first essay empirically examines Becker's (1991) theory of the division of labor and the associated impact on wages with regards to homosexual individuals. His theory posits that there will be less division of labor in same-sex households relative to heterosexual households. His theory also suggests that unmarried partnerships will have less division of labor relative to married households, partly because the costs of dissolution are lower. I estimate the log wages of partnered individuals (heterosexual

marriage, heterosexual unmarried partnerships, homosexual marriages, and homosexual unmarried partnerships) to test for wage differentials between the different types of relationships. To avoid the differing effects on heads of households and their partners, analysis is conducted separately for these individuals. Using pooled, cross-sectional data from the 1990 and 2000 Censuses as well as the 2001 through 2009 American Community Survey, I find the wage differentials of same-sex couples are similar to those of heterosexual couples. Married heads of household in same-sex relationships tend to earn a wage premium relative to their unmarried counterparts while married partners in same-sex relationships tend to earn a wage penalty relative to their unmarried counterparts. Tests of household's division of labor suggest that married same-sex households specialize more than unmarried same-sex households. While the wage differentials and estimates of specialization are not as large as in married, heterosexual households, there is evidence to suggest same-sex marriages do have some similarities to heterosexual marriage with regards to these labor force factors.

The second essay continues the analysis of outcomes for married and unmarried same-sex couples. In this essay, I explore the effect legal recognition of same-sex partnerships has on wage differentials of married and unmarried same-sex couples. Analysis continues to be performed separately for heads of households and their partners. Using the same dataset employed in the first essay, I find considerable differences in how different forms of legal recognition affect the wages of married and unmarried homosexuals. Results indicate that same-sex marriage laws and civil unions result in higher wages for married heads of households and lower wages for married partners.

This effect seems to be greater for male same-sex couples in same-sex marriage states and greater for females in civil union states. There is some evidence that wages are lower for unmarried homosexuals in states with same-sex marriage or civil unions. Wages for married homosexuals are estimated to be considerably lower in states with domestic partnership laws. In these states, there is evidence wages may be higher for unmarried homosexuals. Because these laws may encourage interstate migration of homosexuals, this is controlled for in the analysis. For gay men, there is no evidence to suggest the non-pecuniary benefits of being able to engage in a legally recognized same-sex partnership outweigh the potential pecuniary benefits that are expected to accrue to individuals that invest in mobility. However, married lesbians who move are estimated to have lower earnings than unmarried lesbians who have moved. A potential explanation for this is that lesbians are willing to move to enjoy the non-pecuniary benefits of legally recognized same-sex partnerships.

The third essay expands on the previous work of Leppel (2009) and Gates (2009b) to test the effects of non-discrimination laws based on sexual orientation on labor force outcomes. This essay extends the time frame examined to take into account a greater number of states with non-discrimination laws and increased acceptance of homosexuality. I also include analysis that controls for the likelihood non-discrimination laws may encourage migration into states with such laws. After expanding the time frame and controlling for mobility, results do not suggest there are significant changes in labor force outcomes for gay men and women in states with non-discrimination laws.

Gay men and women who move into states with non-discrimination laws do not appear to have better labor market outcomes than those who have lived in states without these laws.

CHAPTER II

LITERATURE REVIEW

The first two essays in this dissertation test Becker's (1991) theories regarding division of labor in married and unmarried households as they relate to same-sex couples. His theory postulates that in a two-person household, one member will invest relatively more in market human capital, spending more time in workplace production and the other member will invest relatively more in household human capital, allocating more time to household production. Comparative advantages that exist between the sexes encourage this division of labor, with men focusing on workplace production and women focusing on household production. Complementarity in the production of certain household "commodities" will reduce the intra-household division of labor while increasing efficiency. Larger comparative advantages and lower complementarities will result in greater division of labor within the household. Becker (1991) additionally argues that efficient marriage markets will "maximize the aggregate output of household commodities." Partners will match so that household output is maximized. This assortative mating can be positive and/or negative depending upon the traits considered.¹ Imperfect information may result in poor matches that do not maximize household output. In these cases, mutual consent for divorce will result if both partners would be better off. If dissolution and new-partner search costs are low, the gains from divorce would be greater and both members of a household would be more willing to end the marriage. This theory of the costs of dissolution shows the importance that institutions play in wage returns to marriage. Poor matches coupled with high costs of dissolution may result in lower wage premiums for married men.

¹ Becker notes that simple correlations suggest that spouses sort positively into marriage based on age, education, religion, race, and many other factors and negatively based on wage.

Other research has attempted to explain wage differentials of married men and women from a different point of view; marriage does not increase productivity due to increased specialization but employer preference for married males and distaste for married females.² Under this explanation marriage may signal productive characteristics that in turn result in higher wages for married men and lower wages for married women. Some of the proposed characteristics are labor force attachment, work ethic, responsibility, and stability. It is also possible that marriage proxies productive characteristics that are observable to the employer but not the researcher.

Becker notes that homosexual individuals may have comparative advantages that deviate from the norm for their gender (i.e. lesbian women may be more oriented towards workplace production than household production, vice versa for gay men). Because these orientations are more likely to emerge or be recognized after early childhood or even later, early investments in human capital are less likely to complement biology. The individual would be less strongly oriented toward either male or female conventional specialization and less division of labor within a same-sex household. Homosexual households may not be able to benefit to the same degree as heterosexual households from complementarity, reducing household efficiency and the production of marital-specific capital. Becker argues that intra-household factors common to same-sex couples – less investment in marital-specific capital, less extensive division of labor, and lack of legal recognition of partnerships – increase the likelihood of dissolution. Dissolution of partnerships may also be more common among same-sex couples because these

² See Bartlett and Callahan (1984), Korenman and Neumark (1991), and Gray (1997) for examples.

relationships are more likely to result in poorer matches. Social stigma regarding homosexual relationships and a small pool of potential partners reduces the probability a strong match will result.

It may not be necessary for homosexual unions to have a higher frequency of dissolution. If employers perceive these relationships to be less stable, the signal these relationships send is weaker than that of traditional marriage. If employers have a preference for married men, gay men will not be able to take advantage of this. Legal recognition of a homosexual partnership may improve the signal that homosexuals give employers, whether it is stability or labor force attachment. These signals are potentially different based on the varying types of legal same-sex partnerships (full marriages, civil unions, and domestic partnerships). Legally recognized same-sex partnerships also provide a reliable signal of sexual orientation. Discriminating employers would not need to speculate whether these employees are gay or lesbian, resulting in an unintended consequence of lower levels of employment and/or wages for married homosexuals.

Recent literature has begun to examine whether gays and lesbians receive any form of wage differential relative to heterosexuals. Most of the evidence suggests that gay men receive a wage penalty compared to straight men, especially married men, and lesbian women receive a wage premium relative to heterosexual women, especially married women. Most recent literature has attempted to explain the role discrimination plays in these wage differentials. Badgett (1995) was the first to use a nationally representative sample to test for wage differentials for gay men and lesbian women. She

found that gay men earned a significant wage penalty relative to heterosexual men.³ Her hypothesis is that wage differential was attributable to discrimination. It is not uncommon in the literature for identifiable wage differentials for gay men to be attributed to discrimination. Further research into the field has continued to show that wage penalties for gay men though most identify a wage premium for lesbian women.⁴ As a further test to the discrimination hypothesis, Leppel (2009) estimated that anti-discrimination laws have increased unemployment levels of homosexuals, hypothesizing that increased migration of gays and lesbians have flooded these labor markets.

Other tests of the impact discrimination plays in wage differentials focus on differences in human capital and occupation. Becker (1991) argues that homosexuals have biological predispositions more closely aligned to those of the opposite sex (i.e. gay men may enter more nurturing careers while lesbian women may enter more physically demanding careers). His theory of biological deviation from gender norms may in part explain the predominance of gay men in female dominated occupations and lesbian women in traditionally male jobs. Antecol, Jong, and Steinberger (2008) test the role these differences play on the wage differentials of gay men and lesbian women. Using Blinder-Oaxaca and DiNardo-Fortin-Lemieux decompositions, they find that wage premiums are attributable to human capital accumulation. These results also suggest that wage penalties are mostly the results of unobservable. Allegretto and Arthur (2001) and Carpenter (2004) find that the wage premium received by married men is responsible for

³ She also noted a negative coefficient for women identified as lesbians, but this difference was not significantly different from zero

⁴ See Blandford (2003), Clain and Leppel (2001), and Cushings-Daniel and Yeung (2009) for other studies that use larger or different data sets to test the wage differentials for gays and lesbians. Black et al. (2007b) has a good review of the literature on homosexual wage differentials.

the differences in household incomes of heterosexual married couples and partnered homosexual male couples. If access to marriage – or a comparable institution – is available to partnered homosexuals, a marriage premium may be available to these couples as well.

Jepsen (2005) and Lefgren and McIntyre (2006) show that wives' education are correlated with higher earnings for husbands; Zavodny (2008) shows this is also true for partnered gay men. According to Becker's theory, increases in one partner's household productivity should result in an increase the other partner's workplace productivity. Zavodny (2008) tests if selection or specialization could potentially explain the wage penalty for gay men.⁵ She finds selection explains the wage premium cohabitating gay men receive relative to single straight men, but only part of the wage penalty cohabitating gay men receive relative to married men. She finds no evidence of specialization using 2000 Census data, suggesting this may be a result of not parsing the differing impacts on both members.

There is limited empirical and theoretical work on whether legal recognition of homosexual partnerships impacts the wages of gays and/or lesbians. Carpenter and Gates (2008) examine the observable differences of gays and lesbians in California who are single, cohabitating, and officially registered domestic partners, though do not address wage differentials. Their findings indicate that white and highly educated gay men and women are more likely to be partnered than homosexuals in other demographics. They

⁵ Jepsen and Jepsen (2002, 2006) and Carpenter and Gates (2008) address assortative matching and selection in to cohabitation/partnership for homosexual couples. Korenman and Neumark (1991) and Ginther and Zavodny (2001) address the issue of specialization's impact on marriage premiums, though not for homosexual partnerships.

also note that lesbian women are more likely to be in registered domestic partnerships than gay men. Booth and Frank (2004) use a sample of British academics and find no difference in the earnings of partnered and non-partnered homosexuals and bisexuals. Lafrance, Warman, and Woolley (working paper, 2009) examine Canadian households and find that the wage differences between partnered and single homosexual individuals are not significantly different after controlling for work habits. Gates (2009a) discusses the American Community Survey's official profile of same-sex couples that consider themselves married (US Census Bureau, 2009) and notes the problem of miscoded heterosexual couples.

CHAPTER III

DIVISION OF LABOR AND MARITAL INSTITUTIONS: EVIDENCE FROM
SAME-SEX MARRIAGE

3.1 INTRODUCTION

Several studies examine whether married men and women earn different wages than their single and cohabitating counterparts. These studies are in consensus that married men earn a wage premium relative to unmarried men and married women earn a wage penalty to unmarried women. Becker (1991) theorizes that the wage differentials result from division of labor within the household. This essay examines the role specialization and marital institutions play in same-sex households. The division of labor among homosexual couples is examined relative to heterosexuals' intra-household labor decisions. Comparisons of division of labor are made between unmarried homosexual couples and married homosexual couples as well. This is the first study to address the wage differentials for homosexual individuals that consider themselves to be married. I also take into account legal recognition of same-sex partnerships and how their effects may vary for married and unmarried homosexuals. This essay exploits a rather recent trend in the literature of analyzing heads and partners separately. This also follows the example set by Antecol and Steinberger (forthcoming) and Oreffice (2011)¹ and should address any attenuation of results regarding specialization or wage premiums, as addressed in Zavodny (2008).

Results support Becker's theory of division of labor among same-sex household. Evidence suggests that specialization does not play as strong a role in the wage differentials of unmarried and married homosexuals as it does for heterosexual couples.

¹ I follow the Oreffice (2011) naming convention by referring to heads of households/householders as "heads" and spouses/unmarried partners as "partners". Antecol and Steinberger (forthcoming) separate based on primary and secondary earner status.

There is no strong evidence that specialization plays a consistent role in explaining wage differentials between married and unmarried homosexuals, point estimates suggest greater specialization in married same-sex households relative to unmarried same-sex households. Given this, evidence suggests that in homosexual unions married heads earn a wage premium and married partners earn a wage penalty. Legal marriage and civil unions tend to complement these finds while domestic partnerships tend to have negative results for married homosexuals and slightly positive results for unmarried homosexuals.

3.2 DATA AND METHODOLOGY

3.2.1 Data

I use the 5% Public Use Micro-data Sample from the 1990 and 2000 decennial U.S. Census data as well as ACS data from 2001 to 2009.² Both surveys are designed to be nationally representative and are conducted by the United States Census Bureau. The data used in this study was retrieved from the Minnesota Population Center at the University of Minnesota (www.ipums.org; Ruggles, et al. 2010). Census and ACS data give every household sampled a serial number so members of a household are easily identifiable. The surveys ask that the respondent be the owner or person whose name is on the mortgage/lease.³ The respondent – who is considered the head – provides

² The 1990 and 2000 Censuses sample 5% of the population, while the 2001 to 2004 ACS sampled approximately 0.4% of the population. This was expanded to a 1% sample in 2005. Both the decennial Census and the 2006 through 2009 ACS samples include individuals in group quarters. For consistency, all individuals in group quarters are eliminated from analysis. According to Minnesota Population center at the University of Minnesota (www.ipums.org) “Group quarters are largely institutions and other group living arrangements, such as rooming houses and military barracks” and are “generally sampled as individuals” (i.e. separate forms for each person). Persons living in a “household” are sampled as one unit (i.e. one form with responses for each person living in the unit).

³ If this criterion does not apply to anyone in the household, any adult living in the residence can fill out the form.

information for themselves and all other members of the household. This information includes, but is not limited to, relationship to head, gender/sex, and marital status. Only the head and their partner are used in the analysis. All individuals who are under the age of 16 as well as those who have sex or relationship status coding flags are eliminated from analysis. Coding flags indicate that a response was changed or provided by the Census Bureau. To avoid potential measurement error, I follow the common practice in this literature to drop observations with sex or relationship coding flags. I also eliminate households where neither the head nor his/her partner speaks English well⁴, households where more than one partner is identified and heterosexual households where one or both members have marital status flags. Previous literature also drops homosexual households with marital status coding flags. I discuss later how some same-sex couples in these households are identified as married same-sex couples.

Being considered married or in an unmarried partnership is based on relationship status responses. In a male-female household, the couple is considered married if one member is reported as a spouse. If a similar household reports one member as being an unmarried partner, then the couple would be considered a heterosexual unmarried partnership. When a same-sex household refers to one member as being an unmarried partner and neither partner has a marital status allocation flag then the household is considered to be a homosexual unmarried partnership. The Census and ACS do not report any member of a same-sex household as being a spouse.

⁴ Black, et al. (2007a) provides evidence that miscoding is more common when the head of household does not speak English well.

If the respondent identifies a person of the same sex as a spouse, the U.S. Census automatically recodes the couple's data. In the 1990 Census the sex of the person identified as the spouse was changed to reflect a heterosexual marriage. In the 2000 Census and ACS, the relationship status of the person identified as a spouse was changed to unmarried partner and their marital status was changed from married to single. In this case a relationship status flag is not generated but a marital status flag is generated, and a homosexual partnership can still be identified. The reason a relationship status flag is not generated is because the Defense of Marriage Act prohibits federal agencies from recognizing same-sex marriages. As discussed in Black et al. (2007a), recoding homosexual couples that identify as married as a heterosexual married couple causes very limited measurement error because of the small number of same-sex couples that identify as married. To capture some of the variance caused by the procedural change in recoding, heterosexual and homosexual unmarried partnerships are interacted with the year variables. This should also capture some of the variance caused by changing attitudes towards these types of relationships.

In the vast majority of cases, when married same-sex couples are recoded to reflect homosexual unmarried partnerships, no relationship status allocation flag is generated but a marital status allocation flag is. Using original census file data, O'Connell and Gooding (2006) note a high but imperfect correlation between the marital status flag and homosexual couples reporting to be married. Reported same-sex couples with marital status allocation flags could be either a heterosexual married couple where one person's sex

was miscoded⁵ or homosexual couples that want to be identified as married but who have their relationship status and marital status recoded. This confounds the identification of married same-sex couples and the potential impact of legal recognition of same-sex partnerships. It is expected the greatest impact of legal recognition will accrue to homosexual couples that identify as being married. Gates and Steinberger (2009) note that using responses from telephone and in-person interviews allows for the potential to identify married same-sex couples. Beginning with the 2005 ACS, the Census Bureau began reporting whether the respondent was interviewed over the phone or in person rather than returning the mailed survey. During a telephone or in-person interview, when a respondent identifies their spouse as someone of the same sex, the interviewer asks for confirmation of the spouse's gender. As before in these scenarios, a relationship status is changed to "unmarried partner" with no allocation flag generated and the marital status is changed from "married" to "single" with an allocation flag generated. Since the sex of the spouse is confirmed and the marital status flag is a strong signal that the couple intended to be identified as married, homosexual couples interviewed in-person or on the phone, in the 2005 and on ACS samples, with marital status allocation flags are highly likely to be married (or at least want to be identified as such). Gates and Steinberger (2009) actually refer to this group as a "confirmed" sample of same-sex married couples. While this does limit the identification of same-sex married couples to the 2005 through 2009 ACS samples, this is more information than has been previously used.

⁵ See O'Connell and Gooding (2006), Black, et al. (2007a), and Gates and Steinberger (2009) for the potential extent of this problem.

3.2.2. Model

The initial models examine wage differences based on relationship type. I use several different specifications that analyze individuals by relationship type and position within the using the following OLS regressions:

$$(1) \quad \log(\text{real annual wage}) = \alpha + \beta_1 * X + \beta_2 * R + \varepsilon$$

$$(2) \quad \log(\text{real annual wage}) = \alpha + \beta_1 * X + \beta_2 * R + \beta_3 * R * Partner + \varepsilon$$

The dependent variable is the log real annual wages⁶ for non-military, non-self-employed individuals between the ages of 18 and 65 that usually work 35 or more hours per week for 48 or more weeks per year. Models for males and females are estimated separately. X represents the set of control variables: age and its square, potential experience and its square, schooling, race, Hispanic identity, occupation, industry, place of birth, veteran status, English ability, and state-year interactions. In equation (1), variables of interest are denoted by R , a vector of indicator variables for whether a person is in a heterosexual unmarried partnership, a homosexual unmarried partnership, or a self-identified homosexual marriage. Equation (2) includes interactions between the indicator variable $Partner$, whether a person is identified as the partner in the relationship, and R to determine whether there are statistically significant wage differences between heads and partners based on relationship type.⁷ After performing this analysis, estimation is performed separately for male heads, male partners, female heads, and female partners. These models are identical to Equation (1) and take the form

⁶ Wages are set to the base year of 1999 using the Consumer Price Index correction provided in the University of Minnesota's IPUMS dataset.

⁷ This includes an interaction between the $Partner$ variable and a variable indicating the person is in a heterosexual marriage.

$$(3) \quad \log(\text{real annual wages}) = \alpha + \beta_1 * X + \beta_2 * R + \varepsilon$$

This approach eliminates potential differences in returns to productive characteristics between heads and partners. This method also allows for easier identification of wage differentials between married and unmarried homosexuals.

3.3 RESULTS

3.3.1 Wage Differentials by Relationship Type

My first estimates come from OLS regression models on the sample with separate analysis for males and females. Table 3.1 presents results comparing married heterosexuals to individuals in other types of relationship. Columns A through C present results for equation (1) through (3), respectively, for men in the sample and Columns D through F present the results for women in the sample. In line with previous research, married males earn a wage premium relative to men in other types of relationships (Column A) and lesbian women earn a wage premium to other women (Column D).

When partner interactions are included (Column B), estimates suggest that male partners earn less than comparable heads. Heterosexual marriage still maintains its wage advantage when considering only partners as the estimated wage penalty for married male partners is less negative than the wage penalties estimated for male partners in other types of relationships. This wage penalty puts estimated earnings for married heterosexual partners on par with the earnings of comparable gay heads. In this model there are no significant differences between the estimated wage penalties for married and unmarried gay heads or partners. This result suggests that gay men who claim to be

married do not have differing wage profiles than unmarried gay men, providing no evidence for increased specialization or returns to marital institutions.

Controlling for legal recognition and non-discrimination laws (Column C), there are noticeable changes in the parameter estimates for men in same-sex partnerships. Excluding these laws appears to downwardly bias estimates of wage penalties for married gay heads and unmarried gay partners while upwardly biasing estimated parameters for unmarried gay heads and married gay partners. Estimates suggest that married gay heads may actually earn a slight wage premium relative to married straight partners but statistical tests suggest these parameters are not significantly different. There is statistically significant evidence that married gay heads earn a wage premium relative to unmarried gay heads. The wage difference between additional wage penalties to gay partners is not significant but estimates suggest a wage penalty for married gay partners relative to their unmarried counterparts. If married gay heads earn wage premiums and married gay partners earn wage penalties, household wage differentials is similar to what is seen in traditional heterosexual marriages.

Columns D through F show results when the model is estimated for the female sample. Including partner interactions in the female model (Column E), indicates that female partners earn less than comparable female heads. There is evidence in this model that married lesbian heads do earn a higher wage than those who are not married. This result suggests that specialization or marital institutions may play a role in wage differentials between these two groups. While parameter estimates for married lesbian partners are more negative than unmarried lesbian partners, adding these parameters to

those for all lesbians indicates there is not statistical difference between their wages. When non-discrimination and marriage laws are included in the model (Column F), the parameter estimate for married lesbians increases noticeably. This indicates that excluding these variable results in a downward bias on the parameter estimate for this variable. The change in the parameter estimate for married lesbian partners results from the increase in the married lesbian parameter and the need to keep the estimate on par with unmarried lesbian partners, not an increase in their wage penalty.

The most obvious result from this analysis is that partners earn significantly less than comparable heads. To more accurately address this issue, models are estimated separately for heads and partner by sex (equation 4). The results from these models are presented in Table 3.2. I quickly reaffirm that gay men receive a wage penalty to married straight men and most lesbian women receive a wage premium to married straight women. For homosexuals, results suggest that married heads earn more than unmarried heads while married partners earn less than unmarried partners. These differences are statistically significant for all groups except gay male partners. In a traditional marriage with a male head and female partner, the head earns a wage premium and the partner earns a wage penalty. This same pattern is seen in both gay and lesbian households even without the benefits of marital institutions.

3.3.2 Legal Recognition of Same-Sex Partnerships and Wage Differentials

The previous models demonstrated the potential for marriage premiums and penalties in same-sex households. I now examine the wage differentials for homosexual

men and women that have the opportunity to marry or enter into a marriage-like partnership. Table 3.3 list dates when different forms of legal recognition of same-sex partnership went into effect. While there is significant variance in the rights and responsibilities granted by the different forms of same-sex legal recognition, it is reasonable to expect that a domestic partnership would have similar effects as same-sex marriage. To test if the general effect of legal recognition, relationship type is interacted with a variable that equals one (1) if the state has some form of legal recognition and zero (0) otherwise. Controls for a mate's productive characteristics are included to reduce the effect specialization has on the estimated results.⁸ Results for how wages of married and unmarried homosexuals are affected by legal recognition are presented in Table 3.4.

Estimates tend to point to wage penalty for married homosexuals. Point estimates are positive for unmarried homosexuals but only significant for unmarried female partners. One potential explanation for these results is that legal recognition may make it easier for employers to discriminate against people in same-sex couples. It could also reflect increased costs associated with having to pay for insurance or other benefits for a homosexual individual's partner. It could be that there are differences between domestic partnerships and other types of legal recognition, marriages and civil unions. Same-sex marriages and civil unions provided essentially the same rights and responsibilities as traditional marriages where domestic partnerships are more limited in scope.

To test this hypothesis, relationship types are interacted with variable that indicates the state has either same-sex marriage or civil unions laws. Also included in the

⁸ See Bardasi and Taylor (2008) and Zavodny (2008) for examples.

analysis is an interaction between relationship type and the domestic partnership variable. Results of this analysis are presented in Table 3.5. The results for married homosexuals are similar for men and women. In states with same-sex marriage or civil unions estimated returns are positive for heads and negative for partners. This is in-line with what would be expected in a traditional heterosexual marriage – the head of household receives a marriage premium while the spouse receives a marriage penalty. Point estimates tend to be negative for unmarried homosexuals in these states, though this is only statistically significant for male heads. The result may reflect a sentiment within the state that gay men and women who do not marry once the opportunity is available may not be as productive as their matrimonially inclined counterparts. It is also possible that the characteristics that make these individuals less likely to marry also make them less productive in the work force.

In domestic partnership states the results are somewhat reversed. The wages for most married homosexuals are estimated to be significantly lower in domestic partnership states while there is a little evidence wages for unmarried homosexuals may be higher in these states. Again the argument may be that domestic partnerships make it easier to discriminate against homosexuals or may increase the costs to employers in terms of benefits and these costs are passed onto the employees.

3.3.3 Division of Labor

Because specialization is a likely explanation for some of the difference between unmarried and married homosexuals' wages, controls for mate's characteristics have been

included in the above models. I included mate's real earnings in thousands of dollars as a measure of workplace productivity of the other household member. Negative results would suggest that specialization places a significant role in household division of labor while positive estimates would suggest positive assortative mating plays a predominate role. It is also possible to compare results across relationship types to see whether specialization is greater in one group relative to another. This would be the case if the estimate of one relationship type is significant lower than that of another relationship type. These parameter estimates, for married and cohabitating heterosexuals and homosexuals, are presented in Table 3.6.

In almost every case, parameter estimates are positive suggesting that positive assortative mating explains intra-household division of labor better than specialization. This does not eliminate the possibility that married households specialize more than unmarried households, as suggested by Becker. Point estimates for both heterosexual and homosexual married individuals are lower than those for their unmarried counterparts. In most cases the difference between married and unmarried individuals is statistically significant. Becker also argues that heterosexual households would specialize more than homosexual households because the cost of dissolving a marriage is substantially greater than dissolving an unmarried partnership. As a result investing in specialization is cheaper than investing in dissolution. If the parameter estimates are lower in married heterosexual households relative to married homosexual households, there would be more evidence to support Becker's theory. For married male heads and female partners, the parameter estimate for heterosexuals is significantly lower than the estimates for

homosexuals. This since a male head/female partner households is the traditional heterosexual marriage, it is expected specialization would be greater in these households. There is no statistical difference between male partners and female heads. Evidence seems to support a Becker's theory that married households will specialize more than unmarried households and heterosexual households will specialize more than homosexual households.

3.4 CONCLUSION

Some arguments for marital wage differentials are based on increased specialization and that the institution signals certain productive characteristics. Tests of these theories can also be applied to same-sex couples. If legal protections are ignored, there is not a great wage difference between unmarried and married homosexuals. Once non-discrimination laws and legal recognition of same-sex partnerships are considered, there is definite difference in the earning profiles of the two groups. This is true even in states that do not recognize same-sex partnerships. The heads of households in married same-sex couples tend to earn more than their unmarried counterparts while partners in same-sex marriages tend to earn less than their unmarried counterparts. This is similar to what we see in a traditional marriage, where the male head receives a wage premium and the female partner receives a wage penalty. Some evidence suggests a greater degree of specialization in married same-sex households compared to unmarried same-sex households.

The various forms of legal recognition of same-sex partnerships are not equal. Full marriages and civil unions reinforce the wage premiums and penalties of married heads and partners. Domestic partnerships do not provide the same results; it is usually the case that married homosexuals earn less in domestic partnership states while unmarried homosexuals may earn more. This might result from domestic partnerships being a signal of sexual orientation making it easier for employers to discriminate against these individuals.

While part of the wage differential is attributable to access to marital institutions, even married gays and lesbians who do not live in states that recognize same-sex partnerships have wages that differ relative to unmarried homosexuals. Becker (1991) argues that his theory of division of labor will not easily apply to same-sex couples because of the biological benefits between the sexes and complementary aspects of these differences that can result in increased investment in marital-specific capital, as well as other benefits. He also argues that reduced costs of dissolution of homosexual partnerships will result in less investment in division of labor within same-sex households. Using standard tests for intra-household specialization, results confirm increased division of labor in traditional heterosexual married households, in line with Becker's theory. Though the results are not significant, there may be estimates suggest that there is increased division of labor in married gay households. This is not the case for married lesbian households. There is not strong evidence to suggest legal recognition impacts specialization though domestic partnerships seem to result in less division of labor.

Table 3.1: Wage Differentials and Relationship Type

	Males			Females		
	A	B	C	D	E	F
Heterosexual Cohabitation	-0.1295 *** (0.0026)	-0.1048 *** (0.0027)	-0.1091 *** (0.0029)	-0.0406 *** (0.0026)	-0.0434 *** (0.0027)	-0.0427 *** (0.0029)
Homosexual Cohabitation	-0.0868 *** (0.0085)	-0.0547 *** (0.0089)	-0.0787 *** (0.0097)	0.0581 *** (0.0078)	0.0764 *** (0.0082)	0.0860 *** (0.0089)
Homosexual Marriages	-0.0786 *** (0.0136)	-0.0585 *** (0.0169)	-0.0376 * (0.0228)	0.0752 *** (0.0129)	0.1180 *** (0.0166)	0.1609 *** (0.0218)
Partners in Heterosexual Marriages		-0.0519 *** (0.0005)	-0.0540 *** (0.00007)		-0.0271 *** (0.0006)	-0.0254 *** (0.0007)
Partners in Heterosexual Cohabitation		-0.0972 *** (0.0016)	-0.0948 *** (0.0020)		-0.0285 *** (0.0016)	-0.0227 *** (0.0020)
Partners in Homosexual Cohabitation		-0.1012 *** (0.0058)	-0.0953 *** (0.0078)		-0.0733 *** (0.0054)	-0.0730 *** (0.0071)
Partners in Homosexual Marriages		-0.0894 *** (0.0214)	-0.1335 *** (0.0309)		-0.1191 *** (0.0212)	-0.1585 *** (0.0292)
Interactions for Partners	No	Yes	Yes	No	Yes	Yes
Law Variables Included	No	No	Yes	No	No	Yes
Adjusted R ²	0.4244	0.4259	0.4260	0.4254	0.4254	0.4260
Sample Size	4,909,175	4,909,175	4,909,175	3,094,845	3,094,845	3,094,845

Results are robust to how legal recognition is defined. Standard errors are in parentheses. Significance: *** 1%, ** 5%, * 10%.

Table 3.2: Wage Differentials for Heads and Partners Relative to Heterosexual Marriages

	Male Heads	Male Partners	Female Heads	Female Partners
Heterosexual Cohabitation	-0.0910 *** (0.0034)	-0.1646 *** (0.0050)	-0.0364 *** (0.0045)	-0.0468 *** (0.0037)
Homosexual Cohabitation	-0.0883 *** (0.0117)	-0.1013 *** (0.0160)	0.0757 *** (0.0135)	0.0415 *** (0.0111)
Homosexual Marriages	-0.0383 * (0.0227)	-0.1240 *** (0.0316)	0.1807 *** (0.0273)	0.0072 (0.0218)
Homosexual Cohabitation = Homosexual Marriage	No [5.76]	Yes [0.60]	No [16.89]	No [2.83]
Adjusted R ²	0.4141	0.4322	0.4467	0.4147
Sample Size	3,963,572	945,603	749,782	2,345,120

Results are robust to how legal recognition is defined. Standard errors are in parentheses. F-statistics are in brackets. Significance: *** 1%, ** 5%, * 10%.

Table 3.3: Legal Protections of Same-Sex Partnerships, by State: 1990-2009

	Marriage	Civil Union	Broad Domestic Partnership	Narrow Domestic Partnerships
Hawaii				1997
California				1999
Vermont		Jul. 1, 2000		
District of Columbia*				Oct. 1, 2002
Massachusetts	May 17, 2004			
Maine				Jul. 30, 2004
California			Jan. 1, 2005	
Connecticut		Oct. 1, 2005		
District of Columbia			Apr. 4, 2006	
New Jersey		Feb. 19, 2007		
New Hampshire		Jan. 1, 2008		
Oregon			Feb. 4, 2008	
Washington				Jul. 23, 2007
Washington			Jun. 12, 2008	
Connecticut**	Oct. 28, 2008			
Iowa	Apr. 27, 2009			
Vermont ***	Sep. 1, 2009			

* The District of Columbia first enacted domestic partnerships in June 1992, though recognition was prohibited by Act of Congress until fiscal year 2002.

** On October 1, 2010, all existing civil unions were automatically transformed into marriages in Connecticut and the state stopped providing civil unions.

*** While same-sex marriages were available in Vermont in 2009, the date they were first offered falls after the cutoff date of July 1.

Table 3.4: Wage Differentials in States with Legal Recognition of Same-Sex Partnerships

	Male Heads	Male Partners	Female Heads	Female Partners
Unmarried	0.0016	0.0123	0.0046	0.0329 ***
Homosexuals	(0.0115)	(0.0159)	(0.0142)	(0.0115)
Married	-0.1359 ***	0.0210	-0.0285	-0.1388 ***
Homosexuals	(0.0384)	(0.0545)	(0.0510)	(0.0414)

Standard errors are in parentheses. Significance: *** - 1%, ** - 5%, * - 10%.

Table 3.5: Wage Differentials in States with Legal Recognition of Sam-Sex Partnerships – Marriages and Civil Unions vs. Domestic Partnerships

	Male Heads	Male Partners	Female Heads	Female Partners
Panel A: Marriages and Civil Unions				
Unmarried	-0.0521 **	0.0256	-0.0210	-0.0309
Homosexuals	(0.0240)	(0.0322)	(0.0251)	(0.0200)
Married	0.0280	-0.0742	0.0766	-0.0429
Homosexuals	(0.0600)	(0.0764)	(0.0663)	(0.0550)
Panel B: Domestic Partnerships				
Unmarried	0.0094	0.0100	0.0112	0.0450 ***
Homosexuals	(0.0119)	(0.0165)	(0.0151)	(0.0123)
Married	-0.1945 ***	0.0630	-0.1008 *	-0.2017 ***
Homosexuals	(0.0418)	(0.0594)	(0.0586)	(0.0475)

Standard errors are in parentheses. Significance: *** - 1%, ** - 5%, * - 10%.

Table 3.6 Test of the Division of Labor – Parameter Estimates of Mate's Real Wage on Log Real Wages

	Male Heads	Male Partners	Female Heads	Female Partners
Married	0.0004 ***	0.0007 ***	0.0016 ***	0.0015 ***
Heterosexuals	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Unmarried	0.0030 ***	0.0030 ***	0.0028 ***	0.0021 ***
Heterosexuals	(0.0000)	(0.0001)	(0.0000)	(0.0000)
Unmarried	0.0016 ***	0.0014 ***	0.0112 ***	0.0022 ***
Homosexuals	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Married	0.0008 *	-0.0002	0.0010	0.0021 ***
Homosexuals	(0.0004)	(0.0005)	(0.0007)	(0.0004)

Standard errors are in parentheses. Significance: *** - 1%, ** - 5%, * - 10%.

CHAPTER IV

LEGAL PROTECTIONS AND MARITAL INVESTMENT

4.1 INTRODUCTION

For most couples there is very little difference in the rights and responsibilities that marriage provides depending on the state in which they reside. For the typical man and woman a marriage in Vermont provides the same benefits and obligations as a marriage in Wyoming. However this is not the case for couples of the same-sex. While it is possible for homosexual couples to claim to be married regardless of the state in which they live, the legal recognition of that relationship can vary widely across the country. As of 2009, twelve states provided some form of legal recognition for same-sex partners. These laws ranged from narrowly defined domestic partnerships to provision of full marriage rights. The variance in the marital institutions that are available to same-sex couples gives a way to test how the institution of marriage itself impacts the wage differentials of men and women.

The purpose of this essay is to estimate the effects different types of marital institutions have on the wages of gay men and women. I expand the literature by examining the impact these laws have on unmarried and married homosexuals. One potential estimation error would be differing results for heads of households and their partners. If a head receives a positive return while a partner receives a negative return this could result in statistically insignificant estimates. To prevent potential attenuation of results, heads of households – hereafter heads – and their mates/spouses/partners – hereafter partners – are estimated separately. I also control for potential increases in specialization or positive assortative matching by including controls for the significant other in the wage estimations.

4.2 DATA AND METHODOLOGY

4.2.1 Data

Statewide legal recognition of same-sex partnerships is a policy passed recently. I use information on what type of laws states enacted and when these laws went into effect to create variables on legal recognition. Table 4.1 presents the type of legal recognition that is provided by state and when recognition began for each state.

I use the 5% Public Use Micro-data Sample from the 1990 and 2000 decennial U.S. Census data as well as ACS data from 2001 to 2009.¹ Both surveys are designed to be nationally representative and are conducted by the United States Census Bureau. The data used in this study was retrieved from the Minnesota Population Center at the University of Minnesota (www.ipums.org; Ruggles, et al. 2010). Census and ACS data give every household sampled a serial number so members of a household are easily identifiable. The surveys ask that the respondent be the owner or person whose name is on the mortgage/lease.² The respondent – who is considered the head – provides information for themselves and all other members of the household. This information includes, but is not limited to, relationship to head, gender/sex, and marital status. Only the head and their partner are used in the analysis. All individuals who are under the age of 16 as well as those who have sex or relationship status coding flags are eliminated from the analysis. Coding flags indicate that a response was changed or provided by the

¹ The 1990 and 2000 Censuses sample 5% of the population, while the 2001 to 2004 ACS sampled approximately 0.4% of the population. This was expanded to a 1% sample in 2005. Both the decennial Census and the 2006 through 2009 ACS samples include individuals in group quarters. For consistency, all individuals in group quarters are eliminated from analysis.

² If this criterion does not apply to anyone in the household, then any adult living in the residence can fill out the form.

Census Bureau. To avoid potential measurement error, I follow the common practice in this literature to drop observations with sex or relationship coding flags. I also eliminate households where neither the head nor his or her partner speaks English well³, households where more than one partner is identified and heterosexual households where one or both members have marital status flags. Previous literature also drops homosexual households with marital status coding flags. I discuss later how some same-sex couples in these households are identified as married same-sex couples.

Being considered married or in an unmarried partnership is based on relationship status responses. In an opposite-sex household, the couple is considered married if one member is reported as a spouse. If a similar household reports one member as being an unmarried partner, then the couple would be considered a heterosexual unmarried partnership. When a same-sex household refers to one member as being an unmarried partner and neither partner has a marital status allocation flag, the household is considered to be a homosexual unmarried partnership. The Census and ACS do not report any member of a same-sex household as being a spouse.

If the respondent identifies a person of the same sex as a spouse, the U.S. Census automatically recodes the couple's data. In the 1990 Census the sex of the person identified as the spouse was changed to reflect a heterosexual marriage. In the 2000 Census and ACS, the relationship status of the person identified as a spouse was changed

³ Black, et al. (2007a) provides evidence that miscoding is more common when the head of household does not speak English well.

to unmarried partner and their marital status was changed from married to single.⁴ As discussed in Black et al. (2007a), recoding homosexual couples that identify as married as a heterosexual married couple causes very limited measurement error because of the small number of same-sex couples that identify as married. To capture some of the variance caused by the procedural change in recoding, heterosexual and homosexual unmarried partnerships are interacted with the year variables. This should also capture some of the variance caused by changing attitudes towards these types of relationships.

In the vast majority of cases, when married same-sex couples are recoded to reflect homosexual unmarried partnerships, no relationship status allocation flag is generated but a marital status allocation flag is.⁵ Reported same-sex couples with marital status allocation flags could be either a heterosexual married couple where one person's sex was miscoded⁶ or homosexual couples that want to be identified as married but who have their relationship status and marital status recoded. This confounds the identification of married same-sex couples and the potential impact of legal recognition of same-sex partnerships. It is expected the greatest impact of legal recognition will accrue to homosexual couples that identify as being married. Gates and Steinberger (2009) note that using responses from telephone and in-person interviews allows for the potential to identify married same-sex couples. Beginning with the 2005 ACS, the Census Bureau began reporting whether the respondent was interviewed over the phone

⁴ In this case a relationship status flag is not generated but a marital status flag is generated, and a homosexual partnership can still be identified. The reason a relationship status flag is not generated is because the Defense of Marriage Act prohibits federal agencies from recognizing same-sex marriages.

⁵ Using original census file data, O'Connell and Gooding (2006) note a high but imperfect correlation between the marital status flag and homosexual couples reporting to be married.

⁶ See O'Connell and Gooding (2006), Black, et al. (2007a), and Gates and Steinberger (2009) for the potential extent of this problem.

or in person rather than returning the mailed survey. During a telephone or in-person interview, when a respondent identifies their spouse as someone of the same sex, the interviewer asks for confirmation of the spouse's gender. As before in these scenarios, a relationship status is changed to "unmarried partner" with no allocation flag generated and the marital status is changed from "married" to "single" with an allocation flag generated. Since the sex of the spouse is confirmed and the marital status flag is a strong signal that the couple intended to be identified as married, homosexual couples interviewed in-person or on the phone, in the 2005 and on ACS samples, with marital status allocation flags are highly likely to be married (or at least want to be identified as such). Gates and Steinberger (2009) actually refer to this group as a "confirmed" sample of same-sex married couples. While this does limit the identification of same-sex married couples to the 2005 through 2009 ACS samples, this is more information than has been previously used.

4.2.2. Model

The initial models test wage differences based on relationship type, legal recognition, and the interaction between the two.

$$\log(\text{real annual wage}) = \alpha + \beta_1 * X + \beta_2 * R + \beta_3 * Law + \beta_4 * R * Law + \varepsilon$$

The dependent variable is the log real annual wages⁷ for non-military, non-self-employed individuals between the ages of 18 and 65 that usually work 35 or more hours per week for 48 or more weeks per year. Models for males and females are estimated separately. X represents the set of control variables: age and its square, potential experience and its

⁷ Wages are set to the base year of 1999 using the Consumer Price Index correction provided in the University of Minnesota's IPUMS dataset.

square, schooling, race, Hispanic identity, occupation, industry, place of birth, veteran status, English ability, and state-year interactions. R represents a set of indicator variables for whether a person is in a heterosexual unmarried partnership, a homosexual unmarried partnership, or a self-identified homosexual marriage. The control group is those in a heterosexual marriage. Law is an indicator variable for whether the state provides some form of legal recognition of same-sex partnerships. An interaction between R and Law creates a difference-in-difference approach to this model. This difference-in-difference approach is to control for the potential that high wage states are the states that provide legal recognition to same-sex couples. In the working paper, Kirby (working paper A) examines the effects legal recognition of same-sex partnerships has on wages. That analysis does not consider substantial differences in the effects of different forms of legal recognition, pooling different forms into broad categories. This essay expands on that analysis by examining the effects of the different forms of legal recognition separately. Consideration is also given to how these laws may affect division of labor within the household.

4.3 RESULTS

4.3.1 Legal Recognition of Same-Sex Partnerships and Wage Differentials, by Type of Legal Recognition

The initial model tests for differences between individuals who live in states with some form of legal recognition and those who do not. The estimates for unmarried and married gay men and women are presented in Table 4.2. The results suggest that there are wage penalties, sometimes large and significant, for married gay men and women in

states with same-sex marriage, civil unions, or domestic partnerships. If legal recognition makes discrimination easier then that could explain the wage penalties. Another explanation could be that there are differences between the different types of legal recognition and one form is seriously biasing the results for others.

I test the hypothesis that the wages of homosexuals are different in states with legal recognition of same-sex partnerships relative to states without such laws. This analysis begins by limiting the definition of legal recognition to only same-sex marriage. The definition is then expanded to include civil unions, broad domestic partnerships, and then all domestic partnerships. These results are presented in Table 4.3. There appears to be a stark difference in the wage differentials of married gay men and women in domestic partnership states and those in marriage and civil union states. Prior to the introduction of domestic partnerships into the definition of legal recognition, men in same-sex marriages have wages that appear similar to those of people in a traditional marriage – premiums for the head and penalties for the partner. There is evidence of wage premiums for both members of female same-sex marriages when a state provides protection. However, when domestic partnerships are included in the definition wage penalties are more commonly estimated.

Since domestic partnerships do not appear to provide similar wage differentials as full marriages and civil unions, I examine each type of legal recognition separately. Previously the model was estimated sixteen times, expanding the definition of legal recognition with each re-estimation for the four separate groups (male heads, male partners, female heads, and female partners). Now the model is estimated only four

times, once for each group. These regressions contain control variables for each type of legal recognition as well as interactions between each type of legal recognition and the different types of relationships: heterosexual cohabitation, homosexual cohabitation, and homosexual marriage. The estimates reflect wage differentials between homosexuals living in states with legal recognition of same-sex partnerships relative to those who do not. These results are presented in Table 4.4.

Again these results show wage differentials for gays and lesbians living in states with legal recognition relative to those that do not live in these types of states. In full marriage states, there is a wage premium estimated for married gay heads, though it is not significant, and a large and significant wage penalty estimated for married gay partners. This provides some evidence that married gay couples may have division of labor similar to heterosexual married couples. Unmarried gay heads in are still estimated to receive lower wages in marriage and civil union states. This result is not sufficient evidence to support the idea that not being married signals undesirable productive characteristics. The most striking results are the large and significant wage differentials for married gay men in broad domestic partnership states. Married gay heads receive a wage penalty while married gay partners earn a wage premium. This may reflect less division of labor in the household.

Results do not support the hypothesis that full marriage and civil unions can be used as signals for lesbians either. Unmarried lesbians in these states are estimated to receive wage penalties but these are only significant for unmarried lesbian partners in full marriage states. The difference in results for Negative coefficients are estimated for each

group of married lesbian in domestic partnership states, with most being significant. There is slight evidence that domestic partnerships benefit unmarried lesbians, potentially at the expense of married lesbians. Being in a domestic partnership sends a very strong signal about a person's sexual orientation making it easier for employers to discriminate against them. Lesbians not in a domestic partnership may opt not to reveal information regarding sexual orientation, protecting them from discrimination.

4.3.2 Tests of the Division of Labor

So far results have provided evidence that differences exist between types of marital institutions. The question remains whether these results are impacted by the division of labor within households. Results thus far suggest married homosexual heads earn higher wages than their unmarried counterparts while married homosexual partners earn less than comparable unmarried homosexual heads. For gay men these results are more pronounced in full marriage states. The results are more pronounced for lesbian women in both full marriage and civil union states, though statistical significance is greater in civil union states. Domestic partnerships however seem to counteract these wage differentials for married gay men and result in wage penalties for married lesbians.

A potential explanation for these results is differences in how various types of households split household and workplace responsibilities. To test for increased specialization, previous studies⁸ have included the education and predicted hours worked of person's mate (partner or spouse) into the wage equation. Including the mate's education is used to help control for positive assortative mating and productivity gains a

⁸ See Bardasi and Taylor (2008) and Zavodny (2008) for examples.

spouse's/partner's education may provide. The estimates on the mate's education, while not presented, suggest increasing positive returns which is in line with previous literature. Including the mate's hours of work is used to test for specialization as it is expected that as an individual's mate specializes more in workforce production the person will specialize more in household production, reducing their own work force productivity. This result could also be explained by an income effect. To control for the income effect, Tobit models are used to estimate the predicted hours a person's mate would work based on the mate's demographic and productive characteristics⁹. A mate's predicted hours of work are also interacted with the type of relationship. Negative results for a mate's predicted hours worked would indicate that specialization plays a larger role in explaining wage differentials while positive results would say that positive assortative matching plays a more important role. Comparing results between two types of relationship (e.g. heterosexual marriage and heterosexual cohabitation) will also explain if specialization plays a greater role in one type of household relative to the other. Because of the importance in children regarding specialization, the estimated models include variables for number of own children, number of own children under five years old, age of youngest own child in the household, and age of oldest own child in the household. The estimated parameters on mate's predicted hours of work are presented in Table 4.5.

For men positive assortative mating explains more of wages than does specialization. However, specialization explains a greater portion of wages for some

⁹ These include number and age of children, year, state of residence, age, potential experience, education, other real income, veteran status, race, Hispanic origin, and English ability.

groups than it does for others. The results of F-tests show that heterosexual married men almost always have higher levels of specialization than do other men. The only exception is that for male partners in heterosexual and homosexual partners. In this case the estimate on mate's predicted hours is smaller for heterosexual married partners than for homosexual married partners but the difference between the two is not statistically significant. This may be a result of the small sample size of married gay men. The small sample size may also explain the F-test results on the difference between estimates for married and unmarried gay men. The estimates are smaller for married gay heads and partners than their unmarried counterparts but this difference is not significant. There is some evidence of greater specialization in married gay households, though more information is needed to prove or disprove this result.

For women specialization appears to play a larger role in wage determination than it does for men, though it does not always dominate the impact of positive assortative mating. The only group for whom specialization clearly dominates positive assortative mating in wage determination is straight married women. Straight married women also appear to specialize more than any other group with one exception. While there is no statistically significant difference, married lesbian partners have a lower coefficient on mate's predicted hours than their straight counterparts. There is statistically significant evidence, however, to show that wages for married lesbian partners are impacted greater by specialization than their unmarried counterparts. There is an unexpected result for married lesbian heads; the estimate on mate's predicted hours is greater for married lesbian heads than for unmarried lesbian heads. This suggests that specialization has a

less important role for married lesbian heads. This might be an artifact of the small sample size or a result that needs further study to explain more fully. Whatever the potential reason, in this analysis the difference between the estimated coefficients for married and unmarried lesbian heads is not statistically different.

Table 4.5 presents information on the division of labor in all states. Most of these results suggest that married same-sex couples specialize more than cohabitating same-sex couples. Earlier results showed that legal recognition of same-sex partnerships result in significant wage differentials for married and unmarried homosexual couples. Part of these wages differentials may be explained by variances in specialization in these states. To test for these variances, I interact the type of legal recognition with mate's predicted hours worked by relationship type (mate's predicted hours worked * relationship type * legal recognition). The coefficient on each variable is the additional impact specialization (negative estimate) or positive assortative mating (positive estimate) has on each type of relationship in states with a given type of legal recognition. If full marriage rights increases specialization in same-sex households, the estimated coefficients for homosexual heads and partners should be negative. If domestic partnerships result in less specialization, the coefficients would be positive. The results from these estimates are presented in Table 4.6.

In states that provide same-sex marriages or civil unions, the wages of straight married men are affected more greatly by specialization while the wages of straight married women are influenced more by positive assortative mating in domestic partnership states. It is unlikely that legalizing same-sex partnerships caused this. It

seems more likely that this intra-household behavior existed before-hand in these states and may have even influenced the type of legal recognition that was enacted. The wages of unmarried heterosexuals that live in broad domestic partnership states seem to be more greatly influenced by positive assortative mating as well. Estimates suggest there tends to be increased specialization in gay and unmarried lesbian households that live in full marriage and civil union states and less specialization in domestic partnership states though few of these results are statistically significant. These results for married lesbian are inconsistent and not always reasonable.

4.3.3 Controlling for Increased Mobility

Another aspect of wage differentials relevant to this discussion is the mobility of gays and lesbians. The introduction of same-sex marriage and other forms of legal recognition can significantly impact the decision to move and where to move.¹⁰ If these laws are increasing the mobility of same-sex couples, there is likely some bias in the estimated wage differentials of homosexuals. It is typically expected that mobility is a form of human capital such that we expected those that invest in mobility to move to increase wages. With regards to legal recognition of same-sex marriage the results from movement however could be ambiguous. The non-pecuniary benefits movement may be sufficient for individuals to willingly accept lower wages. Movement is also costly however, so we might expect only those individuals with sufficient resources to invest in mobility. Those individuals with the resources may also be people with greater earning

¹⁰ Welfare migration is one area where laws changes are expected to impact the interstate movement of a specified group. Examples of this research include Brueckner (2000), Gelbach (2004), and McKinnish (2005).

potential. Here mobility is measured by whether the individual has moved to another state since birth. There are not enough observations to accurately estimate the impact of moving to states that recognize same-sex partnerships. The results for mobility are presented in Table 4.7.

The first row of results shows the wage differentials for all movers. For most groups, mobility does reflect an investment to increase wages; results are positive and significant for every group except female partners. Female partners may be moving based on a decision by their spouse and therefore less likely to see the significant returns to mobility that others may receive.

The next three rows show how these wage differentials vary by relationship type with the comparison group being people in heterosexual marriages. Unmarried straight men do not earn as large of a wage premium from mobility as married straight men. This is reflected in the negative and significant estimates for both unmarried straight male heads and partners. There is not significant evidence for additional benefits to mobility for gay men. The parameter estimates are positive but are relatively small in most cases. This might be a result of the ambiguous nature of mobility for homosexual individuals or the small sample size, or both. There is no ambiguity for married lesbians; there are large and significant estimates on the additional wage differentials these women receive from mobility. It is apparent the non-pecuniary benefits of mobility outweigh potential financial gains. The parameter estimates are so large that the wages of married lesbians would be higher if they chose not to move.

4.3 CONCLUSION

There is evidence that the wages of married gay men and women are significantly different from those of unmarried homosexuals. And this difference can be affected by whether a state provides legal recognition to a married same-sex couple. However, impact on wages is significantly different – for the married and unmarried, men and women – depending on the type of legal recognition a state provides. Marriages and civil unions seem to accentuate wage premiums and penalties for the heads and partners in a same-sex marriage. In states that have domestic partnerships, married gay men and women are estimated to suffer significant wage penalties. This affect could be that domestic partnerships make discrimination easier by identifying homosexuals. Another result could be the shifting of costs of increased benefits to married gay employees.

While there is not enough information to speak explicitly on mobility and same-sex marriage, I can examine if the non-pecuniary benefits of mobility tend to be greater for married homosexuals. Gay men, married and unmarried, do not tend to earn less or more than other people who move so it should not be assumed that the psychic benefits to marriage laws significantly outweigh pecuniary concerns for married gay men. However, married lesbians who have moved do earn less than other movers. This suggests they are moving more for aspects other than wages. A possible explanation for this is moving to states that recognize same-sex partnerships to enjoy the non-pecuniary benefits of that institution.

Further research is necessary to more adequately explain the wage differentials that are occurring. A larger data set that allows for the identification of more individuals who claim to be in same-sex marriages would add greater variety to the sample and allow

for more in-depth analysis of specialization and mobility. Better information regarding the non-wage forms of compensation, such as health insurance, would help identify if lower wages for married homosexuals in domestic partnership states are due to cost shifting.

Table 4.1: Legal Recognition of Same-Sex Partnerships by State: 1990 – 2009

	Marriage	Civil Union	Broad Domestic Partnership	Narrow Domestic Partnerships
Hawaii				1997
California				1999
Vermont		Jul. 1, 2000		
District of Columbia*				Oct. 1, 2002
Massachusetts	May 17, 2004			
Maine				Jul. 30, 2004
California			Jan. 1, 2005	
Connecticut		Oct. 1, 2005		
District of Columbia			Apr. 4, 2006	
New Jersey		Feb. 19, 2007		
New Hampshire		Jan. 1, 2008		
Oregon			Feb. 4, 2008	
Washington				Jul. 23, 2007
Washington			Jun. 12, 2008	
Connecticut**	Oct. 28, 2008			
Iowa	Apr. 27, 2009			
Vermont ***	Sep. 1, 2009			

* The District of Columbia first enacted domestic partnerships in June 1992, though recognition was prohibited by Act of Congress until fiscal year 2002.

** On October 1, 2010, all existing civil unions were automatically transformed into marriages in Connecticut and the state stopped providing civil unions.

*** While same-sex marriages were available in Vermont in 2009, the date they were first offered falls after the cutoff date of July 1.

Table 4.2: Wage Differentials of Married and Unmarried Homosexuals in States with Same-Sex Legal Recognition Relative to Those in States without Legal Recognition

	Male Heads	Male Partners	Female Heads	Female Partners
Unmarried	0.0016	0.0123	0.0046	0.0329 ***
Homosexuals	(0.0115)	(0.0159)	(0.0142)	(0.0115)
Married	-0.1359 ***	0.0210	-0.0285	-0.1388 ***
Homosexuals	(0.0384)	(0.0545)	(0.0510)	(0.0414)

Standard errors are in parentheses. Significance: *** - 1%, ** - 5%, * - 10%.

Table 4.3: Wage Differentials of Married and Unmarried Homosexuals in States with Same-Sex Legal Recognition, by Increasingly Broad Definitions of Legal Recognition

	Male Heads	Male Partners	Female Heads	Female Partners
<u>Panel A: Same-Sex Marriage</u>				
Unmarried Gay Heads	-0.0441 (0.0314)	-0.0060 (0.0434)	-0.0350 (0.0330)	-0.0667 ** (0.0131)
Married Gay Heads	0.1402 ** (0.0696)	-0.2358 ** (0.0931)	0.0769 (0.0780)	0.0624 (0.0723)
<u>Panel B: Same-Sex Marriage and Civil Unions</u>				
Unmarried Gay Partners	-0.0575 ** (0.0230)	0.0170 (0.0313)	-0.0315 (0.0244)	-0.0579 *** (0.0191)
Married Gay Partners	0.1120 ** (0.0566)	-0.1199 * (0.0707)	0.1348 ** (0.0616)	0.0286 (0.0511)
<u>Panel C: Same-Sex Marriage, Civil Unions, and Broad Domestic Partnerships</u>				
Unmarried Lesbian Heads	-0.0042 (0.0131)	0.0310 * (0.0918)	0.0074 (0.0165)	-0.0088 (0.0132)
Married Lesbian Heads	-0.1528 *** (0.0381)	0.0719 (0.0539)	0.0620 (0.0512)	-0.0357 (0.0412)
<u>Panel D: Same-Sex Marriage, Civil Unions, and All Domestic Partnerships</u>				
Unmarried Lesbian Partners	0.0012 (0.0113)	0.0099 (0.0159)	0.0046 (0.0143)	0.0222 (0.0114)
Married Lesbian Partners	-0.1302 *** (0.0376)	0.0138 (0.0546)	-0.0260 (0.0511)	-0.1408 *** (0.0408)

Standard errors are in parentheses. Significance: *** - 1%, ** - 5%, * - 10%.

Table 4.4: Wage Differentials of Married and Unmarried Homosexuals in States with Same-Sex Legal Recognition, by Type of Legal Recognition, Separately

	Male Heads	Male Partners	Female Heads	Female Partners
<u>Panel A: Unmarried Homosexuals</u>				
Same-Sex Marriage States	-0.0420 (0.0319)	0.0021 (0.0458)	-0.0306 (0.0336)	-0.0543 ** (0.0821)
Civil Union States	-0.0649 ** (0.0330)	0.0458 (0.0443)	-0.0199 (0.0348)	-0.0313 (0.0263)
Broad Domestic Partnership States	0.0121 (0.0148)	0.0288 (0.0208)	0.0260 (0.0199)	0.0285 * (0.0159)
Narrow Domestic Partnership States	0.0060 (0.0148)	-0.0131 (0.0209)	0.0009 (0.0189)	0.0492 *** (0.0154)
<u>Panel B: Married Homosexuals</u>				
Same-Sex Marriage States	0.0659 (0.0718)	-0.2054 ** (0.0979)	0.0545 (0.0821)	-0.0118 (0.0748)
Civil Union States	-0.0169 (0.0903)	0.0360 (0.1016)	0.1371 (0.0895)	-0.0750 (0.0676)
Broad Domestic Partnership States	-0.2327 *** (0.0435)	0.1203 * (0.0635)	-0.0468 (0.0641)	-0.1295** (0.0513)
Narrow Domestic Partnership States	0.0077 (0.0781)	-0.1570 (0.1001)	-0.2991 (0.0990)	-0.4301 *** (0.0804)

Standard errors are in parentheses. Significance: *** - 1%, ** - 5%, * - 10%.

Table 4.5: Impact of Mate's Expected Weekly Hours of Work on a Person's Wages

	Male Heads	Male Partners	Female Heads	Female Partners
Heterosexual Marriage	0.0011 *** (0.0001)	0.0040 *** (0.0002)	-0.0023 *** (0.0001)	-0.0002 * (0.0001)
Heterosexual Cohabitation	0.0081 *** (0.0004)	0.0083 *** (0.0008)	-0.0009 (0.0007)	0.0022 *** (0.0005)
Homosexual Cohabitation	0.0176 *** (0.0018)	0.0130 *** (0.0026)	0.0064 *** (0.0020)	0.0160 *** (0.0015)
Homosexual Marriage	0.0118 ** (0.0048)	0.0062 (0.0109)	0.0132 ** (0.0020)	-0.0036 (0.0056)
F-Tests				
Hetero. Marriage = Hetero. Cohab.	No [251.59]	No [26.41]	No [3.74]	No [19.44]
Hetero. Marriage = Homo. Cohab	No [81.05]	No [12.07]	No [18.00]	No [113.38]
Hetero. Marriage = Homosexual Marriage	No [5.05]	Yes [0.04]	No [4.14]	Yes [0.36]
Hetero. Cohab. = Homo. Cohab.	No [25.65]	No [3.01]	No [11.54]	No [73.07]
Hetero. Cohab. = Homo. Marriage	Yes [0.62]	Yes [0.04]	No [3.42]	Yes [1.06]
Homo. Cohab. = Homo. Marriage	Yes [1.25]	Yes [0.36]	Yes [0.75]	No [11.36]
Adjusted R2	0.4181	0.4376	0.4531	0.4202
Sample Size	3,963,572	945,603	749,725	2,345,120

Standard errors are in parentheses. F- Stats are in brackets. Significance: *** - 1%, ** - 5%, * - 10%.

Table 4.6: Specialization and Positive Assortative Mating in States that Recognize Same-Sex Partnerships

	Male Heads	Male Partners	Female Heads	Female Partners
<u>Heterosexual Marriage</u>				
Same-Sex Marriage States	-0.0030 *** (0.0007)	-0.0038 *** (0.0010)	-0.0013 * (0.0007)	0.0003 (0.0005)
Civil Union States	-0.0044 *** (0.0007)	-0.0075 *** (0.0013)	0.0004 (0.0009)	-0.0003 (0.0006)
Broad Domestic Partnership States	-0.0006 (0.0004)	0.0001 (0.0006)	0.0016 *** (0.0005)	0.0031 *** (0.0004)
Narrow Domestic Partnership States	-0.0006 * (0.0003)	-0.0003 (0.0006)	-0.0006 (0.0005)	0.0017 *** (0.0003)
<u>Heterosexual Cohabitation</u>				
Same-Sex Marriage States	-0.0018 (0.0034)	0.0061 (0.0059)	-0.0009 (0.0007)	-0.0032 (0.0034)
Civil Union States	0.0041 (0.0035)	-0.0081 (0.0057)	0.0027 (0.0042)	-0.0033 (0.0035)
Broad Domestic Partnership States	0.0033 ** (0.0016)	0.0104 *** (0.0030)	0.0043 * (0.0025)	0.0078 *** (0.0020)
Narrow Domestic Partnership States	0.0023 (0.0015)	0.0099 *** (0.0028)	0.0015 (0.0023)	0.0006 (0.0018)
<u>Homosexual Cohabitation</u>				
Same-Sex Marriage States	-0.0094 (0.0131)	-0.0027 (0.0202)	0.0128 (0.0136)	-0.0185 ** (0.0076)
Civil Union States	0.0058 (0.0174)	-0.0210 (0.0175)	-0.0144 (0.0163)	-0.0175 * (0.0090)
Broad Domestic Partnership States	-0.0028 (0.0051)	-0.0067 (0.0071)	0.0072 (0.0070)	0.0025 (0.0053)
Narrow Domestic Partnership States	0.0080 (0.0054)	0.0141 * (0.0080)	0.0092 (0.0066)	0.0136 ** (0.0057)
<u>Homosexual Marriage</u>				
Same-Sex Marriage States	-0.0063 (0.0286)	-0.0087 (0.0372)	-0.0289 (0.0336)	0.0267 (0.3222)
Civil Union States	-0.0464 (0.0341)	-0.0379 (0.0784)	-0.0053 (0.0317)	0.0708 *** (0.0232)
Broad Domestic Partnership States	0.0071 (0.0142)	0.0496 * (0.0267)	0.0352 ** (0.0157)	0.0145 (0.0149)
Narrow Domestic Partnership States	0.0298 (0.0367)	0.0335 (0.0299)	-0.2400 *** (0.0596)	0.0513 *** (0.0160)

Standard errors are in parentheses. F-Stats are in brackets. Significance: *** - 1%, ** - 5%, * - 10%.

Table 4.7: Wage Differentials for Movers

	Male Heads	Male Partners	Female Heads	Female Partners
All Movers	0.0359 *** (0.0006)	0.0232 *** (0.0013)	0.0173 *** (0.0014)	0.0118 (0.0074)
Additional Wage Differential for Movers, by Relationship Type				
Heterosexual	-0.0166 *** (0.0201)	-0.0113 *** (0.0031)	-0.0001 (0.0029)	0.0008 (0.0023)
Homosexual	0.0107 (0.0077)	0.0120 (0.0108)	-0.0035 (0.0090)	0.0118 (0.0074)
Homosexual	0.0155 (0.0279)	0.0439 (0.0395)	-0.0882 ** (0.0364)	-0.1636 *** (0.0285)

Results are robust to how legal recognition is defined. Standard errors are in parentheses. F-statistics are in brackets. Significance: *** 1%, ** 5%, * 10%.

CHAPTER V

NON-DISCRIMINATION LAWS, MOBILITY, AND LABOR OUTCOMES

5.1 INTRODUCTION

As of 2010, 21 states and the District of Columbia have passed laws prohibiting discrimination in employment based on sexual orientation (hereafter non-discrimination laws). The intent of these laws appears consistent – to counteract prejudices existing in the market resulting in negative outcomes for the Lesbian, Gay, Bisexual, and Transgendered (LGBT) community. The purpose of this essay is to examine the effects these laws have on the outcomes of homosexuals and heterosexuals. The analyses presented in this essay use data from the 2000 Census and the 2001 through 2010 American Community Surveys.

The economic literature on the labor market outcomes for gay men and women has grown significantly since the seminal Badgett (1995) paper (e.g. Black 2000, Black et al. 2003, Carpenter 2005, Oreffice 2011, Antecol and Steinberger forthcoming, and Kirby working papers A and B). The most common results are that there are negative labor market outcomes for gay men and positive outcomes for lesbian women relative to their married counterparts. If negative outcomes are driven by employer or coworker discrimination (Becker, 1971), then it is possible that non-discrimination laws may work to counter some of the prejudice.

Previous work has examined how non-discrimination policies have impacted targeted groups. Studies examining the Civil Rights Act of 1964 tend to show positive effects on the wages and occupational mobility of black men and women.¹ Conversely,

¹ See Kwasi Fosu (1992 and 2000).

studies on the labor force effects of the Americans with Disabilities Act find lower levels of employment and unchanged wages for those with disabilities.² Three papers – Klawitter and Flatt (1998), Leppel (2009), and Gates (2009b) – examine the effects non-discrimination laws have on labor market outcomes for homosexuals and find different results. Klawitter and Flatt find no evidence that non-discrimination laws result in wage changes for people in same-sex couples, though Gates estimates positive returns for both gay men and women and that the returns increase the longer the law has been in place. A possible explanation for the different results is that Klawitter and Flatt use the 1990 Census and Gates uses the 2000 Census. Leppel examines labor force participation, employment, and unemployment for homosexual and heterosexual couples. Her results indicate an increase in unemployment for same-sex couples in states with non-discrimination laws. Leppel, like Gates, uses data from the 2000 Census.

This essay expands on the work of these three preceding works. I extend the examination period to include data from 2001 through 2010. This will include more states with non-discrimination laws. I also examine the impact interstate movers have on the estimates. It is possible that non-discrimination laws encourage people to move into these states to obtain better labor market outcomes. If movers tend to have higher levels of human capital, this could upwardly bias estimated returns to non-discrimination laws.

The majority of estimates do not show significant effects on the labor force outcomes of gay men or women. Since this study looks at a longer time period than the three previous papers, it is possible that general attitudes towards homosexuality have

² See DeLeire (2000) and Acemoglu and Angrist (2001).

improved, resulting in a narrowing gap in non-discrimination law states and those without. Early positive results for heterosexual women living in states with non-discrimination laws may be the result of other policies or work-place attitudes that improve labor force outcomes for females, in line with Leppel's (2009) hypothesis.

5.2 DATA AND METHODOLOGY

5.2.1 Data

In 1977, the District of Columbia passed legislation prohibiting employment discrimination based on sexual orientation. Since then 21 states have also enacted state-wide nondiscrimination policies (National Gay and Lesbian Task Force, www.theTaskForce.org). Table 5.1 shows the list of states with employment nondiscrimination laws and in what year these policies went into effect. It is possible these laws proxy for other policies for other policies or reflect a greater tolerance for homosexuals within the state.

I use the 1% and 5% Public Use Micro-data Sample from the 2000 decennial U.S. Census as well as the American Community Survey from 2001 to 2010.³ Both surveys are designed to be nationally representative and are conducted by the United States Census Bureau. The data used in this study was retrieved from the Minnesota Population Center at the University of Minnesota (www.ipums.org; Ruggles, et al. 2010). All individuals who are under the age of 16 as well as those who have sex, marital status, and

³ The 1% and 5% samples from the 2000 Census are mutually exclusive – no individual would be included in both samples. The 2001 to 2004 ACS sampled approximately 0.4% of the population. This was expanded to a 1% sample in 2005. Both the decennial Census and the 2006 through 2009 ACS samples include individuals in group quarters. For consistency, all individuals in group quarters are eliminated from analysis.

relationship status coding flags are eliminated from analysis. Coding flags indicate that a response was changed or provided by the Census Bureau. To avoid potential measurement error, it is common practice in this literature to drop observations with these types of coding flags. Also eliminated are households where neither the head nor his/her partner speaks English well⁴ and households where more than one partner is identified.

This analysis examines individuals who are in an unmarried partnership, married, or single. A respondent who does not indicate that other members of the household are related to him/her as a spouse or unmarried partner and who does not note his/her marital status as married is considered single. If the respondent notes another household member as a spouse, both individuals have marital status responses of married, and the two individuals are not of the same sex, then the couple is considered married. Unmarried partnerships are households where the respondent notes another household member as his/her unmarried partner and neither of the individuals has a marital status listed as married. If the members of an unmarried partnership are of different sexes then they are considered a heterosexual unmarried partnership; if they are of the same sex then they are considered a homosexual unmarried partnership. Individuals of the same sex are not identified as spouses in the U.S. Census or the American Community Survey; if a household reports a same-sex marriage, the relationship status is recoded as unmarried partner and marital status is recoded from married to single.

⁴ Black, et al. (2007a) provides evidence that miscoding is more common when the head of household does not speak English well.

The combined Census and ACS sample has hundreds of thousands of observations for single persons, cohabitating heterosexuals, and married heterosexuals. Performing even simple analyses on such large samples can be time consuming because of the computational processing demands. To reduce the computational burden, the sample of single, married, and heterosexual cohabitating partners are drawn from the 1% 2000 Census and the 2001 through 2010 American Community Survey. When the analysis is limited to individuals between the age 25-54 a random sample of 100,000 is taken for married heads and married partners as well as unmarried heads and partners. This is expanded to 200,000 when the age range is expanded to 18 through 64. A random sample of 200,000 is performed for single individuals regardless of age range. Homosexual cohabitating partners are drawn from the 5% 2000 Census and the 2001 through 2010 American Community Survey to ensure a sufficiently large sample.

5.2.2 Model

The first model of labor force outcomes is similar to Leppel's method, using data from the 2000 Census and estimating the impact of non-discrimination laws. The model takes the form:

$$Y_{is} = NonDis_s\beta_1 + X_{is}\beta_2 + \gamma_r + \epsilon_{is}$$

where i indexes individuals, s indexes states, and r indexes Census regions. Y represents individual labor force participation and unemployment. $NonDis$ is an indicator variable for whether a state has passed a law that prohibits discrimination in employment based on sexual orientation. X is a set of control variables including age, age squared divided by

100, number of own children, number of own children under five, race (excluding white), multiracial, Hispanic origin, English ability, disability, region (excluding South), type of occupation (white collar, blue collar, and farming, fishing, and forestry with service occupations excluded), real non-wage income in thousands, mate's real wage in thousands, and mate's real non-wage income in thousands. This analysis is performed separately for males and females based on relationship type (cohabitating homosexuals, cohabitating heterosexuals, married heterosexuals, and singles). This analysis is also done separately for heads of households and their partners.⁵ In an attempt to replicate Leppel's analysis, in this model individuals are limited to those who have a valid response to the industry⁶ variable and are between the ages of 25 and 54.

Next, I extend Leppel's analysis to include data from the 2001 through 2010 American Community Survey as well as the 2000 Census. This expands the analysis to include more states that have passed non-discrimination laws. It also allows for change in the attitudes towards homosexuality that seem to have across the United States. Laws that may have had significant impact in 2000 may not be as effective in 2010 because there may be less discrimination towards homosexuals in 2010. This model takes the form:

⁵ Respondents are considered heads of households and the person they indicate as either spouse or unmarried partner is considered to be their partner. Obviously a separate analysis based on heads or partners cannot be done for singles and no information on mates can be included in analysis of their labor market outcomes.

⁶ As defined by the Minnesota Population Center at the University of Minnesota (www.ipums.org) "In census usage, 'industry' currently refers to work setting and economic sector, as opposed to the worker's specific technical function, or 'occupation'". In both the Census and the ACS, an individual would not have a valid "industry" response if they had been jobless for the previous five (5) years. For individuals currently employed in more than one industry, they are instructed to report the industry in which they earn the most money. For those that have worked in more than one industry in the past five years, they are to report the industry in which they are currently, or most recently employed.

$$Y_{ist} = NonDis_{ist}\beta_1 + X_{ist}\beta_2 + \gamma_s + \gamma_t + \epsilon_{ist}$$

In this model γ_s is a vector of state indicator variables and γ_t is a vector of year indicator variables.⁷ Initially this model is estimated using Leppel's choice of subjects, those between the ages 25 and 54; then the model is expanded to include those between the ages of 18 and 64. Expanding the age range examined will make results more compatible with other papers examining the labor force outcomes of gay men and women.

It is possible non-discrimination laws may result in interstate migration into the state. To account for this, another model is estimated which takes the form:

$$Y_{ist} = NonDis_{st}\beta_1 + Mover_{ist}\beta_2 + NonDis_{st} * Mover_{ist}\beta_3 + X_{ist}\beta_4 + \gamma_s + \gamma_t + \epsilon_{ist}$$

where *Movers* is an indicator variable that equals 0 if the person lives in the same state as they did in the previous year and 1 otherwise.

5.3 RESULTS

The initial analysis tests for the impact of non-discrimination laws on labor force participation by including an indicator variable for whether the state has a non-discrimination law. The marginal effects are calculated by finding the difference in the estimated probabilities of being in the labor force for individuals in states with non-discrimination laws – the variable has a value of one - minus the probability of being in

⁷ Wyoming is excluded from the vector of state indicator variables to avoid a near singular matrix. This state was chosen because it is last in a list of an alphabetical list of the states. The year of 2000 is excluded from the vector of year indicator variables because it is the year used in the previous studies by Leppel (2009) and Gates (2009b). The coefficients on the year variables would illustrate changes common across all individuals relative to their established base year.

the labor force in a state without a non-discrimination law – the variable has a value of zero. When calculating these probabilities, the other values are measured at their sample means. The first round of analysis is done of eight separate samples based on sex and relationship type.⁸

In the tables that follow the estimates for individuals in homosexual partnerships are presented in column 1, individuals in heterosexual marriages are in column 2, individuals in unmarried heterosexual partnerships are in column 3, and single individuals are in column 4. In the first table that follows (Table 5.2), results for males are presented in Panel A and results for females are presented in Panel B. For all other tables that follow, Panel A presents results for male heads, Panel B presents results for male partners, Panel C presents results for female heads, and Panel D presents results for female partners.

Estimates are on labor force participation presented in Table 5.2, Panel A for males and Panel B for females. Initial logit analysis on whether an individual is in the labor force suggest there is no significant impact on the labor force participation of men regardless of relationship type. The converse appears to be true with respect to women; women's labor force participation is estimated to be greater in states with sexual orientation-based non-discrimination laws. When Leppel finds a comparable result, she suggests it may be that "these laws reflect a climate more tolerant of diversity and more supportive of women's rights".

⁸ These samples are gay males, married males, cohabitating males, single males, gay females, married females, cohabitating females, and single females.

It is plausible that these laws affect heads of households differently than their partners. To test this hypothesis, the analysis is performed separately for heads and partners, so the analysis is performed on 16 separate samples. The results of these estimates are presented in Table 5.3. The baseline for estimated marginal effects is the variable means of the 16 samples, with the marginal effect for the non-discrimination law variable being measured as the estimated probability when that variable equals one minus the estimated probability when that variable equals zero. Single men and women are compared to both heads and partners for consistency. Separate analysis suggests that male partners in unmarried partnerships as well as female partners in homosexual couples experience an increase in labor force participation. The result for gay women is reasonable; the result for unmarried male partners is unusual. This result may be anomalous or could be limited to the states that had non-discrimination laws in 2000. I expand the period examined to 2010 which adds ten additional states with non-discrimination laws.

Expanding the scope to cover 2000-2010, results are presented in Table 5.4. There again seems to be no significant impact for men's labor force participation, except for those in unmarried partnerships. However, rather than the partners in these relationship having higher participation, the heads are now estimated to have lower participation. This could be a result of interstate migration of gays and lesbians into the state competing for jobs with these men. Married and single women, however, tend to have higher labor force participation, in-line with Leppel's hypothesis about states with non-discrimination laws being more tolerant of women in the workplace. Estimates show

that the labor force participation of lesbian partners is lower, though in the previous analysis it was higher. It is also possible that interstate migration may be the cause. Over a greater time period more lesbian couples may have moved in to these states, increasing the supply of labor in job markets they are entering.

I increase the age range from 25 through 54 to 18 through 64. This is to keep analysis consistent with the work of other papers in this field including Gates' (2009b), analysis that will be examined later in the essay. Results for this expanded age group are presented in Table in Table 5.5. In general there is not significant change in the parameter estimates resulting from an expansion of the age range. There are two exceptions to this statement, unmarried male heads and married female partners. Before the expansion unmarried male heads were estimated to have a significantly lower wage in states with non-discrimination and married female partners were estimated to have significantly higher wages. Now neither is estimated to have parameter estimates significantly different from zero.

To test the hypothesis of interstate mobility, the model is expanded to include a variable for whether an individual has moved across state lines in the past year, and an interaction of this variable with the non-discrimination law variable. The marginal effects for interstate mobility are calculate in a similar methods as those for non-discrimination laws - the estimated probability when interstate mobility equals one minus the estimated probability when interstate mobility equals zero, with all other variables measured at their means. For the interaction between non-discrimination laws and interstate mobility, what I am interested in this analysis is the difference between those

who recently moved to non-discrimination law states compared to those who have lived in these states for more than a year. The marginal effect for this variable is calculated by subtracting the estimated probability when the variables non-discrimination law is equal to one, interstate mobility equals zero, and the interaction between non-discrimination laws and interstate mobility equals zero, from when the variables non-discrimination law equals one, interstate mobility equals one, and the interaction between non-discrimination law and interstate mobility equals one, with other variables measured at their mean. The results are presented in Table 5.6.

After controlling for interstate mobility, the estimated marginal effects for gay men are positive and negative for lesbian women, for both heads and partners. These results are not statistically significant, but the results for gay males are interesting when the results for other men are taken into consideration. The estimated marginal effects of non-discrimination laws on the labor force participation of married, cohabitating, and single men are all negative. These results are statistically significant for single men and cohabitating male heads. One possible explanation for this is that the laws have encourage more gay men to enter the work force, and the influx of new workers has discouraged some men from even looking for work. Single women are also estimated to have significantly lower labor force participation but there does not appear to be any consistency in the results for identifiably heterosexual women. For those men who move into states with non-discrimination laws, the heads of households – gay or straight - and single men appear to have higher labor force participation but there is no consistent result for men who are partners in their relationship. For gay women who

move into these states, estimates suggest even lower labor force participation but again these results are not significant. For married and cohabitating female heads that recently moved in non-discrimination law states, labor force participation is significantly higher, though their partner's rates are estimated to be lower. Female partners may not enter these labor markets as quickly though; this may be a result of a move being driven by a husband or boyfriend's new job.

Next I examine the effect the laws have on other labor force outcomes including unemployment, hours worked, and wages. Limiting the sample to those individuals in the labor force, Table 5.7 show the estimated affect non-discrimination laws have on the probability of being unemployed, controlling for mobility. The non-discrimination law is negatively correlated with unemployment for married and gay male heads, so those who enter the labor force are more likely to get a job in these states than in states without non-discrimination laws. There are similar effects for married and gay female partners. Consistently, movers are less likely to be unemployed than people who have not moved in the last year, in line with the idea that people tend to move for jobs. The results also show a negative correlation between movers into non-discrimination law states and unemployment, though this correlation is not usually significant. This correlation is again in line with the theory that people move for jobs, but there is a lack of significance, it does not supply any additional evidence that people who move into state with non-discrimination laws are any better or worse off.

Another possible effect non-discrimination laws may have on individuals is to change the number of hours worked. While labor force participation appears to be

unchanged and unemployment is estimated to be lower for gay males, it is feasible the increase in employment has come at the cost of hours. Results of the selection Tobit model are presented in Table 5.8. After controlling for mobility, there appears to be no significant impact on the hours worked for gay men or women. There is little evidence that non-discrimination laws have a significant impact on the hours worked for other men or women, either. It is noteworthy, though, that the point estimates single and heterosexual partner men and women are almost always positive. As for the individuals who recently moved into states with non-discrimination laws, results do not show a consistent, significant pattern.

The final labor market outcome examined is the effect non-discrimination laws have on wages. Heckman selection models are performed to estimate the potential returns workers receive from these types of laws; log wage regressions are estimated for all workers in the labor force and again for fulltime workers. The results for all workers are presented in Table 5.9. The results usually show wages are significantly higher for individuals who moved across state lines in the past year. This result is expected and is in line with the theory of people moving for higher wages. For gay men and women, there do not appear to be any significant differences in wages for those living in states with non-discrimination laws, though point estimates are positive for homosexuals that live in non-discrimination law states. Men in heterosexual relationships, either married or cohabitating, tend to receive higher wages in states with non-discrimination laws. Point estimates are usually negative for women in heterosexual relationships with the exception of the heads of unmarried partnerships. For these women their wages are

estimated to be significantly greater. Also, those men and women in heterosexual relationships that have moved into non-discrimination law states in the past year, tend to have lower wages.

Limiting the analysis to full-time workers does not greatly change the results, which are presented in Table 5.10. Men who work fulltime are usually estimated to have higher wages. Wage penalties are still commonly estimated for singles and individuals in heterosexual relationships that recently moved in to states with non-discrimination laws, though the coefficients tend to closer to zero. The most obvious difference in the analysis for all workers compared to fulltime workers is there is no longer a tendency for higher wages for men working in states with non-discrimination laws. This suggests that wage gains for men in these states tend to accrue to those who work part-time. If states with non-discrimination laws also have policies that result in higher wages for part-time workers that would explain this difference.

Thus far there do not appear to be any significant gains for gay men and women as a result of non-discrimination laws. What has been presented so far controls for those who move into states with non-discrimination laws, but this assumes there are not systematic differences between people who move into these states, people who move into other states, and those who have not moved recently. Since people moved across state-lines prior to adoption of non-discrimination laws, there are likely other characteristics of these states that draw in people. To control for this I limit the sample to people who move into states that, by the end of the available survey, have passed non-discrimination

laws.⁹ Selection models are performed on the log wages of all workers and those who work fulltime. Results are presented in Table 5.11 for all worker and 5.12 for fulltime workers. There continues to be no evidence of a significant effect on the wages of gay men and women that move in to states with no discrimination laws, whether they are employed fulltime or work fewer hours. What appears more common is a negative correlation between non-discrimination laws and the wages of single men and women. When the sample is limited to fulltime workers, there is a negative point estimates for all groups when they moved in after the passage of the non-discrimination law, though the estimates are not usually significantly different from zero.

5.4 CONCLUSION

Previous work by Leppel and Gates showed evidence that non-discrimination laws have mixed results for gay men and women. Leppel (2009) estimated higher levels of unemployment while Gates' (2009b) results showed higher earnings. The findings in this essay do not support their results and are more in line with earlier work by Klawitter and Flatt (1998). After controlling for interstate mobility, most results are not significantly different from zero. Without statistically significant results, there is uncertainty about any potential benefits or penalties as a result of non-discrimination laws.

Point estimates do provide some evidence of possible effects on homosexual and heterosexual populations as a result of non-discrimination laws. Gay men may be better

⁹ These states are, in order of passage of non-discrimination laws, the District of Columbia, Wisconsin, Massachusetts, Connecticut, Hawaii, California, New Jersey, Vermont, Minnesota, Rhode Island, New Hampshire, Nevada, Maryland, New York, New Mexico, Illinois, Maine, Washington, Colorado, Iowa, Oregon, and Delaware.

off in terms of labor force participation, unemployment, and wages. For other men there is stronger evidence they are worse off in terms of labor force participation, but better off in terms of unemployment, hours worked, and wages. Lesbian women may also be better off in terms of labor force participation and unemployment. Other women may be better off in terms of many metrics of labor force outcomes, except labor force participation. One argument for this is that states with non-discrimination laws may also have other policies or work place attitudes that result in better outcomes for women.

For the recently moved, lower rates of unemployment are consistently estimated though not typically at significant levels. Estimates for other labor force outcomes are either inconsistent or negative. For those who are single or in heterosexual relationships wages are commonly estimated to be lower, and occasionally this result is statistically significant.

The results of this analysis do not in general show significant impacts as a result of non-discrimination laws, especially for gay men and women. This may be a result of greater acceptance of homosexuals in the work force therefore the difference in labor force outcomes in states with non-discrimination laws and those without has diminished. The results may not take into account differences that may exist in different types of occupations or industries. Further analysis is necessary to see if this is likely.

Table 5.1: Statewide Non-Discrimination in Employment Laws

District of Columbia	1977	Minnesota	1993	Illinois	2005
Wisconsin	1982	Rhode Island	1995	Maine	2005
Massachusetts	1989	New Hampshire	1997	Washington	2006
Connecticut	1991	Nevada	1999	Colorado	2007
Hawaii	1991	Maryland	2001	Iowa	2007
California	1992	New York	2002	Oregon	2007
New Jersey	1992	New Mexico	2003	Delaware	2009
Vermont	1992				

9 other states have some form of non-legislative policy prohibiting discrimination based on sexual orientation against state employees: Arizona, Indiana, Kansas, Kentucky, Michigan, Missouri, Montana, Ohio, and Pennsylvania.

Table 5.2: The Impact of Non-Discrimination Laws on Labor Force Participation, Age 25 – 54, Year 2000

	Gay	Married	Cohabiting	Single
	<u>Panel A: Males</u>			
Non-Discrim. Law	0.0054 [0.0421] (0.0440)	0.0006 [0.0055] (0.0205)	-0.0027 [-0.0184] (0.0182)	0.0028 [0.0189] (0.0181)
No. Obs.	13,155	78,976	73,646	76,337
	<u>Panel B: Females</u>			
Non-Discrim. Law	0.0189 *** [0.1128] (0.0378)	0.0153 *** [0.0651] (0.0155)	0.0036 [0.0177] (0.0161)	0.0061 * [0.0344] (0.0176)
No. Obs.	14,725	77,688	73,265	75,405

Marginal effects are presented without parentheses, braces, or brackets. The marginal effects are calculated as the difference in the probability where variables are estimated at their means except for the Non-Discrim. Law variable. The difference is when the Non-Discrim. Law equals one (1) minus when the Non-Discrim. Law equals zero (0). Logit coefficient estimates are reported in braces and corresponding standard errors are reported in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.3: The Impact of Non-Discrimination Laws on Labor Force Participation, Age 25 – 54, Year 2000

	Gay	Married	Cohabiting	Single
<u>Panel A: Male Heads</u>				
Non-Discrim. Law	0.0036 [0.0423] (0.0709)	-0.0024 [-0.0232] (0.0270)	-0.0115 [-0.0117] (0.0257)	0.0028 [0.0189] (0.0181)
No. Obs.	6,381	46,023	40,223	76,337
<u>Panel B: Male Partners</u>				
Non-Discrim. Law	0.0040 [0.0271] (0.0589)	0.0007 [0.0055] (0.0388)	0.0091 ** [0.0605] (0.0264)	0.0028 [0.0189] (0.0181)
No. Obs.	6,774	18,282	32,797	76,337
<u>Panel C: Female Heads</u>				
Non-Discrim. Law	0.0062 [0.0594] (0.0645)	0.0044 [0.0211] (0.0320)	0.0051 [0.0300] (0.0252)	0.0061 * [0.0344] (0.0176)
No. Obs.	6,764	18,152	33,519	75,405
<u>Panel D: Female Partners</u>				
Non-Discrim. Law	0.0248 *** [0.1298] (0.0496)	0.0060 [0.0246] (0.0200)	0.0001 [0.0003] (0.0209)	0.0061 * [0.0344] (0.0176)
No. Obs.	7,961	45,511	39,649	75,405

Marginal effects are presented without parentheses, braces, or brackets. The marginal effects are calculated as the difference in the probability where variables are estimated at their means except for the Non-Discrim. Law variable. The difference is when the Non-Discrim. Law equals one (1) minus when the Non-Discrim. Law equals zero (0). Logit coefficient estimates are reported in braces and corresponding standard errors are reported in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.4: The Impact of Non-Discrimination Laws on Labor Force Participation, Age 25 – 54, Year 2000 – 2010

	Gay	Married	Cohabiting	Single
<u>Panel A: Male Heads</u>				
Non-Discrim. Law	0.0077 [0.0911] (0.0802)	0.0037 [0.0474] (0.0407)	-0.0203 *** [-0.2055] (0.0371)	-0.0009 [-0.0076] (0.0249)
No. Obs.	17,718	99,990	99,971	199,984
<u>Panel B: Male Partners</u>				
Non-Discrim. Law	0.0096 [0.0732] (0.0713)	-0.0025 [-0.0296] (0.0411)	-0.0045 [-0.0360] (0.0328)	-0.0009 [-0.0076] (0.0249)
No. Obs.	18,420	99,996	99,993	199,984
<u>Panel C: Female Heads</u>				
Non-Discrim. Law	-0.0068 [-0.0683] (0.0772)	0.0128 ** [0.0641] (0.0291)	-0.0031 [-0.0190] (0.0303)	0.0069 ** [0.0463] (0.0226)
No. Obs.	18,067	99,994	99,990	199,989
<u>Panel D: Female Partners</u>				
Non-Discrim. Law	-0.0190 * [-0.1233] (0.0648)	0.0196 *** [0.0867] (0.0269)	0.0053 [0.0251] (0.0277)	0.0069 ** [0.0463] (0.0226)
No. Obs.	19,288	99,986	99,966	199,989

Marginal effects are presented without parentheses, braces, or brackets. The marginal effects are calculated as the difference in the probability where variables are estimated at their means except for the Non-Discrim. Law variable. The difference is when the Non-Discrim. Law equals one (1) minus when the Non-Discrim. Law equals zero (0). Logit coefficient estimates are reported in braces and corresponding standard errors are reported in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.5: The Impact of Non-Discrimination Laws on Labor Force Participation, Age 18 – 64, Year 2000 - 2010

	Gay	Married	Cohabiting	Single
<u>Panel A: Male Heads</u>				
Non-Discrim. Law	-0.0013 [-0.0090] (0.0623)	0.0001 [0.0006] (0.0233)	-0.0025 [-0.0194] (0.0236)	-0.0021 [-0.0096] (0.0211)
No. Obs.	22,688	199,985	199,952	199,974
<u>Panel B: Male Partners</u>				
Non-Discrim. Law	0.0103 [0.0523] (0.0557)	-0.0018 [-0.0126] (0.0244)	0.0027 [0.0163] (0.0214)	-0.0021 [-0.0096] (0.0211)
No. Obs.	23,464	199,992	199,974	199,974
<u>Panel C: Female Heads</u>				
Non-Discrim. Law	-0.0197 [-0.1278] (0.0594)	0.0124 ** [0.0397] (0.0185)	0.0007 [0.0028] (0.0194)	0.0014 [0.0053] (0.0197)
No. Obs.	23,539	199,993	199,982	199,979
<u>Panel D: Female Partners</u>				
Non-Discrim. Law	-0.0237 * [-0.1097] (0.0506)	-0.0021 [-0.0061] (0.0172)	0.0172 *** [0.0589] (0.0180)	0.0014 [0.0053] (0.0197)
No. Obs.	25,340	199,983	199,950	199,979

Marginal effects are presented without parentheses, braces, or brackets. The marginal effects are calculated as the difference in the probability where variables are estimated at their means except for the Non-Discrim. Law variable. The difference is when the Non-Discrim. Law equals one (1) minus when the Non-Discrim. Law equals zero (0). Logit coefficient estimates are reported in braces and corresponding standard errors are reported in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.6: The Impact of Non-Discrimination Laws on Labor Force Participation, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility

	Gay	Married	Cohabiting	Single
<u>Panel A: Male Heads</u>				
Non-Discrim. Law	0.0095 [0.0659] (0.1165)	-0.0122 [-0.0830] (0.0537)	-0.0128 ** [-0.1003] (0.0395)	-0.0212 *** [-0.098] (0.0353)
Interstate Movers	0.0254 *** [0.1625] (0.0600)	0.0029 [0.0200] (0.0276)	-0.0027 [-0.0218] (0.0179)	-0.0120 *** [-0.0572] (0.0160)
Movers * Law	0.0027 [-0.1448] (0.0906)	0.0153 * [0.0790] (0.0452)	0.0118 *** [0.1113] (0.0276)	0.0063 *** [0.0856] (0.0259)
No. Obs.	14,662	112,469	119,989	127,122
<u>Panel B: Male Partners</u>				
Non-Discrim. Law	0.0077 [0.0384] (0.1024)	-0.0066 [-0.0456] (0.0409)	-0.0052 [-0.0311] (0.0321)	-0.0212 *** [-0.098] (0.0353)
Interstate Movers	0.0027 [0.0131] (0.0520)	0.0027 [0.0188] (0.0199)	-0.0197 *** [-0.1222] (0.0143)	-0.0120 *** [-0.0572] (0.0160)
Movers * Law	-0.0071 [-0.0488] (0.0777)	0.0087 [0.0391] (0.0330)	-0.0073 *** [0.0770] (0.0226)	0.0063 *** [0.0856] (0.0259)
No. Obs.	14,965	165,286	135,104	127,122
<u>Panel C: Females Heads</u>				
Non-Discrim. Law	-0.0186 [-0.1196] (0.1153)	0.0244 [0.0782] (0.0310)	-0.0087 [-0.0366] (0.0292)	-0.0179 ** [-0.0664] (0.0333)
Interstate Movers	-0.0069 [-0.0455] (0.0568)	0.0196 *** [0.0615] (0.0154)	-0.0081 *** [-0.0346] (0.0128)	-0.0035 [-0.0133] (0.0146)
Movers * Law	-0.0236 [-0.0954] (0.0905)	0.0046 * [-0.0466] (0.0252)	0.0018 ** [0.0423] (0.0203)	0.0092 * [0.0470] (0.0245)
No. Obs.	14,713	165,531	134,473	127,988
<u>Panel D: Female Partners</u>				
Non-Discrim. Law	-0.0310 [-0.1547] (0.1013)	-0.0119 [-0.0332] (0.0391)	0.0218 [0.0747] (0.0286)	-0.0179 ** [-0.0664] (0.0333)
Interstate Movers	-0.0320 *** [-0.1739] (0.0493)	-0.0132 * [-0.0372] (0.0196)	-0.0357 *** [-0.1239] (0.0124)	-0.0035 [-0.0133] (0.0146)
Movers * Law	-0.0280 [-0.0369] (0.0770)	-0.0071 [0.0174] (0.0328)	-0.0332 [0.0041] (0.0196)	0.0092 * [0.0470] (0.0245)
No. Obs.	14,914	113,260	121,349	127,988

Marginal effects are presented without parentheses, braces, or brackets. The marginal effects are calculated as the difference in the probability where variables are estimated at their means except for the Non-Discrim. Law variable. The marginal effect for the Non-discrimination variable is difference between when the Non-Discrim. Law equals one (1) minus when the Non-Discrim. Law equals zero (0). The marginal effect for the interstate movers variable is difference between when moved last year equals one (1) minus when moved last year equals zero (0). To capture the difference between people who have lived in a non-discrimination state for more than one year and those who have migrated in to the state in the past year, the marginal effect for the mover*law variable is the difference the probability measured where Non-Discrim. Law equals one (1), moved last year equals one (1), and movers*law equals one (1) minus the probability measured where Non-Discrim. Law equals one (1), moved last year equals zero (0), and movers*law equals zero (0). Logit coefficient estimates are reported in braces and corresponding standard errors are reported in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.7: The Impact of Non-Discrimination Laws on Unemployment, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility

	Gay	Married	Cohabiting	Single
<u>Panel A: Male Heads</u>				
Non-Discrim. Law	-0.0269 *** [-0.3380] (0.1425)	-0.0118 *** [-0.1837] (0.0679)	0.0042 [0.0327] (0.0396)	-0.0050 [-0.0464] (0.0412)
Interstate Movers	-0.0124 * [-0.1401] (0.0746)	-0.0061 ** [-0.0842] (0.0354)	-0.0069 *** [-0.0527] (0.0174)	-0.0055 *** [-0.0489] (0.0188)
Movers * Law	-0.0003 [0.1358] (0.1094)	-0.0023 [0.0443] (0.0564)	-0.0125 [-0.0418] (0.0269)	-0.0038 [0.0134] (0.0303)
No. Obs.	12,956	97,762	107,769	127,122
<u>Panel B: Male Partners</u>				
Non-Discrim. Law	0.0111 [0.0933] (0.1271)	-0.0007 [-0.0093] (0.0481)	-0.0252 [-0.1463] (0.0320)	-0.0050 [-0.0464] (0.0412)
Interstate Movers	-0.0048 [-0.0400] (0.0656)	-0.0126 *** [-0.1425] (0.0231)	-0.0050 ** [-0.0282] (0.0139)	-0.0055 *** [-0.0489] (0.0188)
Movers * Law	-0.0097 [-0.0347] (0.0943)	-0.0118 [0.0086] (0.0370)	-0.0082 [-0.0243] (0.0220)	-0.0038 [0.0134] (0.0303)
No. Obs.	12,616	97,762	117,145	127,122
<u>Panel C: Female Heads</u>				
Non-Discrim. Law	0.0136 [0.1730] (0.1402)	-0.0033 [-0.0380] (0.0516)	0.0050 [0.0371] (0.0372)	-0.0023 [-0.0233] (0.0424)
Interstate Movers	-0.0080 [-0.0989] (0.0685)	-0.0237 *** [-0.2317] (0.0246)	-0.0149 *** [-0.1075] (0.0162)	-0.0076 *** [-0.0729] (0.0183)
Movers * Law	-0.0129 [-0.0352] (0.1013)	-0.0190 [0.0400] (0.0393)	-0.0243 *** [-0.0654] (0.0252)	-0.0037 [0.0373] (0.0306)
No. Obs.	12,978	122,588	110,742	127,988
<u>Panel D: Female Partners</u>				
Non-Discrim. Law	-0.0186 * [-0.2005] (0.1206)	-0.0099 * [-0.1257] (0.0713)	-0.0074 [-0.0466] (0.0386)	-0.0023 [-0.0233] (0.0424)
Interstate Movers	-0.0155 ** [-0.1497] (0.0600)	-0.0155 *** [-0.1687] (0.0350)	-0.0148 *** [-0.0902] (0.0163)	-0.0076 *** [-0.0729] (0.0183)
Movers * Law	-0.0107 [0.0242] (0.0904)	-0.0089 [0.0588] (0.0572)	-0.0091 [0.0328] (0.0260)	-0.0037 [0.0373] (0.0306)
No. Obs.	12,659	122,588	92,097	127,988

Marginal effects are presented without parentheses, braces, or brackets. The marginal effects are calculated as the difference in the probability where variables are estimated at their means except for the Non-Discrim. Law variable. The marginal effect for the Non-discrimination variable is difference between when the Non-Discrim. Law equals one (1) minus when the Non-Discrim. Law equals zero (0). The marginal effect for the interstate movers variable is difference between when moved last year equals one (1) minus when moved last year equals zero (0). To capture the difference between people who have lived in a non-discrimination state for more than one year and those who have migrated in to the state in the past year, the marginal effect for the mover*law variable is the difference the probability measured where Non-Discrim. Law equals one (1), moved last year equals one (1), and movers*law equals one (1) minus the probability measured where Non-Discrim. Law equals one (1), moved last year equals zero (0), and movers*law equals zero (0). Logit coefficient estimates are reported in braces and corresponding standard errors are reported in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.8: The Impact of Non-Discrimination Laws on Usual Hours Worked, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility

	Gay	Married	Cohabiting	Single
<u>Panel A: Male Heads</u>				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.0060 (0.1300)	-0.0547 (0.0589)	-0.0370 (0.0429)	-0.0808 * (0.0379)
Interstate Movers	0.1541 ** (0.0641)	0.0290 (0.0298)	0.0035 (0.0185)	-0.0041 (0.0167)
Movers * Law	-0.1108 (0.0936)	0.0317 (0.0487)	0.0299 (0.0283)	0.0532 (0.0264)
Second-Stage - Hours Usually Worked				
Non-Discrim. Law	-0.1573 (0.8503)	0.0386 (0.3745)	0.1233 (0.2817)	0.4730 (0.3253)
Interstate Movers	-0.1945 (0.4386)	0.3326 * (0.1865)	0.2996 ** (0.1192)	0.5847 *** (0.1443)
Movers * Law	0.7711 (0.6207)	0.1259 (0.3016)	-0.0280 (0.1785)	-0.2035 (0.2268)
No. Obs.	14,662	112,469	119,989	127,122
<u>Panel B: Male Partners</u>				
First-Stage - Labor Force Participation				
Non-Discrim. Law	0.0442 (0.1111)	-0.0352 (0.0441)	-0.0512 (0.0349)	-0.0808 * (0.0379)
Interstate Movers	0.0263 (0.0544)	0.0707 *** (0.0211)	-0.1171 *** (0.0144)	-0.0041 (0.0167)
Movers * Law	-0.0747 (0.0791)	0.0220 (0.0344)	0.0735 *** (0.0223)	0.0532 (0.0264)
Second-Stage - Hours Usually Worked				
Non-Discrim. Law	1.0233 (0.8709)	0.2151 (0.3021)	0.1116 (0.2746)	0.4730 (0.3253)
Interstate Movers	0.0207 (0.4321)	0.7050 *** (0.1454)	0.0065 (0.1108)	0.5847 *** (0.1443)
Movers * Law	-0.5897 (0.6122)	-0.4642 ** (0.2338)	0.1986 (0.1683)	-0.2035 (0.2268)
No. Obs.	14,965	165,286	135,104	127,122

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.8: The Impact of Non-Discrimination Laws on Usual Hours Worked, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility, Cont’.

	Gay	Married	Cohabiting	Single
Panel C: Female Heads				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.1339 (0.1258)	0.0186 (0.0349)	-0.1260 *** (0.0333)	-0.0645 * (0.0372)
Interstate Movers	-0.0685 (0.0611)	0.0245 (0.0171)	-0.0471 *** (0.0138)	-0.0120 (0.0156)
Movers * Law	-0.0296 (0.0947)	0.0272 (0.0275)	0.0665 (0.0213)	0.0342 (0.0257)
Second-Stage - Hours Usually Worked				
Non-Discrim. Law	-0.6036 (0.7615)	0.3895 (0.3388)	-0.0530 (0.2522)	0.1023 (0.3140)
Interstate Movers	0.1033 (0.3793)	0.4989 *** (0.1677)	0.3202 *** (0.1072)	0.4470 *** (0.1359)
Movers * Law	0.4551 (0.5653)	-0.2032 (0.2635)	0.2164 (0.1600)	-0.0277 (0.2167)
No. Obs.	14,713	165,531	134,473	127,988
Panel D: Female Partners				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.1099 (0.1077)	-0.0542 (0.0430)	0.0430 (0.0322)	-0.0645 * (0.0372)
Interstate Movers	-0.1284 ** (0.0518)	-0.0033 (0.0216)	-0.0949 *** (0.0129)	-0.0120 (0.0156)
Movers * Law	0.0416 (0.0775)	0.0480 (0.0352)	-0.0166 (0.0201)	0.0342 (0.0257)
Second-Stage - Hours Usually Worked				
Non-Discrim. Law	0.2248 (0.7755)	0.7961 * (0.4755)	0.2052 (0.2924)	0.1023 (0.3140)
Interstate Movers	0.3477 (0.3726)	0.2396 (0.2395)	0.0479 (0.1208)	0.4470 *** (0.1359)
Movers * Law	-0.5805 (0.5538)	-0.3571 (0.3883)	-0.0284 (0.1798)	-0.0277 (0.2167)
No. Obs.	14,914	113,260	121,349	127,988

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.9: The Impact of Non-Discrimination Laws on Wages of All Workers, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility

	Gay	Married	Cohabiting	Single
Panel A: Male Heads				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.0969 (0.1127)	-0.0541 (0.0539)	-0.1055 *** (0.0366)	-0.0850 ** (0.0349)
Interstate Movers	0.1214 ** (0.0589)	0.0674 ** (0.0273)	-0.0286 * (0.0164)	-0.0191 (0.0157)
Movers * Law	-0.0031 (0.0869)	0.0572 (0.0453)	0.0938 *** (0.0255)	0.0793 *** (0.0256)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	0.0495 (0.0518)	0.0557 *** (0.0135)	0.0452 *** (0.0161)	-0.0134 (0.0178)
Interstate Movers	-0.0226 (0.0278)	0.0795 *** (0.0018)	0.0827 *** (0.0071)	0.0712 *** (0.0080)
Movers * Law	-0.0449 (0.0404)	-0.0840 *** (0.0021)	-0.0284 *** (0.0110)	0.0114 (0.0131)
No. Obs.	14,099	106,855	144,968	122,372
Panel B: Male Partners				
First-Stage - Labor Force Participation				
Non-Discrim. Law	0.0060 (0.0953)	-0.0108 (0.0408)	-0.0508 * (0.0292)	-0.0850 ** (0.0349)
Interstate Movers	0.0318 (0.0494)	0.0535 *** (0.0195)	-0.0973 *** (0.0128)	-0.0191 (0.0157)
Movers * Law	-0.0524 (0.0725)	0.0321 (0.0327)	0.0663 *** (0.0204)	0.0793 *** (0.0256)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	0.0005 (0.0540)	0.0171 (0.0163)	0.0619 *** (0.0165)	-0.0134 (0.0178)
Interstate Movers	0.0726 ** (0.0283)	0.1091 *** (0.0080)	0.0613 *** (0.0071)	0.0712 *** (0.0080)
Movers * Law	0.0204 (0.0407)	-0.0208 (0.0129)	-0.0092 (0.0112)	0.0114 (0.0131)
No. Obs.	14,441	156,665	130,901	122,372

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.9: The Impact of Non-Discrimination Laws on Wages of All Workers, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility, Cont'.

	Gay	Married	Cohabiting	Single
Panel C: Female Heads				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.1287 (0.1096)	0.0894 *** (0.0316)	-0.0605 ** (0.0267)	-0.0624 ** (0.0299)
Interstate Movers	-0.0423 (0.0529)	0.0857 *** (0.0156)	-0.0375 *** (0.0117)	-0.0080 (0.0131)
Movers * Law	-0.0180 (0.0850)	-0.0478 * (0.0257)	0.0608 *** (0.0185)	0.0549 ** (0.0219)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	0.0358 (0.0456)	-0.0021 (0.0190)	0.0600 *** (0.0168)	0.0295 (0.0200)
Interstate Movers	0.1165 *** (0.0234)	0.0704 *** (0.0097)	0.0808 *** (0.0075)	0.0872 *** (0.0089)
Movers * Law	0.0127 (0.0356)	-0.0174 (0.0153)	-0.0348 *** (0.0116)	-0.0514 *** (0.0147)
No. Obs.	14,340	162,450	132,400	125,786
Panel D: Female Partners				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.1122 (0.1026)	-0.0272 (0.0401)	0.0683 *** (0.0262)	-0.0624 ** (0.0299)
Interstate Movers	-0.0784 (0.0500)	-0.0189 (0.0200)	-0.0911 *** (0.0114)	-0.0080 (0.0131)
Movers * Law	0.0501 (0.0774)	0.0408 (0.0335)	0.0079 (0.0179)	0.0549 ** (0.0219)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	-0.0609 (0.0454)	-0.0061 (0.0259)	-0.0103 (0.0190)	0.0295 (0.0200)
Interstate Movers	0.0508 ** (0.0228)	0.0244 * (0.0131)	0.0741 *** (0.0084)	0.0872 *** (0.0089)
Movers * Law	-0.0143 (0.0346)	0.0064 (0.0216)	-0.0301 ** (0.0129)	-0.0514 *** (0.0147)
No. Obs.	14,581	111,220	119,912	125,786

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.10: The Impact of Non-Discrimination Laws on Wages of Fulltime Workers, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility

	Gay	Married	Cohabiting	Single
Panel A: Male Heads				
First-Stage – Work Fulltime				
Non-Discrim. Law	0.1689 *	0.0167	-0.0226	-0.0223
	(0.0884)	(0.0397)	(0.0266)	(0.0279)
Interstate Movers	-0.0006	0.1267 ***	0.0741 ***	0.0985 ***
	(0.474)	(0.0203)	(0.0118)	(0.0126)
Movers * Law	0.0431	0.0260	0.0443 **	0.0213
	(0.0689)	(0.0328)	(0.0183)	(0.0204)
Second-Stage - Wages of Fulltime Workers				
Non-Discrim. Law	0.0012	0.0551 ***	0.0196	0.0118
	(0.0443)	(0.0183)	(0.0138)	(0.0147)
Interstate Movers	-0.0112	0.0702 ***	0.0390 ***	0.0351 ***
	(0.0236)	(0.0094)	(0.0061)	(0.0067)
Movers * Law	-0.0304	-0.0262 *	-0.0282 ***	0.0053
	(0.0342)	(0.0152)	(0.0094)	(0.0109)
No. Obs.	14,099	106,855	144,968	122,372
Panel B: Male Partners				
First-Stage – Work Fulltime				
Non-Discrim. Law	0.1083	-0.0112	0.0518 *	-0.0223
	(0.0809)	(0.0297)	(0.0232)	(0.0279)
Interstate Movers	0.0665	0.1204 ***	0.0112	0.0985 ***
	(0.0423)	(0.0144)	(0.0101)	(0.0126)
Movers * Law	-0.0106	0.0339	0.0442 ***	0.0213
	(0.0615)	(0.0235)	(0.0158)	(0.0204)
Second-Stage - Wages of Fulltime Workers				
Non-Discrim. Law	0.0055	-0.0284 **	0.0199	0.0118
	(0.0442)	(0.0141)	(0.0131)	(0.0147)
Interstate Movers	0.0426	0.0685 ***	0.0223 ***	0.0351 ***
	(0.0234)	(0.0070)	(0.0056)	(0.0067)
Movers * Law	-0.0356	-0.0015	-0.0250 ***	0.0053
	(0.0335)	(0.0113)	(0.0088)	(0.0109)
No. Obs.	14,441	156,665	130,901	122,372

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.10: the Impact of Non-Discrimination Laws on Wages of Fulltime Workers, Age 18 – 64, Year 2001 – 2010, Controlling for Interstate Mobility

	Gay	Married	Cohabiting	Single
<u>Panel C: Female Heads</u>				
First-Stage – Work Fulltime				
Non-Discrim. Law	-0.0083 (0.0813)	0.0968 *** (0.0287)	0.0557 * (0.0240)	-0.0089 (0.0279)
Interstate Movers	0.0386 (0.0414)	0.1219 *** (0.0144)	0.0755 *** (0.0107)	0.1053 *** (0.0123)
Movers * Law	-0.0375 (0.0631)	-0.0837 *** (0.0232)	0.0129 (0.0166)	-0.0146 (0.0205)
Second-Stage - Wages of Fulltime Workers				
Non-Discrim. Law	-0.0064 (0.0395)	0.0054 (0.0156)	0.0080 (0.0128)	0.0256 * (0.0141)
Interstate Movers	0.0417 ** (0.0201)	0.0421 *** (0.0079)	0.0324 *** (0.0058)	0.0495 *** (0.0064)
Movers * Law	0.0227 (0.0311)	-0.0114 (0.0126)	-0.0139 (0.0089)	-0.0329 *** (0.0105)
No. Obs.	14,340	162,450	132,400	125,786
<u>Panel D: Female Partners</u>				
First-Stage – Work Fulltime				
Non-Discrim. Law	0.0494 (0.0767)	0.0215 (0.0379)	0.0362 (0.0249)	-0.0089 (0.0279)
Interstate Movers	0.1019 *** (0.0387)	0.0101 (0.0190)	0.0164 (0.0110)	0.1053 *** (0.0123)
Movers * Law	-0.0328 (0.0585)	-0.0184 (0.0316)	-0.0235 (0.0169)	-0.0146 (0.0205)
Second-Stage - Wages of Fulltime Workers				
Non-Discrim. Law	-0.0574 (0.0374)	0.0156 (0.0221)	-0.0079 (0.0144)	0.0256 * (0.0141)
Interstate Movers	0.0093 (0.0192)	0.0075 (0.0110)	0.0007 (0.0064)	0.0495 *** (0.0064)
Movers * Law	0.0162 (0.0290)	0.0102 (0.0183)	0.0035 (0.0097)	-0.0329 *** (0.0105)
No. Obs.	14,581	111,220	119,912	125,786

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

Table 5.11: The Impact of Non-Discrimination Laws on Wages of Interstate Movers in the Labor Force, Age 18 – 64, Year 2001 - 2010

	Gay	Married	Cohabiting	Single
Panel A: Male Heads				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.1708 *	-0.0753 ***	-0.0654 **	0.0712 ***
	(0.0971)	(0.0249)	(0.0309)	(0.0203)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	0.0173	-0.0141	-0.0003	-0.0289 **
	(0.0440)	(0.098)	(0.0141)	(0.0124)
No. Obs.	6,356	92,053	56,251	91,501
Panel B: Male Partners				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.0211	-0.0096	0.0215	0.0712 ***
	(0.0829)	(0.0255)	(0.0277)	(0.0203)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	0.0015	-0.0047	0.0307 *	-0.0289 **
	(0.0489)	(0.0103)	(0.0157)	(0.0124)
No. Obs.	6,300	91,241	53,564	91,501
Panel C: Female Heads				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.1958 **	-0.0161	-0.0451 *	0.0155
	(0.0987)	(0.0196)	(0.0245)	(0.0190)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	0.0235	0.0191	0.0394 ***	-0.0279 **
	(0.0382)	(0.0119)	(0.0149)	(0.0125)
No. Obs.	6,194	94,028	59,646	94,238
Panel D: Female Partners				
First-Stage - Labor Force Participation				
Non-Discrim. Law	-0.0143	-0.0354 *	0.0146	0.0155
	(0.0927)	(0.0186)	(0.0236)	(0.0190)
Second-Stage - Wages of All Workers				
Non-Discrim. Law	-0.0480	0.0056	-0.0153	-0.0279 **
	(0.0404)	(0.0121)	(0.0167)	(0.0125)
No. Obs.	6,070	94,419	56,466	94,238

Results for the first stage of the selection model are coefficient estimates, not marginal effects. Standard errors are in parentheses. Significant at the 1% level ***, significant at the 5% level **, significant at the 10% level*.

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