

The Wage Effects of Same-Sex Marriage Legalization

A Difference-in-Differences Analysis Using the American Community Survey

Anqi XU

R0960207

Thesis submitted to obtain the degree of
Master of Economics (Leuven)

Promoter: Prof. Edoardo Ciscato
Academic year 2024-2025

Copyright information:

*student paper as part of an academic education
and examination.*

*No correction was made to the paper
after examination.*

The Wage Effects of Same-Sex Marriage Legalization

A Difference-in-Differences Analysis Using the American Community Survey

This thesis examines how the legalization of same-sex marriage affected wages among same-sex cohabiting adults in the United States. Using American Community Survey (ACS) microdata from 2000–2019 and exploiting the staggered legalization across the U.S. states, I implement a difference-in-differences design that contrasts same-sex with different-sex cohabiting couples before and after state legalization. I found no meaningful participation response on the extensive margin for both men and women. However, the intensive margin shows small but precise, opposite-signed treatment effects on wage: roughly +3% for men and -2.3% for women. As a result, the pre-existing gay "wage penalty" and lesbian "wage premium" both shrank after legalization, but did not reverse. Heterogeneity by earner role is consistent with a household specialization mechanism.

Acknowledgements

I would like to express my gratitude to my thesis supervisor, Prof. Edoardo Ciscato, for his guidance, feedback and support throughout this research process.

Table of Contents

1	Introduction	1
2	Institutional Background	3
2.1	Evolution of legal recognition for same-sex relationships in the US . . .	3
2.2	Implications for labor markets	4
3	Literature Review	6
3.1	Impact of Marriage Equality	6
3.2	Wage Premium and Penalty among Sexual Minorities	7
3.3	Potential Channels Through Which Legalization May Affect Wages . . .	8
3.4	Gaps in the Literature	8
4	Data and Methods	10
4.1	Data	10
4.1.1	Data Source: American Community Survey (ACS)	10
4.1.2	Sample Selection	10
4.2	Empirical Strategy	11
4.2.1	Extensive Margin	12
4.2.2	Intensive Margin	12
5	Results	14
5.1	Descriptive Statistics	14
5.2	Extensive Margin	17
5.3	Intensive Margin	18
5.4	Heterogeneity Analysis and Mechanisms Assessment	20
6	Robustness Check	25
6.1	Model Assumptions and Diagnostic Checks	25
6.1.1	Parallel trends assumption	25
6.1.2	Homoskedasticity and Normality	27
6.2	Alternative specifications of the model	28
6.2.1	Restricting to Post Roll-Out Sample	28
6.2.2	Restricting to Prime Age Couples	28
7	Discussion and Conclusion	29
7.1	Summary of findings	29
7.2	Contribution to existing literature	29
7.3	Limitations of the Study	30
7.4	Suggestion for future research	30
	Bibliography	32

List of figures	35
List of tables	36
Appendix	37
A.1 Heterogeneity Analysis	37
A.2 Parallel Trend Assumption Assessment for Male Subsample	40
A.3 Robustness Check: Post 2005	40
A.4 Robustness Check: Prime Age Couples	43

Chapter 1

Introduction

The legal recognition of same-sex relationships has evolved rapidly within the United States over the period of 2004 to 2015. In 2004, Massachusetts became the first U.S. state to permit same-sex couples to marry, and over the next twelve years other states followed either through legislation, court rulings or ballot initiatives. Prior to the federal recognition, 34 states and the District of Columbia had legalized same-sex marriage. On 26 June 2015 the U.S. Supreme Court handed down its landmark decision in *Obergefell v. Hodges*, holding that the Fourteenth Amendment requires states to license and recognize marriages between two people of the same sex. This ruling made marriage equality a constitutional right nationwide, binding on all 50 states and the District of Columbia.

Marriage is not only a symbol of love, but also confers a host of economic benefits in the U.S., including spousal health insurance coverage, access to Social Security survivor benefits, favorable tax filing options, inheritance rights, and legal parenthood, etc. For same-sex couples who previously lacked access to these benefits, marriage equality could shift both perceived discrimination and intra-household economic dynamics, thereby affecting labor outcomes.

However, the impact of marriage equality on labor market outcomes, especially on wages, remains underexplored. Assessing this relationship is crucial for us to understand the broader implications of the marriage equality, as wages are the primary price of labor and a summary metric of economic opportunity. If marriage equality meaningfully changes how employers, households, and workers behave, those changes should ultimately be reflected in pay. Yet theory delivers ambiguous predictions. On the one hand, legal recognition can reduce discrimination, improve job matching, strengthen bargaining power, and expand access to employer benefits—forces that point to a wage premium. On the other hand, legalization also alters household specialization and insurance within couples; if one partner shifts time toward home production or accepts jobs with more benefits and less pay, an offsetting wage penalty for that partner could arise. Because channels from both directions plausibly operate at once, the net effect on wages is an empirical question.

Studying wage level is also policy-relevant, as it captures longer-run productivity, compensation structure, and returns to human capital—not just the extensive margin of participation. If marriage equality reduces pay gaps or changes the distribution of wages within same-sex households, that informs debates on anti-discrimination enforcement, benefit design, and the broader economic returns to legal inclusion.

The staggered rollout of same-sex marriage across U.S. states provides a quasi-experimental setting to identify these effects. It allows me to compare wage trajectories of individuals in same-sex versus different-sex households before and after legal-

ization, while absorbing time-invariant differences across places and common shocks over time. This design moves the discussion from correlation to credible causality.

Given the two-part nature of labor market outcomes, this study begins by examining whether the legalization of same-sex marriage influenced individuals' likelihood of labor force participation (the extensive margin). Conditional on employment, I then estimate the impact on wages (the intensive margin). The analysis reveals no statistically significant effect on the probability of employment for either gender. However, wage responses diverge by gender: same-sex cohabiting men experienced a modest increase of approximately 3 percentage points, while women faced a decline of about 2.3 percentage points. Consequently, the previously documented "wage penalty" for gay men and "wage premium" for lesbians both narrowed post-legalization, though neither fully reversed. A series of robustness checks support the credibility of these findings.

To explore potential mechanisms, I conduct heterogeneity analyses by earner role and parental status. For men, the positive wage effects are concentrated among primary earners, with little change observed for secondary earners—suggesting a reallocation of household labor in line with intrahousehold specialization theories. For women, wage declines occur across both earner roles, with more pronounced losses among secondary earners. This pattern is consistent with either enhanced household specialization or employer perceptions updating in response to shifting expectations about family formation.

This thesis is structured as follows. Chapter 2 gives an institutional background of the marriage equality in the US; Chapter 3 reviews the existing literature on the impact of marriage equality, wage premium and penalties, potential mechanism, and identifies the research gaps this study addresses; Chapter 4 describes the data and outlines the empirical methodology used in the analysis; Chapter 5 begins with descriptive statistics, presents the results of the econometric models, and conducts heterogeneity analysis to assess potential mechanisms; Chapter 6 conducts robustness checks to assess the validity of the main findings; Chapter 7 provides a broader discussion of the results, explores limitations, gives suggestions for future research, and finally concludes the thesis.

Chapter 2

Institutional Background

2.1 Evolution of legal recognition for same-sex relationships in the US

The legal status of same-sex relationships in the United States has evolved from criminalization to full marriage equality over a relatively short time. The timeline is crucial for identifying treatment and control periods in the empirical analysis. This section therefore highlights the key institutional changes that transformed same-sex relationships from criminal conduct to civil marriage, culminating in nationwide legalization.

Until the early 2000s, consensual same-sex intimacy remained a criminal offense in many states. That changed in *Lawrence v. Texas* (2003), where the U.S. Supreme Court struck down a Texas law that criminalized consensual sexual conduct between adults of the same sex (U.S., 2003). Writing for the majority, Justice Kennedy held that such laws violated the Due Process Clause of the Fourteenth Amendment and recognized that homosexuals have a fundamental right to engage in private sexual activity. Although *Lawrence* did not address marriage directly, it had justified discrimination and signaled that gay and lesbian relationships were protected by constitutional privacy and liberty rights.

Apart from decriminalization, several states created civil unions or domestic partnerships that offered some of the state-level benefits of marriage to same-sex couples. These arrangements were intentionally distinct from marriage and often provided only a subset of spousal rights. Vermont established the first civil union law in 2000. The civil union statute granted many of the same state benefits and protections as marriage. Connecticut became the second state to legalize civil unions in 2005, New Jersey, California and other states also followed and expanded spousal rights gradually (Lambda Legal, n.d.). While these institutions improved access to health insurance, inheritance and hospital visitation rights, they maintained a separate status that many advocates viewed as discriminatory and that offered no federal recognition.

The Massachusetts Supreme Judicial Court's decision in *Goodridge v. Department of Public Health* in 2003 marked the first time a state high court held that same-sex couples have a constitutional right to marry ("*Goodridge*", 2003). The ruling granted full marriage rights to same-sex couples. *Goodridge* catalyzed both progress and backlash: some states moved toward marriage equality, while others enacted constitutional amendments banning same-sex marriage.

Between 2004 and 2015, the United States became a patchwork of marriage laws. After Massachusetts began issuing marriage licenses in May 2004. Connecticut and Iowa followed by judicial decision in 2008 and 2009. Legislative action in Vermont

led to full marriage rights in 2009, making it the first state to enact marriage equality by statute (National Center for Lesbian Rights, 2017). By mid-2015, a majority of states—34 states and the District of Columbia—had legalized same-sex marriage (Movement Advancement Project, 2023).

This patchwork created large regional disparities: a same-sex couple could marry in Massachusetts or New York but would see their union unrecognized if they moved to Texas or Alabama. The variation across states and time provides the quasi-experimental conditions exploited in this thesis.

Even where states allowed same-sex marriage, couples initially lacked federal recognition because of the Defense of Marriage Act (DOMA). Enacted in 1996, DOMA defined marriage for federal purposes as a union between one man and one woman, thereby excluding same-sex spouses from federal benefits. In *United States v. Windsor* in 2013, the Supreme Court held that DOMA's exclusion of same-sex married individuals from the definition of "spouse" violated the Fifth Amendment ("*United States v. Windsor*", 2013). The *Windsor* decision required the federal government to recognize marriages valid under state law, thereby granting access to federal benefits including Social Security survivor benefits, federal tax filing status, immigration sponsorship, and spousal health insurance.

Despite the *Windsor* ruling, the patchwork remained: 14 states still banned same-sex marriage (Pew Research Center, 2019). On 26 June 2015 the U.S. Supreme Court decided *Obergefell v. Hodges*, holding that the Fourteenth Amendment requires all states to license marriages between two people of the same sex and to recognize marriages performed out of state ("*Obergefell v. Hodges*", 2015). The 5–4 decision legalized same-sex marriage nationwide. This ruling eliminated the legal disparities across states.

Although *Obergefell* established marriage equality nationwide, institutional differences persist. Some states have "zombie" constitutional amendments or statutes that would re-ban same-sex marriage if the Supreme Court reversed its decision. In addition, religious-exemption statutes in states such as Colorado, Michigan and New Mexico allow service providers to refuse participation in same-sex weddings. (Long, 2025) These ongoing legal debates highlight the fragility of marriage rights and reinforce why studying the economic impact of legalization is timely.

2.2 Implications for labor markets

Marriage rights in the United States confer numerous economic benefits. Married couples can access spousal health insurance through employers, file joint federal and state tax returns, benefit from Social Security survivor and spousal retirement benefits, and obtain simplified estate and inheritance transfers. Before *Windsor* and *Obergefell*, same-sex couples were excluded from these benefits even when their state recognized their marriages, leading to unequal healthcare coverage and higher tax burdens ("*Obergefell v. Hodges*", 2015; "*United States v. Windsor*", 2013). After federal recognition, married same-sex couples gained access to these benefits and could pool resources more effectively.

At the same time, legal recognition can reduce workplace discrimination and improve job matching. By signaling state endorsement and clarifying employers' legal

obligations, marriage recognition lowers the stigma costs of hiring and promotion, facilitates “outness” at work, and makes it easier for firms to administer spousal benefits uniformly. These forces operate on the demand side (less taste-based or statistical discrimination, clearer compliance) and the matching margin (greater mobility into better-paying firms and occupations), which should raise employment stability, access to full-time contracts, and ultimately wages for at least one partner in same-sex couples.

Moreover, legalization reshapes intra-household bargaining and specialization. By securing property rights, survivorship, and divorce remedies, marriage changes partners’ outside options and the gains from specialization, potentially inducing a reallocation of time toward home production for one partner and greater market work for the other. These adjustments may be amplified where legal recognition expands access to adoption and assisted reproduction, increasing the likelihood of childrearing. In households that form or expand after legalization, one partner may select into jobs with richer benefits but lower pay or reduce hours to provide care, while the other intensifies market work—patterns that could generate a wage penalty for the secondary earner even as the primary earner experiences a premium. Hence, the net wage effect is ambiguous ex ante and is expected to vary by earner role, gender, and parental status.

The incremental nature of U.S. marriage policy offers a unique opportunity to study the causal effects of legal recognition on wages. Variability across states and over time—ranging from decriminalization (*Lawrence*), to civil unions (Vermont 2000), to federal recognition (*Windsor*), and finally nationwide marriage equality (*Obergefell*)—provides quasi-experimental variation. This thesis leverages the different timing of recognition of same-sex marriage by different states in particular to examine whether access to marriage leads to wage premiums or penalties for individuals in same-sex couples and explores the heterogeneity and mechanisms through which legal recognition interacts with labor markets.

Chapter 3

Literature Review

This thesis relates to three strands of literature: First, studies documenting the social and economic impacts of marriage equality; second, research on wage premiums and penalties among sexual minorities; and lastly analyses of the mechanisms through which legal recognition might influence labor-market outcomes. After reviewing these strands, the chapter concludes by highlighting gaps in the existing literature and situating the contribution of this thesis.

3.1 Impact of Marriage Equality

Marriage-equality laws have prompted wide-ranging social and economic changes. Qualitative analyses highlight how feminist and queer scholars remain critical of the marital institution because marriage historically enforces gendered inequality; nevertheless, many acknowledge that marriage equality provides material benefits and may open space for broader political reforms (Bernstein, 2015; Boyd, 2013). These normative questions are important because they shape expectations of how marriage might alter economic behavior. For example, feminist critiques suggest that the traditional marital contract may reinforce gendered divisions of labor, whereas queer critiques highlight heterogeneity in same-sex relationships.

Empirical evidence uses quasi-experimental variation from staggered adoption across U.S. states to identify the broad social and economic impact of marriage equality. Legalization of same-sex marriage increases marriage take-up, health insurance coverage, access to care, healthcare utilization, and the migration of gay men into states that recognize same-sex marriage, with no effect on heterosexual marriage take-up (Carpenter, 2020; Carpenter et al., 2021; Marcén & Morales, 2022). Marital surplus and home ownership also rise sharply when and where same-sex marriage is legal, whereas domestic partnerships or civil unions generate no significant surplus (Delhomme & Hamermesh, 2021). For family formation, it is found that marriage equality increased adoptions by 9–18% and raised the probability that same-sex couples adopt children by 11–55% (Martin & Rodriguez, 2022).

Directly related to my thesis, there are several studies analyze the effect of marriage equality on labor supply and employment. Hansen et al. exploit variation in the timing of legalization across U.S. states and find that gay men do not alter hours worked in response to legalization, whereas lesbian women reduce annual labor supply by about six per cent; lesbians who earn less than their partners reduce hours by more than twice that amount, reallocating work hours to care labor (Hansen et al., 2020). Similarly, in the adoption-law literature, study indicates that after states allowed same-sex couples to adopt, the wage gap between lesbians and heterosexual women shrank

or inverted, while the wage gap for men did not change, supporting a parenthood hypothesis (Levendis & Lowen, 2023). Marriage equality increased employment and full-time contracts in same-sex couples, suggesting that access to marriage reduced occupational segregation and discrimination (Sansone, 2018). Legal environments also influence sorting across employers since same-sex behaving men earn less than comparable heterosexual men because they accept a compensating wage differential to work in tolerant firms (Martell, 2013).

Marriage equality seems to also shape social attitudes and well-being. Survey data show that support for same-sex marriage has grown rapidly over the past three decades (Hart-Brinson, 2016). An analysis of over one million implicit and explicit bias tests finds that anti-gay bias decreased more rapidly after states legalized same-sex marriage and that states that did not pass such laws experienced increased bias immediately after federal legalization (Ofosu et al., 2019). Long-run opinion trends show that support for same-sex marriage increased from 11.1% in 1988 to 66.7% in 2018, driven largely by period effects rather than cohort replacement (Twenge & Blake, 2021). Study shows factors such as personal contact with gay friends or family members and major political events have contributed to this rapid change of public opinion (Cao et al., 2017; Rosenfeld, 2017).

3.2 Wage Premium and Penalty among Sexual Minorities

The second strand of literature compares the wages or broader labour-market experiences of heterosexual and homosexual workers. Meta-analyses and cross-sectional studies consistently show that lesbian women tend to earn more than comparable heterosexual women, whereas gay men earn less than comparable heterosexual men. A widely cited meta-analysis of 31 studies finds that lesbians earn about 9% more while gay men earn about 11% less. (Klawitter, 2015). This reflects both discrimination and household specialization. A more recent synthesis emphasizes that these patterns may be changing: some new studies report wage parity or even higher wages for gay men, while lesbian premiums appear smaller (Drydakis & Zimmermann, 2020).

Early work shows that lesbians often experience a wage premium relative to heterosexual women. The premium is strongest for older white women and is not explained by the presence of children (Daneshvary et al., 2009). The premium is also larger for high earners and increases intrahousehold wage gap, which is consistent with household specialization hypothesis (Martell & Nash, 2020).

However, It is also found recently that cohabiting lesbians earn about 11% less than married heterosexual women, with a 24% penalty for young lesbians and no penalty for older lesbians (Martell, 2019). And recent change of legal environment may also narrow the wage gap for lesbians relative to heterosexual women (Levendis & Lowen, 2023). These mixed results highlight heterogeneity across age, marital status and temporal context.

Research on gay men generally reports a wage penalty. Field experiments and surveys show that partnered gay men are significantly less likely to be working full-time, whereas partnered lesbians are more likely to work full-time than heterosexuals (Arab-

sheibani et al., 2005; Black et al., 2003; US Census Bureau, 2020). Meta-analysis mentioned above estimates a penalty of 11% for gay men (Klawitter, 2015).

On the other hand, some recent studies suggest that wage parity may be emerging. Men in same-sex couples are up to 60% more likely to be unemployed than married heterosexual men, yet differences in hourly wages cannot be fully explained by occupational sorting (Bogusz & Gromadzki, 2024).

3.3 Potential Channels Through Which Legalization May Affect Wages

There are several proposed several mechanisms through which marriage equality might influence wages and labor supply.

First, marriage may increase **household specialization and intrahousehold bargaining**. Marriage equality encourages a division of labor within households, where one partner reduces labour supply to focus on home production. Evidence from heterosexual couples shows that men receive a wage premium after marriage, partly due to specialization. Whether similar dynamics apply to same-sex couples is less clear. Marriage equality also grants access to spousal benefits, joint health insurance, joint tax filing and social security survivor rights, which can affect labor supply and job choice.

Second, the legalization of same-sex marriage may influence labor outcome through **discrimination and signaling**. Marriage equality may reduce discrimination by signaling legitimacy to employers and customers. Also, wage premiums for lesbians in early years may be caused by signaling lower probability of having children and a greater willingness to work longer hours, therefore escape the motherhood penalties.

Third, Legal recognition of marriage can influence decisions around **parenthood**. Related to the household specialization channel and second signaling channel discussed above, this may change household responsibilities and enhanced bargaining power. The legal environment may make adoption and assisted reproduction easier than before for same-sex couples, thereby exposing them to the parenthood hypothesis and to motherhood penalties.

The last mechanism is selection: marriage equality could change the composition of couples observed in surveys if individuals with higher marriage surplus are more likely to formalise their union. However, in this study, I focus on cohabiting behaviors instead of marital status, therefore the effect of selection is limited.

3.4 Gaps in the Literature

Despite extensive literature, several gaps remain. First, the evidence on wage premiums and penalties is mixed and context-dependent. Some analyses report sizable premiums for lesbians and penalties for gay men, while others find parity or even reversals. These discrepancies arise not only from differing sample characteristics but also from the timing of surveys, legal environments and identification strategies. Because most studies rely on cross-sectional or short panels, little is known about how wage differentials evolve over the life course or whether gains and losses persist as couples

age and accumulate experience. A key contribution of the present thesis is to trace wage outcomes over a longer horizon and to examine whether observed premiums or penalties are transient or sustained.

Second, the direct effect of marriage equality on wages remains underexplored relative to other outcomes. Scholars have extensively studied marriage's impact on family formation, health, labor supply, migration, well-being and public opinion, but only a handful of papers explicitly examine wages or earnings dynamics. This thesis addresses that gap by using state-level variation of legalization timing to identify the causal effect of marriage equality on wages, controlling for confounding factors and exploring differences by gender, earner role and partnership status.

Lastly, while household specialization and discrimination are commonly cited mechanisms, the empirical evidence disentangling these channels is still limited. This study draws on large microdata sets that include partnership type, employment status and demographic characteristics and will explore heterogeneity by earner role and the presence of children to shed light on these mechanisms.

Chapter 4

Data and Methods

4.1 Data

4.1.1 Data Source: American Community Survey (ACS)

The analysis relies on microdata from the U.S. Census Bureau's American Community Survey (ACS), a nationally representative survey (Ruggles et al., 2025). Since full implementation in 2005, the ACS has provided a rich set of information on demographic, social, housing, and economic characteristics annually. Public-use microdata (PUMS) are released as anonymized person and household records.

Although it is not panel study, its large sample size provided high statistic power. The 1-year PUMS represent roughly one percent of the U.S. population, yielding sufficient precision for analyses of relatively small subpopulations (e.g., same-sex couples) (US Census Bureau, 2020). The household pointer variables allows me to identify co-resident partners and their sex. The ACS data also contains key labor outcome variables, including the labor income and labor supply. These make the ACS ideal dataset for my research design.

The main analysis window is 2000–2019, therefore, I used ACS data in testing phase from 2000 to 2004. They differ from the later years in sample size, and the data do not include geographic identifiers below the state and individuals in group quarters. (Herman, 2008) Because my identification relies only on state identifiers and I exclude group quarters, these differences are limited in relevance. However, to address potential concerns to the methodology and comparability, I report a robustness specification that restricts the sample to 2005–2019, containing only data after the official launch.

4.1.2 Sample Selection

To construct the analysis sample, I apply several selection criteria and data cleaning steps.

First, the sample is restricted to individuals surveyed from 2000 to 2019. As this period covers the period between 2003 (the first-mover state Massachusetts to first legalized same-sex marriage) and 2015 (the national wide supreme court ruling). At the same time, I exclude more recent waves, particularly those from 2020–2022, to ensure that the results are not affected by the pandemic.

In line with my research design, the unit of observation is individual living with a partner. I identify couples using spouse/partner pointer and classify couples as same-sex or different-sex cohabiting from the partner's reported sex. To focus on active

households in the labor market, I retain couples in which at least one partner is in prime age, defined as 25–54.

For data quality issues, observations with flagged sex and marital status are dropped from the sample. Because in earlier years, the Census Bureau recode the sex of same-sex married couple so that the records are consistent with the Defense of Marriage Act of 1996 (Dillender, 2015; Hansen et al., 2020).

Finally, all monetary variables, including savings amounts and household income, are deflated to 1999 dollars using the national level Consumer Price Index (CPI) to account for inflation over time, as the state level CPI is not available for all states (US Census Bureau, 2020).

4.2 Empirical Strategy

This study exploits the staggered legalization of same-sex marriage across U.S. states to identify its causal effect on outcomes for gays and lesbians using difference-in-differences (DiD). Following the literature, I compare individuals in same-sex cohabiting couples to observationally similar individuals in different-sex couples before vs. after a state legalizes same-sex marriage, while absorbing time-invariant state differences and common shocks with state and year fixed effects. DiD is used to isolate the effect of the marriage equality (Angrist & Pischke, 2008).

I split the sample into two groups - male and female, and analyze separately. As suggested by the literature, the two groups have different baseline traits in labour outcomes compared to their control group and they may response differently to this external shock.

The identification hinges on same-sex cohabiting individuals and different-sex cohabiting individuals forming valid counterfactuals once we condition on a rich set of pre-determined characteristics (Roth et al., 2023). Substantively, the legalization of same-sex marriage directly impacted the same-sex cohabiting households, while different-sex cohabiting households were not directly affected by it, yet both groups are drawn from the same birth cohorts, live in the same macroeconomic, informational and cultural environment. Intuitively, the different-sex cohabiting individuals could provide the counterfactual path of labor outcomes that the same-sex cohabiting individuals would have followed in the absence of the shock, after controlling for a rich set of observables capturing demographics, household composition, human capital and labor-market attachment.¹

However, there might be unobserved and time-varying differences between the two groups, and that might lead to a different pre-trend. Therefore, I assessed the (conditional) parallel trend assumption in Section 6.1.1, and found no evidence of pre-treatment divergence, supporting the design.

¹ Concretely, we condition on: age and age²; number of children, presence of child under 5; race and ethnicity; education level

4.2.1 Extensive Margin

I used a two-part approach distinguishing the impact on the extensive and intensive margins. The decision to enter or exit the labor market might be influenced by household specialization, access to partner's health insurance, social norms, and reservation wage. On the other hand, the determinants on wage level, conditional on working, are conceptually different, potentially include human capital accumulation, job sorting, promotions, and workplace discrimination. Given that the impact of SSM legalization may operate through both labor force participation and wage adjustments, I estimate and interpret the treatment effects on these two margins separately without aggregating them into a single total effect. (Chen & Roth, 2024; McKenzie, 2023),

First, a probit DiD model is used to analyze the extensive margin, that is, the probability of participating in the labor market. The ACS dataset reports weeks worked last year in intervals and usual weekly hours worked. I approximate annual labor supply in the preceding year by multiplying the midpoint of the reported interval by the usual hours worked per week.

I model the binary outcome $D_{ist} = 1\{\text{labor_supply}_{ist} > 0\}$ using a probit model to analyze the impact of marriage equality on the participation for same-sex cohabiting individuals compared to different-sex cohabiting individuals:

$$\Pr(D_{ist} = 1 \mid X_{it}) = \Phi\left(\theta SS_i + \lambda L_{st} + \delta_t + \sigma_s + \beta_{\text{DiD}}^{(E)}(SS_i \times L_{st}) + X'_{ist}\gamma\right). \quad (4.1)$$

via a latent index:

$$\begin{aligned} D_{it}^* &= \theta SS_i + \lambda L_{st} + \delta_t + \sigma_s + \beta_{\text{DiD}}^{(E)}(SS_i \times L_{st}) + X'_{ist}\gamma + \varepsilon_{ist}, \\ D_{ist} &= \mathbf{1}\{D_{it}^* > 0\}, \quad \varepsilon_{ist} \sim \mathcal{N}(0, 1). \end{aligned} \quad (4.2)$$

where $\Phi(\cdot)$ is the standard normal CDF; SS_i indicates if the respondent has a same-sex cohabiting partner, L_{st} denotes if the respondent lives in a state where same-sex marriage is legalized, δ_t is the year fixed effects, σ_s is the state fixed effects, and X_{ist} is a vector of controls, which include age, age squared, number of children, presence of a child under 5 years old, race, ethnicity and education. The variance $\text{Var}(\varepsilon_i)$ is normalized to 1, so the coefficients are on latent scale. Therefore, I also report average marginal effects (AMEs) in probability units for interpretability. The standard errors are clustered at the state level.

4.2.2 Intensive Margin

For the intensive margin, the analysis is focus on the individuals with positive wages. The wage is computed as the ratio of each respondent's total pre-tax wage and salary income to the total hours of labor supplied in the preceding year. All monetary values are deflated to 1999 dollars.

I restrict the sample to individuals with positive wages and estimate the impact on the level of wages using OLS:

$$\ln(W_{ist}) = \theta SS_i + \lambda L_{st} + \sigma_s + \delta_t + \beta_{\text{DiD}}^{(I)}(SS_i \times L_{st}) + X'_{ist}\gamma + \varepsilon_{ist}, \quad i \in \mathcal{A}, \quad (4.3)$$

where $\mathcal{A} \equiv \{i : D_{ist} = 1\}$. Other notations are same as indicated before. Standard errors are clustered at the state level.

This two-part structure accommodates the possibility that SSM legalization affects participation and wage levels differently.

Chapter 5

Results

5.1 Descriptive Statistics

Tables 5.1 and 5.2 report unweighted summary statistics for the analysis sample by (i) the legal status of marriage equality in the state–year (Not legal vs. Legal) and (ii) couple type (different-sex cohabiting vs. same-sex cohabiting), separately for men and women. Monetary variables are normalized to 1999 U.S. dollars (as in the rest of the paper).

For men (Table 5.1), average hourly wages are similar for heterosexual and gay men in states without marriage equality. In legal states, gay men earn slightly more per hour on average. Despite the modest wage gap in legal states, gay men work fewer hours than heterosexual men both before and after legalization, which translates into lower annual labor income. Gay men have substantially fewer children, and are more educated on average. Home ownership rates and non-labor income are also higher for gay men.

For women (Table 5.2), lesbians earn more per hour than heterosexual women in both policy environments and work substantially more hours. Consequently, annual labor income is higher for lesbians. As with men, lesbians have fewer children, has slightly higher education attainment, has higher ratio of being a home owner and higher non-labor income.

Overall, the descriptive patterns are consistent with well-documented differences in composition between same-sex and different-sex couples: same-sex individuals are more highly educated and have fewer children; lesbians have higher wages and work more hours than heterosexual women, while gay men work fewer hours and have lower annual labor income than heterosexual men.

These descriptive patterns highlight notable baseline differences between same-sex cohabiting and different-sex cohabiting individuals, underlining the importance of controlling for covariates and using appropriate empirical strategies. In particular, the same-sex cohabiting women report much higher wage rate on average. While this difference does not necessarily violate the Difference-in-Differences identification strategy, it underscores the importance of carefully considering the parallel trends assumption in the subsequent analysis.

	Not legal		Legal	
	Heterosexual	Gay	Heterosexual	Gay
Men				
Wage rate	22.82 (52.79)	22.09 (33.27)	24.59 (59.91)	25.74 (55.10)
Labor supply (hours)	1996.69 (860.17)	1771.09 (918.48)	2016.55 (833.40)	1797.99 (917.82)
Wage & salary income	41115.12 (47378.34)	36787.05 (49929.77)	45810.90 (54608.68)	43643.84 (58298.37)
Age	43.22 (9.77)	41.73 (10.37)	43.37 (9.86)	42.70 (11.08)
Number of children (HH)	1.35 (1.23)	0.19 (0.64)	1.38 (1.25)	0.23 (0.71)
Presence of Child under 5	0.22 (0.42)	0.03 (0.18)	0.23 (0.42)	0.04 (0.19)
<i>Race/Ethnicity</i>				
White	0.82 (0.38)	0.82 (0.39)	0.80 (0.40)	0.80 (0.40)
Black	0.07 (0.25)	0.05 (0.22)	0.06 (0.24)	0.05 (0.22)
Asian	0.05 (0.21)	0.04 (0.19)	0.07 (0.25)	0.06 (0.24)
Other race	0.06 (0.24)	0.09 (0.29)	0.07 (0.26)	0.09 (0.28)
Hispanic	0.12 (0.32)	0.20 (0.40)	0.14 (0.35)	0.17 (0.37)
<i>Education</i>				
Less than high school	0.09 (0.29)	0.10 (0.31)	0.08 (0.27)	0.05 (0.21)
High school diploma	0.36 (0.48)	0.24 (0.43)	0.33 (0.47)	0.22 (0.41)
Some college	0.23 (0.42)	0.23 (0.42)	0.22 (0.41)	0.22 (0.41)
College graduate	0.20 (0.40)	0.25 (0.44)	0.23 (0.42)	0.29 (0.45)
Graduate degree	0.12 (0.33)	0.17 (0.37)	0.15 (0.36)	0.22 (0.42)
Home owner	0.21 (0.41)	0.34 (0.47)	0.26 (0.44)	0.35 (0.48)
Non-labor income	2881.18 (13339.98)	2823.34 (20851.85)	2753.29 (13923.47)	3048.73 (18779.40)
N	4,480,277	45,748	2,543,405	44,444

Table 5.1: Descriptive statistics (Men). Means reported; standard deviations in parentheses. Monetary values in 1999 USD.

	Not legal		Legal	
	Heterosexual	Lesbian	Heterosexual	Lesbian
Women				
Wage rate	16.50 (46.09)	19.54 (35.22)	18.33 (53.24)	20.44 (70.07)
Labor supply (hours)	1317.57 (957.85)	1696.77 (900.23)	1380.12 (963.98)	1728.77 (896.08)
Wage & salary income	20185.55 (26803.46)	31323.79 (39966.40)	24004.10 (33070.95)	33141.60 (42267.85)
Age	40.82 (9.17)	41.27 (10.43)	41.01 (9.22)	41.31 (10.81)
Number of children (HH)	1.35 (1.23)	0.52 (0.95)	1.38 (1.25)	0.59 (0.98)
Presence of Child under 5	0.22 (0.42)	0.09 (0.28)	0.23 (0.42)	0.10 (0.30)
<i>Race/Ethnicity</i>				
White	0.82 (0.39)	0.84 (0.36)	0.79 (0.40)	0.81 (0.40)
Black	0.06 (0.24)	0.07 (0.25)	0.05 (0.23)	0.08 (0.26)
Asian	0.06 (0.23)	0.02 (0.15)	0.08 (0.27)	0.04 (0.19)
Other race	0.06 (0.24)	0.07 (0.25)	0.07 (0.26)	0.08 (0.27)
Hispanic	0.12 (0.33)	0.11 (0.31)	0.15 (0.35)	0.14 (0.34)
<i>Education</i>				
Less than high school	0.08 (0.27)	0.05 (0.22)	0.06 (0.24)	0.04 (0.19)
High school diploma	0.34 (0.47)	0.25 (0.44)	0.27 (0.44)	0.24 (0.42)
Some college	0.25 (0.43)	0.25 (0.43)	0.24 (0.43)	0.25 (0.43)
College graduate	0.22 (0.41)	0.25 (0.43)	0.26 (0.44)	0.25 (0.44)
Graduate degree	0.11 (0.32)	0.20 (0.40)	0.17 (0.37)	0.23 (0.42)
Home owner	0.21 (0.41)	0.29 (0.45)	0.26 (0.44)	0.36 (0.48)
Non-labor income	1257.52 (8463.98)	2011.91 (22592.86)	1254.97 (9064.49)	2143.83 (16963.95)
N	4,480,277	39,952	2,543,405	42,934

Table 5.2: Descriptive statistics (Women). Means reported; standard deviations in parentheses. Monetary values in 1999 USD.

5.2 Extensive Margin

The extensive margin captures the participation of labor market. For both men and women, I find a negative but statistically insignificant treatment effect, suggesting no clear evidence of behavioral change in participation.

For men, same-sex couples are less likely to participate than different-sex couples, and this baseline gap actually grows in magnitude when richer controls are added. (See Table 5.3) The interaction between same-sex status and legalization—is small and not robust: it is negative and statistically significant only in the most parsimonious model (col. 1), but flips sign or becomes indistinguishable from zero once demographics, education, and state/year fixed effects are included (cols. 2–4). In probability space (See table 5.5), the average marginal effects (AME) is at most 0.6% in the leanest model and fades to roughly 0.2% in the preferred, fully controlled specification, with confidence intervals spanning zero. Substantively, there is no credible evidence that marriage equality altered men’s participation among same-sex couples relative to comparable different-sex men.

Table 5.3: Labor Supply (Participation), Men — Probit DiD

	Dependent variable: <i>labor supply participation</i>			
	(1)	(2)	(3)	(4)
Same-sex couple	-0.23*** (0.02)	-0.24*** (0.02)	-0.31*** (0.02)	-0.31*** (0.02)
Legalization	0.01 (0.01)	0.02** (0.01)	-0.01* (0.01)	0.00 (0.01)
Same-sex × Legalization	-0.03** (0.01)	0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
Log-likelihood	-1,887,237	-1,718,137	-1,656,498	-1,643,950
Pseudo R^2	0.001	0.090	0.123	0.129
Observations	7,113,874	7,113,874	7,113,874	7,113,874

Notes: standard errors clustered by state in parentheses;
sym* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

For women, same-sex couples start out more likely to participate than different-sex couples; this positive baseline difference shrinks as controls are added. (See Table 5.4) The coefficient of the interaction term is modestly negative in simple model (col. 1) but becomes statistically and economically negligible once demographics, education, and state/year fixed effects are included (col. 2-4). The interaction average marginal effects (AMEs) tell the same story: about -0.7% in the sparse specifications, converging to 0 with richer controls, and becoming statistically insignificant. Thus, there is no robust differential change found in women’s participation for same-sex couples after legalization relative to the control group.

Table 5.4: Labor Supply (Participation), Women — Probit DiD

	Dependent variable: <i>labor supply participation</i>			
	(1)	(2)	(3)	(4)
Same-sex couple	0.43*** (0.02)	0.31*** (0.01)	0.26*** (0.02)	0.27*** (0.02)
Legalization	0.02* (0.01)	0.05*** (0.01)	-0.00 (0.01)	0.01* (0.01)
Same-sex × Legalization	-0.02* (0.01)	-0.02 (0.02)	-0.00 (0.01)	-0.01 (0.01)
Demographic controls	No	Yes	Yes	Yes
Education	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
Log-likelihood	-3,802,848	-3,652,471	-3,538,881	-3,515,041
Pseudo R^2	0.001	0.040	0.070	0.076
Observations	7,106,568	7,106,568	7,106,568	7,106,568

Notes: standard errors clustered by state in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5.5: Interaction AME: Effect of Legalization for Same-Sex vs. Different-Sex Couples

	(1)	(2)	(3)	(4)
Men	-0.006** (0.003)	0.003 (0.002)	-0.003 (0.002)	-0.002 (0.002)
Women	-0.007** (0.003)	-0.007** (0.003)	-0.000 (0.003)	-0.002 (0.003)

Notes: Columns (1)–(4) align with the specifications in Tables 5.3–5.4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, the DiD estimates imply no meaningful effect of marriage equality on the extensive margin of labor supply for either gender: all AMEs are well under one percentage point and statistically insignificant. This pattern shows that marriage-equality legalization did not affect labor-force participation among same-sex couples relative to different-sex couples.

5.3 Intensive Margin

The intensive margin captures within-employment wage adjustment. Conditioning on employed individuals receiving positive wages, I estimate OLS DiD models of log wages with the same covariates and state/year fixed effects as in the extensive margin analysis.

In the absence of legalization, same-sex cohabiting men earn less than different-sex cohabiting men. (See Table 5.6) After legalization, the relative wage of same-sex men

risers: the interaction term is consistently positive and statistically precise, attenuating as controls and fixed effects absorb composition and common shocks but remaining significant in the preferred specification. Quantitatively, the preferred estimate implies a 3% relative wage increase for same-sex men. The evidence points to a small but meaningful positive intensive-margin response for men.

Table 5.6: Wage Intensive Margin (ln wage), Men — OLS DiD

	Dependent variable: ln(wage)			
	(1)	(2)	(3)	(4)
Same-sex couple	-0.091*** (0.018)	-0.004 (0.013)	-0.081*** (0.008)	-0.096*** (0.006)
Legalization	0.050*** (0.013)	0.052*** (0.011)	0.012 (0.008)	-0.002 (0.005)
Same-sex × Legalization	0.080*** (0.016)	0.063*** (0.014)	0.033*** (0.011)	0.029** (0.012)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.001	0.078	0.224	0.242
Observations	6,121,193	6,121,193	6,121,193	6,121,193

Notes: robust SEs clustered by state in parentheses; Coefficients are in log points. Approximate % effect is $100 \cdot (\exp(\beta) - 1)$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Before legalization, same-sex women earn more than different-sex women (See Table 5.7). Post-legalization, their relative wage declines: the coefficient of the interaction term is negative and precisely estimated throughout, with magnitude reduced once demographics, education, and state/year FE are included but remaining statistically significant in the preferred model. The preferred estimate corresponds to a 2.3% relative wage decrease for same-sex women. As with men, the legalization main effect is near zero once fixed effects are included, indicating the movement is not a common shock but a differential shift specific to same-sex women. Thus, the intensive-margin response for women is negative and robust, albeit modest in size.

Table 5.7: Wage Intensive Margin (ln wage), Women — OLS DiD

	Dependent variable: ln(wage)			
	(1)	(2)	(3)	(4)
Same-sex couple	0.161*** (0.012)	0.158*** (0.010)	0.078*** (0.006)	0.066*** (0.003)
Legalization	0.088*** (0.012)	0.090*** (0.011)	0.018** (0.009)	0.000 (0.005)
Same-sex \times Legalization	-0.082*** (0.007)	-0.061*** (0.008)	-0.024*** (0.006)	-0.023*** (0.006)
Demographic controls	No	Yes	Yes	Yes
Education	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.004	0.049	0.199	0.217
Observations	5,197,634	5,197,634	5,197,634	5,197,634

Notes: robust SEs clustered by state in parentheses; Coefficients are in log points. Approximate % effect is $100 \cdot (\exp(\beta) - 1)$. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Taken together, marriage-equality legalization is associated with **opposite-signed intensive-margin wage adjustments: +3%** for men and **2.3%** for women, conditional on employment and after rich controls and fixed effects. These magnitudes are economically small but statistically significant. As a results, for both men and women, after marriage equality, the baseline wage gap (wage penalty for men and wage premium for women) shrank, but not reversed.

Given that no treatment effect is found on the extensive margin, these results suggest that any behavioral adjustment to legalization operates primarily **within jobs** rather than through entry/exit from employment.

5.4 Heterogeneity Analysis and Mechanisms Assessment

Motivated by the household specialization channel, I split the employed sample into primary earners and secondary earners within households and re-estimate the OLS DiD wage models with the same covariates and fixed effects. The parameter of interest remains the Same-sex \times Legalization coefficient, which captures the differential post-legalization change in wages for same-sex couples relative to different-sex couples, now separately by household earner role.

Among men, the treatment effect is concentrated among primary earners. In the preferred specification with controls and state/year fixed effects, an around +3.0% relative wage increase is found for primary earners. (See Table 5.8 and 5.9) Whereas the effect on secondary earner is statistically indistinguishable from zero. This indicates that the overall positive intensive-margin result for men is driven by primary earners, not secondary earners.

Legal recognition reduces transaction costs inside the household and unlocks risk-sharing via pooled benefits. With a more secure household budget, couples can re-optimize. Primary earners can invest more on market work, target career-track roles, or accept riskier or higher-return matches. These changes show up as higher wages through promotions, better matches, or steeper pay ladders.

This evidence support the household specialization hypothesis: households tilt market effort (and wage growth) toward the primary earner, while the secondary earner prioritizes non-wage job attributes.

Table 5.8: Wage Intensive Margin (ln wage), Men — Primary Earner, OLS DiD

	Dependent variable: ln(wage)			
	(1)	(2)	(3)	(4)
Same-sex couple	0.032 (0.020)	0.079*** (0.014)	-0.027*** (0.009)	-0.044*** (0.006)
Legalization	0.061*** (0.014)	0.064*** (0.011)	0.023*** (0.008)	-0.003 (0.005)
Same-sex × Legalization	0.075*** (0.017)	0.059*** (0.014)	0.034*** (0.012)	0.030** (0.013)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.002	0.096	0.273	0.293
Observations	4,674,971	4,674,971	4,674,971	4,674,971

Notes: OLS on employed sample; SEs clustered by state; person weights applied. Coefficients in log points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5.9: Wage Intensive Margin (ln wage), Men — Secondary Earner, OLS DiD

	Dependent variable: ln(wage)			
	(1)	(2)	(3)	(4)
Same-sex couple	0.055*** (0.015)	0.089*** (0.012)	0.053*** (0.010)	0.038*** (0.007)
Legalization	0.065*** (0.011)	0.064*** (0.010)	0.033*** (0.009)	0.003 (0.008)
Same-sex × Legalization	0.029** (0.014)	0.026** (0.012)	0.005 (0.010)	0.004 (0.010)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.002	0.034	0.104	0.123
Observations	1,268,483	1,268,483	1,268,483	1,268,483

Notes: OLS on employed sample; SEs clustered by state; person weights applied. Coefficients in log points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

However, among women, the treatment effect is negative and statistically significant in both earner groups but larger for secondary earners. In the preferred specification, I found a 1.4% decrease for primary earner and a 3.6% decrease for secondary earners. (See Table 5.10 and 5.11)

This significant wage drop for both earner roles might be caused by employers updating signaling. As the wage premium in earlier years could be explained by social norm signaling and absence of motherhood penalties. With legalization of same-sex marriage employers may expect higher probability of family formation, leave, and parenthood among partnered women. As a results, wages might adjust downward and promotions might be hindered regardless of earner role.

At the same time, this difference in effect magnitude between primary and secondary earners also supports the household specialization hypothesis. Within female couples, the household may tilt career investment toward one partner while the secondary earner trades off wage for flexibility or amenities, consistent with the larger negative effect for secondary earners.

Table 5.10: Wage Intensive Margin (ln wage), Women — Primary Earner, OLS DiD

	Dependent variable: ln(wage)			
	(1)	(2)	(3)	(4)
Same-sex couple	0.106*** (0.012)	0.115*** (0.011)	0.044*** (0.007)	0.030*** (0.005)
Legalization	0.108*** (0.012)	0.109*** (0.011)	0.032*** (0.009)	0.001 (0.007)
Same-sex × Legalization	-0.079*** (0.008)	-0.058*** (0.008)	-0.017** (0.006)	-0.014** (0.006)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.006	0.053	0.247	0.273
Observations	1,902,473	1,902,473	1,902,473	1,902,473

Notes: OLS on employed sample; SEs clustered by state; person weights applied. Coefficients in log points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5.11: Wage Intensive Margin (ln wage), Women — Secondary Earner, OLS DiD

	Dependent variable: ln(wage)			
	(1)	(2)	(3)	(4)
Same-sex couple	0.080*** (0.012)	0.085*** (0.010)	0.014** (0.005)	0.003 (0.005)
Legalization	0.068*** (0.013)	0.069*** (0.012)	0.008 (0.009)	-0.000 (0.005)
Same-sex × Legalization	-0.086*** (0.008)	-0.067*** (0.009)	-0.035*** (0.007)	-0.036*** (0.007)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.002	0.043	0.161	0.179
Observations	3,118,156	3,118,156	3,118,156	3,118,156

Notes: OLS on employed sample; SEs clustered by state; person weights applied. Coefficients in log points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The heterogeneity analysis clarifies where the intensive-margin adjustments occur: same-sex men's wage gains are mainly driven by primary earners, while same-sex women's wage losses are driven by both primary and secondary earners, with larger loss for secondary earners. The pattern supports the intrahousehold tradeoff channel for both genders. For lesbians, signaling might also play a role.

In order to probe within household specialization, motivated by the parenthood hypothesis, I next examine heterogeneity by the presence of children, re-estimating the

DiD models separately for households with and without children. If a parenthood channel is operative, effects should be stronger in households with children—especially more negative for female.

Contrary to that prediction, the positive treatment effects for men are concentrated among households with children, whereas the negative treatment effects for women arise mainly among childless households (Appendix Tables 1–2). This pattern does not support household specialization or contemporaneous “parenthood penalty” explanation for women’s wages.

A plausible explanation is selection into parenthood after legalization. By removing legal barriers to adoption/parental recognition, marriage equality likely made parenthood more feasible for couples with stronger labor-market prospects. If higher-earning or faster-growing couples were more likely to become parents post-legalization, then the “with children” group will mechanically contain couples on steeper wage trajectories. This is consistent with men’s larger gains in the “with children” group. At the same time, the “without children” group includes couples who either remain childless or are still pre-parenthood, among whom women’s wages show the relative decline. In short, the kids heterogeneity appears descriptive of sorting rather than causal evidence of a parenthood mechanism.

Chapter 6

Robustness Check

6.1 Model Assumptions and Diagnostic Checks

To further assess the validity of the estimated treatment effects, I examine key assumptions underlying the empirical models used. This includes testing for parallel trends in the Difference-in-Differences framework, and checking for heteroskedasticity, normality assumptions in nonlinear specifications. These diagnostic checks help to ensure that the reported results are not driven by violations of core modeling assumptions.

6.1.1 Parallel trends assumption

A key identifying assumption of the Difference-in-Differences framework is that, absent the marriage equality, the treatment and control groups would have followed parallel trends in wage level. Here, I report the pre-trend analysis for female subsample. The patterns for male are similar, see Appendix A.2.



Figure 6.1: Raw Trend with confidence intervals

Figure 6.1 plots the raw mean wage trends with 90% confidence intervals for same-sex and different-sex cohabiting individuals. Prior to the legalization of marriage equality, the two groups exhibit relatively stable and non-intersecting trajectories, same-sex cohabiting women showing a clear advantage on average wage relative to different-

sex counterpart. The wage gap shrank with relative year increases, and disappeared around 8 years after legalization of same-sex marriage.

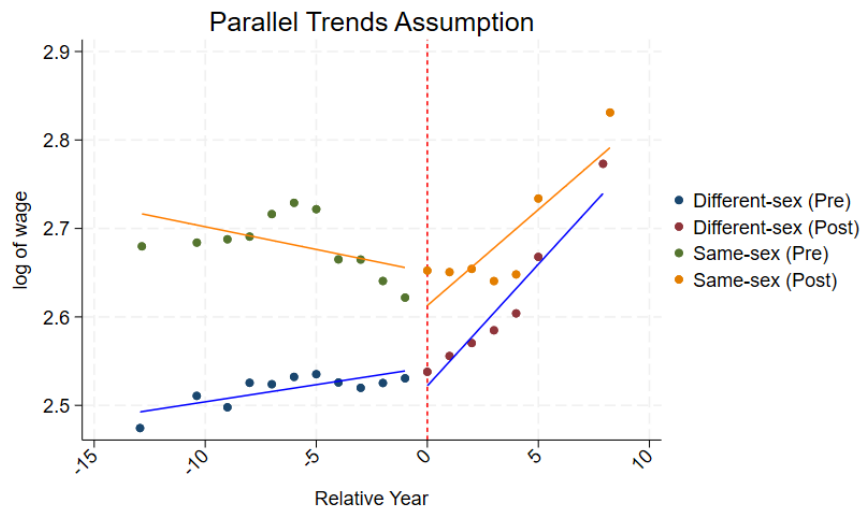


Figure 6.2: Linear Trend Without Controls

Next, Figure 6.2 compares fitted linear trends before and after the legalization shock without conditioning on covariates. While the visual evidence points to broadly similar slopes across groups in the pre-treatment period, some divergence appears both pre- and post-legalization.

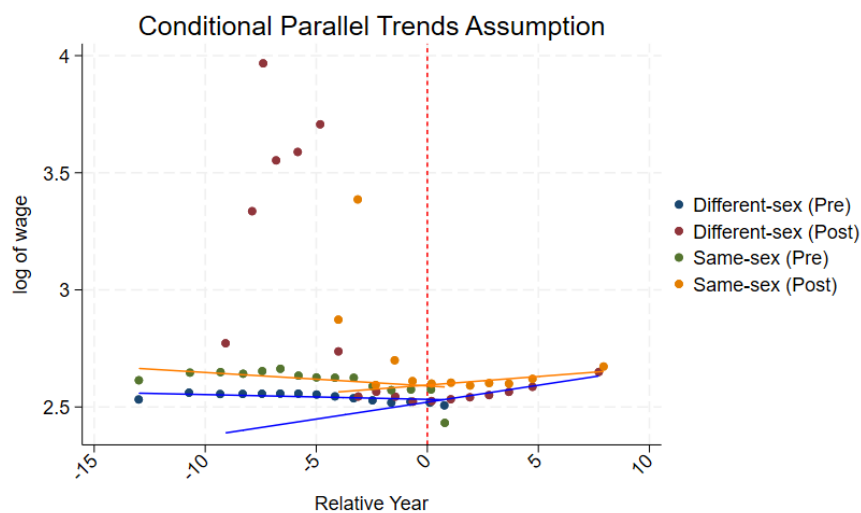


Figure 6.3: Linear Trend With Controls

Finally, Figure 6.3 reports conditional trends after adjusting for the same set of covariates included in the main DiD regressions (age, education, race, presence of children, etc.). Once these observables are accounted for, the pre-treatment slopes align more closely, supporting the validity of the conditional parallel trends assumption. This provides reassurance that the estimated DiD effects can be interpreted as causal responses to the legalization of marriage equality.

6.1.2 Homoskedasticity and Normality

To examine the validity of the homoskedasticity assumption in the Probit model, I estimate a heteroskedastic Probit specification using the `hetprobit` command in Stata. The variance of the error term is modeled as a function of several covariates, including age, age squared, number of children, presence of child under 5, race, and education, while clustering standard errors at the state level. I then conduct a Wald test of the joint significance of these variables in the variance equation. The test rejects the null hypothesis of homoskedasticity, with a test statistic of $\chi^2(10) = 56.43$ and a p-value of 0.00. This indicates strong evidence of heteroskedasticity, and the assumption of constant variance in the Probit model is rejected.

The probit model also relies on the assumption that the latent error term follows a normal distribution. To assess the validity of this assumption, I implement a score test for probit model proposed by Davidson. (Davidson & MacKinnon, 1984; Silvapulle & Silvapulle, 1995) The procedure involves augmenting the probit model with squared and cubic terms of the linear index function $X\beta$. Under the null hypothesis of normality, these higher-order terms should not be jointly significant.

I estimate the augmented probit model and conduct a joint test of the coefficients on the added terms. The test yields a test statistic of $\chi^2(2) = 664.42$ with a corresponding p-value of 0.00. This result implies that I reject the null hypothesis of normality, suggesting that the standard probit model is not appropriately specified and the normality assumption appears to be violated in our setting.

To remedy this, I re-estimate the extensive margin using a linear model instead of probit model. The estimated treatment effects remain qualitatively similar, reporting very small and not statistically significant coefficients of the interaction term for both men and women, except for the baseline model that did not control for any covariate (See Table 6.1).

Table 6.1: Linear DID Estimates of Labor Supply (Extensive Margin)

	Women				Men			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Same-sex \times Legalized	-0.007** (0.003)	-0.003 (0.003)	0.002 (0.003)	0.001 (0.003)	-0.006** (0.003)	0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)
Controls	No	+demographic	+education	+state and year FE	No	+demographic	+education	+state and year FE
R^2	0.001	0.044	0.077	0.082	0.000	0.069	0.086	0.090
N	7,106,568	7,106,568	7,106,568	7,106,568	7,113,874	7,113,874	7,113,874	7,113,874

Notes: Dependent variable is an indicator for positive labor supply. Estimates from linear probability models. Standard errors clustered at the state level are reported in parentheses.

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

Overall, the linear probability estimates confirm that marriage equality did not significantly alter labor market participation for either men or women.

6.2 Alternative specifications of the model

6.2.1 Restricting to Post Roll-Out Sample

As mentioned in previous chapter, ACS sampling and questionnaire design stabilize from 2005 onward. Restricting to 2005–2019 enhances cross-year comparability and removes pre-roll-out noise. I therefore re-estimate the extensive- and intensive-margin DiD models on the 2005+ sample only.

Overall, limiting the sample to 2005–2019 leaves the paper's conclusions intact: no economically meaningful participation effects, and opposite-signed wage effects by gender of modest size on the intensive margin. (See Appendix A.3) On the extensive margin, the restricted-sample AMEs are tiny and insignificant across all models, which is consistent with main results. On the intensive margin, The gender-opposite wage pattern is robust. I found a roughly +2.8% relative wage increase for same-sex cohabiting men and a roughly about a -2.2% relative change for same-sex cohabiting women post-legalization. These magnitudes are close to the main results, confirming that legalization is associated with small but precise within-employment wage adjustments—up for men, down for women.

6.2.2 Restricting to Prime Age Couples

While in the main analysis, the sample is restricted to compabiting partners with at least 1 in prime age. Here restricting to couples both in prime age reduces life-cycle heterogeneity and focuses on peak working years. I re-estimate the DiD models on this subsample with the same covariates and state/year FE. Overall, the pattern is consistent with main results. (See Appendix A.4)

On the extensive margin, there is no participation response found among prime-age couples for both women and men. the AMEs are tiny and insignificant through all specifications. On the intensive margin, the gender-opposite wage pattern persists. In the preferred spec, the coefficient of interaction term is +0.026 for men and -0.030 for women, both precisely estimated. Thus, among prime-age couples, legalization is associated with modest within-employment wage gains for same-sex cohabiting men and losses for same-sex cohabiting women, relative to different-sex controls.

Chapter 7

Discussion and Conclusion

7.1 Summary of findings

This thesis asked whether marriage equality translated into a wage premium or penalty for individuals in same-sex couples, using staggered legalization across U.S. states and ACS microdata (2000–2019). The evidence points to no meaningful participation response (extensive margin) for either men or women. However, the intensive margin shows small but precise, opposite-signed wage adjustments: in preferred OLS DiD specifications with state/year FE and full controls, same-sex men experience a modest wage increase (about +3%), while same-sex women exhibit a wage decrease (about -2.3%). As a result, the previous “wage penalty” for gay men and “wage premium” for women both shrank but did not reverse after legalization. These patterns are stable in robustness checks including diagnostic checks and alternative samples.

Heterogeneity clarifies where the adjustment occurs and helps explore mechanisms. Among men, gains are concentrated among primary earners, with secondary earners showing no differential change—consistent with intrahousehold specialization of market effort after legalization. Among women, both primary and secondary earners see declines, with larger losses for secondary earners, supporting household specialization channel or employer updating signaling about family formation risk.

However, heterogeneity by children does not support a contemporaneous parenthood-penalty mechanism: men’s gains are larger with children, while women’s losses are more evident without children, showing a pattern more consistent with selection into parenthood post-legalization than with immediate caregiving shocks.

Taken together, the results indicate that marriage equality primarily reallocated returns within employment, rather than altering entry/exit decisions. Within employment, same-sex legalization has caused wage rate convergence between same-sex cohabiting and different-sex cohabiting individuals, with a positive effect on gay men and a negative effect on lesbian women. Through heterogeneity analysis within earner roles, household specialization and employer beliefs are plausible channels through which legalization maps into wages.

7.2 Contribution to existing literature

Whereas prior works emphasize the impact of same-sex marriage legalization on family formation, health insurance, migration, and labor supply, this study offers one of the first causal identification of wages effects with a two-margin design, filling the gap of this underexplored labor outcome. By separating participation from within-employment

wages, this thesis shows legalization's economic footprint is intensive, with positive effect on gay men and negative effect on lesbian women.

Heterogeneity analysis with earner-role decomposition uncovers who benefits: primary earners in male couples capture gains; women in same-sex couples—especially secondary earners—face losses. This provides wage-based evidence consistent with specialization documented on labor supply in related work.

7.3 Limitations of the Study

Several limitations should be acknowledged. First, the identification of sexual orientation relied on the cohabiting status reported from the ACS. Only wage of cohabiting individuals can be observed and studied, restricting the external validity of the research conclusion. At the same time, without directly reporting sexual orientation, misclassification and measurement errors could occur during the survey. Related to measurement errors, the wage construction in the ACS (pre-tax salary from last year divided by usual hours per week and weeks-worked intervals) inherits imprecision, which can further dampen treatment effect magnitudes.

Second, as described in Chapter 2, “marriage equality” coincides with broader institutional and social shifts including variation in anti-discrimination enforcement, domestic partnership and adoption laws. Even with state and year fixed effects, residual confounding from evolving local attitudes or unobserved policy complements cannot be fully ruled out.

Third, legalization plausibly affects mobility and sorting—particularly among gay men—which can alter the local composition of workers and firms. If higher-wage same-sex couples disproportionately move to legal states, the analysis may partly capture compositional shifts rather than within-person changes.

Finally, the heterogeneous treatment effects and mechanisms are not fully explored. This study utilized the heterogeneity analysis by earner role to contribute to informative evidence for the household specialization mechanism, but no definitive channel is proposed and assessed.

7.4 Suggestion for future research

Building on the findings of this thesis, several suggestions for future research are given. First, given the complicated policy movement, future work could better disentangle the wage effects of marriage equality from contemporaneous institutional changes and behavioral sorting, such as parental recognition laws, migration selection.

Second, more detailed heterogeneity analyses—beyond earner roles and child status—are warranted to identify sensitivity and vulnerability within different socio-economic groups. Analysis by race/ethnicity, education, and occupation could reveal whether legalization narrows or widens gaps for groups facing compounded disadvantages, and suggest appropriate complementary policies and initiatives.

Finally, external validity deserves careful evaluation. Because the analysis relies on cohabiting couples observed in the ACS, generalization to singles, non-cohabitators, or newly married same-sex couples should be established rather than assumed. Outside

the United States, comparative studies in settings with different benefit regimes, family policies, and anti-discrimination enforcement would help separate institutional from cultural channels and gauge the portability of the findings across populations and policy environments.

Bibliography

- Angrist, J. D., & Pischke, J.-S. (2008, December). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press. <https://doi.org/10.2307/j.ctvc4j72>
- Arabsheibani, G. R., Marin, A., & Wadsworth, J. (2005). Gay Pay in the UK. *Economica*, 72(286), 333–347. <http://www.jstor.org/stable/3549058>
- Bernstein, M. (2015). Same-Sex Marriage and the Future of the LGBT Movement: SWS Presidential Address. *Gender & Society*, 29(3), 321–337. <https://doi.org/10.1177/0891243215575287>
- Black, D. A., Makar, H. R., Sanders, S. G., & Taylor, L. J. (2003). The Earnings Effects of Sexual Orientation. *Industrial and Labor Relations Review*, 56(3), 449. <https://doi.org/10.2307/3590918>
- Bogusz, H., & Gromadzki, J. (2024). Labor Market Outcomes of Same-Sex Couples in Countries with Legalized Same-Sex Marriage. *SSRN Electronic Journal*. <https://doi.org/10.2139/ssrn.4883866>
- Boyd, S. B. (2013). "Marriage Is More than Just a Piece of Paper": Feminist Critiques of Same Sex Marriage. *NTU Law Review*, 8, 263.
- Cao, H., Zhou, N., Fine, M., Liang, Y., Li, J., & Mills-Koonce, W. R. (2017). Sexual Minority Stress and Same-Sex Relationship Well-being: A Meta-analysis of Research Prior to the U.S. Nationwide Legalization of Same-Sex Marriage. *Journal of Marriage and Family*, 79(5), 1258–1277. <https://doi.org/10.1111/jomf.12415>
- Carpenter, C. S. (2020). The Direct Effects of Legal Same-Sex Marriage in the United States: Evidence From Massachusetts. *Demography*, 57(5), 1787–1808. <https://doi.org/10.1007/s13524-020-00908-1>
- Carpenter, C. S., Eppink, S. T., Gonzales, G., & McKay, T. (2021). Effects of Access to Legal Same-Sex Marriage on Marriage and Health. *Journal of Policy Analysis and Management*, 40(2), 376–411. <https://doi.org/10.1002/pam.22286>
- Chen, J., & Roth, J. (2024). Logs with Zeros? Some Problems and Solutions. *The Quarterly Journal of Economics*, 139(2), 891–936. <https://doi.org/10.1093/qje/qjad054>
- Daneshvary, N., Jeffrey Waddoups, C., & Wimmer, B. S. (2009). Previous Marriage and the Lesbian Wage Premium. *Industrial Relations: A Journal of Economy and Society*, 48(3), 432–453. <https://doi.org/10.1111/j.1468-232X.2009.00567.x>
- Davidson, R., & MacKinnon, J. G. (1984). Convenient specification tests for logit and probit models. *Journal of Econometrics*, 25(3), 241–262. [https://doi.org/10.1016/0304-4076\(84\)90001-0](https://doi.org/10.1016/0304-4076(84)90001-0)
- Delhommer, S. M., & Hamermesh, D. S. (2021). Same-Sex Couples and the Gains to Marriage: The Importance of the Legal Environment. *Journal of Policy Analysis and Management*, 40(4), 1120–1139. <https://doi.org/10.1002/pam.22287>
- Dillender, M. (2015). HEALTH INSURANCE AND LABOR FORCE PARTICIPATION: WHAT LEGAL RECOGNITION DOES FOR SAME-SEX COUPLES. *Contemporary Economic Policy*, 33(2), 381–394. <https://doi.org/10.1111/coep.12076>

- Drydakis, N., & Zimmermann, K. F. (2020). Sexual Orientation, Gender Identity and Labour Market Outcomes: New Patterns and Insights. *International Journal of Manpower*, 41(6), 621–628. <https://doi.org/10.1108/IJM-03-2020-481>
- Goodridge v. Department of Public Health. (2003).
- Hansen, M. E., Martell, M. E., & Roncolato, L. (2020). A labor of love: The impact of same-sex marriage on labor supply. *Review of Economics of the Household*, 18(2), 265–283. <https://doi.org/10.1007/s11150-019-09454-1>
- Hart-Brinson, P. (2016). The Social Imagination of Homosexuality and the Rise of Same-sex Marriage in the United States. *Socius: Sociological Research for a Dynamic World*, 2, 2378023116630555. <https://doi.org/10.1177/2378023116630555>
- Herman, E. (2008). The American Community Survey: An introduction to the basics. *Government Information Quarterly*, 25(3), 504–519. <https://doi.org/10.1016/j.giq.2007.08.006>
- Klawitter, M. (2015). Meta-Analysis of the Effects of Sexual Orientation on Earnings. *Industrial Relations: A Journal of Economy and Society*, 54(1), 4–32. <https://doi.org/10.1111/irel.12075>
- Lambda Legal. (n.d.). Civil Unions. <https://legacy.lambdalegal.org/publications/civil-unions>
- Levendis, J., & Lowen, A. (2023). What Same-Sex Adoption Laws Can Tell Us About the Gender Wage Gap in the United States. *Journal of Family and Economic Issues*, 44(2), 473–489. <https://doi.org/10.1007/s10834-022-09835-2>
- Long, J. T. (2025, June). The History of Same-Sex Marriage in the United States, and What Might Come Next. Retrieved August 17, 2025, from <https://statecourtreport.org/our-work/analysis-opinion/history-same-sex-marriage-united-states-and-what-might-come-next>
- Marcén, M., & Morales, M. (2022). The effect of same-sex marriage legalization on interstate migration in the USA. *Journal of Population Economics*, 35(2), 441–469. <https://doi.org/10.1007/s00148-021-00842-5>
- Martell, M. E. (2013). Differences Do Not Matter: Exploring the Wage Gap for Same-Sex Behaving Men. *Eastern Economic Journal*, 39(1), 45–71. <https://doi.org/10.1057/eej.2012.4>
- Martell, M. E. (2019). Age and the new lesbian earnings penalty. *International Journal of Manpower*, 41(6), 649–670. <https://doi.org/10.1108/IJM-10-2018-0322>
- Martell, M. E., & Nash, P. (2020). For Love and Money? Earnings and Marriage Among Same-Sex Couples. *Journal of Labor Research*, 41(3), 260–294. <https://doi.org/10.1007/s12122-020-09305-4>
- Martin, J., & Rodriguez, Z. (2022). The Effect of Same-Sex Marriage Legalization on Adoptions and Family Formation in the U.S. *SSRN Electronic Journal*. <https://doi.org/10.2139/ssrn.4307175>
- McKenzie, D. (2023). Interpreting treatment effects on an inverse hyperbolic sine outcome variable and alternatives. Retrieved November 26, 2024, from <https://blogs.worldbank.org/en/impactevaluations/interpreting-treatment-effects-inverse-hyperbolic-sine-outcome-variable-and>
- Movement Advancement Project. (2023). *Relationship & Parental Recognition Laws: Marriage and Domestic Partnerships* (tech. rep.). <https://www.lgbtmap.org/img/maps/citations-marriage-relationship-positive.pdf>

- National Center for Lesbian Rights. (2017). *Marriage, domestic partnerships, and civil unions: Same-sex couples within the United States* (tech. rep.). National Center for Lesbian Rights. <http://www.nclrights.org/legal-help-resources/resource/marriage-domestic-partnerships-and-civil-unions-an-overview-of-relationship-recognition-for-same-sex-couples-within-the-united-states/>
- Obergefell v. Hodges. (2015, June). <https://supreme.justia.com/cases/federal/us/576/644/#:~:text=Michigan%2C%20Kentucky%2C%20Ohio%2C%20and%20Tennessee,state.%20The%20Court%20noted%20other>
- Oforu, E. K., Chambers, M. K., Chen, J. M., & Hehman, E. (2019). Same-sex marriage legalization associated with reduced implicit and explicit antigay bias. *Proceedings of the National Academy of Sciences*, 116(18), 8846–8851. <https://doi.org/10.1073/pnas.1806000116>
- Pew Research Center. (2019). 5 facts about same-sex marriage. <https://www.pewresearch.org/short-reads/2019/06/24/same-sex-marriage/>
- Rosenfeld, M. J. (2017). Moving a Mountain: The Extraordinary Trajectory of Same-Sex Marriage Approval in the United States. *Socius: Sociological Research for a Dynamic World*, 3, 2378023117727658. <https://doi.org/10.1177/2378023117727658>
- Roth, J., Sant'Anna, P. H., Bilinski, A., & Poe, J. (2023). What's trending in difference-in-differences? A synthesis of the recent econometrics literature. *Journal of Econometrics*, 235(2), 2218–2244. <https://doi.org/10.1016/j.jeconom.2023.03.008>
- Ruggles, S., Flood, S., Sobek, M., Backman, D., Cooper, G., Drew, J. A. R., Richards, S., Rodgers, R., Schroeder, J., & Williams, K. (2025). IPUMS USA: Version 16.0. <https://doi.org/10.18128/D010.V16.0>
- Sansone, D. (2018). Pink Work: Same-Sex Marriage, Employment and Discrimination [Version Number: 1]. <https://doi.org/10.48550/ARXIV.1807.06698>
- Silvapulle, M. J., & Silvapulle, P. (1995). A Score Test against One-Sided Alternatives. *Journal of the American Statistical Association*, 90(429), 342–349. <https://doi.org/10.1080/01621459.1995.10476518>
- Twenge, J. M., & Blake, A. B. (2021). Increased Support for Same-sex Marriage in the US: Disentangling Age, Period, and Cohort Effects. *Journal of Homosexuality*, 68(11), 1774–1784. <https://doi.org/10.1080/00918369.2019.1705672>
- United States v. Windsor. (2013).
- U.S. (2003). *Lawrence v. Texas*.
- US Census Bureau. (2020). *Understanding and using American Community Survey data: What all data users need to know* (tech. rep.). US Department of Commerce, Economics and Statistics Administration, US Census Bureau.

List of Figures

6.1	Raw Trend with confidence intervals	25
6.2	Linear Trend Without Controls	26
6.3	Linear Trend With Controls	26
1	Raw Trend with CI (Male)	39
2	Linear Trend Without Controls (Male)	39
3	Liner Trend With Controls (Male)	40

List of Tables

5.1	Descriptive statistics (Men). Means reported; standard deviations in parentheses. Monetary values in 1999 USD.	15
5.2	Descriptive statistics (Women). Means reported; standard deviations in parentheses. Monetary values in 1999 USD.	16
5.3	Labor Supply (Participation), Men — Probit DiD	17
5.4	Labor Supply (Participation), Women — Probit DiD	18
5.5	Interaction AME: Effect of Legalization for Same-Sex vs. Different-Sex Couples	18
5.6	Wage Intensive Margin (ln wage), Men — OLS DiD	19
5.7	Wage Intensive Margin (ln wage), Women — OLS DiD	20
5.8	Wage Intensive Margin (ln wage), Men — Primary Earner, OLS DiD	21
5.9	Wage Intensive Margin (ln wage), Men — Secondary Earner, OLS DiD	22
5.10	Wage Intensive Margin (ln wage), Women — Primary Earner, OLS DiD	23
5.11	Wage Intensive Margin (ln wage), Women — Secondary Earner, OLS DiD	23
6.1	Linear DID Estimates of Labor Supply (Extensive Margin)	27
1	Wage Intensive Margin (ln wage), Men — Heterogeneity by Children (OLS DiD)	37
2	Wage Intensive Margin (ln wage), Women — Heterogeneity by Children (OLS DiD)	38
3	Extensive Margin (Participation), Men — Probit DiD, ACS 2005–2019	40
4	Extensive Margin (Participation), Women — Probit DiD, ACS 2005–2019	41
5	Interaction AME (pp change in $Pr(\text{has.wage})$), ACS 2005–2019	41
6	Intensive Margin (Log Wages), Men — OLS DiD, ACS 2005–2019	42
7	Intensive Margin (Log Wages), Women — OLS DiD, ACS 2005–2019	42
8	Extensive Margin (Participation), Prime-Age Men — Probit DiD	43
9	Extensive Margin (Participation), Prime-Age Women — Probit DiD	43
10	Interaction AME (pp change in $Pr(\text{has.wage})$), Prime-Age Couples	44
11	Intensive Margin (Log Wages), Prime-Age Men — OLS DiD	44
12	Intensive Margin (Log Wages), Prime-Age Women — OLS DiD	45

Appendix

A.1 Heterogeneity Analysis

Table 1: Wage Intensive Margin (ln wage), Men — Heterogeneity by Children (OLS DiD)

Panel A. No children in household				
	(1)	(2)	(3)	(4)
Same-sex couple	-0.019 (0.018)	0.022 (0.015)	-0.050*** (0.010)	-0.066*** (0.006)
Legalization	0.030*** (0.010)	0.046*** (0.010)	0.013* (0.007)	-0.001 (0.005)
Same-sex × Legalization	0.072*** (0.016)	0.051*** (0.014)	0.027** (0.012)	0.023 (0.014)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.001	0.047	0.173	0.191
Observations	1,896,345	1,896,345	1,896,345	1,896,345
Panel B. With children in household				
Same-sex couple	-0.351*** (0.019)	-0.182*** (0.014)	-0.157*** (0.012)	-0.163*** (0.011)
Legalization	0.059*** (0.015)	0.049*** (0.011)	0.012 (0.008)	-0.003 (0.006)
Same-sex × Legalization	0.316*** (0.028)	0.180*** (0.023)	0.092*** (0.019)	0.089*** (0.019)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.002	0.095	0.246	0.264
Observations	4,224,848	4,224,848	4,224,848	4,224,848

Notes: OLS on employed sample; standard errors clustered by state (in parentheses); person weights applied. Coefficients in log points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Wage Intensive Margin (ln wage), Women — Heterogeneity by Children (OLS DiD)

Panel A. No children in household				
	(1)	(2)	(3)	(4)
Same-sex couple	0.176*** (0.009)	0.172*** (0.010)	0.079*** (0.005)	0.066*** (0.004)
Legalization	0.062*** (0.012)	0.071*** (0.011)	0.012 (0.009)	-0.001 (0.005)
Same-sex × Legalization	-0.082*** (0.007)	-0.069*** (0.008)	-0.029*** (0.007)	-0.029*** (0.007)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.003	0.027	0.177	0.200
Observations	1,777,705	1,777,705	1,777,705	1,777,705
Panel B. With children in household				
Same-sex couple	0.091*** (0.017)	0.129*** (0.014)	0.086*** (0.011)	0.078*** (0.009)
Legalization	0.102*** (0.013)	0.087*** (0.010)	0.019** (0.009)	0.001 (0.006)
Same-sex × Legalization	-0.029** (0.014)	-0.031** (0.012)	-0.009 (0.009)	-0.009 (0.008)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.004	0.070	0.209	0.226
Observations	3,419,929	3,419,929	3,419,929	3,419,929

Notes: OLS on employed sample; standard errors clustered by state (in parentheses); person weights applied. Coefficients in log points. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.



Figure 1: Raw Trend with CI (Male)

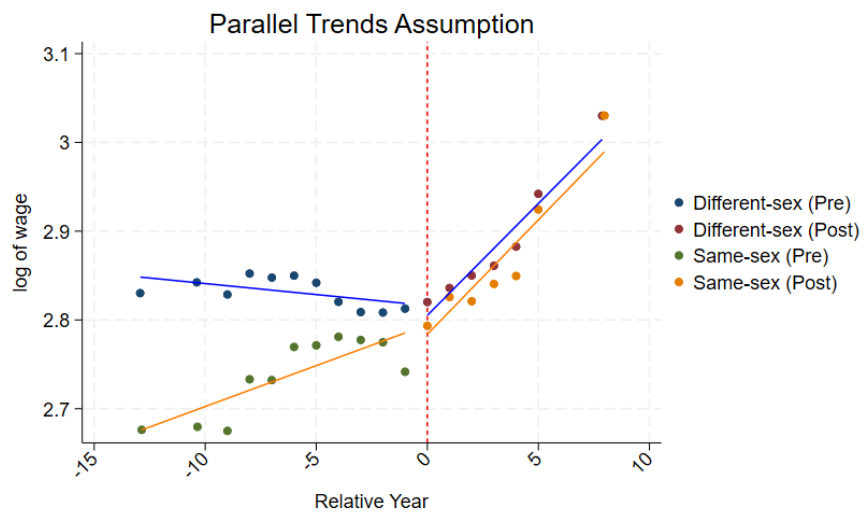


Figure 2: Linear Trend Without Controls (Male)

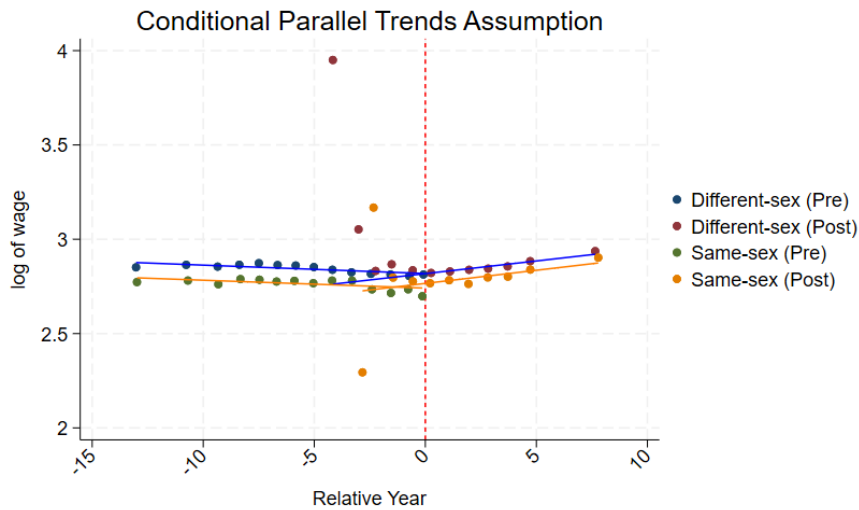


Figure 3: Linear Trend With Controls (Male)

A.2 Parallel Trend Assumption Assessment for Male Subsample

A.3 Robustness Check: Post 2005

Table 3: Extensive Margin (Participation), Men — Probit DiD, ACS 2005–2019

	Dependent variable: <i>has_wage</i>			
	(1)	(2)	(3)	(4)
Same-sex couple	-0.114*** (0.019)	-0.135*** (0.018)	-0.199*** (0.018)	-0.196*** (0.014)
Legalization	0.062*** (0.006)	0.067*** (0.005)	0.040*** (0.005)	0.001 (0.008)
Same-sex × Legalization	-0.039*** (0.013)	-0.007 (0.013)	-0.031** (0.013)	-0.030** (0.012)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
Log-Likelihood	-2,547,340.10	-2,425,229.63	-2,360,180.98	-2,352,099.06
Pseudo R^2	0.001	0.048	0.074	0.077
Observations	6,297,088	6,297,088	6,297,088	6,297,088

Notes: Probit coefficients; standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Extensive Margin (Participation), Women — Probit DiD, ACS 2005–2019

	Dependent variable: <i>has_wage</i>			
	(1)	(2)	(3)	(4)
Same-sex couple	0.367*** (0.016)	0.246*** (0.013)	0.188*** (0.014)	0.201*** (0.013)
Legalization	0.035*** (0.010)	0.056*** (0.008)	0.010 (0.007)	0.008 (0.006)
Same-sex × Legalization	-0.008 (0.015)	-0.002 (0.017)	0.012 (0.017)	0.009 (0.017)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
Log-Likelihood	-3,664,156.90	-3,536,985.96	-3,437,553.71	-3,419,781.56
Pseudo R^2	0.001	0.035	0.063	0.067
Observations	6,291,020	6,291,020	6,291,020	6,291,020

Notes: Probit coefficients; standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Interaction AME (pp change in $Pr(\text{has_wage})$), ACS 2005–2019

	(1)	(2)	(3)	(4)
Men	-0.007** (0.003)	0.003 (0.002)	-0.003 (0.002)	-0.002 (0.002)
Women	-0.007** (0.003)	-0.007** (0.003)	-0.000 (0.003)	-0.002 (0.003)

Notes: AME of legalization for same-sex *relative to* different-sex couples (i.e., $\Delta\text{AME} = \text{AME}(\text{legal} \mid \text{SS} = 1) - \text{AME}(\text{legal} \mid \text{SS} = 0)$). Entries are probability changes on the 0–1 scale (pp = percentage points). Standard errors clustered by state in parentheses.

Table 6: Intensive Margin (Log Wages), Men — OLS DiD, ACS 2005–2019

	Dependent variable: $\ln(\text{wage})$			
	(1)	(2)	(3)	(4)
Same-sex couple	-0.088*** (0.017)	-0.005 (0.012)	-0.083*** (0.008)	-0.096*** (0.006)
Legalization	0.058*** (0.014)	0.058*** (0.011)	0.019** (0.008)	-0.004 (0.005)
Same-sex \times Legalization	0.080*** (0.016)	0.063*** (0.014)	0.033*** (0.011)	0.029** (0.012)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.002	0.080	0.230	0.247
Observations	5,416,017	5,416,017	5,416,017	5,416,017

Notes: OLS on employed sample; robust standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Intensive Margin (Log Wages), Women — OLS DiD, ACS 2005–2019

	Dependent variable: $\ln(\text{wage})$			
	(1)	(2)	(3)	(4)
Same-sex couple	0.153*** (0.011)	0.152*** (0.010)	0.077*** (0.006)	0.066*** (0.004)
Legalization	0.090*** (0.013)	0.090*** (0.012)	0.024** (0.009)	-0.002 (0.005)
Same-sex \times Legalization	-0.082*** (0.007)	-0.069*** (0.008)	-0.029*** (0.007)	-0.029*** (0.007)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.004	0.051	0.202	0.221
Observations	4,594,502	4,594,502	4,594,502	4,594,502

Notes: OLS on employed sample; robust standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Robustness Check: Prime Age Couples

Table 8: Extensive Margin (Participation), Prime-Age Men — Probit DiD

	(1)	(2)	(3)	(4)
Same-sex couple	-0.138*** (0.018)	-0.148*** (0.019)	-0.208*** (0.018)	-0.205*** (0.015)
Legalization	0.053*** (0.006)	0.054*** (0.006)	0.022*** (0.005)	0.004 (0.009)
Same-sex × Legalization	-0.007 (0.013)	0.001 (0.013)	-0.023* (0.013)	-0.022* (0.013)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
Log-Likelihood	-2,269,492.97	-2,238,456.83	-2,176,335.37	-2,168,532.09
Pseudo R^2	0.000	0.014	0.041	0.045
Observations	6,225,425	6,225,425	6,225,425	6,225,425

Notes: Probit coefficients; standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Extensive Margin (Participation), Prime-Age Women — Probit DiD

	(1)	(2)	(3)	(4)
Same-sex couple	0.443*** (0.013)	0.276*** (0.011)	0.223*** (0.012)	0.237*** (0.011)
Legalization	0.028*** (0.010)	0.053*** (0.008)	0.005 (0.007)	0.011** (0.006)
Same-sex × Legalization	-0.019 (0.017)	-0.016 (0.019)	0.003 (0.019)	-0.002 (0.019)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
Log-Likelihood	-3,864,570.26	-3,740,792.58	-3,639,216.48	-3,620,216.13
Pseudo R^2	0.001	0.033	0.059	0.064
Observations	6,683,745	6,683,745	6,683,745	6,683,745

Notes: Probit coefficients; standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Interaction AME (pp change in $Pr(\text{has.wage})$), Prime-Age Couples

	(1)	(2)	(3)	(4)
Men	0.000 (.)	0.002 (0.003)	-0.004 (0.003)	-0.005 (0.003)
Women	-0.007 (0.004)	-0.007 (0.005)	0.001 (0.005)	-0.001 (0.005)

Notes: Interaction AME is the AME of legalization for same-sex *relative to* different-sex couples (i.e., $AME(\text{legal} \mid SS = 1) - AME(\text{legal} \mid SS = 0)$). Entries are probability changes (pp) on the 0–1 scale; clustered SEs by state.

Table 11: Intensive Margin (Log Wages), Prime-Age Men — OLS DiD

	(1)	(2)	(3)	(4)
Same-sex couple	-0.075*** (0.018)	-0.009 (0.013)	-0.082*** (0.008)	-0.096*** (0.005)
Legalization	0.049*** (0.014)	0.052*** (0.011)	0.008 (0.008)	-0.002 (0.005)
Same-sex \times Legalization	0.067*** (0.015)	0.059*** (0.013)	0.030*** (0.010)	0.026** (0.011)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.001	0.079	0.228	0.247
Observations	5,485,243	5,485,243	5,485,243	5,485,243

Notes: OLS on employed sample; standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Intensive Margin (Log Wages), Prime-Age Women — OLS DiD

	(1)	(2)	(3)	(4)
Same-sex couple	0.166*** (0.012)	0.152*** (0.010)	0.076*** (0.005)	0.064*** (0.004)
Legalization	0.087*** (0.012)	0.090*** (0.011)	0.018** (0.009)	0.001 (0.005)
Same-sex × Legalization	-0.094*** (0.008)	-0.073*** (0.008)	-0.032*** (0.007)	-0.030*** (0.006)
Demographic controls	No	Yes	Yes	Yes
Education dummies	No	No	Yes	Yes
State & Year FE	No	No	No	Yes
R^2	0.004	0.039	0.192	0.211
Observations	4,909,289	4,909,289	4,909,289	4,909,289

Notes: OLS on employed sample; standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

