Title
Essays in Applied Microeconomics

Author
Swete, Chelsea Lauren

Publication Date
2020

UNIVERSITY OF CALIFORNIA SAN DIEGO

Essays in Applied Microeconomics

A dissertation submitted in partial satisfaction of the
requirements for the degree Doctor of Philosophy

in

Economics

by

Chelsea Swete

Committee in charge:

Kate Antonovics, Co-Chair
Julie Cullen, Co-Chair
Jeffrey Clemens
Todd Gilmer
Isaac Martin

2020

The Dissertation of Chelsea Swete is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Co-Chair

Co-Chair

University of California San Diego

2020

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

I would like to thank my committee members for their invaluable feedback on my research. I would also like to thank the women of the Econ 117 office for their support and my cohort's Applied Picnic research group.

Chapter 1, "Increasing Health Insurance Coverage for Same-Sex Couples: The Roles of the ACA and Same-Sex Marriage", is currently being prepared for submission for publication of the material. The dissertation author was the sole author of this paper.

Chapter 3, "The Distributional Impacts of Taxes on Health Products: Evidence from Diaper Sales Tax Exemptions", is coauthored with Kye Lippold. The dissertation author was a primary author of this chapter. It is currently being prepared for submission for publication of the material.

VITA

2011	B. A. in Economics, Wesleyan University
2011-2014	Research Assistant/Programmer, Mathematica Policy Research
2014	Senior Programmer Analyst, Mathematica Policy Research
2014-2020	Graduate Teaching Assistant/Associate Instructor/Reader, University of California San Diego
2016	M. A. in Economics, University of California San Diego
2020	Ph. D. in Economics, University of California San Diego

ABSTRACT OF THE DISSERTATION

Essays in Applied Microeconomics

by

Chelsea Swete

Doctor of Philosophy in Economics

University of California San Diego, 2020

Kate Antonovics, Co-Chair
Julie Cullen, Co-Chair

This dissertation is composed of three chapters addressing the economics of health, discrimination, and inequality. The first chapter examines the roles of the Affordable Care Act (ACA) and employer spousal coverage made possible by same-sex marriage in increasing health insurance coverage for same-sex couples in the United States. It uses data from the American Community Survey in a difference-in-differences model on same-sex couples in ACA Medicaid expansion states to determine if people who did not have access to legal same-sex marriage at the time of the ACA had larger gains in insurance coverage post-ACA than those who already had access to marriage. I find a 1.8pp larger increase in Medicaid coverage and a 3.5pp larger increase

in overall coverage in late gay marriage states. These differences are the combined effects of the initial differential response to the ACA and the subsequent direct response to same-sex marriage passage in the post period. When I directly control for same-sex marriage passage, I find a 2.8pp larger increase in Medicaid and suggestive evidence for increased crowd-out of employer coverage in late gay marriage states.

The second chapter explores the link between homophobic attitudes and the wage gap between men in same-sex couples and men in opposite-sex couples in the US. I use average responses to General Social Survey questions to create a state-year level prejudice index and show that it has a positive relationship with the wage gap. An increase of one standard deviation in the homophobia index is associated with a 4% wage penalty for men in same-sex relationships.

In the third chapter, we examine taxation of diapers, usually seen as an inelastic health product, and find substantial income heterogeneity in responsiveness to taxes using retail scanner data. Exploiting changes to sales tax exemptions for diapers in New York and Connecticut, we find that diaper sales rise by 5.4% in low-income areas when taxes are removed, accompanied by reduced spending on children's pain medications. These results imply that sales tax exemptions for diapers can have positive spillover effects on health and well-being.

Chapter 1

Increasing Health Insurance Coverage for Same-Sex Couples: The Roles of the ACA and Same-Sex Marriage

1.1 Introduction

Marriage is an important institution that comes with over 1,000 benefits and legal rights (CBO 2004). One important category of these benefits is access to insurance. In addition to the informal insurance implicit in pooling household resources, marriage is also a pathway to accessing employer health insurance in the US. This benefit is widespread. For example, conditional on offering any health insurance to employees, 99% of large firms and 96% of small firms also offered spousal coverage options in 2014 (KFF 2014). Marriage and household formation also influence eligibility for social safety net programs because many are administered at the household level. For means-tested programs, the Federal Poverty Line (FPL) is defined with respect to household size and is not linear in number of household members, so public provision is influenced by both the number of people in a household and their income.

Marriage can play a role in determining who is eligible for public provision of insurance but also for affecting who is in the pool of uninsured people targeted by public insurance to begin with. When establishing or expanding public programs, economists and policy makers are often concerned about the extent to which people are "crowded out" of existing private provision into government provision. The ability to gain private employer health insurance through a spouse means that marriage may play a role in mediating crowdout. In addition to government concern about the cost of providing public insurance, the source of insurance also matters for individuals. Private insurance may offer advantages in quality of care and availability of specific medical professionals, but also involves paying monthly premiums.

How marriage influences access to health insurance is difficult to study because of the layers of inherent selection. People select both whether to get married at all and who they marry in endogenous ways. There is evidence of positive selection into marriage and assortative matching of skill levels between spouses.[1] This paper examines a unique context when there

[1] Some examples that address this question include Antonovics and Town (2004), Ginther and Zavody (2001), and Greenwood et al (2014).

was unequal access to marriage at the time of a major public insurance expansion: at the time of the Affordable Care Act (ACA) Medicaid expansion, the ability of same-sex couples to get married varied by state.

I focus on two policies that changed health insurance options for same-sex couples. Same-sex marriage could have increased employer coverage by opening up the spousal coverage option and it could have decreased Medicaid coverage either by people switching out of Medicaid to their spouse's employer insurance or by changing eligibility.[2] The Affordable Care Act increased the number of people eligible for Medicaid and several papers have shown that it increased Medicaid coverage in expansion states.[3] The public insurance expansion aspect of the ACA could have resulted in decreased employer insurance through crowdout, or could have increased employer insurance through either the individual mandates to have health insurance or firm mandates to offer health insurance. As the policies had the potential to exert opposite effects, the overall effect of the combination of the ACA and same-sex marriage is ambiguous and the interaction of the two is unknown.

The main question addressed in this paper studies how the response to the ACA unfolded differently for same-sex households in states where same-sex couples did not have access to marriage yet compared to those who already did. Was takeup of public insurance and crowdout from employer insurance greater? What happened to overall insurance rates? The variation I use for this question is which pathway to obtaining insurance was available to people in same-sex couples first. The possible mechanism is that if a person had access to health insurance through marrying their partner, then they would not need to take up the Medicaid expansion when it passed. Essentially, same-sex marriage could have kept people off of Medicaid. The order of the

[2]Carpenter et al (2018) provide evidence that legal same-sex marriage increased overall rates of health insurance coverage for only men in two-male households. Dillender (2015) and Buchmuller and Carpenter (2012) find increases in health insurance for only women following legal recognition of partnerships. Gonzales (2015) documents increases in employer coverage and decreases in Medicaid coverage following New York state's same-sex marriage law in 2011 for both men and women. Medicaid eligibility may change with marriage for cohabiting partners if neither had claimed the other as a tax dependent before.

[3]Including Cohen and Martinez (2014), Courtemanche et al (2016), Duggan et al (2017), Frean et al (2017), Long et al (2014), Smith and Medalia (2015), and Sommers et al(2015)

policies could also have been associated with different distributions in the sources of insurance.

To answer these questions, I group states by whether or not they had legal same-sex marriage by January 1, 2014, the main implementation date of the ACA. I refer to "early" states as those that passed same-sex marriage earlier than the ACA and "late" states as those that passed it later than the ACA. I restrict my analysis to states that accepted the expansion of Medicaid, the government-provided insurance program for the poor in the US, to ensure that the cross-state comparison is across states enacting similar changes to the health insurance landscape. I use the 2010-2016 waves of the American Community Survey (ACS) in order to compare the pre-ACA 2010-2013 period to the post-ACA 2014-2016 period.

First, I document that same-sex couples in both "early" and "late" same-sex marriage states saw gains in both overall insurance and Medicaid coverage after the ACA. These increases were larger in the "late" states. I then formalize this into a difference-in-differences model controlling for other characteristics that influence health insurance coverage, where pre/post is defined by "early" versus "late" states. I find that "late" states had a 1.8pp larger increase in Medicaid coverage for same-sex couples in the post period, which maps to a 53% larger increase than the increase for the "early" states. The increased gains in overall insurance rates were larger; the additional 3.5pp gain in overall insurance maps to a 67% larger increase than the gain for the "early" states. The interpretation of the post-ACA gains combines any immediate differential response to the ACA by the "late" states compared to the "early" states with the subsequent passage of same-sex marriage in the "late" states. Same-sex marriage was made legal in all US states in the Supreme Court *Obergefell v Hodges* 2015 decision, which falls in the post period of the analysis.

To start to separate out the combined policies, I then use a more disaggregated model by adding direct controls for policy-year adoption of same-sex marriage while keeping the difference-in-differences style "late" same-sex marriage states in the post period interaction term. I let the direct same-sex marriage term vary by late/early same-sex marriage states because if the availability of one coverage option matters for another, as I argue it does for the availability of same-sex marriage at the time of the ACA, same-sex marriage may matter differentially based on whether the ACA provisions were in effect. I add a third level of difference by comparing same-sex couples to opposite-sex couples to account for any state health insurance trends that could affect both groups, such as early state Medicaid expansions.

I find that the increased takeup of Medicaid in "late" same-sex marriage states at the time of the ACA holds in these specifications. In addition, this approach separates out the changes in employer insurance. The change in employer insurance at the time of the ACA for same-sex couples in "late" same-sex marriage states compared to "early" same-sex marriage states is negative, which suggests that crowdout could have been increased by the lack of access to marriage.

Finally, I test whether these insurance patterns are heterogeneous across different demographic groups. The effects are stronger for non-parents, which is consistent with the ACA Medicaid expansion eligibility limits changing mostly for non-parents. I also find results are stronger for women.

Same-sex couples are a part of the LGBTQ community, which is a particularly vulnerable group when it comes to health insurance and health outcomes. In quarter 4 of 2013, the last quarter before the ACA, 22% of LGBT respondents in a Gallup poll did not have health insurance, compared to 16.7% of non-LGBT respondents (Gates 2014). These differences in insurance rates are particularly relevant because of significant health disparities between the LGBTQ and non-LGBTQ communities: the LGBT community has higher rates of smoking, substance abuse, HIV/AIDS, and worse mental health outcomes (Kates et al 2018). In addition, the ACA Medicaid expansion was focused on adults without children, which makes it particularly relevant for same-sex couples, who are far less likely to have children than opposite-sex couples.

Understanding this history and learning more about the role of marriage in health insurance access in the US has implications for policy. The health disparities experienced by the LGBTQ community make it particularly important to understand the history of health insurance coverage for this population at a time of rapid policy change. Contributing to the knowledge of how same-sex marriage and health insurance are related is important because there are major constraints in data availability; it is difficult to find a sufficiently large dataset that records sexual orientation and health insurance outcomes in the correct time period. It is also important to understand how the policy context of same-sex marriage affected the roll-out of the ACA because as its provisions are weakened and attempts to overturn it continue, predictions of how this would change insurance rates for the LGBTQ community need to account for this difference.

More generally, a better understanding of how marriage and public insurance expansions are related can inform how we think about recent policy proposals and experiments changing access to public health insurance such as undoing the ACA, expanding public insurance access through "Medicare for All", or adding Medicaid work requirements. Marriage can play a role in determining need for public insurance through changing access to private insurance, eligibility for public insurance through household-level provision, and crowdout through need and eligibility.

The structure of the paper is as follows: Section 2 gives more background on the two policies studied and reviews the relevant literature and this paper's contributions, Section 3 describes the data and trends, and Section 4 explains the empirical strategies. Section 5 describes the results, Section 6 reviews the robustness and heterogeneity tests, and Section 7 concludes.

1.2 Background

1.2.1 Policy Background

The Affordable Care Act (ACA, popularly known as "Obamacare") was signed into law on March, 23, 2010. In *National Federation of Independent Businesses vs Sibelius* on June 28, 2012, the Supreme Court upheld the ACA but made the Medicaid expansion optional for each state. 24 states plus DC chose to accept the Medicaid expansion on the January 1, 2014 ACA implementation date. The federal funding for the expansions was tied to making all households up to 138% of the Federal Poverty Line (FPL) eligible for Medicaid, including non-parents. For many states, non-parents were not eligible at any income level for Medicaid before the ACA. There were myriad changes to the health insurance landscape of the US with ACA implementation. Chief among them were the individual mandate[4] and the establishment of individual exchange markets. The individual exchanges were set up by each state and people in households earning between 100-400% of the FPL were eligible for subsidies to buy health insurance. Other changes included making denial of coverage for pre-existing conditions illegal and making the Medicaid application process easier.

The passage of same-sex marriage took place in different states at different times, starting in 2004 with Massachusetts. Individual states and district courts established same-sex marriage state by state. The Supreme Court decision on June 26, 2015 on *Obergefell v Hodges* forced the remaining fifteen states to legalize same-sex marriage.

1.2.2 Same-Sex Marriage and Health Insurance

The relationship between same-sex marriage and health insurance is not clear ex ante since employers can respond as well. Obtaining health insurance coverage for a same-sex spouse requires revealing one's sexual orientation to the employer, which risks harassment or getting fired, since sexual orientation is not a protected class of worker in 26 states even as of 2019 (MAP project). Firms could also respond by withdrawing coverage options for domestic partnerships. Unfortunately, it is rare for large data sets to include direct questions about sexual orientation.

Carpenter et al (2018) use the Behavioral Risk Factor Surveillance System (BRFSS) to show that health insurance coverage and health outcomes were better for men in two-male households after state passage of same-sex marriage. Since the BRFSS does not include information on whether two same-sex people sharing housing are in a relationship or roommates until after most states passed same-sex marriage, the paper looks at households that have two men or two women. They then scale the estimates by the post-period percentage of two-male or two-female households that are actually couples. It is more difficult to detect effects for women because while about 27% of two men households are couples, only 11% of two women households are.

[4]The penalty was less than $100, but the law did require people to carry health insurance.

While my paper is more specific in the scope and is not able to include health outcomes because the ACS does not contain them, I am able to detect changes for individuals directly recorded as part of a same-sex couple and to trace changes in the source of insurance, whether it is Medicaid or employer-provided, in addition to the overall rates.

Two studies find evidence for increased health insurance for women following legal recognition of same-sex partnerships. Dillender (2015) finds this for recognition of legal same-sex marriage in the 1996-2011 CPS. Buchmuller and Carpenter (2012) find increased health insurance coverage for women following California's 2005 law that requires firms offering spousal health insurance coverage to extend it to domestic partnerships that could include same-sex couples.

One study looks at the pattern of health insurance in the American Community Survey for New York State's passage of same-sex marriage in 2011. New York is a uniquely useful state to analyze because it has several years of pre-period data starting in 2008 when the ACS started collecting health insurance information and several years of post-period data before the 2014 ACA. It finds that men in same-sex relationships had a 6.3pp increase in employer coverage and a 2.2pp decrease in Medicaid coverage; the respective numbers for women were an 8.9pp increase and a 3.9pp decrease (Gonzalez 2015).

I add to the evidence that same-sex marriage was associated with an increase in health insurance coverage and show how it had an indirect effect on insurance takeup through changing responses to the ACA.

1.2.3 ACA Expansion and Health Insurance

Studies generally find that the ACA Medicaid Expansion increased health insurance coverage in states that took up the program, including Cohen and Martinez (2014), Courtemanche et al. (2016), Duggan et al (2017), Frean et al. (2017), Long et al. (2014), Smith and Medalia (2015), and Sommers et al. (2015). One recent study documented the role of the ACA in decreasing the health insurance coverage gap between married and unmarried people in the US

(Courtemanche et al 2019).

Gonzalez and Henning-Smith (2017) examines how the ACA Medicaid Expansion affected the LGBTQ population used the Behavioral Risk Factor Surveillance System. Using data from 19 states in 2014, it finds that LGBTQ adults in Medicaid expansion states were more likely to have coverage than LGBTQ adults in non-expansion states. Because the BRFSS only records same-sex couples after 2014, this analysis is restricted to a post-period comparison.

1.3 Data

I use the American Community Survey (ACS) data for this project. The ACS disinguishes between the sources of health insurance, including whether an individual is on Medicaid, individually purchased insurance, or employer-provided insurance. It also has information on households that can be used to determine if two people living together are married or unmarried romantic partners rather than roommates.[5] When combined with gender, this information can be used to determine if individual is part of a same-sex or opposite-sex cohabiting couple. This means my analysis is limited to people who are in cohabiting or married couples, not single people. In examining the role of marriage access, couples are an informative group to use.[6] The ACS is collected monthly in a uniform distribution over months[7], but the publicly available data only identifies the year.

This paper uses the 2010-2016 ACS in order to start the year the ACA was signed and end before some provisions were weakened starting in 2017. The sample is restricted to people in same-sex couples aged 26-64 and residing in Medicaid expansion states. The age restriction is because individuals under the age of 26 were allowed to be insured under a parent's plan starting in 2010 and individuals 65 and older are covered by Medicare. One restriction in this data is that the information on whether a same-sex couple is married is only available starting in 2013, even

[5]The text of this question that separates roommates and romantic partners can be found in Figure 1.4

[6]There is no discontinuous change in reporting of cohabiting couples following same-sex marriage in the ACS. Carpenter et al (2018) also finds no discontinuous jump in the BRFSS.

[7]There was a disruption in followup for data collection in October 2013 due to the government shutdown.

for states where it was legal before 2013.

The descriptive statistics for my sample can be found in Table 2.1, a comparison of the demographic characteristics for people in same-sex versus opposite-sex couples. There are several key differences between these groups that might be expected to influence health insurance. One is that people in cohabiting same-sex couples are more educated than people in cohabiting opposite-sex couples; 26.4% of people in same-sex couples have attended graduate school compared to 15.6% of people in opposite-sex couples and the college rates are similarly higher, 27.7% versus 22.6%. Another relevant variable is the proportion of couples without children. 77.6% of people in same-sex couples do not have children compared with 39.1% of opposite-sex couples. This makes the Medicaid expansion's focus on non-parents particularly relevant for people in same-sex couples.

1.3.1 Trends Over Time

Figure 1.5 shows the overall trends in health insurance coverage over time for people in same-sex couples in expansion states. Health insurance coverage is increasing over time. The majority of coverage in all years is through employers for this group of cohabiting couples. However, the interaction between the LGBTQ community overall and Medicaid is significant: The Williams Institute estimates that as of January 2018, 1,171,000 LGBT adults are covered by Medicaid (Williams Institute 2018). Medicaid coverage increases in 2014, as seen by the increasing width of the middle green band in Figure 1.5. It is possible for an individual to report multiple types of coverage or coverage other than Medicaid, individually purchased, or employer-provided. Other types of coverage may include VA, TRICARE, or Indian Health Services. The breakdown of types of multiple coverage is in Table 1.2, which shows that the most common type of multiple coverage is employer and individually purchased. The vast majority of people in this sample have a single type of coverage.

While this paper is concerned with how access to marriage and public insurance expansions are related, the ACS only records same-sex marriages starting in 2013, which does not span the pre-period of the analysis. It is still informative to break down the coverage gains where possible, so Figure 1.6 separates the gains in Medicaid by whether the individual is in a cohabiting or married partnership. An individual is recorded as unmarried if they live in a state where marriage is not yet legal or if they are in a state where marriage is legal and they choose not to get married. The increase in Medicaid coverage is almost entirely driven by unmarried couples, which is suggestive that marriage could have influenced who took up Medicaid.

1.4 Empirical Strategies

The time variation used in this paper is whether or not a state had passed same-sex marriage by January 1, 2014. A policy timeline for the Medicaid expansion states used in my sample is available in Figure 1.1 and in map form in Figure 1.2. In both figures, purple corresponds to "early" same-sex marriage states and green represents "late" same-sex marriage states. Figure 1.3 shows the ACA and same-sex marriage events on the same timeline to illustrate the order and timing of these policies for different states.

1.4.1 Difference-in-Differences Model

The initial model is run on a sample of people in same-sex couples aged 26-64 in Medicaid expansion states. The intuition is similar to a difference-in-differences model where the pre/post time period is with respect to the January 1, 2014 ACA implementation. The "treated" group is late same-sex marriage states and the "control" group is early same-sex marriage states where late/early indicates the order in which the policies were passed.

$$Insurance_{ist} = \alpha + \beta LateGayMar * PostACA + \eta_s + \gamma_t + X_i \nu + \varepsilon_{ist} \qquad \text{(DD)}$$

S is state, t is year, and i is individual. X is a vector of individual demographic characteristics that is comprised of education categories, race indicators, gender, and age. The standard errors are clustered at the state level.

The relevant insurance variables for this analysis are a series of indicators for whether an individual is covered by Medicaid, individually purchased insurance, employer-provided insurance, or any insurance at all. β is the coefficient of interest on the interaction between indicator variables for late same-sex marriage states and for whether the year of observation is 2014-2016, where "Late same-sex marriage" states are defined as states that passed same-sex marriage after January 1, 2014. [8] The interpretation of β is the additional increase in coverage in states where people had access to the ACA before they had access to same-sex marriage. All states passed same-sex marriage by 2015, so the interpretation of the coefficient is the combined difference due to any differential take-up of the ACA and the direct effect of same-sex marriage. Table 1.3 spells out the comparison in chart form.

1.4.2 Triple Differences Composite Model

If there exist state-specific insurance trends or policies that affect insurance differentially in late same-sex marriage and early same-sex marriage states, the interpretation of the difference-in-differences model will be compromised. In this context, a possible complication is the early partial expansion of Medicaid eligibility in several states before the ACA expansion. A triple difference model with the third comparison made to opposite-sex couples, both married and cohabiting, can address some of these concerns. In the model below, SSC is an indicator for whether an individual is in a same-sex couple.

$$\begin{aligned} Insurance_{ist} = \alpha &+ \theta_1 LateGayMar * PostACA * SSC \\ &+ \theta_2 LateGayMar * PostACA \\ &+ \theta_3 LateGayMar * SSC + \theta_4 PostACA * SSC + \theta_5 SSC \\ &+ \eta_s + \gamma_t + X_i \nu + \varepsilon_{ist} \end{aligned} \tag{DDDc}$$

[8] Illinois expanded Medicaid on 1/1/2014 but is not included in this sample because the state announced that same-sex marriage would be legal in November 2013 but it did not go into effect until June 2014. The results are robust to including Illinois, but the interpretation is different based on whether it is the expectation or implementation of same-sex marriage.

I use a triple difference framework using the opposite-sex couples. For the triple difference model, λ_1 is the coefficient of interest as the additional change in insurance coverage observed in the post period for individuals in late same-sex marriage states compared to the change in those states for individuals in opposite-sex couples. This post period still contains both the ACA and gay marriage, so the interpretation is a composite effect.

1.4.3 Triple Differences Decomposed Model

Same-sex marriage became legal in different states in different years over the time period in this model. To take advantage of this variation, I use a more disaggregated model that controls for the year same-sex marriage is passed. The argument of this paper is that the order of the policies made a difference for take-up patterns, so the direct effect of same-sex marriage could be different based on whether or not the ACA was already in effect. To allow for this difference, I use separate terms for early versus late passers of same-sex marriage. I continue to use the triple difference with opposite-sex couples to account for any state health insurance trends that would have affected both groups.

$$\begin{aligned} Insurance_{ist} = \alpha &+ \lambda_1 LateGayMar * PostACA * SSC \\ &+ \lambda_2 EarlyGayMar * GayMarAtTime * SSC \\ &+ \lambda_3 LateGayMar * GayMarAtTime * SSC \\ &+ \lambda_4 LateGayMar * PostACA + \lambda_5 EarlyGayMar * GayMarAtTime \\ &+ \lambda_6 LateGayMar * GayMarAtTime + \lambda_7 LateGayMar * SSC \\ &+ \lambda_8 PostACA * SSC + \lambda_9 SSC + \eta_s + \gamma_t + X_i \nu + \varepsilon_{ist} \end{aligned} \tag{DDDd}$$

GayMarAtTime is an indicator for whether the state has same-sex marriage legally available for at least some part of the year of observation, so it turns from 0 to 1 in the year same-sex marriage becomes legal and stays 1 for all subsequent years. λ_2 and λ_3 are the direct change in insurance coverage for same-sex couples compared to opposite sex couples in the year same-sex marriage is allowed, allowing different sizes for early versus late states. λ_1 is then the differential response of non-same-sex marriage states the the ACA, controlling for same-sex marriage passage. State fixed effects, year fixed effects, and the same set of demographic controls are used. Standard errors are clustered at the state level.

Mapping this approach to the difference-in-differences intuition Table 1.3 means that by adding separate terms for (b) and (c), I allow them to be different effects and also for the Treat*Post coefficient to describe the difference between (a) and (d). This difference between (a) and (d) shows the different response to the ACA by whether or not the state had same-sex marriage.

1.5 Results

1.5.1 Raw Data

First, it is illustrative to look at the raw difference-in-differences tables and graphs to understand the overall changes in the early and late states before and after January 1, 2014. Table 1.4 shows the relative gains in Medicaid coverage for same-sex couples in expansion states. Medicaid coverage in late same-sex marriage states increased by 5.5pp and coverage in early same-sex marriage states increased by 3.6pp, so the increased gains in coverage were 1.9pp, or a 53% larger increase. Hypothetically, the change in Medicaid coverage with the combined policies for the late states could have gone either way; the ACA could have increased coverage by expanding eligibility and same-sex marriage could have decreased coverage by opening up a path to private insurance that may be more desirable than public insurance. We see in this table that the overall change in Medicaid coverage was positive. Figure 1.7 shows these differences by year residualized by age, gender, and race.[9] Rates of Medicaid coverage were similar before the ACA, but in 2014, there was a larger increase in Medicaid coverage in late same-sex marriage

[9]The raw versions of these figures can be found in the online appendix.

states. The results from the raw data are suggestive that legal same-sex marriage access at the time of the ACA was associated with increased Medicaid takeup.

In theory, the state individual exchanges, as a new path to health insurance at the time of the ACA, could have seen differential enrollment based on the availability of same-sex marriage. In the raw difference-in-differences Table 1.5, there is a slightly larger gain for the early passers.

Employer coverage is another type of insurance coverage where the policies could have pushed in opposite directions. Same-sex marriage should increase employer coverage as spousal coverage becomes a possibility for some people who did not have access to employer coverage before. The ACA could have decreased coverage if crowdout was happening or could have increased coverage if the employer mandate was effective. Table 1.6 shows that people in same-sex couples in early same-sex marriage states were slightly less likely to have employer coverage after the ACA with a decrease of .1pp. The net change for late states in the post period was positive, with a 1.4pp increase. This addresses part of the main question of what happened with employer coverage with these two policies: the net change was positive for the late same-sex marriage states that combined the policies in the post-period.

While it is useful for this paper to break down the types of insurance gains, it is also important to keep overall health insurance/uninsurance rates in mind. Table 1.7 shows the differences in having any type of health insurance. After 2014, late same-sex marriage states increased their overall insurance rate by 8.2pp, which is 3.3pp or 67% higher than the 4.9pp increase in early same-sex marriage states. Late same-sex marriage states started from a lower coverage rate, 84.5% versus 90%, which is supportive of the idea that marriage may have played a role in determining who was uninsured. Figure 1.8 graphs the residualized differences for any coverage. The trends seem to be relatively stable and similar in the pre-period, with the late same-sex marriage states having lower coverage. In 2014, both groups of states have increases in any coverage, but the gain in the late same-sex marriage states is much larger, closing much of the pre-existing coverage gap.[10]

[10] The raw versions of these figures can be found in the online appendix.

1.5.2 Difference-in-Differences Model

The results for the motivating difference-in-differences model in equation (DD) are in Table 1.8. The interpretation of the coefficients on *LateGayMarriage*PostACA* are the combined change due to:

- The differential response to the ACA based on not having same-sex marriage as an option
- Passing same-sex marriage in 2014 or 2015

In terms of the intuition Table 1.3, it's the difference in moving from nothing to (a) and (b) for the late same-sex marriage states and from (c) to (d) for the early same-sex marriage states. The overall changes could move in either direction because same-sex marriage could increase employer coverage and decrease Medicaid while the ACA could increase Medicaid and decrease employer coverage.

The results show that late same-sex marriage states had a statistically significant 1.8pp larger increase in Medicaid coverage than early same-sex marriage states, which is very similar to the 1.9pp raw increase. This shows that the combined changes due to differential takeup of the ACA and same-sex marriage passage in the post period were positive. The event study for the Medicaid dependent variable is found in Figure 1.9 and there are no statistically significant differences in the pre-period.[11]

Employer coverage, the third column of Table 1.8, shows that late same-sex marriage states had a 1.8pp larger increase than early same-sex marriage states. This reinforces that the change following the combination of differential ACA response and direct same-sex marriage passage was positive. There is no statistically significant difference in the level of individually purchased coverage. The fourth column shows the differential results for having any type of insurance coverage, which was a 3.5pp larger gain in late same-sex marriage states (equivalently,

[11] There is a possible downward trend after 2015 in the post-period, which is after all states passed same-sex marriage, which is consistent with a story that people temporarily used the Medicaid expansion to gain insurance until they could marry their partner. I cannot statistically reject that the 2015 coefficient and 2016 coefficient are the same, so I will be adding 2017 data to see if this trend continues.

the uninsurance rate decreased 3.5pp more). Since it is possible to have multiple types of insurance or another type of insurance, it is not necessary for the sum of Medicaid, individual coverage, and employer coverage to equal the any coverage column. The event study for any type of coverage, found in Figure 1.11, does not reject parallel trends. While same-sex marriage is becoming legal in the early same-sex marriage "control" group in the pre-period, it is possible that gains in employer coverage may be partially cancelled out by decreases in Medicaid coverage that would be masked by the overall coverage. For that purpose, it is useful to start to separate out the policies and types of coverage to get a more complete picture of the changes.

1.5.3 Triple Differences Composite Model

The results of this specification, estimating equation (DDDc), can be found in Table 1.9. The coefficients are consistent with the positive significant coefficients in the DD model: the 1.2pp larger increase in Medicaid compared to 1.8pp, 1.5pp larger increase in employer coverage compared to 1.8pp, and the 2.8pp larger increase in any coverage compared to 3.5pp. The significance level of the Medicaid coefficient falls from significance at the 1% level to the 10% level.

1.5.4 Triple Differences Decomposed Model

Table 1.10 shows the results for the decomposed triple differences model, equation (DDDd). In this case, the "treat"*"post" variable is *LateGayMar*PostACA*SSC*, same-sex couples in late same-sex marriage states in 2014-2016 as compared to opposite-sex couples, early same-sex marriage states, and 2010-2013. It can be interpreted as the additional change in coverage at the time of the ACA in states where same-sex marriage was not legal before the ACA, controlling for health insurance trends also experienced by opposite-sex couples and the direct effects of same-sex marriage passage. The intuition behind this model using Table 1.3 is that by putting in terms for (b) and (c), the direct impact of same-sex marriage, then the "treat"*"post" variable can be interpreted as the difference between a and d, the different responses to the ACA based on whether same-sex marriage was legal by January 1, 2014. The coefficient on this variable for the Medicaid regression is a statistically significant 2.8pp. This is a bigger effect than the simple combined model, which is consistent with the differential response being only part of the combined effect.

The coefficient on the differential ACA response variable in the employer-provided insurance regression, shown in the first row of the third column in Table 1.10, is -5.1pp. This is suggestive that there was increased crowdout out of employer-provided insurance in places where same-sex marriage wasn't an option for individuals in same-sex couples at the time of a public insurance expansion.

For the more direct responses to same-sex marriage becoming legal, there are separate terms for passing same-sex marriage before or after the ACA for same-sex couples, *EarlyGayMar*GayMarAtTime*SSC* and *EarlyGayMar*GayMarAtTime*SSC*. Both of these coefficients are positive and significant for employer insurance. The employer insurance coefficient for late states is higher than those for early states and I can reject that the two coefficients are equal. I can also reject that the sum of the two terms concerning same-sex couples in late states is 0, *LateGayMar*PostACA*SSC* and *LateGayMar*GayMarAtTime*SSC*, so the net effect of the policies is positive in this model as well. For both same-sex marriage variables, the Medicaid coefficients are negative but not significant.

The event study figures for the LateGayMar*PostACA*SSC for this model are in Figures 1.12, 1.13, and 1.14. Figure 1.12 shows the Medicaid results, with parallel trends in the pre-period and an increase in the post-period. Figure 1.13 shows the employer coverage results that show a clear decrease in employer coverage in all years in the post-period. This is suggestive of increased crowdout from employer insurance into public insurance for states that did not allow same-sex marriage at the time of the ACA.

1.6 Robustness and Heterogeneity

1.6.1 Heterogeneity by Demographic Characteristics

The Medicaid expansion and same-sex marriage could have different sized effects on different groups. Since the Medicaid expansion was particularly focused on non-parent households, we would expect childless households to have larger gains and larger differential responses. The previous literature has found mixed results on whether men or women increase health insurance coverage following same-sex marriage, so gender is an important axis of heterogeneity. While income and occupation are endogenous to health insurance coverage because a job comes bundled with all of those aspects, we can look at different responses by education level, which is less likely to change quickly in response to these policies surrounding health insurance. We would expect the Medicaid results to be concentrated in less educated populations, where individuals are more likely to be poor and in jobs that do not offer health insurance. We may also expect heterogeneity by age because younger people are generally less likely to have health insurance and marriage patterns may be different with age.

First, I test these dimensions of heterogeneity for the simple model by splitting the sample based on the demographic characteristic. I find that the significant increases in Medicaid are concentrated in individuals that do not have children in Table 1.11, although the standard errors are higher for parents in part because there are fewer of them. Table 1.12 shows that in contrast to Carpenter et al. 2018's results, I find differences for women and not men. This could be in part because of the differences in empirical strategy made necessary by the data constraints. Since more women who are not in same-sex couples live together as roommates, it is harder to pick up effects for women as measured by two-female households in the BRFSS.

The Medicaid coefficient for the individuals without a college degree in Table 1.13 is positive and consistent with the overall results, but not significant. There is no change in the college degree or higher group. When I separate the results by age in Table 1.14, the Medicaid result is positive and significant for the middle third of the ages, 29-51. It is positive but not significant for the top third, 52-64. The net employer results load onto the youngest age group, 26-38.

1.6.2 Heterogeneity by Predicted Risk

Another approach to heterogeneity testing is to use pre-period data to determine which characteristics best predict falling into uninsured categories and see if people who face higher predicted risk of not having insurance demonstrate higher changes. I run 5-fold LASSOs on individuals in 2008-2009 in same-sex couples in states that did not have gay marriage at the time but would expand Medicaid in 2014. The LASSO determines the best prediction of the dependent variable, not having health insurance and having an income below 138% FPL, based on combinations of all possible demographic characteristics variables. I use the coefficients from this LASSO to create a risk index for the 2010-2016 data and run the model on the sample with above median risk and below median risk for falling into this category. Results for this test are in Table 1.15. The above median risk has significant positive results for employer and any insurance. It has higher results for Medicaid, but the difference is not statistically significant.

1.7 Conclusion

This paper examines the health insurance coverage levels and sources changes associated with the policy changes of same-sex marriage and the Affordable Care Act in Medicaid expansion states for same-sex couples. I find that both policies played a role in increasing health insurance coverage for individuals in same-sex couples from 2010-2016, but there were different implications for which type of insurance people gained. Compared to people in states with had access to same-sex marriage before the passage of the ACA, there were larger gains in Medicaid and overall coverage for same-sex couples in states that did not yet have access to marriage. There is suggestive evidence that there was increased crowdout from employer coverage at the time of the ACA Medicaid expansion where marriage was not an option, which implies that spousal employer health insurance provision is important for evaluating insurance expansions. The results also suggest that same-sex marriage led to increases in employer coverage for same-sex couples. The combined effects are particularly salient for non-parents and women.

1.8 Acknowledgements

Chapter 1, “Increasing Health Insurance Coverage for Same-Sex Couples: The Roles of the ACA and Same-Sex Marriage”, is currently being prepared for submission for publication of the material. The dissertation author was the sole author of this paper.

References

Andrews, Michelle. 2014. "Gay Couples Entitled To Equal Family Health Coverage, Fed Says." *NPR*, March 15, 2014. https://www.npr.org/sections/health-shots/2014/03/15/290259565/same-sex-couples-entitled-to-equal-family-health-insurance

Antonovics, Kate, and Robert Town. 2004. "Are All the Good Men Married? Uncovering the Sources of the Marital Wage Premium." *The American Economic Review* 94(2): 317-321.

Buchmueller, Thomas and Christopher Carpenter. 2012. "The Effect of Requiring Employers to Extend Health Benefit Eligibility to Same-Sex Partners of Employees: Evidence from California." *Journal of Policy Analysis and Management* 31(2): 388-403.

Carpenter, Christopher, Samuel T. Eppink, Gilbert Gonzales, and Tara McKay. 2018. "Effects of Access To Legal Same-Sex Marriage on Marriage and Health: Evidence from BRFSS," NBER Working Paper 24651, National Bureau of Economic Research, Cambridge, MA.

Cohen, Robin A. and Michael E. Martinez. 2014. "Health Insurance Coverage: Early Release of Estimates from the National Health Interview Survey, January–March 2014." National Center for Health Statistics.

Congressional Budget Office. "The Potential Budgetary Impact of Recognizing Same-Sex Marriages." Attachment to a letter to the Honorable Steve Chabot, June 21, 2004. https://www.cbo.gov/publication/15740.

Courtemanche, Charles, Ishtiaque Fazlul, James Marton, Benjamin D. Ukert, Aaron Yelowitz, and Daniela Zapata. 2019. "The Impact of the ACA on Insurance Coverage Disparities After Four Years." NBER Working Paper 26157, National Bureau of Economic Research, Cambridge, MA.

Courtemanche, Charles, James Marton, Benjamin D. Ukert, Aaron Yelowitz, and Daniela Zapata, 2016. "Impacts of the Affordable Care Act on Health Insurance Coverage in Medicaid Expansion and Non-Expansion States." NBER Working Paper 22182, National Bureau of Economic Research, Cambridge, MA.

Dawson, Lindsey, Jennifer Kates, and Matthew Rae. "Access to Employer-Sponsored Health Coverage for Same-Sex Spouses." 2016. *Kaiser Family Foundation*, Nov 2 2016. https://www.kff.org/disparities-policy/issue-brief/access-to-employer-sponsored-health-coverage-for-same-sex-spouses/

Dillender, Marcus. 2015. "Health Insurance and Labor Force Participation: What Legal Recognition Does for Same-Sex Couples." *Contemporary Economic Policy* 33(2): 381-394.

Duggan, Mark, Gopi Shah Goda, and Emilie Jackson. 2017. "The Effects of the Affordable Care Act on Health Insurance Coverage and Labor Market Outcomes." NBER Working Paper 23607, National Bureau of Economic Research, Cambridge, MA.

Frean, Molly, Jonathan Gruber, and Benjamin D. Sommers. 2017. "Premium subsidies, the mandate, and Medicaid expansion: Coverage effects of the Affordable Care Act." *Journal of Health Economics* Volume 53(May 2017): 72-86.

Gates, Gary. "In U.S., LGBT More Likely Than Non-LGBT to Be Uninsured." 2014. Gallup, August 26, 2014. http://news.gallup.com/poll/175445/lgbt-likely-non-lgbt-uninsured.aspx

Ginther, Donna K. and Madeline Zavodny. "Is the male marriage premium due to selection? The effect of shotgun weddings on the return to marriage". 2001. *J Popul Econ* 2001(14): 313-328.

Gonzales, Gilbert. "Association of the New York State Marriage Equality Act With Changes in Health Insurance Coverage.".2015. *JAMA*. 2015;314(7):727–728.

Gonzales, Gilbert and Carrie Henning-Smith. "The Affordable Care Act and Health Insurance Coverage for Lesbian, Gay, and Bisexual Adults: Analysis of the Behavioral Risk Factor Surveillance System." *LGBT Health*. February 2017, 4(1): 62-67.

Greenwood, Jeremy, Nezih Guner, Georgi Kocharkov, and Cezar Santos. 2014. "Marry Your Like: Assortative Mating and Income Inequality." *The American Economic Review*, vol. 104(5): 348-353, May.

International Foundation of Employee Benefit Plans. 2017. "Domestic Partner Benefits." August 1, 2017. https://www.ifebp.org/aboutus/pressroom/releases/Pages/Employers-Dropping-Domestic-Partner-Benefits.aspx

Kates, Jennifer, Usha Ranji, Adara Beamesdurfer, Alina Salganikoff, and Lindsay Dawson. 2018. "Health and Access to Care and Coverage for Lesbian, Gay, Bisexual, and Transgender (LGBT) Individuals in the U.S." *Henry J. Kaiser Family Foundation*, May 3, 2018. https://www.kff.org/report-section/health-and-access-to-care-and-coverage-lgbt-individuals-in-the-us-health-challenges/

KFF (Kaiser Family Foundation). 2014. "2014 Employer Health Benefits Survey Summary of Findings." *Kaiser Family Foundation*. http://files.kff.org/attachment/ehbs-2014-abstract-summary-of-findings.

Long, Sharon K., Nathaniel Anderson, Dana Goin, Michael Karpman, Genevieve M. Kenney, Douglas A. Wissoker, and Stephen Zuckerman. 2014. "QuickTake: Number of Uninsured Adults Falls by 5.4 Million since 2013." *Urban Institute*, April 7, 2014. https://www.urban.org/research/publication/quicktake-number-uninsured-adults-falls-54-million-2013

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas and Matthew Sobek. IPUMS USA: Version 9.0 [dataset]. Minneapolis, MN: IPUMS, 2019. https://doi.org/10.18128/D010.V9.0

Smith, Jessica C. and Carla Medalia. 2015. "Health Insurance Coverage in the United States: 2014." *U.S. Census Bureau*, September 16, 2015. https://www.census.gov/library/publications/2015/demo/p60-253.html

Sommers, Benjamin D., Munira Z. Gunja, Kenneth Finegold, and Thomas Musco. 2015. "Changes in self-reported insurance coverage, access to care, and health under the Affordable Care Act." *JAMA* 314(4): 366–374.

Williams Institute. 2018. "Media Advisory: Medicaid and LGBT Adults." *Williams Institute* January 12, 2018. https://williamsinstitute.law.ucla.edu/williams-in-the-news/28504/

Table 1.1. Descriptive Statistics

Variable	Mean Same-Sex	Mean Opp-Sex
Insurance: Any	0.922	0.906
Insurance: Employer	0.738	0.728
Insurance: Individually Purchased	0.117	0.11
Insurance: Medicaid	0.087	0.086
Less than High School	0.033	0.079
High School	0.203	0.309
Some College	0.222	0.231
College	0.277	0.226
Graduate School	0.264	0.156
Age	45.86	46.672
Female	0.498	0.512
Black	0.047	0.047
Other race	0.073	0.076
Asian	0.05	0.087
Hispanic	0.124	0.143
No Children	0.776	0.391
Observations	48008	3247857

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples and opposite-sex couples, aged 26-64, residing in Medicaid Expansion states.

Table 1.2. Coverage Breakdown

Overall	**Frequency**	**Percent**
No Insurance	3741	7.79
One Type		
Medicaid	3467	7.22
Indiv. Purch.	4216	8.78
Employer	33766	70.33
Other	953	1.99
Multiple Types		
Employer and Indiv	1161	2.42
Employer and Medicaid	452	0.94
Indiv and Medicaid	204	0.42
Employer and Medicaid and Indiv	48	0.1

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Table 1.3. Difference in Differences Intuition

	2010-2013	**2014-2016**
Late Gay Marriage States	neither	a. ACA first b. Gay Marriage second
Early Gay Marriage States	c. Gay marriage first	d. ACA second

Table 1.4. Medicaid Coverage Difference in Differences Table

Medicaid	**Year 2010-2013**	**Year 2014-2016**	**Difference**
ForestGreen**Late Gay Marriage**	0.073	0.128	0.055
Purple**Early Gay Marriage**	0.068	0.104	0.036
Difference	0.005	0.024	0.019

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Late Gay Marriage refers to individuals living in a state that had not passed same-sex marriage before January 1, 2014 (the main implementation date of the Affordable Care Act). Early Gay Marriage refers to individuals living in a state that did pass same-sex marriage before January 1, 2014.

Table 1.5. Individually Purchased Coverage Difference in Differences Table

Individually Purchased	**Year 2010-2013**	**Year 2014-2016**	**Difference**
ForestGreen**Late Gay Marriage**	0.101	0.104	0.003
Purple**Early Gay Marriage**	0.111	0.125	0.014
Difference	-0.01	-0.021	-0.011

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Late Gay Marriage refers to individuals living in a state that had not passed same-sex marriage before January 1, 2014 (the main implementation date of the Affordable Care Act). Early Gay Marriage refers to individuals living in a state that did pass same-sex marriage before January 1, 2014.

Table 1.6. Employer Coverage Difference in Differences Table

Employer	**Year 2010-2013**	**Year 2014-2016**	**Difference**
ForestGreen**Late Gay Marriage**	0.684	0.698	0.014
Purple**Early Gay Marriage**	0.745	0.744	-0.001
Difference	-0.061	-0.046	0.015

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Late Gay Marriage refers to individuals living in a state that had not passed same-sex marriage before January 1, 2014 (the main implementation date of the Affordable Care Act). Early Gay Marriage refers to individuals living in a state that did pass same-sex marriage before January 1, 2014.

Table 1.7. Any Coverage Difference in Differences Table

Any Insurance	**Year 2010-2013**	**Year 2014-2016**	**Difference**
ForestGreen**Late Gay Marriage**	0.845	0.927	0.082
Purple**Early Gay Marriage**	0.9	0.949	0.049
Difference	-0.055	-0.022	0.033

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Late Gay Marriage refers to individuals living in a state that had not passed same-sex marriage before January 1, 2014 (the main implementation date of the Affordable Care Act). Early Gay Marriage refers to individuals living in a state that did pass same-sex marriage before January 1, 2014.

Table 1.8. Difference-in-Differences Model

	Medicaid	Indiv	Employer	Any insurance
LateGayMar*PostACA	0.018	-0.010	0.018	0.035
	(0.008)**	(0.007)	(0.006)***	(0.012)**
N	48,008	48,008	48,008	48,008
Demo. Controls	Yes	Yes	Yes	Yes
State, Year FE	Yes	Yes	Yes	Yes
Exp State Pre-Mean	.068	.111	.738	.897

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.9. Triple Differences, Composite Model

	Medicaid	Indiv	Employer	Any Ins
LateGMar*PostACA*SSC	0.012	-0.009	0.015	0.028
	(0.006)*	(0.006)	(0.006)**	(0.008)***
LateGMar*PostACA	0.006	-0.001	0.004	0.007
	(0.010)	(0.003)	(0.005)	(0.014)
LateGMar*SSC	0.009	-0.011	-0.015	-0.020
	(0.005)	(0.005)**	(0.013)	(0.010)*
PostACA*SSC	-0.004	0.003	0.007	0.003
	(0.005)	(0.003)	(0.004)*	(0.006)
SSC	0.013	0.011	-0.029	-0.007
	(0.003)***	(0.003)***	(0.006)***	(0.005)
N	3,295,865	3,295,865	3,295,865	3,295,865

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married couples (both same-sex and opposite-sex, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.10. Triple Differences, Decomposed Model

	Medicaid	Indiv	Employer	Any Ins
LateGayMar*PostACA*SSC	0.028***	-0.006	-0.051***	-0.012
	(0.010)	(0.006)	(0.017)	(0.015)
EarlyGayMar*GayMarAtTime*SSC	-0.004	0.003	0.024**	0.018
	(0.005)	(0.006)	(0.011)	(0.011)
LateGayMar*GayMarAtTime*SSC	-0.018	-0.002	0.081***	0.050***
	(0.011)	(0.007)	(0.016)	(0.011)
LateGayMar*PostACA	0.002	-0.003	0.004	0.003
	(0.008)	(0.004)	(0.005)	(0.011)
EarlyGayMar*GayMarAtTime	-0.008	-0.004	-0.007	-0.016
	(0.006)	(0.003)	(0.005)	(0.011)
LateGayMar*GayMarAtTime	0.002	0.002	-0.002	-0.000
	(0.006)	(0.001)	(0.003)	(0.005)
SSC	0.016***	0.009	-0.047***	-0.021**
	(0.005)	(0.006)	(0.010)	(0.009)
PostACA*SSC	-0.003	0.003	0.001	-0.002
	(0.005)	(0.003)	(0.004)	(0.006)
LateGayMar*SSC	0.005	-0.009	0.004	-0.007
	(0.006)	(0.007)	(0.015)	(0.013)
Observations	3,295,865	3,295,865	3,295,865	3,295,865
R-squared	0.073	0.015	0.116	0.121
Num. Clusters	24	24	24	24
P-value Early = Late	.260	.589	.008	.044
P-value LateGayMar+PostACA = 0	.198	.211	.003	.000

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Medicaid, Indiv, Employer, and Any Ins are 4 separate dependent variables for separate regressions

SSC is an indicator for Same-Sex Couple status.

P-value Early = Late reports the p-value on the test that the coefficients LateGayMar*GayMarAtTime*SSC and EarlyGayMar*GayMarAtTime*SSC are the same.

P-value Late GayMar+PostACA = 0 reports the p-value on the test that the sum of the coefficients LateGayMar*PostACA*SSC and LateGayMar*GayMarAtTime*SSC is 0.

Table 1.11. DD Model: Heterogeneity by Nonparents

	Medicaid	Indiv	Employer	Any insurance
No Children				
LateGayMar*PostACA	0.018	-0.011	0.016	0.036
	(0.006)**	(0.009)	(0.009)*	(0.012)***
N	37,262	37,262	37,262	37,262
Children				
LateGayMar*PostACA	0.013	-0.004	0.019	0.028
	(0.028)	(0.017)	(0.023)	(0.021)
N	10,746	10,746	10,746	10,746

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.12. DD Model: Heterogeneity by Gender

	Medicaid	Indiv	Employer	Any insurance
Women				
LateGayMar*PostACA	0.032	-0.020	0.037	0.051
	(0.014)**	(0.006)***	(0.013)**	(0.015)***
N	23,897	23,897	23,897	23,897
Men				
LateGayMar*PostACA	-0.000	0.002	-0.000	0.018
	(0.008)	(0.014)	(0.016)	(0.015)
N	24,111	24,111	24,111	24,111

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.13. DD Model: Heterogeneity by Education

	Medicaid	Indiv	Employer	Any Ins
Less than College Degree				
LateGayMar*PostACA	0.013	-0.014	0.016	0.038
	(0.014)	(0.011)	(0.013)	(0.019)*
N	21,401	21,401	21,401	21,401
College Degree or more				
LateGayMar*PostACA	0.004	-0.002	0.017	0.014
	(0.007)	(0.012)	(0.013)	(0.010)
N	25,610	25,610	25,610	25,610

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.14. DD Model: Heterogeneity by Age

	Medicaid	Indiv	Employer	Any insurance
26 to 38				
LateGayMar*PostACA	-0.003	-0.013	0.040	0.033
	(0.016)	(0.011)	(0.020)*	(0.022)
N	13,082	13,082	13,082	13,082
39 to 51				
LateGayMar*PostACA	0.024	-0.001	0.004	0.031
	(0.011)**	(0.016)	(0.021)	(0.016)*
N	17,881	17,881	17,881	17,881
52 to 64				
LateGayMar*PostACA	0.017	-0.003	0.006	0.037
	(0.015)	(0.012)	(0.024)	(0.018)**
N	16,048	16,048	16,048	16,048

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.15. DD Model: Heterogeneity by LASSO-Predicted Risk of No Insurance Under 138% FPL

	Medicaid	Indiv	Employer	Any Ins
Above Median Risk				
LateGayMar*PostACA	0.0153	-0.0169*	0.0249**	0.0361**
	(0.0148)	(0.00884)	(0.00982)	(0.0158)
N	24,065	24,065	24,065	24,065
Below Median Risk				
LateGayMar*PostACA	0.0104	-0.00259	0.00763	0.0166
	(0.00860)	(0.0120)	(0.0119)	(0.0147)
N	23,943	23,943	23,943	23,943

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Weight: perwt from American Community Survey.

Standard Errors: clustered at state level.

LateGayMar refers to individuals living in a state that had not passed gay marriage before January 1, 2014. PostACA is post-Affordable Care Act, 2014-2016.

Table 1.16. Eligibility

Projected Income Eligibility	On Medicaid	Frequency	Percent
Yes	Yes	1924	4.01
Yes	No	2831	5.9
No	Yes	2247	4.68
No	No	41006	85.41

Source: American Community Survey, 2010-2016

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, residing in Medicaid Expansion states.

Note: Projected Income Eligibility for Medicaid based only on FPL, parent status, and state-year. Does not account for SSI/disability.

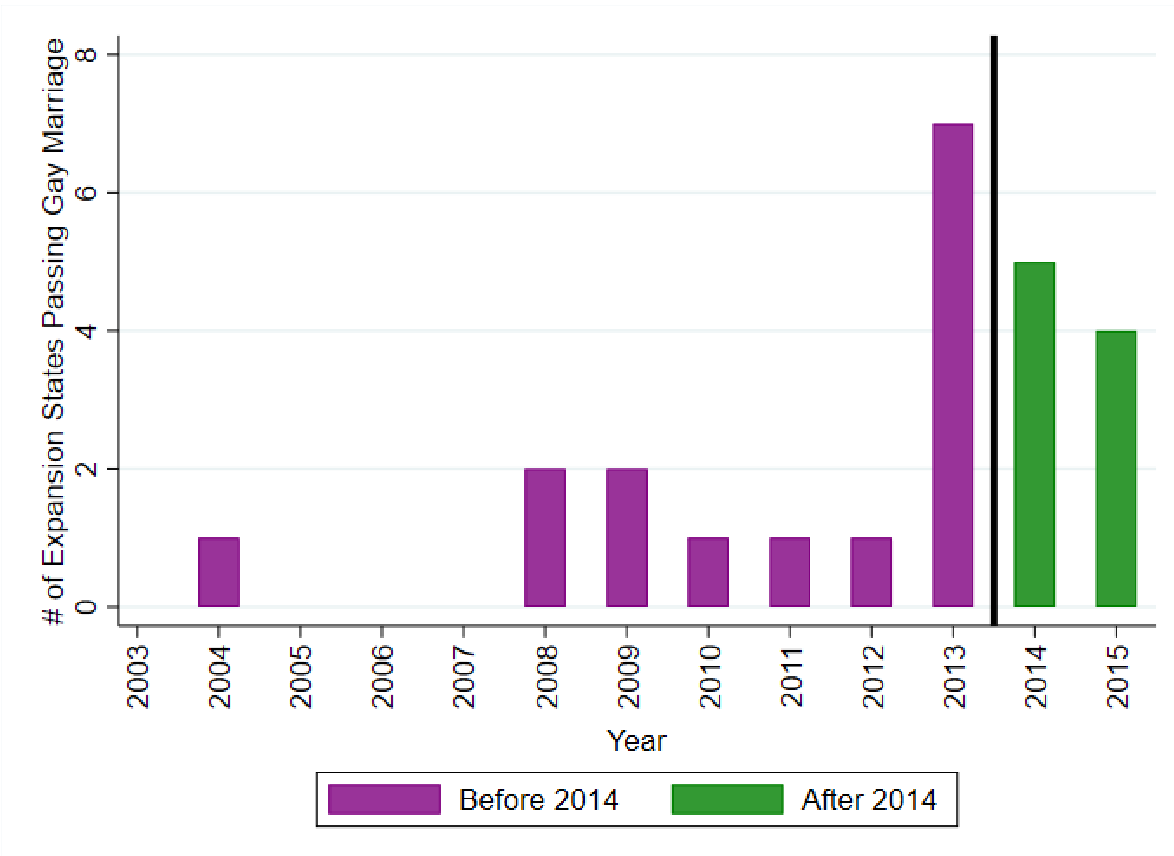


Figure 1.1. Timeline of Same-Sex marriage Passage for Expansion States

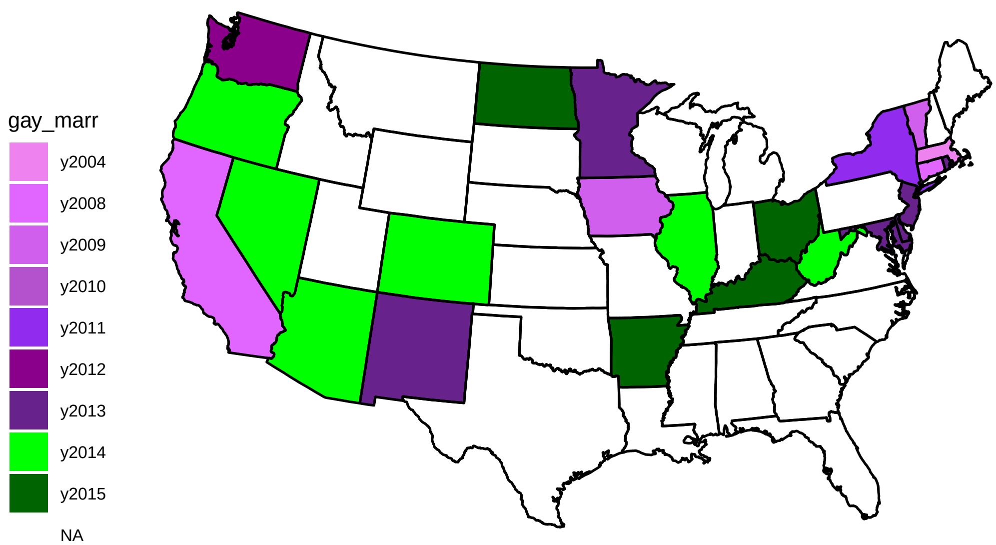


Figure 1.2. Map timeline of Same-Sex marriage Passage for Expansion States

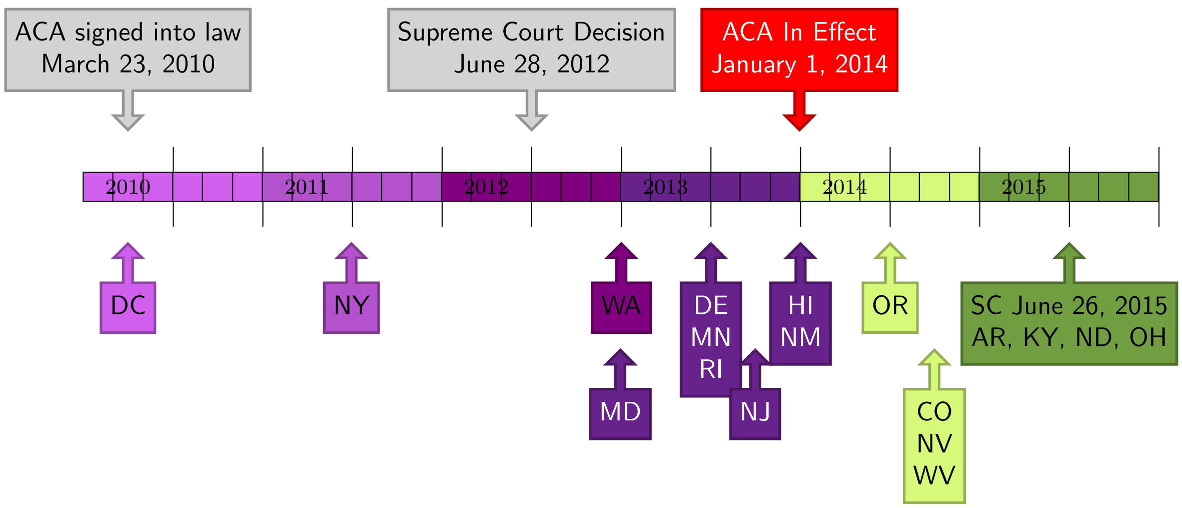


Figure 1.3. Timeline of Same-Sex marriage Passage for Expansion States

2. How is this person related to Person 1?

[ ] Husband or wife

[ ] Biological son or daughter

[ ] Adopted son or daughter

[ ] Stepson or stepdaughter

[ ] Brother or sister

[ ] Father or mother

[ ] Grandchild

[ ] Parent-in-law

[ ] Son-in-law or daughter-in-law

[ ] Other relative

[ ] Roomer or boarder

[ ] Housemate or roommate

[ ] Unmarried partner

[ ] Foster child

[ ] Other nonrelative

Figure 1.4. Options for relationship to head of household question

Source: American Community Survey, from IPUMS

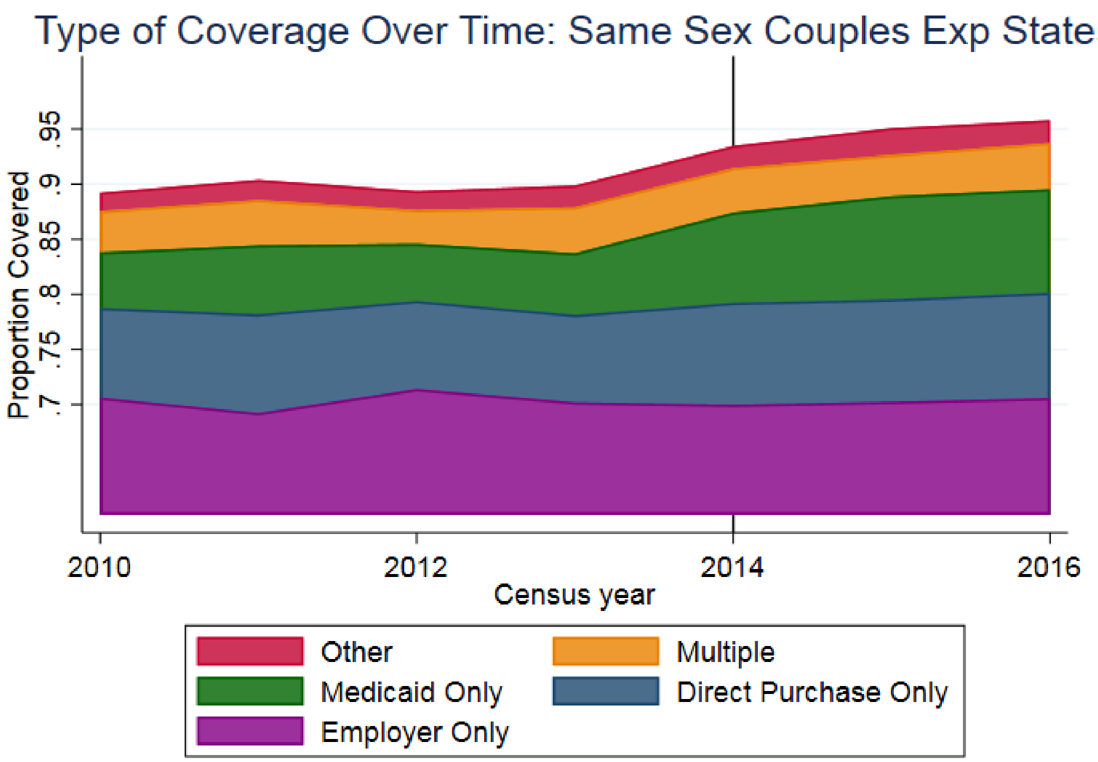


Figure 1.5. Decomposition of Insurance types over time for people in Same-Sex Couples

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

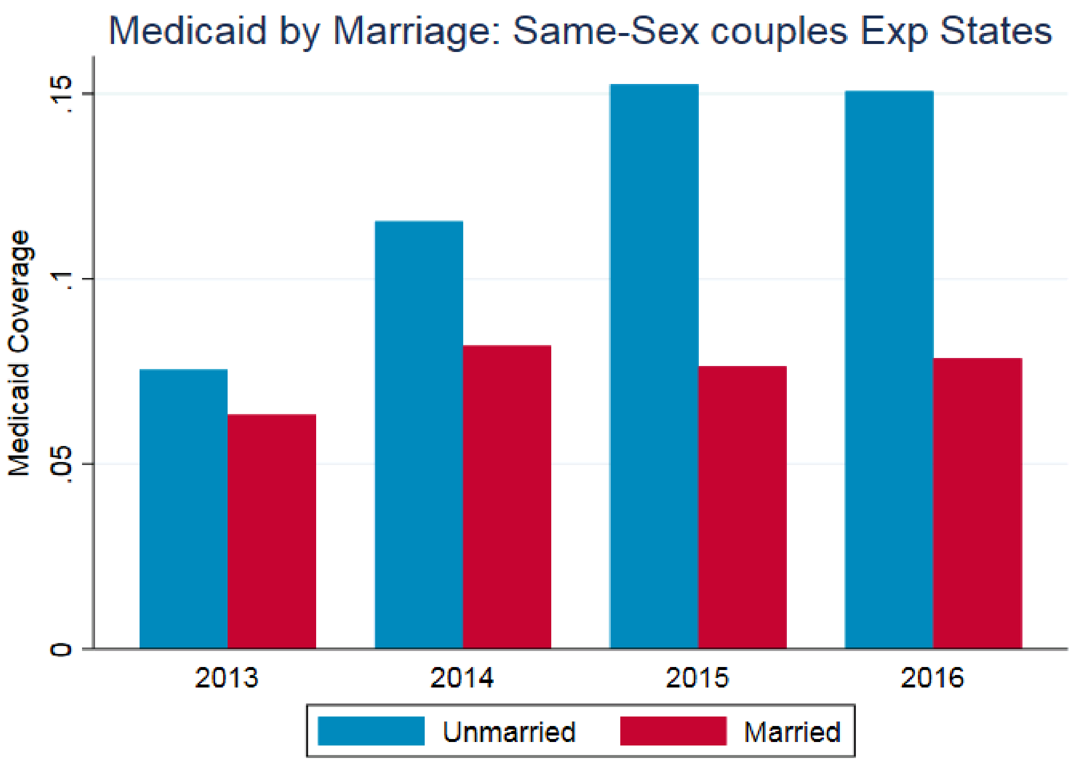


Figure 1.6. Medicaid Coverage in Expansion States

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

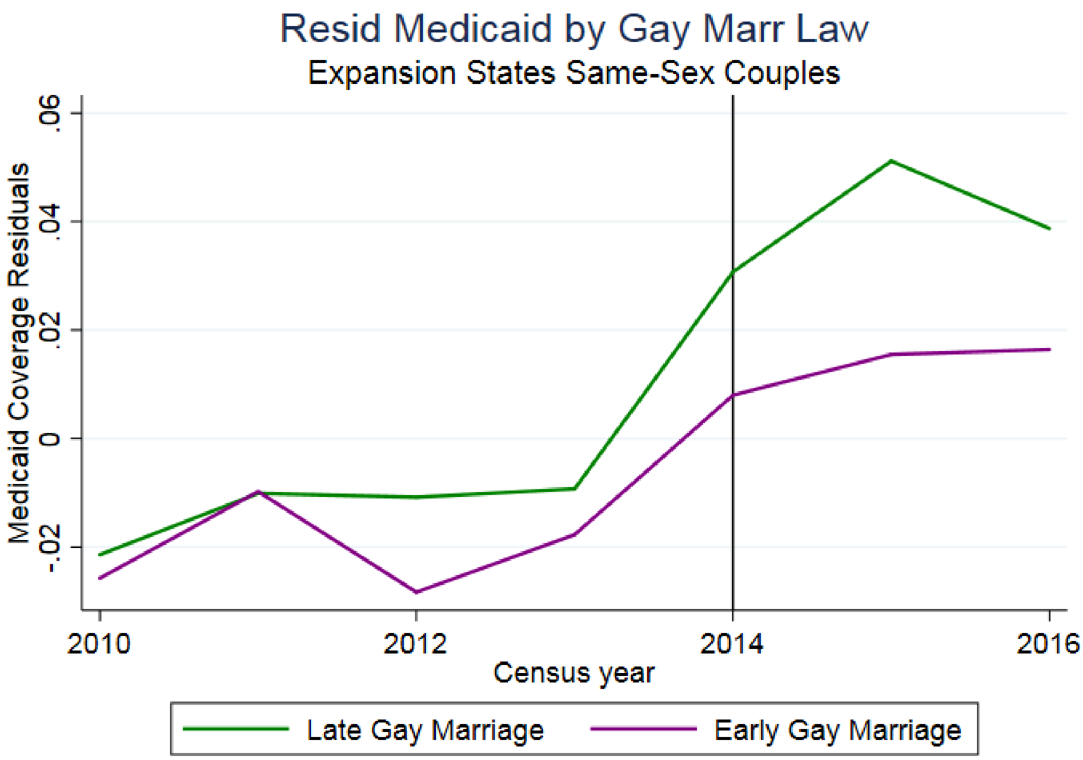


Figure 1.7. Medicaid in Expansion States, by Same-Sex Marriage Law Status, Residualized

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

Residualized by age, race, and gender.

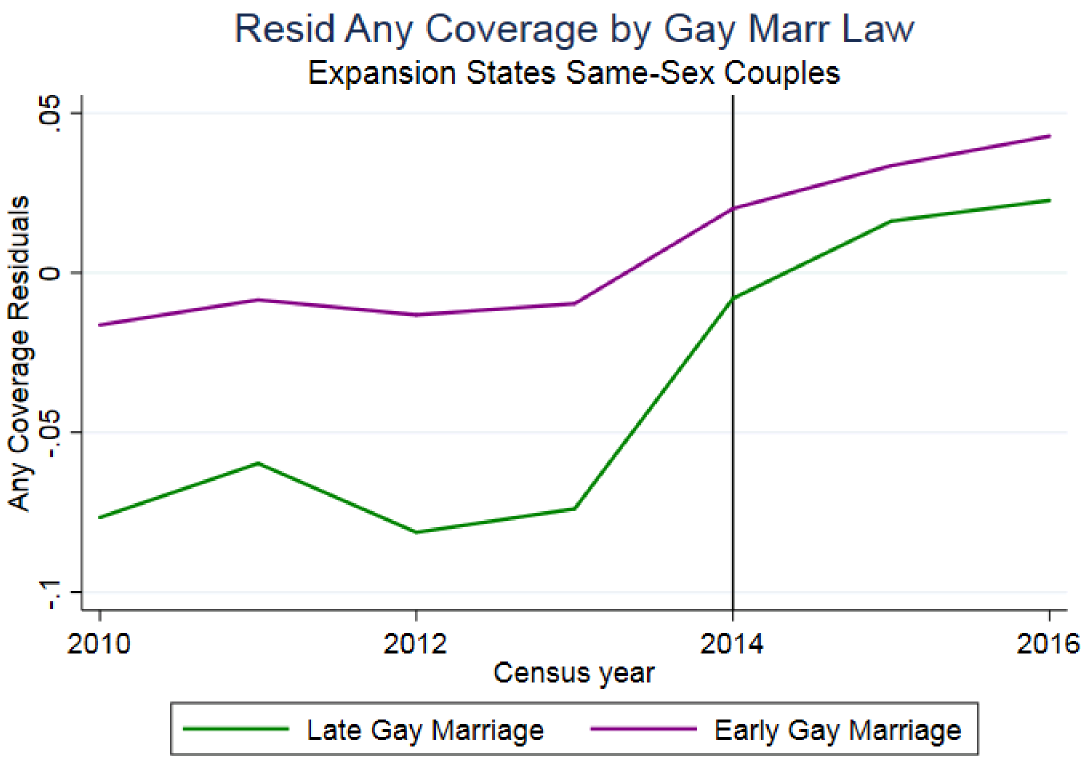


Figure 1.8. Any Coverage in Expansion States, by Same-Sex Marriage Law Status, Residualized

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

Residualized by age, race, and gender.

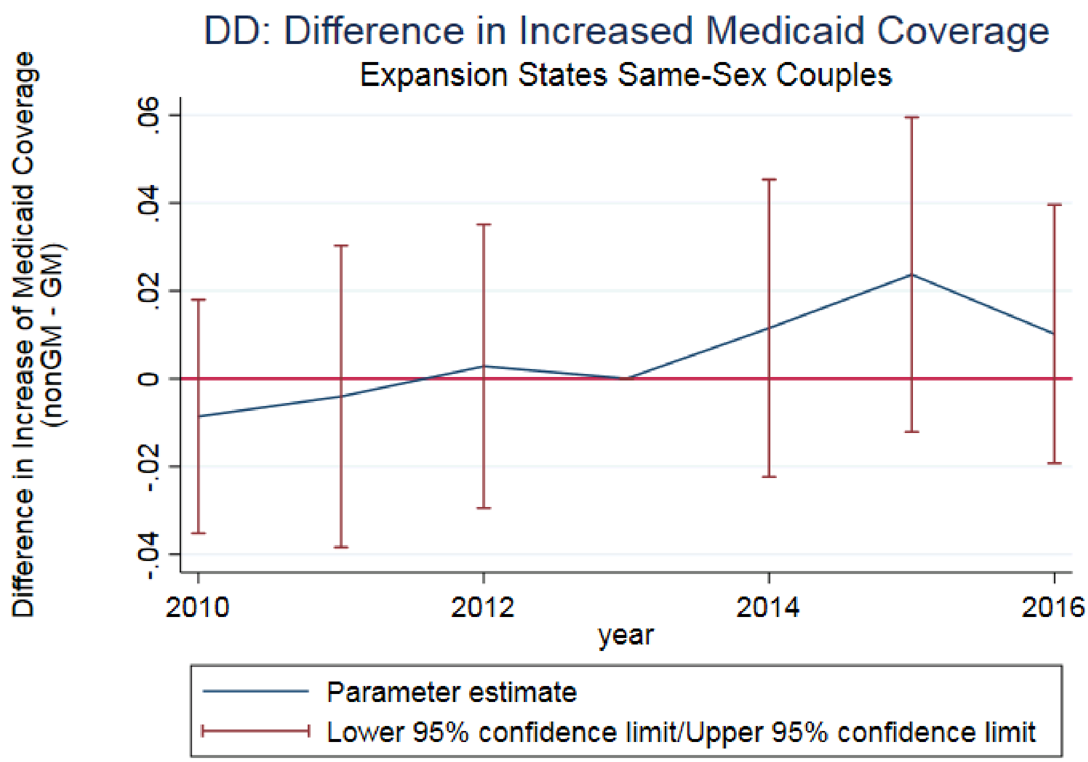


Figure 1.9. Event Study for Medicaid Simple Model

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

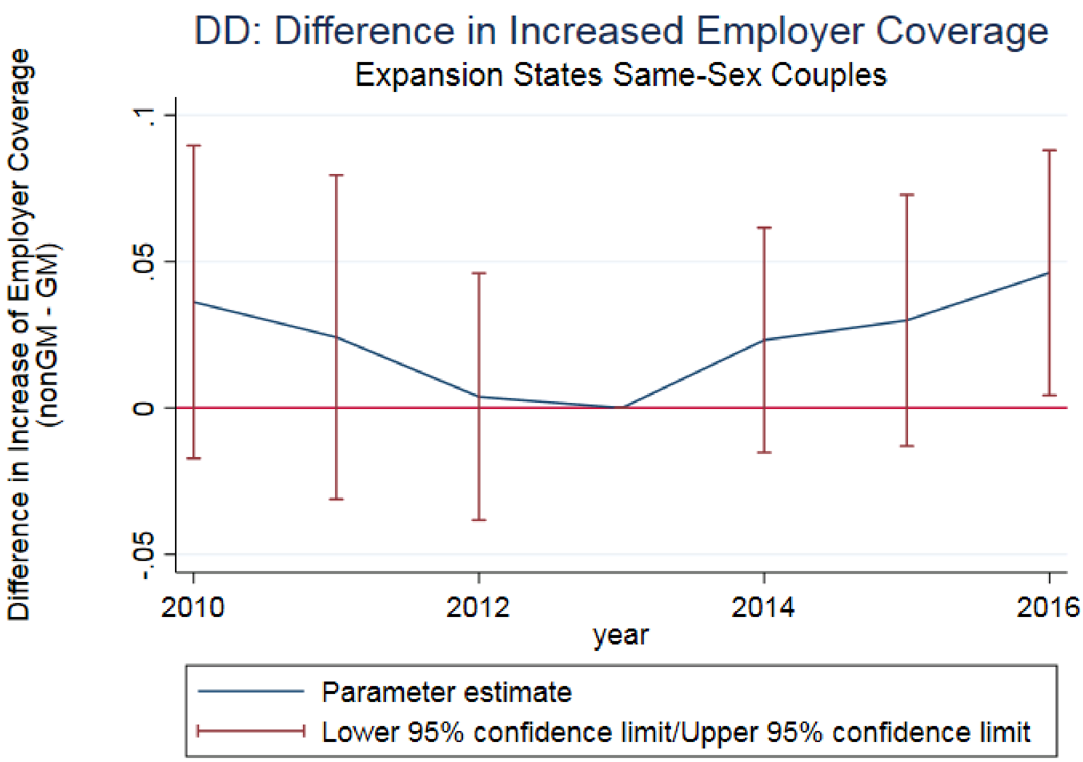


Figure 1.10. Event Study for Employer Coverage Simple Model

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

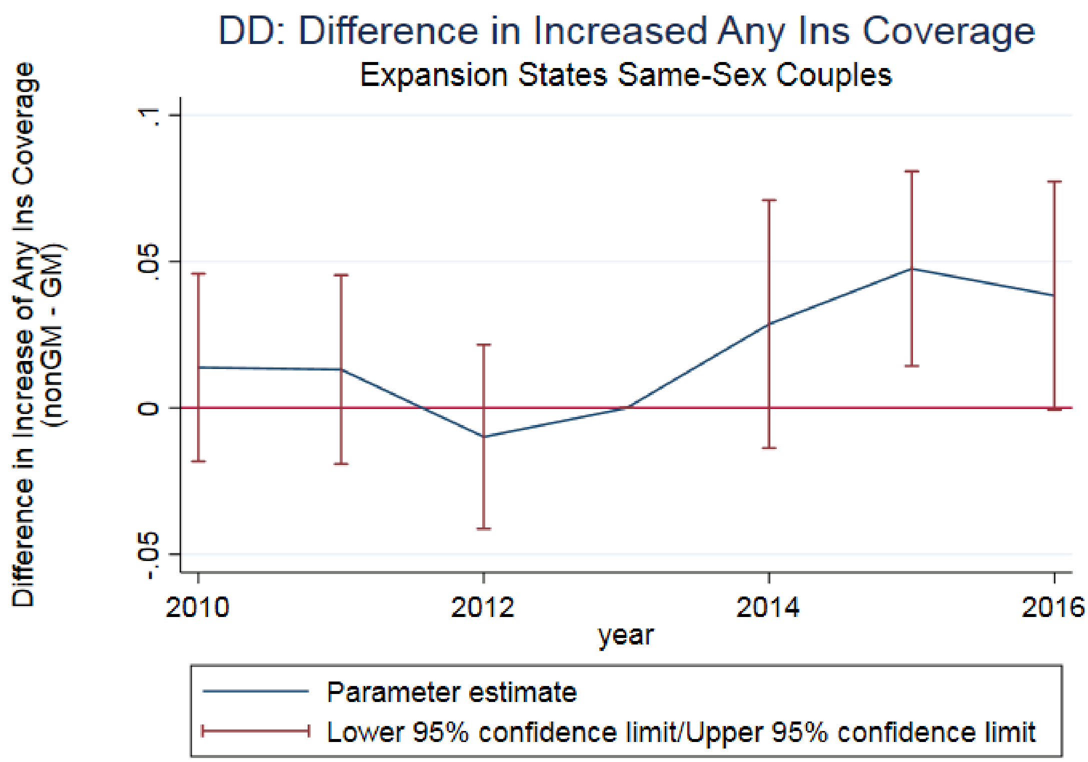


Figure 1.11. Event Study for Any Coverage Simple Model

Sample: Individuals in cohabiting or married same-sex couples, aged 26-64, in Medicaid expansion states.

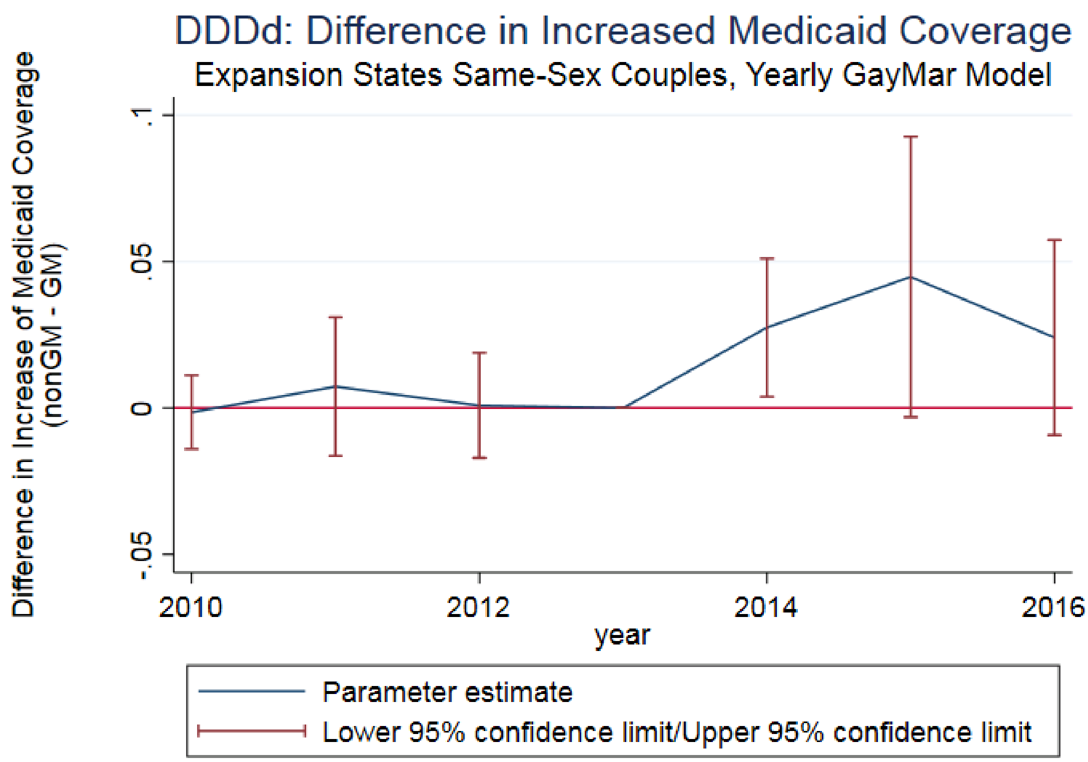


Figure 1.12. Event Study for Medicaid, Same-Sex Marriage Years Model

Sample: Individuals in cohabiting or married same-sex or opposite-sex couples, aged 26-64, in Medicaid expansion states.

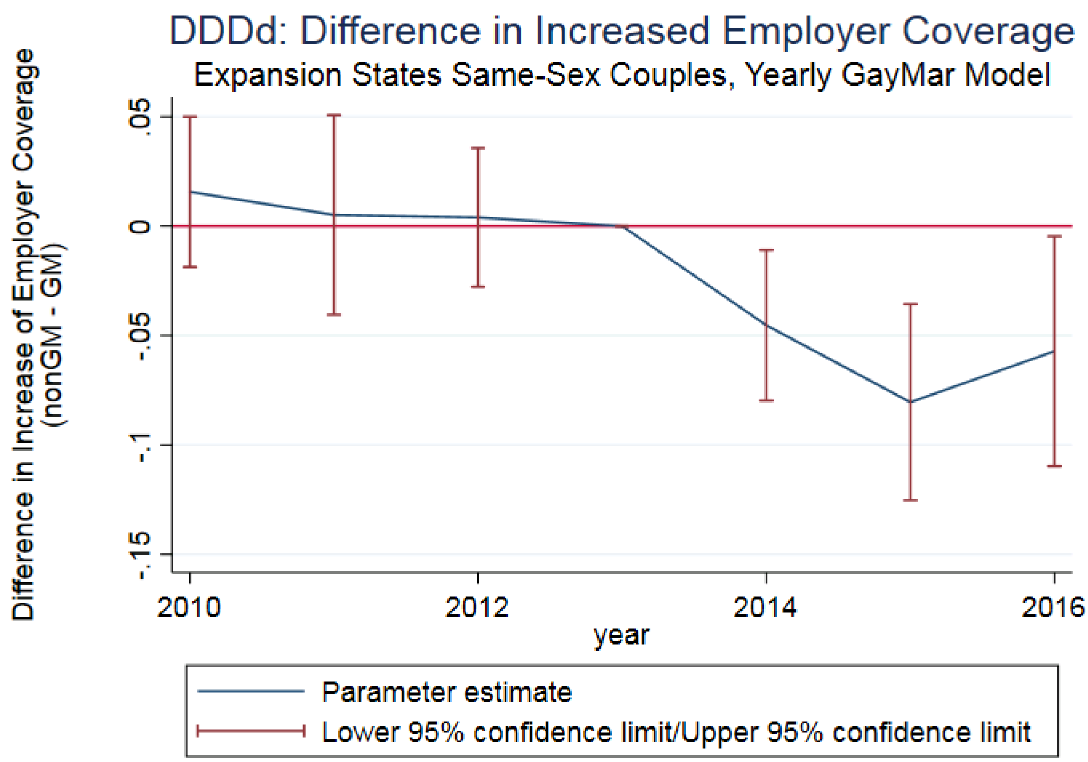


Figure 1.13. Event Study for Employer Coverage, Same-Sex Marriage Years Model

Sample: Individuals in cohabiting or married same-sex or opposite-sex couples, aged 26-64, in Medicaid expansion states.

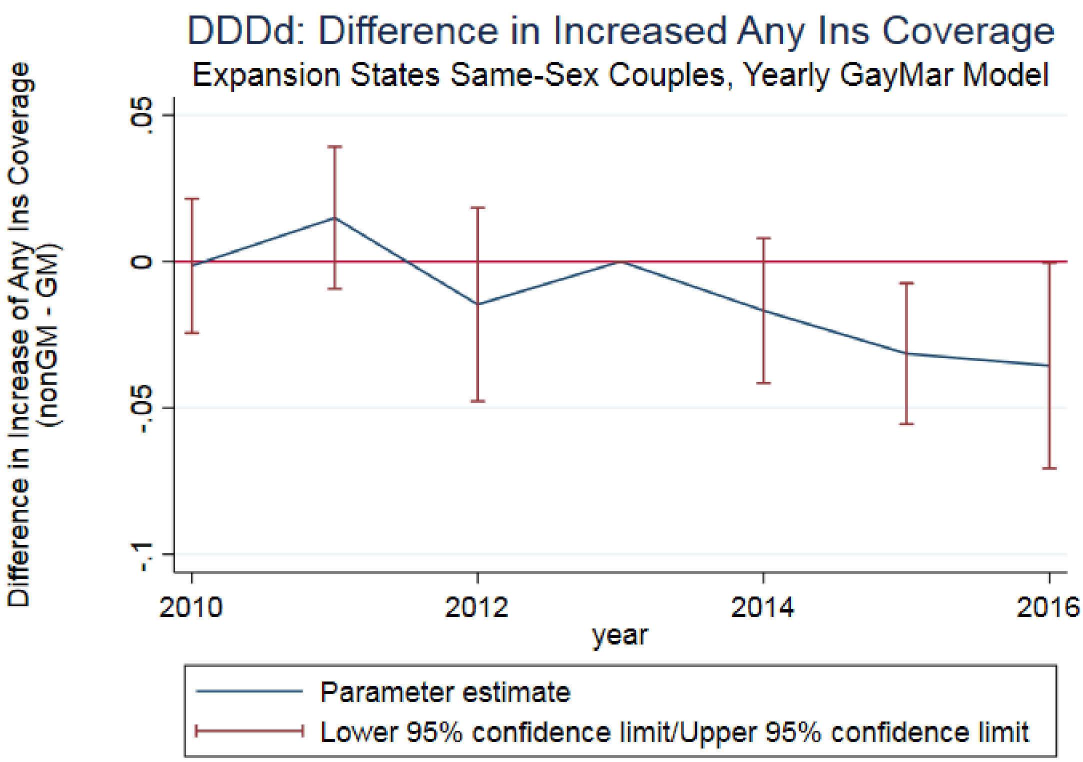


Figure 1.14. Event Study for Any Coverage, Same-Sex Marriage Years Model

Sample: Individuals in cohabiting or married same-sex or opposite-sex couples, aged 26-64, in Medicaid expansion states.

Chapter 2

Homophobia and the Gay-Straight Wage Gap

2.1 Introduction

In the past couple of years, there have been significant changes for the LGBT community in terms of increasing public acceptance and rights like gay marriage. However, these changes have not been reflected in laws concerning employment discrimination. As of 2018, it is legal to fire or discriminate against an employee because of their sexual orientation in 28 US states (see Figure 2.1). In this context it is interesting to examine the relationship between homophobic attitudes, which vary by state and over time, and wages of gay men.

Existing research has worked on documenting wage differences between gay and straight workers. Most have found a wage penalty for gay men (Antecol, Jong, Steinberger 2008; Badgett 1995; Blandford 2003; Carpenter 2007; and Algretto and Arthur 2007), although a recent paper by Carpenter and Eppink 2017 found a wage premium. A main challenge in this vein of literature is the lack of data that identifies sexual orientation. In large data sets, the best available methods have been to identify same-sex cohabiting couples or to use questions on past sexual partners. There are some smaller data sets in more recent years that include survey questions about sexual orientation, but those can be limited in sample size or with which control variables are available. Calculated wage gaps vary with how sexual orientation is identified in the data set, whether couples or single people are included, whether partnered gay men are compared to married or unmarried straight men, the time period of the sample, the size of the sample, and what controls are available.

To answer the question of how variation in attitudes toward gay people is related to gay men's wages, I use the American Community Survey (ACS) in order to be able to match to state-year level homophobia index measures. In the ACS, it is possible to tell if people who indicate they are unmarried partners are the same sex. Therefore, the wage gap I calculate will be the gap between men with opposite-sex cohabiting partners and same-sex cohabiting partners. To my knowledge, this data set and time frame have not been used to calculate the wage gaps, so the first step in this paper is to establish that the wage penalty for men in same-sex couples in this data is approximately 7.5%.

To measure attitudes, I create a geographic homophobia index from normalized responses to questions about attitudes toward gay people in the General Social Survey.[1] My final contribution is to show that this prejudice index is negatively associated with the wages of men in same-sex couples. An increase of one standard deviation of this prejudice index is associated with a 4% wage penalty for men in same-sex relationships.

In the wage gap literature, the work of Charles and Guryan (2008) is relevant for the methods used in this paper. In Gary Becker's 1957 *The Economics of Discrimination*, he puts forth the theory of the "marginal discriminator" to explain the black-white wage gap. The theory models discrimination as an extra cost that prejudiced employers face in hiring African-Americans. It implies that because African-Americans will be hired by those with the least extra discrimination cost, the wage gap will be determined by the level of prejudice of the last employer along the distribution to hire African-Americans. This marginal employer, by construction, will have the level of prejudice at the p^{th} percentile of the prejudice distribution, where p is the percent of African-Americans in the population. Charles and Guryan directly test this theory using GSS data to determine the distribution of prejudice by state and the CPS to determine the wage gap by state in their 2008 paper "Prejudice and Wages: An Empirical Assessment of Becker's *The Economics of Discrimination*".

The theory of the marginal discriminator is not necessarily the optimal model in the case of sexual orientation. A key implicit assumption that allows the sorting of a marginalized group into the least discriminatory employers is that the characteristic is identifiable by employers at the time of hire. Sexual orientation is a concealable characteristic on a resume and at an interview. The implicit assumption for the model in this paper is that while sexual orientation is not observable at time of hire, there may be a certain probability of coworkers finding out each year because concealing such an important aspect of identity can be costly in both effort and

[1] Some of the data used in this analysis are derived from Sensitive Data Files of the GSS, obtained under special contractual arrangements designed to protect the anonymity of respondents. These data are not available from the author. Persons interested in obtaining GSS Sensitive Data Files should contact the GSS at GSS@NORC.org.

psychological toll. This will complicate sorting, so the more relevant measure of prejudice for this paper is the average discriminator rather than the marginal discriminator. This paper does use the method from Charles and Guryan 2008 of using GSS questions to construct distributions of prejudice. [2]

The structure of the paper is as follows: Section 2 describes the data sources, Section 3 explores the wage gaps in the 2008-2016 ACS data and compares the results to Carpenter and Eppink 2017, Section 4 details the construction of the homophobia index, Section 5 looks at the relationship between the homophobia index and the wage gaps, Section 6 analyzes the possibility of some non-discrimination explanations for the results, and Section 7 concludes.

2.2 Data

2.2.1 American Community Survey

Individual-level information for this analysis is from the American Community Survey (ACS). Starting in 1990, the Census added a category for relationship to head of household called "Unmarried Partner"; this category remained as an option on the ACS post-2000. When both the head of the household and the unmarried partner are male, the men are in a same-sex relationship.[3] It is impossible to identify bisexual men using this strategy because they could be in relationships with either men or women. This means that the results speak to the differences between men in cohabiting same-sex couples and men in cohabiting or married opposite-sex couples. I can not draw conclusions about single people.

Variables from this data include wages, education, age, state, occupational category, and race.[4] Wages are adjusted to be in 2012 dollars[5] and occupation is coded into 25 categories.[6]

[2]At the time this paper was written, this prejudice index was a novel contribution to the literature. However, a similar competing paper that was written concurrently and independently has since been published in Burn(2020).

[3]Cohabiting partner is a category separate from roommate

[4]Potential experience is calculated as age minus years education minus six as in Antecol, Jong & Steinberger 2008

[5]source:
http://www.bls.gov/regions/mid-atlantic/data/ConsumerPriceIndexAnnualandSemiAnnual_Table_PDF.pdf

[6]source: https://usa.ipums.org/usa/volii/occ_acs.shtml

While gay marriage was legal during parts of this time period in several states starting in 2004, it was not included as an option on the ACS for same-sex couples until 2013. Therefore, it is not possible to tell which of the same-sex couples in the sample are married before 2013.

The time period used in these models is from 2008-2016. After an analysis of misallocation in the ACS due to issues like those, Gates and Steinberger suggest that the post-2005 ACS is the most accurate in terms of identifying same-sex couples (Gates and Steinberger 2010). This time period was chosen because of the changing recoding policies of the ACS. These policies are important because even small errors in reporting gender for couples who are in reality opposite-sex can cause large errors in estimating characteristics of same-sex couples because there are so few same-sex couples compared to opposite-sex couples. Another important policy is how the ACS recodes same-sex couples who report that they are married. Further, changes made in the layout, design, and reconciling of multiple markings for the 2008 ACS made the counts of same-sex couples more accurate (Lofquist et al. 2010).

Table 2.1 documents differences between men with same-sex cohabiting partners and men with opposite-sex cohabiting partners in the ACS. The raw difference in log wages is not large. However, men in same-sex couples are more educated; 30.8% have a college level education and 25.4% have graduate level education compared with 22.8% and 15.7% for men in opposite-sex couples. Men in same-sex couples are more likely to live in urban areas and on average have fewer children.

2.2.2 General Social Survey

The index of prejudice is created from several questions on the General Social Survey (GSS) from 2004-2016. Data at the state level were used.

Five questions are consistently asked from 2004-2016 that can be used to measure homophobic attitudes. They concern the morality of same-sex sexual relations and attitudes about the civil liberties of gay people including speech, marriage, ability to be professors, and book censorship. These questions can be found in Appendix A.

A nuance to the interpretation of this index is that people systematically underreport homophobic attitudes in surveys (Coffman et al. 2013). This implies that the homophobia index is most likely picking up the answers people think are societally acceptable rather than their actual level of prejudice. This is still a useful measure because it should be strongly correlated with average individual prejudice. The interpretation of the results can shift from the relationship between actual prejudice and wages to the relationship between perceptions of societally acceptable levels of prejudice and wages.

2.3 Wage Gaps

2.3.1 Model

Existing work estimating wage gaps for men in same-sex couples was performed on the 1990 Census and 2000 Census. Estimating overall wage gaps for 2008-2016 using the ACS will be useful as well:

$$logwages_i = \beta_0 + \beta_1 SS_i + \beta_2 X_i + \eta_s + \varepsilon$$

This equation is estimated both pooled and with additional interactions between the same-sex couple indicator and years. β_1 is the variable of interest. X is a vector of characteristics that includes education level indicator variables, experience, experience squared, a series of race and ethnicity indicator variables, a series of indicator variables for occupational categories, urban status, and number of children. The regressions are estimated at the individual level, *i*, and include fixed effects for states.[7] The sample is restricted to full-time workers aged 30-65 in order to measure the differences after most education is completed.

In a similar vein to Blau and Khan 2017's "human capital specification" vs "full specification", I first run the model with only state fixed effects, year fixed effects, race indicators, education, experience, and experience squared. As in their paper exploring the male-female wage gap over time, the interpretation of occupation controls is potentially ambiguous. Previous

[7] All regressions using the ACS data are weighted by the given person weight (PERWT).

work in Antecol, Jong, and Steinberger 2008 documented how men in same-sex couples disproportionately select into occupations that are majority female. This is also true in the 2008-2016 data: 56% of men in same-sex couples work in occupations that are majority female, where 33% of men in opposite-sex couples do. Since people who work in majority female occupations have lower wages on average, this could contribute to a wage gap. However, this selection could be driven by discrimination because women in general are less homophobic.

The third specification adds urban status and number of children, which differ greatly between men in same-sex couples and men in opposite-sex couples. It is important to note that states often pass adoption laws designed to prevent same-sex couples from adopting children and this is more likely to occur in states with more homophobic citizens and lawmakers.

2.3.2 Results

The overall wage gap for the pooled 2008-2016 time period is found in Table 2.2. Adding occupational controls in specification 2 and controlling for number of children and urban status in specification 3 reduce the observed wage gap. For this paper, I will focus on the controls used in specification 3, the full specification, where the wage penalty against men in same-sex couples is about 7.5%. Table 2.3 are the results when the regression is run with interactions between the same-sex couple indicator and the year to determine if the wage gap is changing over this time period. The wage gaps are not significantly different from each other except for 2014, when men in same-sex couples earn more.

2.3.3 Marriage

Much of the previous research has separated the opposite-sex comparison group into married and cohabiting couples. The case for doing so in this time period is less clear because while some same-sex couples could and did select into marriage, they cannot be identified in the data for 2008-2012. Married men in opposite-sex couples make more than cohabiting men in opposite-sex couples; previous literature has looked into the marriage premium for straight men and selection into marriage. Table 2.4 breaks out these groups and is largely consistent with the previous literature in that men in same-sex couples make less than married men in opposite-sex couples but more than unmarried men in opposite-sex cohabiting couples.

2.3.4 Comparison with NHIS

Carpenter and Eppink 2017 found a wage premium for gay men using the National Health Interview Survey (NHIS) from 2013-2015. One of the main differences between using the data in the ACS and the NHIS is how sexual orientation is identified. In the ACS, it must be inferred from the sex of a cohabiting partner, but in the NHIS it is asked directly. This allows the NHIS estimates to identify single gay men and bisexual men. Carpenter and Eppink point out that in the work on the wage gap, estimates using data sources that identify single people tend to find smaller wage gaps. This observation could be consistent with a model where it is more costly to conceal sexual orientation when you have a committed cohabiting partner, so possible discrimination could affect gay men in couples more than single gay men. It could also be consistent with household specialization.

There are some differences in available data in the NHIS and the ACS. In the NHIS, the data are only available at the Census Region level (Northeast, South, West, and Midwest). Urban status is not available. Variables that are available and used as controls in the NHIS that are not available in the ACS include firm size category, tenure at firm, and employment sector.

In order to make a closer comparison between the ACS and NHIS data, I ran the ACS model with the controls, sample, and time frame as close to the Carpenter and Eppink paper as possible. The results are in Table 2.5. The first column uses controls consistent with the rest of this paper and the second uses controls as similar to the NHIS as possible. The restriction to the same sample as the NHIS, including the time period of 2013-2015, reduces the wage gap from 7.5% to 6.6%. Using controls more consistent with the NHIS further reduces the penalty by 2.8pp to 3.8%.

2.4 Homophobia Index

2.4.1 Construction

A homophobia index is calculated using a method similar to the one used by Charles and Guryan 2008 to create their racial prejudice index. First, the responses are normalized by question:

$$dnorm_{it}^{k} = \frac{d_{it}^{k} - E\left[d_{i,2006}^{k}\right]}{\sqrt{Var\left(d_{i,2006}^{k}\right)}}$$

In this equation, d_{it}^{k} is individual i's response to question k in year t. I normalize using the first year of the sample, 2006. These normalized answers were averaged by person to calculate the prejudice index:

$$D_{it} = \sum_{k} \frac{dnorm_{it}^{k}}{5}$$

The prejudice measure is the average level of homophobia for all individuals in a region in the year that the wages were earned as well as the previous three years. Because the GSS is conducted in even-numbered years in the 2000s, this means that starting in 2006, the prejudice measure averages two waves of the survey for each state. The state-year index is then standardized so that an increase of 1 in the index corresponds to a 1 standard deviation increase in prejudice for the time period.

2.4.2 Trends

Table 2.6 shows descriptive statistics for the state-year level index. Overall, answers to these questions are becoming less homophobic over time, with the average yearly index decreasing from .292 in 2006-2007 to -.266 in 2014-2015. Figure 2.2 (2.3 in greyscale) shows a map of the prejudice measure in 2006 and Figure 2.4 (2.5 in greyscale) maps the 2015 levels of the prejudice measure. There is considerable regional variation in the level of homophobia displayed in the answers to these questions. The general range for the state-year measure is about -3 standard deviations to 3 standard deviations.

2.4.3 Relationship to Policy

One check of this measure is to see its relation to policy changes related to gay rights. The correlation of the region-year prejudice variable and an indicator about whether an Employment Non-Discrimination Act (ENDA) that covered sexual orientation was in place in that state-year combination is -.58: the more prejudiced an area is, the less likely they are to have passed an ENDA.

The correlation of the prejudice variable with the passage of gay marriage in that state and year is -.35. The weaker negative correlation makes sense because gay marriage was often decided by court cases rather than the legislature or popular vote and court cases can be less directly connected to public opinion.

2.5 Homophobia and Wage Gaps

2.5.1 Model

The main model is as follows:

$$logwages_{isy} = \beta_0 + \beta_1 SS_i + \beta_2 Avg.Prej_{sy} + \beta_3 SS * Avg.Prej_{isy} + \\ \beta_4 SS * Year_{iy} + \beta_5 Year + \beta_6 X_i + \gamma_s + \varepsilon_{isy}$$

The coefficient of interest is β_3, the interaction between whether a man is in a same-sex relationship (SS) and the level of prejudice he faces in his state in that year. This regression is run at the individual level with fixed effects for year (y), state (s), and the interaction of same-sex and year. X is the full list of controls that were used in the wage gap calculation.

2.5.2 Results

The results for the main specification are in Table 2.7. The coefficient of interest, the interaction of whether a man is in a same-sex cohabiting couple and the level of prejudice in his state in that year, is statistically significant and negative. An increase of 1 standard deviation in the homophobia index is associated with a 4% decrease in wages for men in same-sex couples in that state and year.

Table 2.8 separates the control group of men in opposite-sex couples into married and cohabiting. The interaction term remains consistent at about a 4% wage penalty for men in same-sex couples with a one standard deviation increase in prejudice. The remaining wage gaps are consistent with the earlier results that men in same-sex couples make less than men in opposite-sex marriages. The coefficient on the same-sex indicator for the cohabiting control group is small and positive, but not statistically significant.

2.6 Exploring Alternative Explanations

There are several possible explanations for these findings that are not discrimination. Some of those explanations can be tested with observable data.

2.6.1 Occupational Selection

Previous findings have indicated that men in same-sex couples disproportionately select into occupations that are majority female. If states that are homophobic also pay lower wages in female-dominated occupations, that could lead to the observed relationship between homophobia and wages of gay men. To test whether men in female-dominated occupations in homophobic states make less, I test the following model:

$$logwages_{isy} = \beta_0 + \beta_1 MajFem_i + \beta_2 Avg.Prej_{sy} + \beta_3 MajFem * Avg.Prej_{isy} + \beta_4 SS * Year_{iy} + \beta_5 Year + \beta_6 X_i + \gamma_s + \varepsilon_{isy}$$

This is the main model with an indicator for whether a man works in a majority female occupation substituted for the same-sex couple indicator. β_3 is again the coefficient of interest.

The results of this specification are in Table 2.9. With a coefficient of 0.0002 that is not statistically significant, it is not the case that more homophobic areas are paying workers in female-dominated occupations less than non-homophobic areas. As expected, the coefficient on the majority female occupation indicator is statistically significant and negative.

2.7 Conclusion

In the 2008-2016 time period, there is a wage penalty for men in same-sex couples in the ACS. This contrasts with the findings of Carpenter and Eppink 2017 in the NHIS of a wage premium for gay men in 2013-2015 and the differences could be explained by who is in the sample and what different data sets allow researchers to control for. This is illustrative of the need mentioned in their paper for more data sets to collect information on sexual orientation and identity so that these questions can be more fully investigated. Increasing data availability is particularly important in a context where discrimination on the basis of sexual orientation is legal in most states in employment, housing, and other aspects of life.

This paper shows that homophobic attitudes are negatively associated with wages of men in same-sex couples in the United States. An increase of one standard deviation in a prejudice index constructed based on responses to questions concerning attitudes toward gay men is associated with a 4% drop in wages for men in same-sex relationships. This holds true whether you compare men in same-sex couples to cohabiting men in opposite-sex couples or married men in opposite-sex couples. It is not caused by sorting into female-dominated occupations and it is not fully explained by selective mobility. Further work is needed investigate this relationship between prejudiced attitudes and wages.

Appendix

2.A Appendix A

For all parts of question 77, “Don’t know” was recoded to be the middle level of prejudice. If a person cannot decide whether a gay person should have basic civil rights, that indicates some level of prejudice. The context for the phrasing of questions 77 and 218 in 1973 is that same-sex sexual activity was a felony in the majority of US states at the time.
77. And what about a man who admits that he is homosexual?
A. Suppose this admitted homosexual wanted to make a speech in your community. Should he be allowed to speak, or not?

- 1 = Yes, allowed to speak
- 2 = Not allowed
- 8 = Don’t know

B. Should such a person be allowed to teach in a college or university, or not?

- 4 = Yes, allowed to teach
- 5 = Not allowed
- 8 = Don’t know

C. If some people in your community suggested that a book he wrote in favor of homosexuality should be taken out of your public library, would you favor removing this book, or not?

- 1 = Remove
- 2 = Not remove
- 8 = Don't know

218. What about sexual relations between two adults of the same sex- do you think it is always wrong, almost always wrong, wrong only sometimes, or not wrong at all?

- 1 = Always wrong
- 2 = Almost always wrong
- 3 = Wrong only sometimes
- 4 = Not wrong at all

1513. Do you agree or disagree?

J. Homosexual couples should have the right to marry one another.

- 1 = Strongly agree
- 2 = Agree
- 3 = Neither agree or disagree
- 4 = Disagree
- 5 = Strongly disagree

References

Allegretto, Sylvia A., and Michelle M. Arthur. 2001. “An Empirical Analysis of Homosexual/Heterosexual Male Earnings Differentials: Unmarried and Unequal?” *Industrial and Labor Relations Review* 54(3):631-46.

Antecol, Heather, Anneke Jong and Michael Steinberger. 2008. “The Sexual Orientation Wage Gap: The Role of Occupational Sorting and Human Capital.” *Industrial and Labor Relations Review* 61(4): 518–543.

Badgett, M. V. Lee. 1995. “The Wage Effects of Sexual Orientation Discrimination.” *Industrial and Labor Relations Review* 48(4): 726–739.

Black, Dan A., Seth G. Sanders, and Lowell J. Taylor. 2007. “The Economics of Lesbian and Gay Families.” *The Journal of Economic Perspectives* 21(2): 53–70.

Blandford, John M. 2003. “The Nexus of Sexual Orientation and Gender in the Determination of Earnings.” *Industrial and Labor Relations Review* 56(4): 622-42.

Blau, Francine D. and Lawrence M. Kahn. 2017. “The Gender Wage Gap: Extent, Trends, and Explanations.” *Journal of Economic Literature* 55(3):789-865.

Burn, Ian. (2020). “The Relationship between Prejudice and Wage Penalties for Gay Men in the United States.” *Industrial Labor Relations Review* 73(3): 650–675.

Carpenter, Christopher. 2007. “Revisiting the Income Penalty for Behaviorally Gay Men: Evidence from NHANES III.” *Labour Economics* 14(1): 25-34.

Charles, Kerwin Kofi and Jonathan Guryan. 2008. “Prejudice and Wages: An Empirical Assessment of Becker’s The Economics of Discrimination.” *Journal of Political Economy* 116(5): 773-809.

Coffman, Katherine B., Lucas C. Coffman, and Keith M. Marzilli Ericson. “The Size of the LGBT Population and the Magnitude of Anti-Gay Sentiment are Substantially Underestimated.” NBER Working Paper 19508, National Bureau of Economic Research, Cambridge, MA.

Gates, Gary J and Michael Steinberger. 2010. “Same-Sex Unmarried Partner Couples in the American Community Survey: The Role of Misreporting, Miscoding and

Misallocation." http://economics-files.pomona.edu/steinberger/research/Gates_Steinberger_ACS_Miscode_May2010.pdf

Hunt, Jerome. "A State-by-State Examination of Nondiscrimination Laws and Policies." June 2012. *Center for American Progress Action Fund*. https://cdn.americanprogress.org/wp-content/uploads/issues/2012/06/pdf/state_nondiscrimination.pdf

Jepsen, Christopher and Lisa Jepsen. 2017. "Self-employment, earnings, and sexual orientation." *Review of Economics of the Household* 15(1): 287–305

Klawitter, Marieka. 2015. "Meta-analysis of the effects of sexual orientation on earnings." *Industrial Relations: A Journal of Economy and Society* 54(1): 4–32

Lofquist, Daphne, Terry Lugaila, Martin O'Connell, and Tavia Simmons. "New Estimates of Same-Sex Couple Households from the American Community Survey." *U.S. Census Bureau Housing and Household Economic Statistics Division*. Annual meeting of the Population Association of America, Dallas, Texas, April 15-17, 2010. https://www.census.gov/population/www/socdemo/hh-fam/SS_new-estimates.pdf

Movement Advancement Project. "Non-Discrimination Laws." *Movement Advancement Project*. http://www.lgbtmap.org/equality-maps/non_discrimination_laws

Ruggles, Steven, Sarah Flood, Ronald Goeken, Josiah Grover, Erin Meyer, Jose Pacas and Matthew Sobek. IPUMS USA: Version 9.0 [dataset]. Minneapolis, MN: IPUMS, 2019. https://doi.org/10.18128/D010.V9.0

Table 2.1. Descriptive Statistics

Variable	N	Mean	St Dev
In Opposite-Sex Couple			
Ln Yearly Wage	2884212	10.992	0.726
Edu: Less than HS	3068458	0.064	0.245
Edu: HS Grad	3068458	0.325	0.468
Edu: Some College	3068458	0.226	0.418
Edu: College	3068458	0.228	0.42
Edu: Grad Degree	3068458	0.157	0.363
Experience	3068458	27.198	9.973
Black	3068458	0.059	0.236
Hispanic	3068458	0.112	0.315
Asian	3068458	0.054	0.225
Other Race	3068458	0.051	0.22
Urban	2678758	0.839	0.368
Number Children	3068458	1.261	1.23
In Same-Sex Couple			
Ln Yearly Wage	31729	11.047	0.762
Edu: Less than HS	33259	0.024	0.153
Edu: HS Grad	33259	0.193	0.395
Edu: Some College	33259	0.221	0.415
Edu: College	33259	0.308	0.462
Edu: Grad Degree	33259	0.254	0.435
Experience	33259	25.169	9.239
Black	33259	0.043	0.202
Hispanic	33259	0.112	0.316
Asian	33259	0.041	0.198
Other Race	33259	0.049	0.216
Urban	31246	0.942	0.233
Number Children	33259	0.238	0.713

Source: American Community Survey 2008-2015

Sample: Men in marriages or cohabiting relationships, working full time, aged 30-65

Table 2.2. Overall Wage Gap

	(1)	(2)	(3)
Dependent Variable	ln wages	ln wages	ln wages
Part of Same-sex couple	-0.098***	-0.094***	-0.075***
	0.004	0.004	0.004
Number Children			0.021***
			0
Urban			0.126***
			0.001
			0
Occupation Controls	No	Yes	Yes
Demographic Characteristics	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	2554195	2554195	2554195
R Sq.	0.2901	0.3596	0.3632

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

Source: American Community Survey 2008-2016.

Sample: Men aged 30-65 in marriages or cohabiting relationships and working full time.

Demographic Characteristics include education dummy variables, race indicators, experience, and experience squared.

Survey weights used.

Table 2.3. Overall Wage Gap, Years Pooled

Dependent Variable	ln wages
Part of Same-Sex Couple	-0.087
	0.011
Part of Same-Sex Couple*2009	0.01
	0.016
Part of Same-Sex Couple*2010	0.005
	0.016
Part of Same-Sex Couple*2011	-0.006
	0.016
Part of Same-Sex Couple*2012	-0.002
	0.016
Part of Same-Sex Couple*2013	0.024
	0.015
Part of Same-Sex Couple*2014	0.043***
	0.015
Part of Same-Sex Couple*2015	0.007
	0.015
Part of Same-Sex Couple*2016	0.017
	0.014
Number Children	0.021***
	0
Urban	0.126***
	0.001
Occupation Controls	Yes
Demographic Characteristics	Yes
State FE	Yes
Year FE	Yes
Observations	2554195
R Sq.	0.3632

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

Source: American Community Survey 2008-2016.

Sample: Men aged 30-65 in marriages or cohabiting relationships and working full time.

Demographic Characteristics include education dummy variables, race indicators, experience, and experience squared.

Survey weights used.

Table 2.4. Overall Wage Gaps

	Coef on SS	SE on SS	N
Comparison: Married	-0.089***	0.004	2380179
Comparison: Cohabiting	0.050***	0.004	203853

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

Source: American Community Survey 2008-2015

Sample: Men aged 30-65 in marriages or cohabiting relationships and working full time.

Control for: Education, Experience, Experience Squared, Race Indicators, Occupational Categories, Urban Status, Number Children

Survey weights used.

Table 2.5. Overall Wage Gap 2013-2015 Comparable Controls

	(1)	(2)
Dependent Variable	ln wages	ln wages
Part of Same-sex couple	-0.066***	-0.038***
	0.006	0.006
Controls Used	Regular Controls	NHIS controls
Observations	874538	874538
R Sq.	0.3766	0.3835

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

Source: American Community Survey 2013-2015.

Sample: Men in marriages or cohabiting relationships and working full time, aged 25-64.

Both controls contain: education dummy variables, race indicators, experience, experience squared, and occupation dummies.

Regular controls also contain: state fixed effects, number of children, and urban status.

NHIS controls also contain: industry indicators, census region dummies, an indicator for if there is at least one child under 5, and an indicator for if there is at least one child aged 5-17.

Table 2.6. State-Year Prejudice Index Descriptive Statistics

	Mean	**Std Dev**	**Minimum**	**Maximum**
2006-2007	0.292	0.988	-1.286	2.622
2008-2009	0.252	0.938	-1.249	2.504
2010-2011	-0.029	0.917	-2.444	1.863
2012-2013	-0.206	0.922	-2.209	2.363
2014-2015	-0.266	1.144	-2.78	3.048
pooled	0	1	-2.78	3.048

Source: General Social Survey 2004-2014

Index is an average of the current year plus the three previous years, so each represents two waves of the biannual survey.

Table 2.7. Homophobia and the Gay-Straight Wage Gap: Current Prejudice

Dependent Var	ln wages
Avg Prej Used	Current Prej
Same-Sex	-0.069***
	(0.018)
Avg Prej	-0.006
	(0.007)
SS*Avg Prej	-0.043***
	(0.013)
Individual Controls	Yes
Occupation Controls	Yes
State FE	Yes
Year FE	Yes
SS*Year FE	Yes
Observations	3127402
R Sq.	0.3869

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

Source: American Community Survey 2006-2015

Sample: Men in marriages or cohabiting relationships working full time

Clustered by state.

Survey weights and heteroskedasticity weights.

Individual controls: Education, Experience, Experience Squared, Race Indicators, Urban Status, Number Children

Table 2.8. Separate Opposite-Sex Couples into Married and Cohabiting

Dependent Var	ln wages	ln wages
Avg Prej Used	Current Prej	Current Prej
Opposite Sex Sample	Cohabiting	Married
Same-Sex	0.032**	-0.081***
	(0.015)	(0.018)
Avg Prej	0.011	-0.007
	(0.005)	(0.007)
SS*Avg Prej	-0.039***	-0.042***
	(0.012)	(0.013)
Individual Controls	Yes	Yes
Occupation Controls	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes
SS*Year FE	Yes	Yes
Observations	296914	2867021
R Sq.	0.3819	0.3748

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

Source: American Community Survey 2006-2015

Sample: Men in marriages or cohabiting relationships working full time

Clustered by state.

Survey weights and heteroskedasticity weights.

Individual controls: Education, Experience, Experience Squared, Race Indicators, Urban Status, Number Children

Table 2.9. Majority Female Occupations

Dependent Var	ln wages
Avg Prej Used	Current Prejudice
Maj. Fem.	-0.169***
	(0.005)
Avg Prej	-0.005***
	(0.001)
Maj. Fem*Avg Prej	0.0002
	(0.001)
Individual Controls	Yes
Occupation Controls	Yes
State FE	Yes
Year FE	Yes
Observations	3127402
R Sq.	0.3864

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

Source: American Community Survey 2006-2015

Sample: Men in marriages or cohabiting relationships working full time

Clustered by state.

Survey weights and heteroskedasticity weights.

Individual controls: Education, Experience, Experience Squared, Race Indicators, Urban Status, Number Children

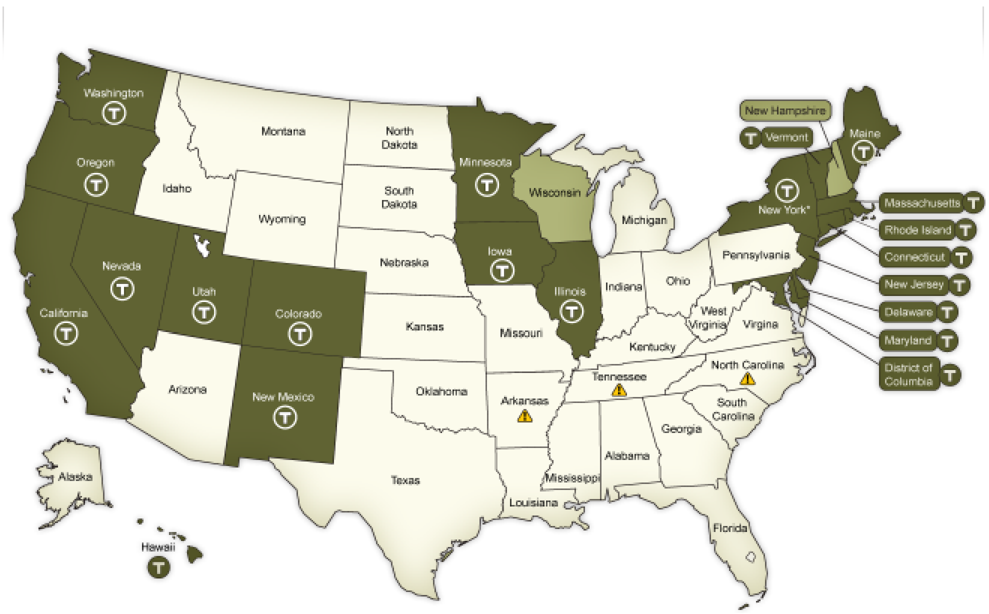


Figure 2.1. Employment Non-Discrimination Acts (ENDAS)
source: http://www.lgbtmap.org/equality-maps/non_discrimination_laws
This map is accurate for the time period of 2015-2018, since Utah passed an ENDA in 2015.

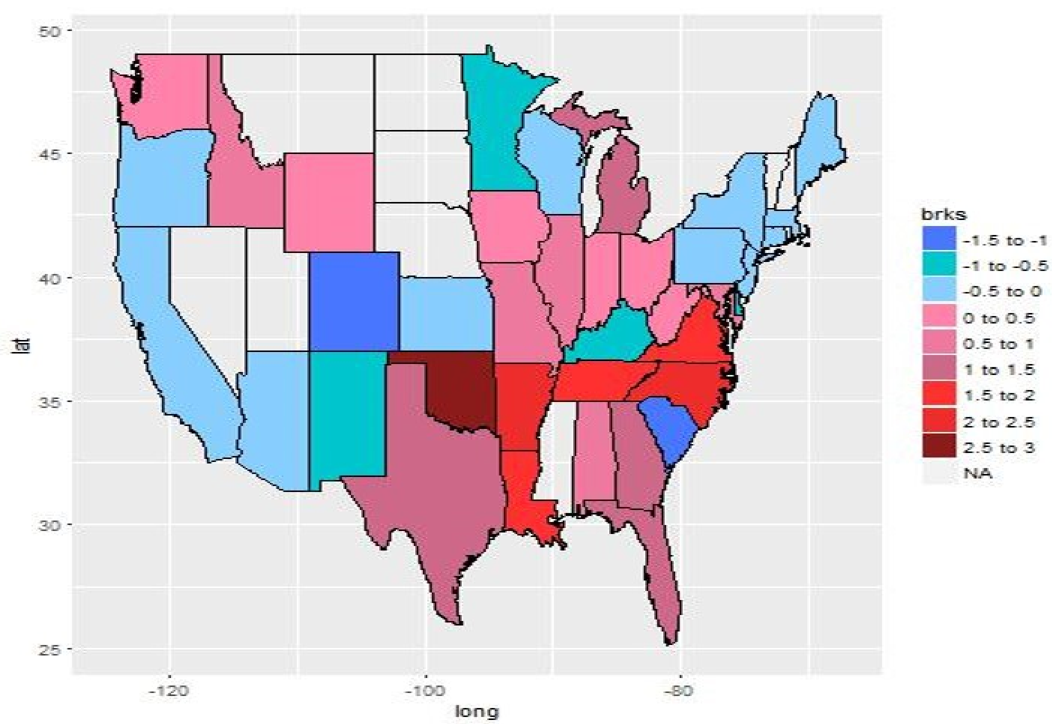


Figure 2.2. Prejudice Measure 2006

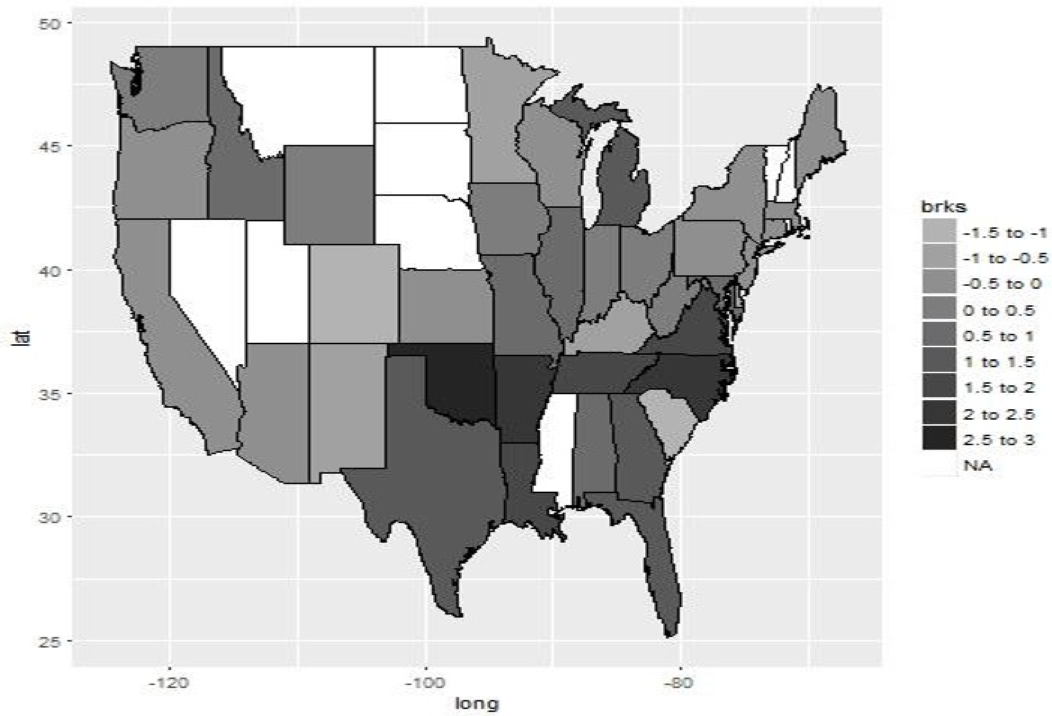


Figure 2.3. Prejudice Measure 2006 Grayscale

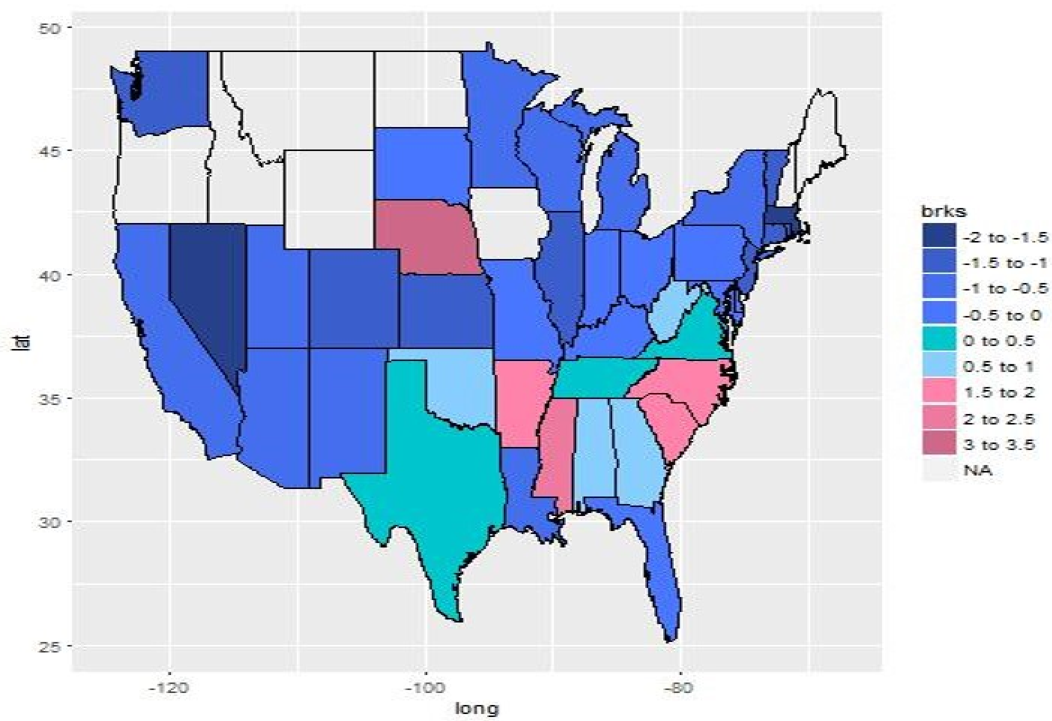


Figure 2.4. Prejudice Measure 2015

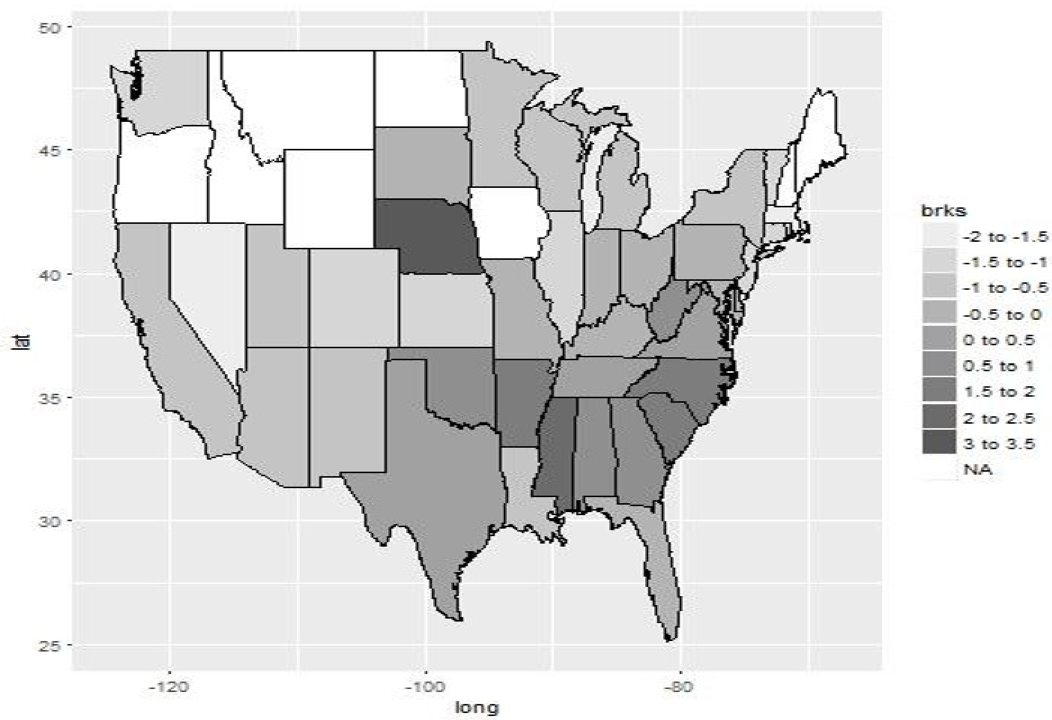


Figure 2.5. Prejudice Measure 2015 Grayscale

Chapter 3

The Distributional Impacts of Taxes on Health Products: Evidence from Diaper Sales Tax Exemptions

3.1 Introduction

Many economists argue that commodity taxes should be uniform across products, as in the classic results of Atkinson and Stiglitz (1976). There are two main ways that policies often alter these recommendations: excise or "sin" taxes on goods such as cigarettes or gas, and sales tax exemptions on goods such as groceries or health products. In this paper, we analyze sales taxes on diapers, a product that has direct health implications but is not always considered a health product in tax codes.

Policies determining sales taxes are subject to a trade-off between efficiency and equity. Since lower income households spend a greater share of their budget on consumption goods, sales taxes are regressive, which decreases equity. However, it is more efficient to tax goods for which demand is relatively inelastic, as this minimizes distortions to behavior. This has led some commentators to argue that health products should be subject to tax without exemptions (Kaeding 2017).

There is evidence from the cigarette tax literature that while all consumers respond to the more salient posted excise tax, only poorer consumers respond to the sales tax applied at the register (Goldin and Homonoff 2013). This opens the possibility that poorer families could be disproportionately responsive to sales taxes on diapers as well. Adda and Cornaglia (2006) find that smokers respond to cigarette taxes by purchasing fewer in number but using them more intensely, therefore inhaling higher concentrations of nicotine and carcinogens. A related concern for diapers is the risk that parents may purchase too few diapers for their children, requiring a longer duration for each diaper use and leading to potential health consequences.

There is reason to believe that the elasticity of demand for diapers is particularly variable in income. For high-income households, diaper purchases may already be at their optimum level regardless of price, as there are only so many diaper changes necessary per day. However, many American families struggle to afford enough diapers for their children. A survey of low-income mothers in New Haven found that 27.5% reported diaper need (Smith et al. 2013), and an Obama

White House blog post concurred that one in three American families face this difficulty (Muñoz 2016). If low-income households are liquidity constrained, their demand for diapers could be especially elastic (as diapers represent a greater share of their budget), which would imply a higher excess burden of sales taxes on diapers for this group. Thus, setting sales tax policy based on inelastic responses by high-income households could mean low-income households may be under-spending on diapers relative to the socially optimal amount.

Both survey and medical evidence suggest that diaper need is associated with negative health consequences. When poor families are not able to purchase enough diapers, they are forced to decrease the frequency of changing their children's diapers. Infrequent diaper changes lead to increased risks of urinary tract infections, diaper dermatitis, and secondary diaper dermatitis infections such as staph (Sugimura et al. 2009; Adalat, Wall, and Goodyear 2007; Fernandes, Machado, and Oliveira 2009). These increased infections lead to more doctors' visits and emergency room visits. Smith et al. (2013)'s survey of low socioeconomic status women showed a correlation between diaper need and increased stress and depression for mothers. The proposed mechanism for this relationship is that the financial inability to change a child's diaper when necessary increases their discomfort and crying, which can reinforce feelings of inadequacy and helplessness for their mothers.

To assess these issues, we study the response of diaper purchasing behavior to changes in taxes on diapers between 2006 and 2013. In addition to state-level changes in sales tax rates, there were four larger changes to diaper sales tax exemptions in this period. Connecticut exempted diapers from sales taxes under a clothing exemption until it was repealed in 2011. Meanwhile, New York eliminated their state sales taxes on diapers in 2006 as part of an overall sales tax exemption for clothing, and temporarily reinstated these taxes between 2010 and 2011 (creating three changes in policy). New York counties were allowed to individually decide whether to apply this exemption to local taxes, which created county-level variation in the changes in tax rates over this period. We use this variation to examine how diaper purchasing patterns respond after sales tax changes, and whether these responses are higher in low-income households and communities.

Our data sources are the Nielsen consumer and retail panel datasets, which are uniquely positioned to study the effects of sales taxes given their detailed product-level information (as previously used by Harding, Leibtag, and Lovenheim (2012) and Kroft et al. (2019)). We first use the Nielsen Homescan Consumer Panel data to investigate diaper purchasing patterns at the household level. We demonstrate that low-income households buy fewer diapers and spend less on diapers than high-income households.

We then analyze the effects of taxes on diapers by examining Nielsen Retail Scanner data, which provides information on the universe of point-of-sale transactions at a large panel of retailers. Overall, we find that stores in lower (below median) income counties show a high (1.6) elasticity of diaper purchases with respect to sales tax rates, with minimal responses in high income areas. Focusing on the four main changes to diaper tax exemptions in New York and Connecticut, we find the tax changes are almost completely passed through to consumers by retailers, leading to a 5.4% increase in the quantity of diapers sold at stores in low-income areas when diapers are tax-exempt. There is only a small (-1%) change in sales for high income areas, implying that low-income consumers are indeed more responsive to sales taxes on diapers.

Finally, we observe changes in spending on health-related products that are consistent with taxes on diapers inducing negative health outcomes; sales of children's pain medication fall by 6.2% in low-income exempt areas. These results imply that recent efforts to expand sales tax exemptions for diapers could have positive spillover effects.

The structure of this paper is as follows: Section 2 provides more detailed policy context about the tax changes we analyze and describe current tax changes; Section 3 describes our methods, including the data and models; and Section 4 summarizes the results of the descriptive Consumer Panel analysis, price changes, and main findings from the Retail Panel diaper sales analyses. Section 5 describes future directions for the project and Section 6 concludes.

3.2 Policy Context

Federal transfer programs such as the Supplemental Nutrition Assistance Program (SNAP) and Women, Infants, and Children (WIC) do not cover diapers with their benefits. Thus, low-income households potentially face liquidity constraints when buying diapers that are not addressed by current programs.

This paper focuses on changes in diaper taxes between 2006 and 2013. Six states exempted diapers from sales taxes prior to 2006: Massachusetts, Minnesota, New Jersey, Pennsylvania, Rhode Island, and Vermont (Weir et al. 2014). Also, several states (Alaska, Delaware, Montana, New Hampshire, and Oregon) do not have sales taxes. These states did not change their tax treatment of diapers during the study period. Our first source of variation involves the remaining states, which experienced small changes in taxes on diapers due to variations in their regular sales tax rates.

Additionally, there were several larger changes to sales taxes on diapers in New York. First, diapers were included in an overall state sales tax exemption for clothing under $110 starting April 1, 2006. New York took the unusual step of allowing counties to decide whether local sales taxes would also exempt clothing; some did and some did not. The state sales tax rate in New York at the time was 4% and counties levied local sales taxes ranging from 0 to 5.5%. Additionally, New York repealed this exemption temporarily between October 1, 2010 and March 31, 2011 (with the phased ending of the exemption suspension announced at the time of implementation). The variation in county-level taxes this created is graphed in Figure 3.1, and shown as a map in Figure 3.2; there was considerable geographic heterogeneity in diaper taxes.

As of 2006, Connecticut exempted clothing purchases under $50 from sales tax, and diapers are categorized as clothing in their tax code. The clothing exemption was repealed effective July 1, 2011. This meant that diapers went from not being taxed to being taxed at the state rate of 6.35%. Connecticut would later pass a diaper-specific sales tax exemption in 2016 that went into effect July 1, 2018, but this occurred after the time period covered in this paper.

The New York and Connecticut tax changes applied to all clothing purchases under certain amounts, so the effects of the tax changes cannot be attributed to diapers alone. Annual spending by families in the lowest quintile of incomes on apparel averaged $845 in 2006 and $774 in 2010, meaning the income change from this channel should be relatively modest at $50-$60.[1] However, to the degree that the tax reduction generates income effects, our results will overstate the effects of diaper taxes alone on behavior. We address this issue through model specifications comparing stores in high and low income regions, which should help control for economic trends such as changing incomes.

Connecticut's diaper-specific exemption is part of a recent trend in states considering specific policies to exempt diapers from sales taxes, often in conjunction with exempting feminine hygiene products such as tampons. In 2017 alone, 18 states introduced bills that would eliminate or decrease sales taxes on diapers (Loughead 2018). A potential law that would add diapers to sales tax exemptions failed in California in 2016 after it was vetoed by the governor, but a later effort passed an exemption for 2020-2021. In 2020, Virginia lowered taxes on diapers and other hygiene products to the lower (2.5%) rate at which food is taxed. Washington D.C. eliminated sales taxes on diapers in October 2019, and Louisiana will no longer tax diapers starting in 2021. Findings on earlier changes to diaper taxes could help inform debates about these current laws.

3.3 Methods

3.3.1 Data

Our first data source is the Nielsen Consumer Panel (Homescan) data, a panel that tracks the purchases of 40,000-60,000 households. Participants scan in all purchases, amounts, and trips for both food and non-food spending intended for in-home use. Demographic information about the panelists includes years of birth for children, race, geography, and lagged income categories. We use this purchasing data from 2004-2013 to illustrate diaper purchasing patterns

[1] Figures from "Quintiles of income before taxes: Average annual expenditures and characteristics, Consumer Expenditure Survey" (https://www.bls.gov/cex/csxstnd.htm.)

by income at the household level, restricting the sample to household-years with a child born within the previous 3 years and at least one diaper purchase.[2]

As the sample size of the Homescan data is limited for small geographic areas, we also use the Nielsen Retail Scanner Data to investigate diaper purchases. This data includes the universe of scanned point-of-sale retail transactions for a large sample of businesses, including about 50% of U.S. grocery and drug stores. Information is recorded on sales and average prices at the store-UPC-week level, which we aggregate into information about product categories at the store-month level (from 2006-2013).

While the size of the scanner data provides considerable precision, one limitation is that the data contains information only on stores, without information on individual households purchasing at those stores. To focus on low income households, we use the location of stores in low income areas as a proxy for customer characteristics. Low income areas are defined as those in the bottom 50% of median household income, as measured by American Community Survey data (from 2005-2010) at the county level. This corresponds to a median household income of $42,224. Our sample includes all stores with at least one sale of diapers in the study period.

Figure 3.3 displays the distribution of these low income areas across New York State (the main area where treatment varies in the sample); as the figure shows, the low income areas are geographically diverse, including both the Bronx and more rural counties upstate.

To gather information on diaper sales tax exemptions, we consulted a report by the National Diaper Bank Network (Weir et al. 2014), supplemented by tax rate notices in the state of New York. We also gathered information on sales tax rates by county over time from the Avalara AvaTax API (a commercial software product used by retailers to compute sales taxes due).

[2]Since children with the birth year in that panel year are not reported until the following year, the sample is also restricted to households that are in the data the following year.

3.3.2 Models

When analyzing the consumer response to a sales tax change, it is important to consider how much of these tax changes are passed through to consumers. Diapers are a very concentrated market with two main producers (Procter & Gamble and Kimberly-Clark), but they are also often sold as a loss leader at stores (Neff 2006). Dellavigna and Gentzkow (2019) demonstrate that there are largely uniform prices across grocery store chains, so we might not expect large local price responses to local tax changes. In order to test whether prices changed at stores following the changes in tax exemption status, we run the following model in the retail panel data for each change to statewide tax exemptions of diapers:

$$\ln(p_{iwu}) = \alpha_i + \theta_w + \gamma_u + \beta \text{ TaxChange}_{sw} + \varepsilon_{iwu} \tag{3.1}$$

where *TaxChange* indicates the first full week of the tax change through the rest of the calendar year, p is price in store i in week w for UPC code u (UPC codes identify a specific brand, type, and count of a package of diapers). By having store, week, and UPC fixed effects, the coefficient of interest on *TaxChange* shows how prices at the UPC level changed after the change to the state diaper exemption policy.

To investigate the effects of sales taxes on diaper purchases in the retail panel data, we use a treatment intensity model exploiting spatial and temporal variation in the tax rates applied to diapers. This method has the advantage of using the most variation in treatment status (as sales tax changes other than a full repeal contribute to identification). We use the following model:

$$\ln(y_{it}) = \alpha_i + \theta_t + \beta \ln(1 + \tau_{ct}) + \varepsilon_{it} \tag{3.2}$$

$$\iff \text{low}_{ct} = 1$$

where i indexes stores located in state s and county c, t indexes (monthly) time periods, y_{it} is an outcome, and τ_{ct} is the *ad valorem* tax rate on diapers in county c at time t. Outcomes include the count of diapers sold, units of diapers sold (packages or boxes), sales revenue from diapers, price per diaper, and price per unit. The coefficient of interest β thus captures the elasticity of the measure of interest with respect to the change in the diaper sales tax rate. This specification limits the sample to stores located in low-income counties (indicated by low_{ct}).

We also examine the relative response in low and high income areas with a triple-difference model,

$$\ln(y_{it}) = \alpha_i + \theta_t \cdot \text{low}_{at} + \gamma \ln(1 + \text{Tax}_{ct}) + \beta \ln(1 + \text{Tax}_{ct}) \cdot \text{low}_{at} + \varepsilon_{it} \tag{3.3}$$

where stores in all areas are included, and the tax variable and time fixed effects θ_t are interacted with the indicator low_{ct} for a store being located in a low-income county. The coefficient of interest β thus captures the change in elasticity for low-income stores relative to higher income stores in counties with a tax change.

To examine effects of a discrete change in exemption status (rather than marginal changes), we also use a difference-in-differences model,

$$\begin{gathered} \ln(y_{it}) = \alpha_i + \theta_t + \beta \ \text{Notax}_{st} + \varepsilon_{it} \\ \iff \text{low}_{ct} = 1 \end{gathered} \tag{3.4}$$

where Notax_{st} is an indicator for whether state s has no sales tax on diapers at time t. As discussed above, only New York and Connecticut vary the exemption status of diapers during the study period, so identification of the coefficient of interest β comes from within-store changes in outcomes before and after the policy changes in those states. The sample is still limited to stores located in low income areas.[3]

We use a triple-difference model analogous to Equation 3.3 to investigate distributional

[3]Connecticut has no counties in the bottom half of median incomes, so all variation in this regression comes from New York.

effects:

$$\ln(y_{it}) = \alpha_i + \theta_t + \theta_t \cdot \text{low}_{ct} + \gamma\, \text{Notax}_{st} + \beta\, \text{Notax}_{st} \cdot \text{low}_{ct} + \varepsilon_{it} \tag{3.5}$$

and to see how treatment effects vary over time, we use an event study model building on Equation 3.4,

$$\ln(y_{it}) = \alpha_i + \theta_t + \sum_{d=-q, d\neq -1}^{p} \beta_d \cdot I(t = e_s + d) + \varepsilon_{it} \tag{3.6}$$
$$\iff -q \leq t \leq p \text{ and } \text{low}_{ct} = 1$$

where the model includes indicators $I(\cdot)$ of time to the treatment event e_s, ranging from q periods before the event to p periods after, giving time varying coefficients β_d. There are two possible treatments in the policy context during our sample period: going from no sales tax exemption to having an exemption (New York in 2006 and 2011), or losing an existing exemption (New York in 2010 and Connecticut in 2011). We run separate models for the two cases.

3.4 Results

3.4.1 Differences By Household Income

First, we examine the Nielsen Consumer Panel (Homescan) data for diaper purchasing patterns at the household level. Although there is insufficient power to analyze changes across the two tax changes of interest, the advantage of this data is the ability to look at diaper purchases by household income.

Descriptive statistics on demographics by income level are in Table 3.1. The average number of children 3 and under is relatively similar at 1.25 for households with income over \$35,000 and 1.27 for households with income under \$35,000.[4] This number is over 1 in both

[4]Child age is inferred from year of birth; this variable includes children with the concurrent year of birth and previous 3 calendar years.

cases, so to facilitate interpretation, further results are divided by the number of children 3 and under in the household. The lower income group has more members of racial minorities and lower levels of education for female heads of household.

The Consumer Panel data shows that low-income families buy fewer diapers per child than high-income families. Table 3.2 reports a t-test on the mean of yearly diaper quantity purchased. Higher income families purchase nearly 1000 diapers per child each year while lower income families purchase about 880; this difference is statistically significant. A further breakdown by more granular income levels is shown in Figure 3.4.[5] Diaper quantity monotonically increases with household income category. Yearly diaper spending is also lower for low-income households; Table 3.3 shows that households with yearly income over $35,000 spend $32.05 more per year than households with yearly income under $35,000. We would expect our observed levels of yearly spending to be an underestimate for a year of a child's life because our sample restriction of a child born within the past 3 years depends on calendar year. A child could be born part way through the year, so diapers would not be needed until then, or a child could stop using diapers part way through the year, so diapers would not be needed after. Additionally, spending incorporates both price and quantity, so differences in spending could reflect a combination of lower quantities and cheaper options.

There is some seasonality in diaper purchases. Figure 3.5 shows monthly purchases by income level.[6] Although there are some differences by month, there are not drastically different monthly patterns by income.

3.4.2 Pricing and Passthrough

Looking at the effects of sales taxes on prices at the product level, results from the model of price passthrough (from Equation 3.1) appear in Table 3.4. Two of the tax changes, NY in

[5] These levels map to the stratified sampling of the Nielsen Consumer Panel.

[6] The Nielsen panel years do not map perfectly to calendar years because of a cutoff in reporting the last Saturday of December, so only household-months with full coverage of a month are used. Household-months are counted from the first diaper purchase to the last diaper purchase, with any intervening months without diaper purchases assumed to be 0.

2006 and NY in 2010, show a small but statistically significant change in price. The signs on the coefficients are consistent with less than complete passthrough to consumers; following a sales tax decrease, prices slightly rose, and following a sales tax increase, prices slightly decreased. However, these changes are very small. The tax changes were 4% at minimum (because of the state tax changes, with additional 0-5.5% changes in county tax rates) and the responses in prices were both less than 0.6%. Neither tax change in 2011 was associated with a statistically significant change in prices. Because there was slightly less than perfect passthrough, our estimates for responses to the stated tax rates and changes could be considered underestimates of responses to overall prices.

3.4.3 Primary Regression Results

Turning now to the primary regression specification in the retail data, we measure elasticities of diaper sales outcomes with a treatment intensity model (as given in Equation 3.2). This method has the advantage of using all variation in tax rates that do not represent a complete repeal, including county variation in New York and changes to overall sales tax rates in other states. Table 3.5 shows results consistent with our hypothesis that low income households will have relatively elastic demand; the elasticity of diapers sales with respect to the tax rate is 1.6, indicating these sales are relatively elastic. This stands in sharp contrast to the effects on stores in high income areas in the triple difference (Table 3.6), which show little or no response to the tax change; there is no effect on diapers sold or total sales, and only a small change in the number of packages sold.

To examine discrete effects of tax changes, we consider the difference-in-differences model of Equation 3.4. This method reduces the amount of variation used (as it considers only the changes in state-level exemptions in New York and Connecticut), but focuses on areas where the tax change may be more salient (as diapers are completely exempt, rather than changes occurring through overall changes to sales tax rates). Table 3.7 shows a 5.4% increase in diapers sold in stores in low-income areas after diaper taxes are repealed.

We obtain similar results when measuring the number of package units (unique UPC codes) sold or total dollar sales. Prices per diaper sold fall very slightly (0.7%), consistent with the results in Table 3.4 on nearly complete passthrough. Since we have ruled out large retailer responses on UPC-level prices, price per diaper could vary with changes in bulk purchasing (with higher count packages having lower prices per diaper), or quality (with changes in purchasing patterns toward brands or product lines having higher prices per diaper). The price of the average package sold rises by 0.7%, which is consistent with an increase in bulk purchasing (average package size rises by 1.2%).

3.4.4 Effects on Health-Related Products

Table 3.8 examines how sales of diapers may affect health outcomes by examining sales of health products related to diaper need. We find an 6.2% drop in purchases of children's liquid pain medications in stores in low-income counties with a diaper sales tax exemption, suggesting that children are less likely to experience painful medical conditions as diaper sales increase. This result is particularly striking if children's pain medication is a normal good; the sales tax exemption was part of an overall clothing exemption and that income effect, although small, would increase sales of normal goods. We also see a 3.2% increase in sales of baby powder, a complimentary product to diapers. Other products such as ointments do not show a price response.

3.4.5 Triple Difference Results

One potential concern is that because the difference-in-difference models compare stores in low-income areas across states, there may be underlying changes in New York or Connecticut over time that contaminate the results. Table 3.9 presents results of a triple-difference specification (Equation 3.5), and finds that in the states that changed their diaper tax exemptions, there is a small (1.1%) decrease in sales of diapers, along with a small (0.4%) fall in price per diaper. It is only for stores in low income areas that sales increase (a relative 5.8% difference).

This supports the hypothesis that low income consumers are more responsive to the diaper tax exemptions.

We also apply this methodology to child-related health products. Table 3.10 shows that while low and high income areas generally had precise and small (1-3%) changes in spending on health products after diaper taxes were removed, low income areas see a relative decrease of 8.9% in spending on liquid children's pain medication. This implies that the increase in diaper purchases induced by the tax reduction could have positive health effects in low income areas.

3.4.6 Event Study

To examine how the sales tax removal affects outcomes over time, Figure 3.6 shows an event study (as in Equation 3.6) of changes in diaper sales before and after statewide exemptions were applied in New York (in April 2006 and again in April 2011). Because the Retail Scanner data only begins in January 2006, we are only able to examine 3 months of pre-period observations. Diaper sales are slightly higher in New York in the months prior to the exemption, which does raise some concerns about parallel trends; however, the increase in sales after the tax exemption is higher in magnitude and appears to be sustained.

Figure 3.7 displays similar information for the times exemptions were ended (New York in 2010 and Connecticut in 2011).[7] In these cases, we see roughly parallel trends before the change (with treated areas having slightly lower diaper sales), and lower levels after the change (although results are noisy). Because the October 2010 change in New York was reversed 6 months later, the time period to observe effects is limited.

3.5 Directions for Future Work

- Analyze further breakdowns of responses by income level.
- Investigate potential sources for health outcomes data.

[7]Connecticut has no counties in the bottom half of median incomes, so all variation in the current specification comes from New York.

- Consider a synthetic control model.
- Include further robustness checks using alternate definitions of low-income (including high poverty rate) and controlling for local unemployment rates.

3.6 Conclusion

Since diapers are a product for which low-income areas have a higher elasticity of demand, are not covered under current federal aid programs, and have potential health consequences for children, the policies surrounding regressive sales taxes on diapers are important to study. We find that consumers in low income areas are quite responsive to diaper taxes, and removing taxes on diapers can have potential positive spillovers in reducing use of children's pain medication. Our findings have policy relevance for states that are currently considering adding diapers to their lists of tax-exempt products.

3.7 Acknowledgements

This chapter is coauthored with Kye Lippold. The dissertation author was a primary author of this chapter. It is currently being prepared for submission for publication of the material.

Thanks to Kate Antonovics, Julie Cullen, Alyssa Brown, and Rebecca Fraenkel for comments. Researchers own analyses calculated (or derived) based in part on data from The Nielsen Company (US), LLC and marketing databases provided through the Nielsen Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

References

Adalat, Shazia, David Wall, and Helen Goodyear. 2007. "Diaper Dermatitis-Frequency and Contributory Factors in Hospital Attending Children". *Pediatric Dermatology* 24 (5): 483–488. doi:10.1111/j.1525-1470.2007.00499.x.

Adda, Jérôme, and Francesca Cornaglia. 2006. "Taxes, Cigarette Consumption, and Smoking Intensity". *American Economic Review* 96 (4): 1013–1028. doi:10.1257/aer.96.4.1013.

Atkinson, A.B., and J.E. Stiglitz. 1976. "The Design of Tax Structure: Direct versus Indirect Taxation". *Journal of Public Economics* 6:55–75. doi:10.1016/0047-2727(76)90041-4.

Dellavigna, Stefano, and Matthew Gentzkow. 2019. "Uniform Pricing in U.S. Retail Chains". *Quarterly Journal of Economics* 134 (4): 2011–2084. doi:10.1093/qje/qjz019.

Fernandes, Juliana Dumet, Maria Cecília Rivitti Machado, and Zilda Najjar Prado de Oliveira. 2009. "Quadro clínico e tratamento da dermatite da área das fraldas: parte II [Clinical Presentation and Treatment of Diaper Dermatitis: Part II]". *Anais Brasileiros de Dermatologia* 84 (1): 47–54. doi:10.1590/S0365-05962009000100007.

Goldin, Jacob, and Tatiana Homonoff. 2013. "Smoke Gets in Your Eyes: Cigarette Tax Salience and Regressivity". *American Economic Journal: Economic Policy* 5 (1): 302–336. doi:10.1257/pol.5.1.302.

Harding, Matthew, Ephraim Leibtag, and Michael F Lovenheim. 2012. "The Heterogeneous Geographic and Socioeconomic Incidence of Cigarette Taxes: Evidence from Nielsen Homescan Data". *American Economic Journal: Economic Policy* 4 (4): 169–198. doi:10.1257/pol.4.4.169.

Kaeding, Nicole. 2017. *Tampon Taxes: Do Feminine Hygiene Products Deserve a Sales Tax Exemption?* Fiscal Fact 547. Washington, DC: Tax Foundation. https://files.taxfoundation.org/20170425105103/Tax-Foundation-FF547.pdf.

Kroft, Kory, Jean-William P Laliberté, René Leal-Vizcaíno, and Matthew J Notowidigdo. 2019. *Salience and Taxation with Imperfect Competition*. Working Paper. http://faculty.wcas.northwestern.edu/noto/research/KLLN%7B%5C_%7DTaxation%7B%5C_%7Doct2019.pdf.

Loughead, Katherine. 2018. *Sales Tax Complexity: Diaper Edition*. Tax Foundation. Visited on 05/29/2020. https://taxfoundation.org/sales-tax-complexity-diapers/.

Muñoz, Cecilia. 2016. *The Diaper Divide*. White House Domestic Policy Council. Visited on 05/21/2020. https://obamawhitehouse.archives.gov/blog/2016/03/10/diaper-divide.

Neff, Jack. 2006. “The Battle for the Bottom Line; P&G, K-C push innovations in $5B diaper category”. *Advertising Age*, August 28, 2006. LexisNexis.

Smith, Megan V., Anna Kruse, Alison Weir, and Joanne Goldblum. 2013. “Diaper Need and Its Impact on Child Health”. *Pediatrics* 132 (2): 253–259. doi:10.1542/peds.2013-0597.

Sugimura, Tetsu, Yoshifumi Tananari, Yukiko Ozaki, Yasuki Maeno, Seiji Tanaka, Shinichi Ito, Keiko Kawano, and Kumiko Masunaga. 2009. “Association between the Frequency of Disposable Diaper Changing and Urinary Tract Infection in Infants”. *Clinical Pediatrics* 48 (1): 18–20. doi:10.1177/0009922808320696.

Weir, Alison M, Matthew Smith, Irina Vaynerman, and Tara Rice. 2014. *Fifty State Survey on the Sales Tax Treatment of Diapers*. New Haven, CT: National Diaper Bank Network. http://nationaldiaperbanknetwork.org/wp-content/uploads/2015/01/Sales-Tax-Survey-Mar-2015.pdf.

Table 3.1. Descriptive Statistics By Income Level, Consumer Panel

	Mean Over 35k	**Mean Under 35k**
Num Children 3 & under	1.25	1.27
Race & Ethnicity		
White Non-Hispanic	0.76	0.72
Black	0.07	0.11
Asian	0.07	0.04
Other	0.06	0.09
Hispanic	0.09	0.11
Female Head Education		
Less Than HS	0.01	0.05
HS	0.10	0.25
Some Coll	0.22	0.32
College	0.48	0.34
Post College	0.20	0.06

Source: Nielsen Consumer Panel Dataset.

Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.2. Yearly Diaper Quantity Per Child Under 3, by Income Level

	Mean	**Std Err**	**Obs**
Over 35k	994.57	8.22	6071
Under 35k	879.89	21.13	1228
Difference	114.68	7.72	
Pr 2-sided T-test	0.000		

Source: Nielsen Consumer Panel Dataset.

Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.3. Yearly Diaper Spending Per Child Under 3, by Income Level

	Mean	**Std Err**	**Obs**
Over 35k	227.65	2.00	6071
Under 35k	195.60	4.67	1228
Difference	32.05	1.85	
Pr 2-sided T-test	0.000		

Source: Nielsen Consumer Panel Dataset.

Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Table 3.4. Prices After Tax Changes, UPC Level

	NY 2006 4-9.5% Tax Dec. Ln Unit Price	NY 2010 4-9.5% Tax Inc. Ln Unit Price	NY 2011 4-9.5% Tax Dec. Ln Unit Price	CT 2011 6.35% Tax Inc. Ln Unit Price
Tax Change	0.00587***	-0.00371***	0.0000361	-0.00188
	(0.000768)	(0.000948)	(0.00106)	(0.00270)
Constant	2.418***	2.436***	2.478***	2.476***
	(0.0000297)	(0.0000106)	(0.0000387)	(0.0000131)
N	59891011	64149230	64176220	61612013
Clusters	30947	33639	33428	31805

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store, week, and UPC fixed effects. Treated indicates area has no sales tax on diapers in effect. The sample for each regression contains the store-week-UPC sales for the whole year. The NY 2011 sample does not contain CT and the CT 2011 sample does not contain NY.

Table 3.5. Effects on Diaper Purchases, Treatment Intensity Model

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Tax Rate	-1.644***	-1.651***	-1.686***	-0.0406	-0.0300	0.0221
	(0.346)	(0.351)	(0.354)	(0.0555)	(0.0583)	(0.0634)
N	533919	533919	533919	533919	533919	533919
Clusters	6621	6621	6621	6621	6621	6621
R^2	0.929	0.920	0.932	0.811	0.875	0.792

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects. Tax rate is measured as $\ln(1 + \tau_{ct})$. Sample is limited to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

Table 3.6. Effects on Diaper Purchases, Treatment Intensity Model, Triple Difference

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Tax Rate	0.0178	-0.195*	-0.0204	-0.0505**	0.164***	0.219***
	(0.116)	(0.112)	(0.117)	(0.0217)	(0.0274)	(0.0265)
Tax x Low Inc	-1.642***	-1.437***	-1.643***	0.00680	-0.192***	-0.197***
	(0.363)	(0.366)	(0.371)	(0.0592)	(0.0642)	(0.0684)
N	3092075	3092075	3092075	3092075	3092075	3092075
Clusters	38657	38657	38657	38657	38657	38657
R^2	0.957	0.947	0.957	0.843	0.881	0.890

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects interacted with low income indicator. "Low Income" refers to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data). Tax rate is measured as a proportion.

Table 3.7. Effects on Diaper Purchases, Difference-in-Differences

	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Treated	0.0535***	0.0433***	0.0473***	-0.00764*	0.00732**	0.0122**
	(0.0146)	(0.0126)	(0.0137)	(0.00442)	(0.00367)	(0.00552)
N	533919	533919	533919	533919	533919	533919
Clusters	6621	6621	6621	6621	6621	6621
R^2	0.929	0.920	0.932	0.811	0.875	0.792

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

Table 3.8. Effects on Log Sales of Health Products, Difference-in-Differences

	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0316*	-0.0290	-0.0186	0.00124	-0.0623***
	(0.0178)	(0.0220)	(0.0187)	(0.0259)	(0.0177)
N	530435	522658	523103	357289	544537
Clusters	6591	6511	6491	6190	6824
R^2	0.765	0.715	0.849	0.810	0.874

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. Outcomes are log sales of each product. All models include store and (monthly) date fixed effects. Treated indicates area has no sales tax on diapers in effect. Sample is limited to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

Table 3.9. Effects on Diaper Purchases, Triple Difference

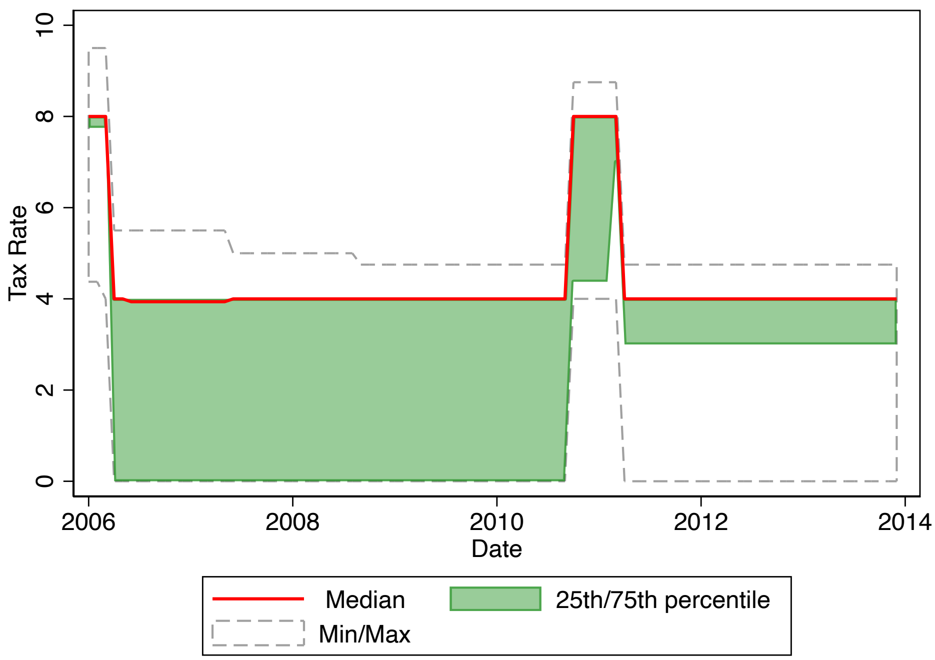
	(1) Ln Diapers Sold	(2) Ln Units	(3) Ln Sales	(4) Ln Diaper Price	(5) Ln Unit Price	(6) Ln Pkg Size
Treated	-0.0108**	-0.00694	-0.0135**	-0.00370***	-0.00528***	-0.00424***
	(0.00546)	(0.00526)	(0.00552)	(0.00118)	(0.00134)	(0.00132)
Treated x Low Inc	0.0581***	0.0437***	0.0547***	-0.00188	0.0132***	0.0166***
	(0.0157)	(0.0145)	(0.0153)	(0.00455)	(0.00407)	(0.00560)
N	3097203	3097203	3097203	3097203	3097203	3097203
Clusters	38671	38671	38671	38671	38671	38671
R^2	0.957	0.947	0.957	0.843	0.881	0.890

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. All models include store and (monthly) date fixed effects interacted with low income indicator. Treated indicates area has no sales tax on diapers in effect. “Low Income” refers to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).

Table 3.10. Effects on Log Sales of Health Products, Triple Difference

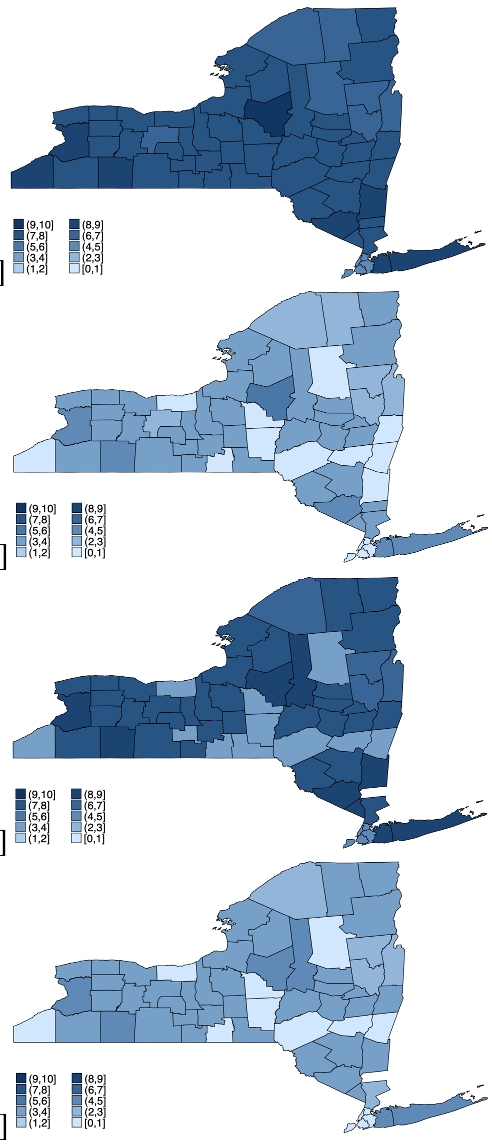
	(1) Baby Powder	(2) Baby Oil	(3) Baby Ointments	(4) UTI Pain Meds	(5) Child Pain Meds
Treated	0.0137**	-0.00883	-0.0228***	0.0255***	0.0317***
	(0.00646)	(0.00538)	(0.00537)	(0.00583)	(0.00703)
Treated x Low Inc	0.0160	-0.0271	0.000220	-0.0256	-0.0889***
	(0.0187)	(0.0232)	(0.0192)	(0.0257)	(0.0196)
N	3082971	3054452	3051801	2343532	3139240
Clusters	38174	37494	37425	35738	39151
R^2	0.850	0.811	0.902	0.793	0.913

Source: Nielsen Retail Scanner Data.
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors (clustered by store) in parentheses. Outcomes are log sales of each product. All models include store and (monthly) date fixed effects interacted with low income indicator. Treated indicates area has no sales tax on diapers in effect. “Low Income” refers to stores in counties in bottom 50% of median incomes (measured in 2005-2010 ACS data).



Source: New York Department of Taxation and Finance, Publication 718-C, various years.

Figure 3.1. Distribution of County Tax Rates on Diapers in New York State over Time



[Jan. 2006]

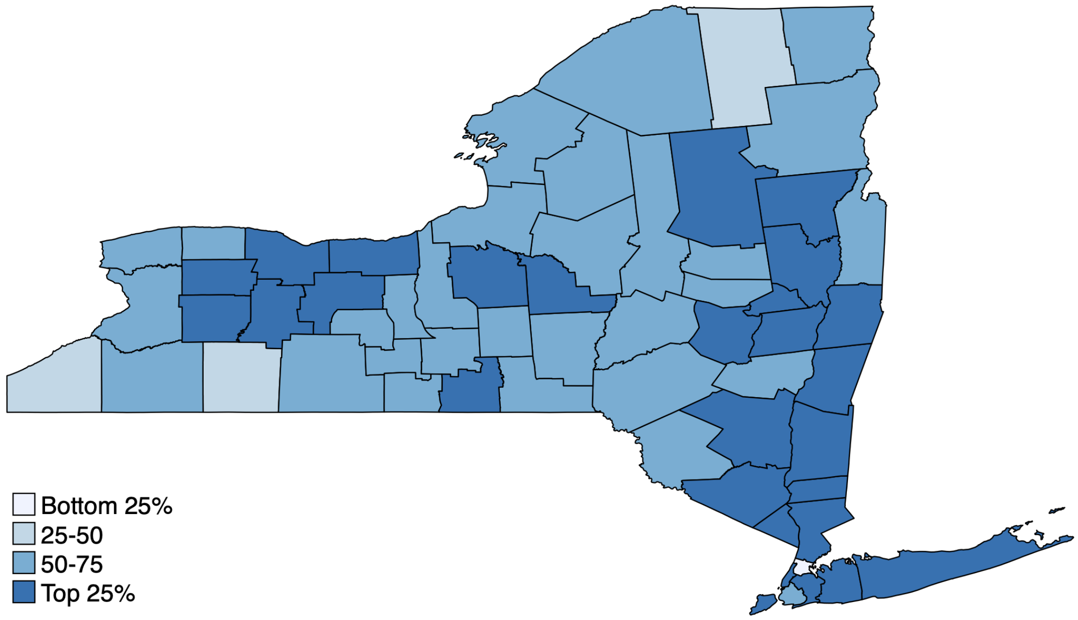
[Apr. 2006]

[Oct. 2010]

[Apr. 2011]

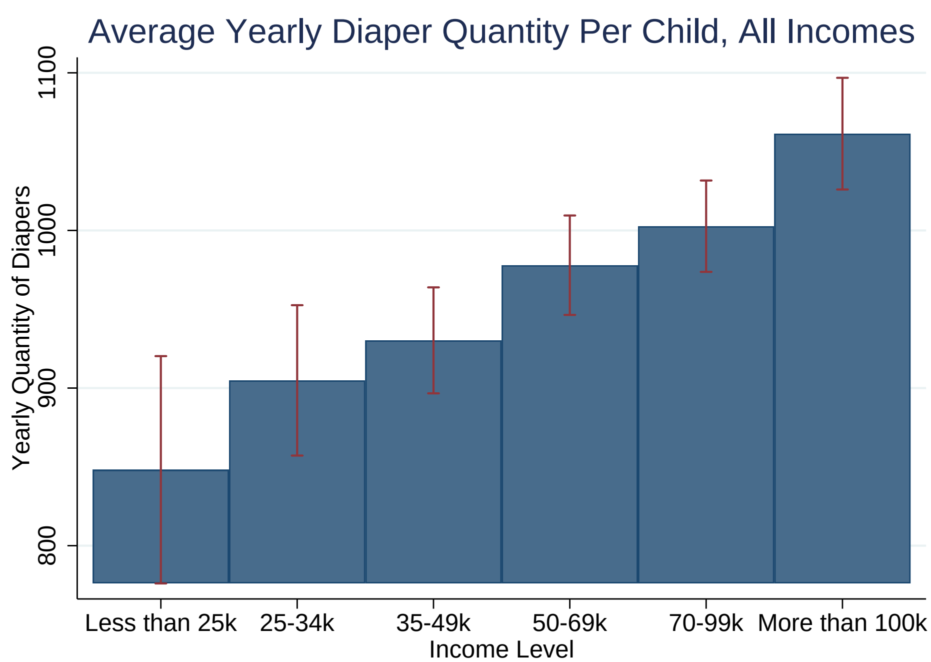
Source: New York Department of Taxation and Finance, Publication 718-C, various years.

Figure 3.2. Tax Rates by County in New York State over Time



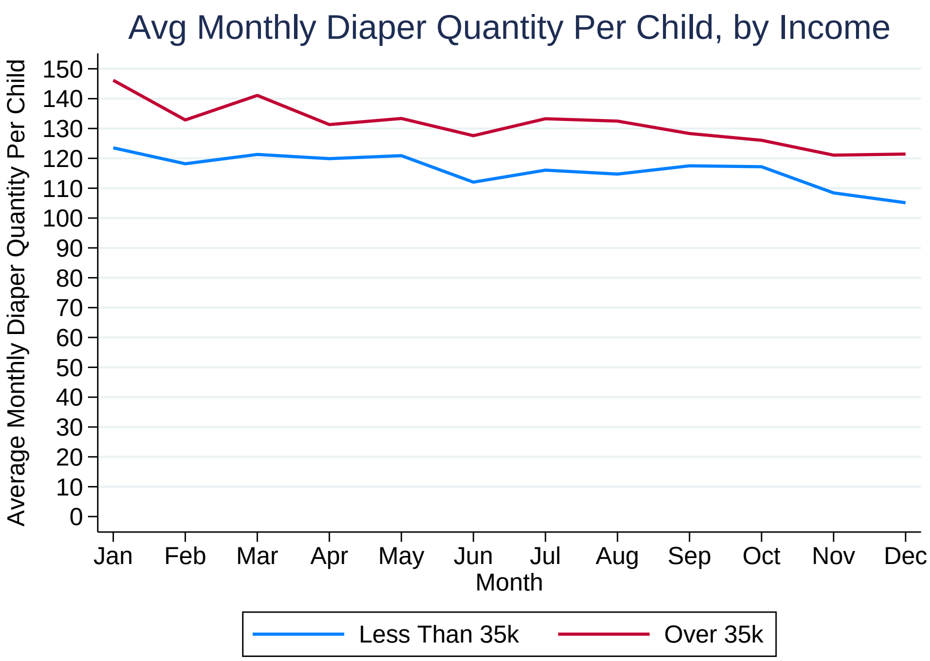
Source: American Community Survey, 2005-2010.

Figure 3.3. Counties in New York State by Quartile of Median Income



Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year, that made at least 1 diaper purchase that year.

Figure 3.4. Consumer Panel Yearly Quantity of Diapers Per Child, By Income Levels

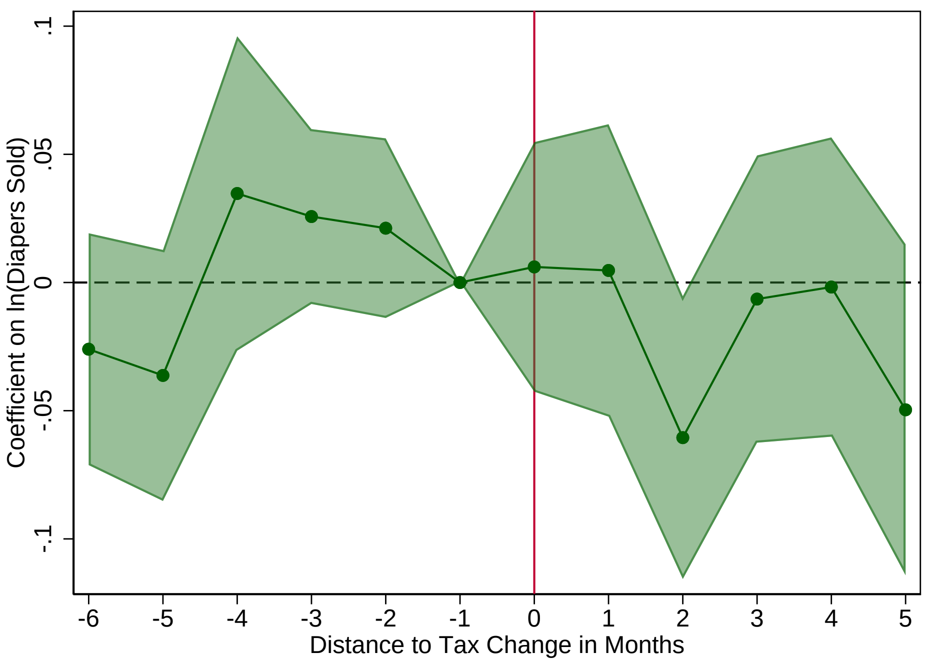


Source: Nielsen Consumer Panel Dataset.
Sample: Households 2004-2013 with at least 1 child born within past 3 years, in the data the following year and for whole month, that made at least 1 diaper purchase that year.

Figure 3.5. Consumer Panel Monthly Quantity of Diapers Per Child, By Income

Source: Nielsen Retail Scanner Data.
Notes: Graph depicts coefficients from a regression of diaper sales on treatment status (NY in April 2006 or April 2011). Shaded ares are 95% confidence intervals.

Figure 3.6. Event Study at Beginning of Exemption for Diapers



Source: Nielsen Retail Scanner Data.
Notes: Graph depicts coefficients from a regression of diaper sales on end of treatment status (NY in October 2010 or CT in July 2011). Shaded ares are 95% confidence intervals.

Figure 3.7. Event Study at End of Exemption for Diapers