ABSTRACT

Title of Dissertation: THREE ESSAYS IN APPLIED PUBLIC ECONOMICS: APPLICATIONS TO VULNERABLE POPULATIONS

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In this dissertation, I study the effects of state and federal programs and policies aimed to help vulnerable populations.

In the second chapter, I estimate the effect of immigration enforcement on prenatal safety net programs and birth outcomes. I compare participation in prenatal WIC and Medicaid between likely undocumented mothers and US born non-Latina mothers. I find an increase in immigration enforcement lowers participation in Medicaid but has a null effect on WIC participation. Because undocumented people are ineligible for Medicaid except in special circumstances, using Medicaid to pay for the delivery of a newborn may signal a person's immigration status. WIC eligibility does not have any restrictions regarding a person's citizenship or legal status. This may explain the chilling effect observed in Medicaid but not in WIC. I find that undocumented mothers reduce their prenatal care. There are also improvements in infant birth weight and a decline

in undocumented women's birth rate. This suggests positive selection into birth when immigration enforcement intensifies.

Chapter three examines the effect of Deferred Action for Childhood Arrivals (DACA) on living arrangements. DACA provides temporary relief from deportation for undocumented immigrants who arrived to the United States as children. DACA recipients receive a social security number, work permit, and may obtain a driver's license in their home state. Previous studies have found that DACA improves beneficiaries' economic well-being. Since housing decisions are closely linked to economic security, I compare DACA eligible and ineligible immigrants to estimate changes in living arrangements. I find DACA increases the incidence of living with a parent in a rented home by 1.9 percentage points and lowers the incidence of living with other family members by 2.4 percentage points. The economic benefits and mobility that DACA provides along with the lowered fear of deportation may change beneficiaries' living arrangements but does not increase the likelihood of moving into a home without a family member.

In chapter four, I re-examine the effect of Naloxone Access Laws on opioid mortality. In response to the opioid crisis, states adopted laws increasing the availability of Naloxone, an overdose reversal drug. The theoretical effect of these policies is ambiguous due to the potential for moral hazard. The current literature contains mixed results when using a difference-in-differences model to estimate the effect of Naloxone Access Laws on mortality. I revisit these studies and establish that the discrepancies in the findings stem from both different time periods studied and the policy definitions used. I then make a methodological correction by adjusting for the staggered policy adoption and find that Naloxone Access Laws increase opioid mortality by 39%. Finally, I discuss the validity of the results. A sharp rise in opioid mortality preceded the adoption of Naloxone Access Laws. Therefore the estimated results from a difference-in-differences model will not be causal. I propose using a contiguous county-border model to establish the causal effect of Naloxone Access Laws. The findings from this chapter emphasize the challenges of establishing causal estimates when evaluating public policies that inherently are not exogenous.

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by

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Dedication

En memoria de Jorge Constantino Perez-Rico, con mucho amor siempre.

To my family: Vinnie, my mom, Elena, Elias, and Jose.

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List of Abbreviations

ACS	American Community Survey
CDC	Center for Disease Control
CHIP	Children's Health Insurance Program
DACA	Deferred Action for Childhood Arrivals
DREAM	Development, Relief, and Education for Alien Minors
FHA	Federal Housing Administration
GED	General Educational Development
GSL	Good Samaritan Law
ICE	Immigration and Custom's Enforcement
IPUMS	Integrated Public Use Microdata Series
ITIN	Individual Taxpayer Identification Number
JEEPS	Justice Expenditure and Employment Extract Series
MML	Medical Marijuana Laws
NAL	Naloxone Access Law
PDMP	Prescription Drug Monitoring Programs
PEP	Priority Enforcement Program
SCP	Secure Community Program
TED	Treatment Episode Data
TRAC	Transactional Records Access Clearing House
WIC	Special Supplemental Nutrition Program for Women, Infants, and Children

Chapter 1: Introduction

In this dissertation, I study the effects of state and federal programs and policies aimed to help vulnerable populations. The goal of this dissertation is to use applied econometric methods to establish causal effects to better understand elements of program and policy design.

I begin with the first two chapters focusing on the undocumented population in the United States. In 2016, an estimated 10.7 million people in the United States were undocumented and 7 million US born people lived with an undocumented family member [1, 2]. I study the role immigration enforcement has on pregnant women's participation in safety net programs and the adverse effects on their infants' health. In chapter 3, I study the role of legal status on living arrangements. In particular, I use Deferred Action for Childhood Arrivals (DACA) to understand the role temporary legal status and relief from deportation has on cohabitation decisions. Then in the last chapter I shift my focus away from immigration to the opioid crisis. The opioid related death rate has rapidly risen in the past two decades and was over 16 per 100,000 people in 2019 [3]. One manner in which states have responded to the opioid crisis is by passing legislation to increase access to Naloxone, an overdose reversal drug. I re-examine what we know about the effect these laws have on opioid related deaths.

Although immigration enforcement is not directly linked to safety net programs, deterrence could occur among households with undocumented people. In chapter 2, I estimate the effect of the deportation rate during pregnancy on participation in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) and the use of Medicaid to pay for the delivery. Women are eligible for WIC regardless of their immigration status, but undocumented immigrants usually do not have access to public health insurance. In special circumstances, including the delivery of a newborn, an undocumented person may use Medicaid. I find a null effect of immigration enforcement on an undocumented mother's prenatal WIC participation, but I find a log point increase in the deportation rate lowers Medicaid payments by 4.4 percentage points. The differences in program eligibility may explain the deterrence, or chilling effect, seen in Medicaid payments but not observed in WIC participation. A rise in immigration enforcement lowers prenatal care. I also find immigration enforcement causes an improvement in birth weight, but this is due to positive selection into birth by mothers. The reduction in births by likely undocumented mothers suggests women who would have had weaker births either choose not to be become pregnant during times of greater immigration enforcement or do not carry the pregnancy to term due to a miscarriage or an early termination.

I contribute to the literature by first estimating the effect of immigration enforcement on two safety net programs. My focus on WIC is particularly interesting given the unique feature that undocumented mothers are eligible beneficiaries. Previous studies focused on deterred safety net program participation of US born children with undocumented parents [4, 5, 6, 7, 8]. Second, I examine birth outcomes other than birth weight and use a continuous measure immigration enforcement. My measure of deportations captures the effect of routine immigration enforcement instead of a large one-time event. Finally, I examine the effect of immigration enforcement on US born mother with likely undocumented family members. The potential spillover effects of immigration enforcement to other adults has been understudied. I find US born Latinas are less likely to participate in Medicaid, and I find some evidence of positive selection into birth as well.

In chapter 3, I study the effect of DACA on beneficiaries' living arrangements and housing characteristics. In June 2012, the Obama Administration announced DACA. This program provides temporary deferred action for undocumented youth who meet certain criteria. While deferred action is not a permanent legal status, DACA recipients receive a social security number, a work permit, and are able to apply for a driver's license in their home state. DACA has been found to improve educational attainment, labor market outcomes, and reduce poverty [9, 10, 11, 12]. I contribute to the existing literature by examining the effect of DACA on the likelihood of living with a parent and other living arrangements. I additionally estimate the effect of DACA on a variety of proxies for housing quality such as the household size, the number of bedrooms, whether the home is rented or owned, and the cost of rent.

In the main model, I estimate the effects of DACA using data from 2005 to 2019. I use a regression discontinuity model that exploits the birth date eligibility cutoff of DACA. A person must be under the age of 31 when the policy was announced, in June 2012, to be eligible. I compare likely eligible immigrants to likely undocumented immigrants who meet all of the other eligibility requirements except for the birth date criteria. I find DACA lowers the likelihood of living with family members other than a parent by 2.4 percentage points; this is a 20% reduction in this living arrangement. I find DACA causes a similar magnitude increase in the likelihood of living with a parent in a rented house. Overall this suggests DACA induces people to move from living with other family members to living with their parents. When I limit the sample to 2016, before an attempt to eliminate DACA, I find DACA causes the amount paid for housing among renters to increase by $73. This suggests DACA led to housing quality improvements but the attempted removal of DACA in 2017 dampened the effect.

Finally in chapter 4, I re-examine the effect of opioid mortality prevention policies that were designed to increase the availability of Naloxone. Despite the intent of these laws, concerns arose regarding the potential of moral hazard. Individuals may be more likely to engage in riskier behavior, such as taking higher doses of opioids, if they believe the risk of dying from an overdose is lower. The literature on Naloxone Access Laws (NALs) reports mixed evidence on the effect of these policies on opioid mortality. The estimated effect on the opioid mortality rate in the literature ranges from -15% to 14% despite similar estimation strategies [13, 14, 15]. I first evaluate the differences across the studies and determine that the mixed conclusions result both from the time period of the studies and the manner in which they defined the legislation.

I then make a methodological improvement by adjusting for the staggered policy adoption. In the presence of heterogeneous treatment effects and variation in the timing of the intervention, a difference-in-differences model will not result in the average treatment effect. I implement the correction for the staggered policy adoption based on Callaway and Sant'Anna [16] and Sun and Abraham [17]. Using the corrected method, I find that having an opioid mortality prevention policy *increases* the opioid mortality rate by 39%. While I have corrected for the variation in the timing of the policy, in order to interpret the point estimate as a casual effect the policies must be exogenously adopted without any anticipatory behavior occurring. I find strong evidence that NALs were adopted in states after they experienced a sharp rise in the opioid mortality rate. Thus we cannot interpret the estimated effects as causal evidence. I propose an alternative estimation strategy that future work on this topic should consider.

Overall my dissertation uses three distinct settings to study public policies and vulnerable populations. Together these studies underscore program design and the need to consider potential adverse effects of policies. Additionally, my dissertation highlights the importance of using appropriate estimation strategies to establishing causal effects. One of the challenges in estimating the effects of public policies is the inherent non-randomness of adoption. There must be careful consideration of the model assumptions when employing different empirical strategies.

Chapter 2: Immigration Enforcement and Deterrence: The Effect on the Safety Net and Birth Outcomes

2.1 Introduction

The undocumented population steady rose from 5.7 million to over 12 million people between 1995 and 2007. As seen in Figure 2.1, since 2007 the estimated number of undocumented people has slowly declined to 10.7 million in 2016 [1]. During this period, there has also been a compositional shift away from newly arrived immigrants towards those who have been in the United States for over a decade. As undocumented immigrants permanently reside in the United States, there is a growing number of mixed status families. A mixed status family has members who are undocumented as well as those with legal status, either through naturalization or birth. Between 2011 and 2014, an estimated 5.9 million children and 1.1 million adults born in the United States lived with an undocumented family member. Figure 2.2 and Appendix Figure A.1 depict the geographic distribution of children with legal status in mixed status households [2]. Both undocumented individuals and US citizens with undocumented family members may be deterred from participating in public programs due to fear of local immigration

[0] This chapter uses confidential data from the National Center for Health and Statistics. Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views of the National Center for Health and Statistics.

enforcement. This chilling effect on the use of publicly provided services can have long term consequences, especially for low-income, first-generation children.

I use state level variation in deportations to estimate the effect of immigration enforcement on two key safety programs for pregnant women, WIC and Medicaid. Unlike most public programs, WIC eligibility does not depend on citizenship or legal status. Undocumented mothers and children under the age of 5 must be below 185% of the federal poverty line to qualify for a monthly food stipend. Undocumented immigrants are not usually eligible for public health insurance, but Medicaid may be used to pay for the delivery of a pregnancy regardless of a person's legal status. I also examine the effect of immigration enforcement on prenatal care and the birth weight of the baby. I use the National Center for Health Statistics' confidential Natality Files from 2009 to 2018. I restrict the sample to first time mothers with less than a bachelors degree to proxy for belonging to a low-income, WIC eligible household. I use information on maternal country of birth and ethnicity to construct groups of likely undocumented mothers and comparison groups. I additionally examine the effect of deportations on US born Latina mothers. They can be indirectly effected by immigration enforcement if they belong to a mixed status household. In a difference-in-differences framework, I compare babies born to US born mothers and babies born to likely undocumented mothers during times of varying deportation intensity.

I find deportations have a null effect on WIC participation but do cause a decline in Medicaid payments among likely undocumented mothers. Likely undocumented mothers are also less likely to receive prompt or any prenatal care during times of greater immigration enforcement. I find that a rise in immigration enforcement causes improvements in birth weight. When I examine the birth rate, I find that a rise in the deportation per non-citizens rate lowers the birth rate among likely undocumented women. This evidence suggests positive selection into pregnancy by undocumented mothers. A potential explanation is that undocumented mothers who would have had weaker births decide not to have a child, miscarry, or terminate the pregnancy during times of greater immigration enforcement. These results are robust to alternative measures of deportation enforcement. The results for US born Latina mothers are mixed depending the definition of immigration enforcement. I find inconclusive evidence on the effect of immigration enforcement on WIC participation and the incidence of prenatal care prior to the third trimester. I estimate that Medicaid participation falls among US born Latinas. I also find improvements in birth weight and some evidence of a decline in the quarterly birth rate among US born Latinas.

The different response to immigration enforcement by undocumented mothers and US born Latinas highlights the different incentives and costs faced by each group of women. Prior literature has found immigration enforcement causes a decline in employment, greater poverty rates, and low participation in safety net programs in likely undocumented households [4, 5, 6, 7, 8]. Undocumented people experience fear for themselves, which may make them less likely to drive without a license or less likely to work without proper documentation. US born Latinas do not face those direct barriers but may fear for their undocumented family members. Furthermore in mixed status households, immigration enforcement could alter the overall household economic security. Alsan and Yang [18] recently examined the effect of the Secure Community Program roll-out among citizen Latinos. The Secure Community Program shares fingerprints obtained by local law enforcement with Homeland Security. They find a decline in participation in the Supplemental Nutrition

Assistance Program and the Supplemental Security Income among citizen Latinos.

There are four ways in which my chapter builds on the relevant existing literature. First, I look separately at US born Latinas and Latinas who are likely to be undocumented. Both of these groups are affected by increased deportations through distinct channels, and it is useful to empirically examine whether their outcomes appear to respond differently. Second, I investigate the effect of an increase in the deportation rate on participation in WIC. To the best of my knowledge, this is the first paper to investigate this outcome. This is particularly of interest because unlike other safety net programs, undocumented women are eligible for WIC; it is not only their US born children that are eligible for the program. Third, I investigate the impact of an increase in the deportation rate on fertility and pregnancy outcomes. Fourth, I use a routine immigration enforcement metric as my main explanatory variable. The few papers examining the effect of immigration enforcement on birth weight rely on a specific immigration policy or a large immigration raid [19, 20, 21].

The remainder of the chapter proceeds as follows. Section 2 provides background information on immigration enforcement in the United States. I describe the link between enforcement and deterrence and discuss existing evidence. I additionally highlight the importance and differences between the two safety net programs, WIC and Medicaid. In Section 3 and Section 4, I discuss the data and the empirical model. I present results in Section 5, and in Section 6 I conclude with a discussion of the significance of my findings.

2.2 Background

2.2.1 Immigration Enforcement in the United States

In the 1990s, the undocumented population residing in the United States steadily increased. During this time, immigration enforcement efforts were focused on the border between Mexico and the United States. In 2003 the Department of Homeland Security replaced the Immigration and Naturalization Services and the U.S. Customs Services. The Department of Homeland Security established the Immigration and Custom's Enforcement (ICE) agency. ICE became responsible for both internal immigration enforcement as well as securing the border. At this time, ICE began the Enforcement and Removal Operation, which actively identified and deportation undocumented individuals living within the United States [22].

Prior to the creation of ICE, the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 established 287(g) Programs that granted limited abilities to law enforcement officers [23]. These programs allow local law enforcement to voluntarily identify and remove undocumented people in certain situations. The 287(g) agreements can be broadly classified into two types, the jail model and the task force/warren model. The jail model limits the police's ability to questioning only arrested non-citizen while task force/warren model allows local law enforcement to act as ICE. The task force/warren model previously allowed local law enforcement to interrogate non-citizen about their legal status. Now law enforcement can only hold undocumented immigrants and execute ICE warrants.[24].

More recently, in 2008 the Bush Administration piloted the Secure Community

Program (SCP), which automatically shared fingerprints of arrested individuals from local police stations with the Department of Homeland Security. The Obama Administration continued to expand coverage of the SCP. Despite some local resistance to this policy, by early 2014 all U.S. counties implemented the SCP. After multiple challenges related to racial profiling and discrimination caused by the SCP, the Department of Homeland Security temporarily suspended the program in November 2014 [25]. The Priority Enforcement Program (PEP) immediately replaced the SCP. Using the same fingerprint screening methods, the Department of Homeland Security continued to locate undocumented individuals with a more limited capacity. The new limitations of the PEP were meant to address concerns that the SCP discouraged undocumented individuals from reporting crime and domestic abuse due to the fear of deportation. The PEP ended in January 2017, when President Trump signed an Executive Order to reinstate the SCP [26].

In addition to the local agreements linking law enforcement officers to immigration enforcement, states have increased immigration enforcement through Omnibus legislation and E-Verify mandates. Arizona first introduced an Omnibus immigration law in 2010 that included a clause allowing police to request identification proving a person's legal immigration status during a lawful stop. Five other states adopted similar Omnibus laws including a lawful stop clause by 2012 [27]. In addition to these policies that link state and local law enforcement officers to immigration enforcement, the E-Verify program is used in some workplaces to confirm the legal status of employees. The E-Verify program was initially used in 1996 among government employees, but states began mandating the use of E-Verify among some or all employers in the state. Less than 160,000 business in the United States used E-Verify in 2009. By 2018, over 810,000 businesses were confirming employees' legal work status [28]. All of these policies have contributed to the local variation in immigration enforcement and deportation intensity experienced by undocumented people over time.

2.2.2 The Safety Net: WIC and Medicaid

Congress permanently established WIC in 1975 to provide supplemental food for low income women and children under the age of five. Although the federal government provides discretionary funds for WIC, the program has received sufficient funding to provide services to all participants since 1997 [29]. Today, WIC beneficiaries include pregnant and postpartum women, infants, and children. WIC eligibility requires households to earn less than 185% of the Federal poverty line. In the fiscal year 2017, about 14 million individuals participated in WIC; of which, 1.4 million were pregnant women. Although national estimates suggest about half of all pregnant women qualify for WIC, participation among eligible pregnant women is about 45% [29]. WIC beneficiaries receive supplemental food packages or stipends. These food packages can include baby food, iron-fortified adult cereal, fruits and vegetables, milk, and eggs. In addition to the food packages or stipend, postpartum women benefit from breast feeding support services, immunizations for children, and referrals to other health services. The average monthly food benefit of WIC per person has remained steady over time at \$43-\$48 in 2013 real dollars. In comparison, the average monthly benefit for the Supplemental Nutrition Assistance Program in 2013 was \$133 per person [30].

Previous work examining the effect of WIC participation on birth outcomes has found that the benefits vary across participants. Figlio et al. [31] apply propensity matching techniques to respondents in the Panel Study of Income Dynamics and fail to find a significant effect of WIC participation on gestation age and birth weight. Although average birth weight appears unaltered, there is a statistically significant reduction in the incidence of low weight births, defined as a birth under 2500 grams. In an attempt to overcome omitted variable bias, Hoynes et al. [32] exploit the quasi-random variation in WIC roll-out to study birth outcomes. They find that WIC increases birth weight, especially among babies born to mothers with low levels of educational attainment. This evidence is consistent with work by Bitler and Currie [33], who observe negative selection into WIC. Even after controlling for this negative selection, WIC benefits are larger among more vulnerable women.

Unlike WIC, the eligibility requirements for public health insurance programs exclude undocumented immigrants. In half of the states, even non-citizen immigrants with legal status must wait 5 year before enrolling in Medicaid. There are two ways in which undocumented pregnant women may qualify for health insurance. First Emergency Medicaid covers an infant's delivery. Even though Medicaid typically covers 60-days of postpartum care, Emergency Medicaid does not provide financial support for prenatal or postpartum care [34]. The second way is through the unborn child option of the Children's Health Insurance Program (CHIP). Beginning in 2002, states could choose to offer an unborn child option that provided health insurance coverage for undocumented mother's prenatal care and the infant's delivery [35]. Some states have further opted into covering 60-days of postpartum care for the mother through the unborn child option [34]. Since undocumented women are ineligible for any other health insurance, both Emergency

Medicaid and the CHIP unborn child option may be important financial resources. Between 2001 and 2004, 90% of Emergency Medicaid recipients in North Carolina were pregnant women and 99% of recipients were undocumented [36].

The government does not consider either WIC or Medicaid as a public charge. Although undocumented immigrants do not have many options for legalization, they may opt out of any public program that could hinder future legalization attempts. An immigrant will be denied a visa or green card if they are deemed to be reliant on the government. Participation in certain public safety net programs, such as receiving cash assistance, would count against a person. The 1999 guidance explicitly excludes health insurance and nutritional programs as qualifying public charges, therefore public charge concerns do not apply to these programs [37].

2.2.3 Conceptual Framework: Immigration Enforcement as a Deterrence

Immigration enforcement policies may have adverse impacts for citizens, particularly children in mixed-status households. Household with undocumented family members may be deterred from sharing household information with the government despite their eligibility for public assistance programs. As the fear and risk of deportation rises, undocumented people may be less willing to drive without a license, preventing them from easily accessing health care. During an in-dept interview regarding the 287(g) agreements in North Carolina, a woman recounts that her sister-in-law delayed seeing a doctor until she was 7 months pregnant because she was undocumented. Another participant describes the increased fear of driving without a license and being stopped at a checkpoint [38]. Friedman and Venkataramani [39] find that ICE detention requests are negatively associated with routine health care visits among Hispanics. It is not just these behavioral changes but the stress of being deported could have negative impacts on a newborn's health. The adverse effects of maternal stress have been well documented in the medical literature [40]. The fear, stress, and increased transportation barriers would imply a decline in participation in safety net programs, lower prenatal care, and ultimately worsened birth outcomes.

Alternatively greater risk of deportations may change an undocumented person's willingness or ability to work without proper documentation. If a household member is deported, there may be an immediate impact on household resources. If this is the case, immigration enforcement may cause a greater need for WIC benefits and health insurance. Amuedo-Dorantes et al. [5] find immigration enforcement increases the poverty rate among US born children with undocumented parents. The benefit of WIC and health insurance during times of hardship may overcome the cost of interacting with the government. If an undocumented mother is more likely to enroll in WIC due to a decline in household income during heightened immigration enforcement, the nutritional advice from WIC and emphasis on prenatal care could mitigate the negative affects of immigration enforcement on an infant's health.

The benefit of WIC initially appears to be lower than that of Medicaid since the national average cost of a vaginal birth without complications is about $\$15,000$ before insurance while WIC provides each beneficiary less than $\$50$ per month [30, 41]. Despite the high price of childbirth, a low-income undocumented mother would not likely pay the full price despite lacking health insurance. The hospital would be financially incentivized to enroll eligible mothers in Emergency Medicaid or CHIP's unborn child option to avoid the costs associated with uncompensated care [42]. From the perspective of the mother, the financial benefit of health insurance for the delivery compared to WIC is unclear and depends on the actual price they would be charged for the delivery.

The cost of participation in WIC is lower than the use of health insurance. Unlike most safety net programs, undocumented mothers are WIC eligible despite their immigration status. Thus participation in WIC does not disclose any information about a person's legal status. Using Emergency Medicaid or the CHIP unborn child option could signal a lack of legal status. Although immigrants with legal status who have yet to meet their state's waiting period are also eligible for these health insurance options, an undocumented mother could fear the government may deduce that she is in the United States without documentation. Overall the impact of the deportation rate on safety net participation, prenatal care, and birth weight is ambiguous for undocumented women.

US citizen with undocumented family members may also experience fear due to increased immigration enforcement. Hainmueller et al. [43] find that having an undocumented parent increases the likelihood a US born child is diagnosed with an adjustment and anxiety disorder. This finding highlights the mental health toll of immigration enforcement among citizen family members. The challenges faced by citizens in mixed status households are lower than undocumented people. US citizens have access to a drivers license, are more likely speak fluent English, and do not fear their own deportation. They do not face the same transportation and communication challenges. Despite this, US citizen with undocumented family members may be partially impacted by immigration enforcement through concern for their family members and through any resources changes in the household resulting from greater immigration enforcement.

The effect of immigration enforcement on birth weight has been estimated for newborns with undocumented mothers. In 2008, Postville, Iowa experienced the largest immigration raid in the United State history to-date. Novak et al. [20] compare newborns of Latina mothers before and after the raid, finding the incidence of low birth weight rises by 24% after the ICE raid. The raid could have altered household resources and caused greater stress for undocumented mothers, adversely impacting infant's health. Ambrozek and Hill [19] evaluate the effect of 287(g) and the SCP on birth outcomes; they find birth weight declines by 13.7 grams. In addition to stress, the 287(g) and SCP could deter undocumented women from driving without a license to prenatal care appointments. Lower health care could adversely impact an infant's birth weight. In Arizona, the proposal of SB1070, which was commonly referred to as the "show your papers" policy, allows state law enforcement to stop anyone and require evidence of legal documentation. Exposure to the law being signed caused Latina immigrants to have lower birth weight newborns [21]. Since the law had yet to be enacted, the change in birth weight is a result of altered behaviors due to fear and maternal stress.

I contribute to the existing literature on the effect of immigration enforcement and policies on birth outcomes by exploiting variation in local deportations. Compared to these studies, I am detecting the effect of continued, local immigration enforcement as opposed to a singular event or policy on WIC and Medicaid participation and birth outcomes. I also distinguish between the effect on undocumented mothers and US born Latinas with likely undocumented family members.

2.3 Data

I obtain data on state-level deportations between 2003 and 2019 from the Transactional Records Access Clearing House - Syracuse University [44]. The TRAC collects information on immigration enforcement statistics through regular Freedom of Information Act requests. I restrict the measure of deportations to internal deportations, excluding any detentions that occur at the border. If I were to include border activity, I would overstate the level of immigration enforcement experienced by an undocumented person living within the United States. I construct the non-citizen state population from the Current Population Survey to estimate the deportations per 1000 non-citizen rate [45]. Nationally the deportations per 1000 non-citizen rate doubled from approximately 9 to 18 between 2005 and 2009. Deportation rates remained high until 2014, when enforcement fell to 10 deportations per 1000 non-citizen as seen in Figure 2.3. In Appendix Figure A.2, we can see that the deportation intensity over time varies across different states.

As a second measure of immigration enforcement, I follow Watson [46] and Amuedo-Dorantes and Lopez [47] to construct a state measure of immigration enforcement policies. I obtain immigration enforcement policy information from Gelatt et al. [48]. I combine the jail model and task force/warren model of 287(g) programs, SCP and PEP, E-verify, and Omnibus immigration laws in the following way.

$$EI_{sq} = \sum_{p} \frac{1}{n_{s,2000}} \sum_{c \in s} n_{c,2000} \times \frac{1}{3} Agreement_{pcq} \tag{2.1}$$

For each policy, $Agreement_{pct}$ is the number of months in a quarter that the policy p was active in county c. This is weighted by the county's population, $n_{c,2000}$ in 2000 obtained from the Surveillance, Epidemiology, and End Results Program[1] [49]. I sum over all counties in a state and divide by that state's total 2000 population, $n_{s,2000}$. By finally summing over each policy, I obtain EI_{st}, which can be interpreted as the number of immigration enforcement policies that an average person in that state would face during a particular year-quarter. Once the index is aggregated to the state level, a person faced between 0 and a maximum of 4.3 policies in a given state between 2009 and 2017. In my analysis, I use this immigration enforcement index as a robustness test to confirm the results I find when using the deportation per 1000 non-citizen rate.

Information on birth outcomes and health behaviors comes from the National Center for Health Statistics Natality Files from 2009 to 2018 [50]. This confidential data provides me with individual records of all births. I restrict the sample to first births to mothers with less than a bachelors degree. Since I do not have any information on income, I rely on the positive correlation between income and education to increase the likelihood that the sample of mothers are WIC and Medicaid eligible. I use two measures of safety net participation: prenatal WIC participation and whether the delivery was paid using Medicaid. I use the number of prenatal visits and the month prenatal care began to construct indicators for whether a mother engaged in any prenatal care during the pregnancy and prompt prenatal care defined as any prenatal care prior to the third trimester. I observe birth weight and the incidence of healthy birth weight, over than 2500 grams. Lastly I construct the quarterly birth rate per 1000 women using the number of all births in

[1]To construct the weighted enforcement index, I obtain population data from the Surveillance, Epidemilogy, and End Results Program because they accurately measure the county level population. This data set does not include information such as citizenship. The Current Population Survey does allow me to measure a state's non-citizen population but does not provide me with accurate county populations.

the Natality Files and the Current Population Survey [45]. From the Current Population Survey, I estimate the female population between the ages of 14-45 for each relevant group. The relevant groups are the comparison groups described below by educational attainment and marital status. I then divide the total number of births by my estimate of women of childbearing age. A limitation of the Natality Files during this period is that the US Standard Certificate of Live Birth was modified in 2003. Although my sample begins in 2009, all states did not adopt the 2003 form until 2014 [51]. The changes in the birth certificate impact the following key variables: maternal education, race and ethnicity, Medicaid, WIC, and prenatal care.

I must assign mothers as likely undocumented based solely on their place of birth and ethnicity. I do not have information on their year of arrival to the United States or citizenship status, thus I present the results with various assignment alternatives. As done in the literature previously, I use foreign born Latina mothers as a proxy for undocumented mothers [19, 20, 21]. Since foreign born Latina mothers includes those with legal status, the estimated effect of immigration enforcement among undocumented women will be attenuated. In efforts to more precisely capture undocumented status, I use foreign born mothers from select countries where undocumented people originate from most frequently: Mexico, El Salvador, Guatemala, India, and Honduras [52]. I estimate the model with two different comparison groups: US born mothers and non-Latina US born mothers. While US born Latinas may initially appear to be a good comparison group due to their similarities with foreign born Latinas, US born Latinas may alter their behavior due to immigration enforcement if their spouse or other household member is undocumented. Due to the potential spillover effects to US born Latinas, my preferred control group is

US born non-Latinas. I also compare US born Latina mothers to US born non-Latinas to measure the indirect effect of immigration enforcement.

Table 2.1 shows the summary statistics for each assigned group. Foreign born mothers are more likely to participate in WIC, pay for the delivery using Medicaid, and give birth to infants with a higher birth weight. They are less likely to receive prompt prenatal care. Table 2.1 also reports two measures of immigration enforcement. The deportation per 1000 non-citizen rate encompasses all internal removals that occurred between the month of birth and one year prior. This measure captures the cumulative deportation intensity experience by a mother right before and during the pregnancy. The second measure is the enforcement index, which can be interpreted as the average active removal policies experienced by a person at the time of birth. This policy index only goes through 2017.

2.4 Empirical Strategy

2.4.1 Main Specification

I estimate Equation 2.2 with collapsed data using the different combination of likely undocumented mother and US born mother comparison groups. I also estimate the model using US born Latinas and non-Latinas to capture the indirect effect of immigration enforcement. I convert the individual records to a birth quarter-state frequency for each maternal group by educational attainment and marital status. I exploit variation across states and over time to estimate the causal effect of the deportation per 1000 non-citizens rate. The estimation equation is as follows:

$$
\begin{aligned}
Y_{gsq} =& \alpha_0 + \alpha_1 \ln DeportRate_{sq} + \alpha_2 \mathbb{1}Undoc_g + \alpha_3 \mathbb{1}Undoc_g \times \ln DeportRate_{sq} \\
&+ \alpha_4 \mathbb{1}married_g + \alpha_5 \mathbb{1}LTHS_g + \alpha_6 \mathbb{1}HS_g + \delta_s + \phi_y + \rho_q + \delta_s \times years + \epsilon_{gsq},
\end{aligned}
\tag{2.2}
$$

where Y_{gsq} is the fraction of prenatal WIC participation for births in group g in state s born in quarter q of year y. The deportation per 1000 non-citizens rate is measured as the cumulative deportations in the birth quarter and the three previous quarters, which captures the intensity of immigration enforcement during the entirety of the birth and the three months prior. I include controls for a mother being married, obtaining less than high school education, and earning a high school degree. The excluded education category is some college but less than a bachelors degree. Both educational attainment and marital status are included in the model to capture household resources and familial stability. I include fixed effects for the state, year, and birth quarter. I additionally allow for a state specific linear time trend, but the results are robust to the exclusion of the time trend. To address serial correlation in the error term, I cluster the standard errors by state-year and use relevant count of births in each group as weights.[2]

The coefficient on the interaction between the deportation rate and being likely undocumented, α_3, is the parameter of interest. When the deportation per 1000 non-citizens rate increases, I expect WIC participation to fall if undocumented mothers experience an increase in their fear of sharing information and interacting with government agencies.

[2]Weighting the regression by birth count is equivalent to estimating the same model using individual observations. By collapsing the data and using weighted groups, I save significant computation time.

Alternatively if an undocumented mother experiences changes in household income caused by the rise in local deportations, we might see WIC participation increase due to higher household need. Thus the sign of α_3 is ambiguous. I expect α_1 to equal zero since deportations should have no impact on mothers with legal status. The key identifying assumption of the difference-in-differences model requires that absent deportations, the trend in the fraction of WIC participation for likely undocumented mothers and mothers born in the United States would be unaltered.

I also estimate Equation 2.2 on the fraction of deliveries paid using Medicaid, prenatal care, birth weight, and fertility. The use of Medicaid payments is another safety net program that will test the chilling effect of immigration. Since undocumented mothers are only eligible for health insurance during pregnancy, I expect to see a stronger chilling effect for Medicaid payments than for WIC participation. The amount of prenatal care is another behavioral outcome that could be altered by a rise in immigration enforcement. I examine overall birth weight and the incidence of a health birth weight as two measures of infant health. Lastly, I estimate the effect of immigration enforcement on the birth rate to test for selection into birth.

2.4.2 Robustness

In my main specification, I measure immigration enforcement as the deportation per 1000 non-citizens during the pregnancy. The benefit of using this measure with a contemporaneous denominator is that it reflects a more accurate measure of the deportation intensity experienced by a person living in a particular state. A drawback is that changes in the non-citizen population within a state could produce variation in the measurement of immigration enforcement even if there is not a true rise or fall in deportations. As a way to validate my results, I estimate the model using the following alternative measures. First I use the deportation rate per 1000 non-citizen in 2000. By using the non-citizen population in 2000, I ensure variation in the measure is solely a result of changes in deportations. I use the population in 2000 because it captures the non-citizen population prior to the increase in internal deportations by ICE. The second alternative measure of deportation enforcement is an enforcement index. This index represents the average number of state and local immigration enforcement policies active in the state at the time that the mother delivers her baby. Lastly, I use the presence of the individual policies instead of a combined index. With these alternative measures, I estimate my main results for likely undocumented mothers based on their country of origin compared to US born non-Latinas. I also estimate the model using births to US born Latinas compared to non-Latinas.

I additionally estimate the model using the deportation rate per 1000 non-citizen during alternative time frames. Instead of using the deportations during the pregnancy, I also use the cumulative deportations two, three, and five years prior to the birth quarter. It is unclear whether the relevant measure of deportations is the contemporaneous rate or the mother's lifetime exposure. While I do not have information on a person's year of arrival to the United States, the longer time spans of cumulative deportations capture a broader measure of immigration enforcement. I also estimate the main results using all births instead of limiting the sample to first births.

Due to limited information in the Natality birth files, I cannot produce placebo groups such as citizen immigrants or proxy for a mother's legal status using her year of arrival to the United States. Instead, I estimate the Equation 2.2 on two sub-samples of states with high and low concentrations of undocumented residents. I obtain the share of undocumented people in a state from Passel and Cohn [53]. The ten states with the highest share of undocumented people in 2016 were Nevada, Texas, California, New Jersey, Maryland, Arizona, Florida, Massachusetts, the District of Columbia, and Georgia. Undocumented people account for 3.8% to 7.1% of these states' population. The ten states with the lowest share of undocumented people were Vermont, West Virginia, Montana, Maine, Mississippi, North Dakota, South Dakota, New Hampshire, and Ohio. Undocumented people account for less than 1% of these states' population. I expect to find amplified point estimates when using the states with a high concentration of undocumented immigrates and little to no effect when using the low concentration states. In the low concentration states, there is less likely to be undocumented mothers in my sample. It is important to note that there could also be heterogeneous effects based on geography. I will not be able to separate changes in the point estimate attributed to differences in the responsiveness to deportations or the accuracy of my proxy for undocumented mothers.

2.5 Effects of Deportations

2.5.1 Maternal Behaviors: The Safety Net and Prenatal Care

Table 2.2 reports the effect of deportations on likely undocumented mothers' prenatal WIC participation and the use of Medicaid as a payment for the delivery of the newborn.

Each column represents a model using different comparison groups and proxies for undocumented mothers. In Columns 1 and 2, the comparison group is all US born mothers. Columns 3 and 4 restrict the comparison group to non-Latina US born mothers; the exclusion of US born Latina mothers from the comparison group lowers the potential of spillover effects. As a proxy for undocumented mothers, Columns 1 and 3 use foreign born Latina mothers, and in Columns 2 and 4 the sample is restricted to mothers born in select countries. Results for all outcomes regarding undocumented mothers will be presented in this manner in Tables 2.2 -2.5.

As seen in Panel A, I find a reduction in WIC participation among likely undocumented mothers only when using all US born mothers as the comparison group. Once I use my preferred comparison group of non-Latina mothers, I find no significant effect of the deportations per 1000 non-citizens rate on likely undocumented mothers' participation in WIC. In Panel B, I report the effect of immigration enforcement on the share of deliveries paid with Medicaid. Although undocumented individuals are not typically eligible for public health insurance, Medicaid includes coverage for pregnant undocumented women if they meet the necessary income eligibility. As seen in Columns 1-4 across the alternative comparison and treatment groups, I find a log point increase in the deportations per 1000 non-citizens rate causes a 4.1 to 5.4 percentage point decline in Medicaid payments for the delivery. Note that given the measurement error in identifying a mother's undocumented status, these point estimates are attenuated and the true effect is likely larger in magnitude.

The chilling effect found in Medicaid payments could be caused by the potential signal of the mother's immigration status. All mothers, regardless of legal immigration status, are eligible for WIC if they meet the income or nutritional risk requirements of the program. Therefore participation in WIC does not disclose any information about a person's legal status. Immigrants with legal status are usually eligible for Medicaid after a 5-years waiting period [54]. Deliveries of newborns can be covered by Emergency Medicaid or the CHIP unborn child option for mothers who would be eligible for Medicaid except for the immigration requirements. Although immigrants with legal status who have not met the required waiting period could also use Emergency Medicaid or the CHIP unborn child option, participation by undocumented mothers in the program signals their legal status more explicitly than participation in WIC. Furthermore the realized benefit of participating in WIC may be higher than using temporary health insurance. The existence of uncompensated care implies low-income individuals unable to pay for the medical service may not be charged for the full cost of their treatment. The higher benefit of WIC relative to Medicaid along with the lower cost of applying in terms of the amount of information disclosed is consistent with the stronger chilling effect present in Medicaid payments.

Next I examine the effect of the deportation rate on prenatal care. In Panel A of Table 2.3, I report the effect on prompt prenatal care. I define prompt prenatal care as having a prenatal care appointment prior to the third trimester. Prompt prenatal care is 5.1 percentage points higher among US born non-Latinas (93.9%) as opposed to foreign born Latinas (88.8%). I find that a log point increase in deportations per 1000 non-citizens decreases prompt prenatal care by 0.45 to 0.68 percentage points; this increases the gap in prompt prenatal care between US born non-Latinas and foreign born Latinas by approximately 10%. Similarly in Panel B, I find that likely undocumented mothers are less likely to have any prenatal care during their pregnancy, a rise between 0.43 to

0.48 percentage points, when the deportations per 1000 non-citizen rate increase. For undocumented mothers, the additional barriers to transportation as well as fear during times of greater immigration enforcement could causes lower prenatal care.

2.5.2 Infant Health: Birth Weight and Fertility

In Table 2.4 I report the effect of the deportations per 1000 non-citizens rate on an infant being born with a healthy birth weight, over 2500 grams, and the average birth weight of the infant. Any impact on birth weight may be a result of a combination of behaviors induced by deportations, such as changes in parental care and opting out of safety net programs, as well as the direct effect of greater maternal stress caused by deportations. As seen in Panel A, I find a log point increase in the deportations per 1000 non-citizens rate causes a rise in the incidence of healthy birth weight by 0.1 percentage points when using my preferred comparison group in Columns 3 and 4. Panel B shows that across all comparison groups and proxies for undocumented mothers, a log point increase in the deportations per 1000 non-citizen rate causes an increase in birth weight by 5.2-8.3 grams among newborns with undocumented mothers. Given the worsened prenatal care and chilling effect, I would expect a decline in birth weight. This suggests immigration enforcement causes a positive selection in births to undocumented mothers.

I further examine the potential of selection into birth by estimating the effect on fertility. Unlike the prior results, I construct the quarterly birth rate per 1000 women for each group of mothers using all births, not only first births. As seen in Table 2.5, I find the deportation rate lowers the birth rate among likely undocumented mothers. In my preferred specification, Column 4, I find a log point increase in deportations per 1000 non-citizen rate lowers the quarterly birth rate by 1.2 per 1000 women. This suggests the birth weight improvement seen in Table 2.4 can be attributed in part to the behavioral choice of pregnancy induced by the deportation intensity. The positive selection into birth means that either undocumented mothers with weaker births have a miscarriage or the undocumented women that would have had a weaker birth decided not to become pregnant or terminated the pregnancy during times of greater deportations.

2.5.3 Spillover Effects: US Latinas

The effects of immigration enforcement may indirectly impact an undocumented individual's family member or spouse even if they have legal status in the United States. Although they may face less barriers accessing public benefits and interacting with government agencies, US born Latinas with undocumented family can be fearful on their family's behalf. US born Latinas also may experience changes in household resources if undocumented family members are less likely to work during periods of heightened immigration enforcement. In Table 2.6, I present results estimating this effect of having legal status but being indirectly impacted by immigration enforcement by comparing non-Latina and Latina US born mothers' outcomes.

I find a log point increase in the deportations per 1000 non-citizens rate increases WIC participation by 0.93 percentage points among Latina born mothers. US born Latina mothers are 0.37 percentage points more likely to have prompt prenatal care when the deportations per 1000 non-citizens rate increase by a log point. Since the WIC program emphasizes the importance of prenatal care, the prompt care by US born Latina mothers might be caused by encouragement from their increased participation rates in WIC. I do not find a statistically significant effect on Medicaid payments for the delivery. Compared to undocumented mothers, the cost of participating in Medicaid would be lower for US born mothers experiencing the spillover effect of immigration. I also find no statistically significant on obtaining any prenatal care. I find immigration enforcement improves the infant's birth weight and a decline in the quarterly birth rate. The improved birth weight could either be a consequence of the increased WIC participation and the prompt prenatal or selection into birth.

2.5.4 Robustness

I estimate Equation 2.2 using my preferred comparison group and proxy for undocumented mothers, non-Latina US mothers and foreign mothers from select countries, using the two alternative measures of deportation enforcement. In the previously described results, the measure of immigration enforcement is the deportation per 1000 non-citizen rate during the entire pregnancy and three months prior. In Table 2.7, Panel A reports the estimates using the deportations per non-citizens in 2000. Panel B reports the estimates using the enforcement index representing the number of local removal policies enacted in a state at the time of birth. Both alternative deportation measures confirm the main results across all of the outcomes for undocumented mothers. One notable difference is that when I use the enforcement index, I find a chilling effect for *both* WIC participation and a Medicaid payment for the delivery. I previously only find a chilling effect for

WIC participation. One additional enforcement policy at the time of birth lowers likely undocumented mothers' prenatal WIC participation by 3.4 percentage points and lowers Medicaid payments by 7.7 percentage points. In Appendix Table A.1, I additionally report the effect of the introduction of each individual policy at the time of birth. The chilling effect for WIC appears to be driven by the two state level policies, E-Verify and Omnibus Acts. The chilling effect for Medicaid appears to be driven 287G agreements and the E-Verify policy. Omnibus Acts, 287G agreements, and E-Verify all have a negative effect on prenatal care measures and the latter two improve birth weight. The presence of the SCP at the time of birth is the only policy that has a statistically significant effect on the birth rate, a decline of 13.2 births per 1000 women in a birth quarter.

Next I estimate the main results among the 10 states with the highest and lowest percent of undocumented people. The purpose of this analysis is to estimate the effect on a sample where the proxy for undocumented mothers is more and a sample that is less likely to be accurate. As seen in Table 2.8, I find the estimated effect of deportations on undocumented mothers to increase in magnitude when I use the the top 10 states with the highest percent percent of undocumented people. Panel B shows a null effect across all outcomes, except prompt prenatal care, when using births in the 10 states with the lowest percent of undocumented people in their state population. The prompt prenatal result has a lower magnitude point estimate (-3.8 percentage points) compared to the main result (-4.5 percentage points) in Table 2.3. I cannot separate whether these results are caused solely by the change in the precision of my proxy for undocumented mothers or are due to heterogeneous effects across states.

Undocumented women are exposed to immigration enforcement during their entire time in the United States. My main measure of deportations captures cumulative removals during the pregnancy and three months prior. As seen in Appendix Table A.2, the results are robust to alternative timing measures of cumulative deportations. I first divide my main measure into two to capture removals during the first trimester and three months prior and removals during the second and third trimester. Next I use three longer duration of cumulative enforcement: two years, three years, and five years prior to the birth. If I had information on the mother's year of immigration, I could more accurately tailor the measure of cumulative immigration enforcement to represent lifetime exposure. The results are robust to the alternative timing of deportation. The point estimates are similar in magnitude when using deportations during the pregnancy and the point estimates attenuate as the cumulative measure increases in duration. I also estimate the effect of deportations at different horizons in the same model and report the results in the Appendix Table A.3. The model includes the annual deportation rate in the prior three years, the contemporaneous rate, and one year after the birth. Once all of these deportation rate horizons are included in the specification, I do not find a statistically significant effect on any time for any outcome examined. This is likely due to the correlation over time of the deportation rate.

I also estimate the effect of deportations on undocumented mothers using all births instead of just first births. I chose to restrict the sample to first births for my main results because these mothers might be less aware of resources or less comfortable sharing their information with the government. When I use the sample of all births, the results become attenuated or statistically insignificant for some outcomes as seen in Appendix Table A.4.

The results for US Latina moms are more sensitive than those for likely undocumented mothers. When I use the deportations per 1000 non-citizens in 2000 as the measure of immigration enforcement, the estimated effect match the main results. This means the main estimated results are not driven by state migration of the non-citizen population. If instead I measure enforcement as the policy index, I now find US Latina mothers do experience a chilling effect for both WIC and Medicaid. I find an additional immigration enforcement policy lowers WIC participation and Medicaid payments by 1.1 and 1.3 percentage points, respectively. I previously found a null effect on Medicaid and an increase in WIC participation. Furthermore I no longer find that increases in immigration enforcement increase the incidence of prompt prenatal care (see Table 2.9). In the Appendix Table A.5, I individually examine each immigration policy instead of the aggregated index. I find the chilling effect is driven by Omnibus Acts and E-Verify policies; this is consistent with the results when estimated the effect among undocumented mothers. I find evidence of worsened prenatal care for all polices except for the SCP. Only the E-Verify appears to improve birth weight and the SCP lowers the birth rate. Additionally as seen in Table 2.10, all of the results become statistically insignificant when examining either ten state sub-sample. The main results are robust to the use of all births and alternative cumulative timing of deportations as reported in Appendix Tables A.4 and A.6, but I do not estimate a statistically significant effect of deportations on any outcome in the model including multiple horizons (see Appendix Table A.7).

2.6 Conclusion

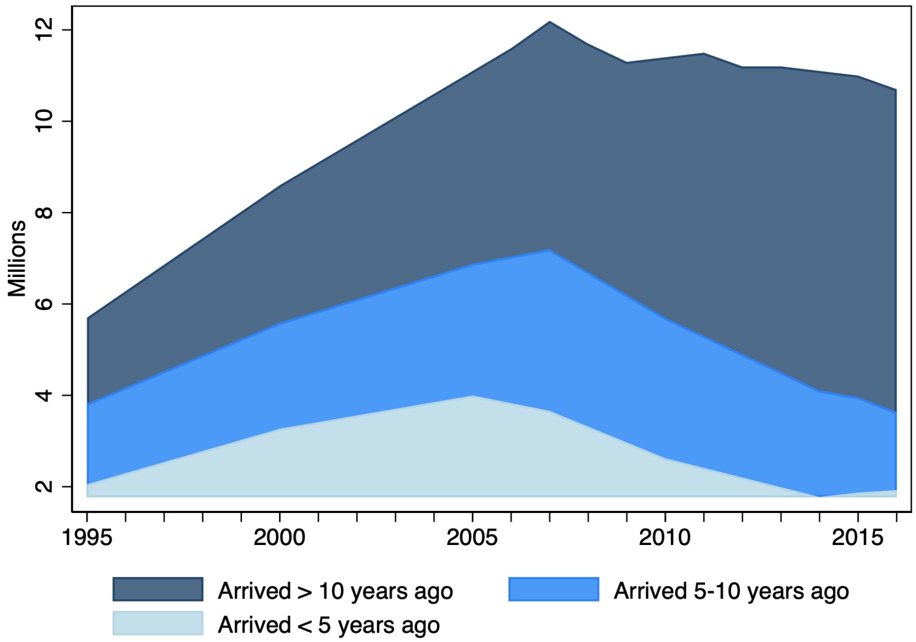
Heightened immigration enforcement can deter participation in safety net programs. Children of undocumented parents are particularly vulnerable to this chilling effect. In this chapter, I estimate the effect of deportations during pregnancy on two safety net programs, prenatal WIC and the use of Medicaid to pay for the delivery. I find a log point increase in the deportations per 1000 non-citizens rate decreases Medicaid payments by 4.4 percentage points among undocumented mothers. There does not appear to be a chilling effect for participation in WIC. Mothers are eligible for WIC regardless of their legal immigration status. Undocumented mothers can use Emergency Medicaid or the unborn child CHIP option to pay for the delivery. Given the eligibility restrictions of Medicaid, WIC is less likely to signal a mother's undocumented immigration status. WIC's lower cost of participation relative to Medicaid could explain the lack of a chilling effect for WIC.

I additionally examine the effect of immigration enforcement on prenatal care and infant's birth weight. I find undocumented mother are 0.55 and 0.45 percentage points less likely to receive prompt or any prenatal care during times of heightened deportations. Furthermore I find an increase in immigration enforcement causes newborns to have improved birth weight and a decline in the birth rate among likely undocumented women. The improved birth weight and declined fertility among undocumented women suggests either deportations deters undocumented women from becoming pregnant or undocumented women experience an increase in miscarriages or obtain abortions among weaker births. These results are highly robust to alternative measure of immigration enforcement.

Deportations and immigration enforcement do not only impact undocumented individuals but also those with undocumented family members. I examine the spillover effect deportations have on US born Latina mothers. While these mothers have citizenship status, deportations may increase maternal stress due to fear for their undocumented family members or may decrease their household resources if undocumented family members are deterred from working or deported. The results for US born Latinas are mixed and sensitive to the measure of immigration enforcement. The chilling effect and prompt prenatal care among Latina mothers is inconclusive as the results vary across enforcement definitions. I find an overall null effect on the incidence of any prenatal care. I consistently find immigration enforcement improvements in birth weight and lowers in the birth rate. This suggests positive selection into birth for US born Latinas, which is also evident among undocumented women. Overall my results highlight how immigration enforcement can impact take-up rates in public programs and alter behavior not only among undocumented people but potentially among US born citizens too.

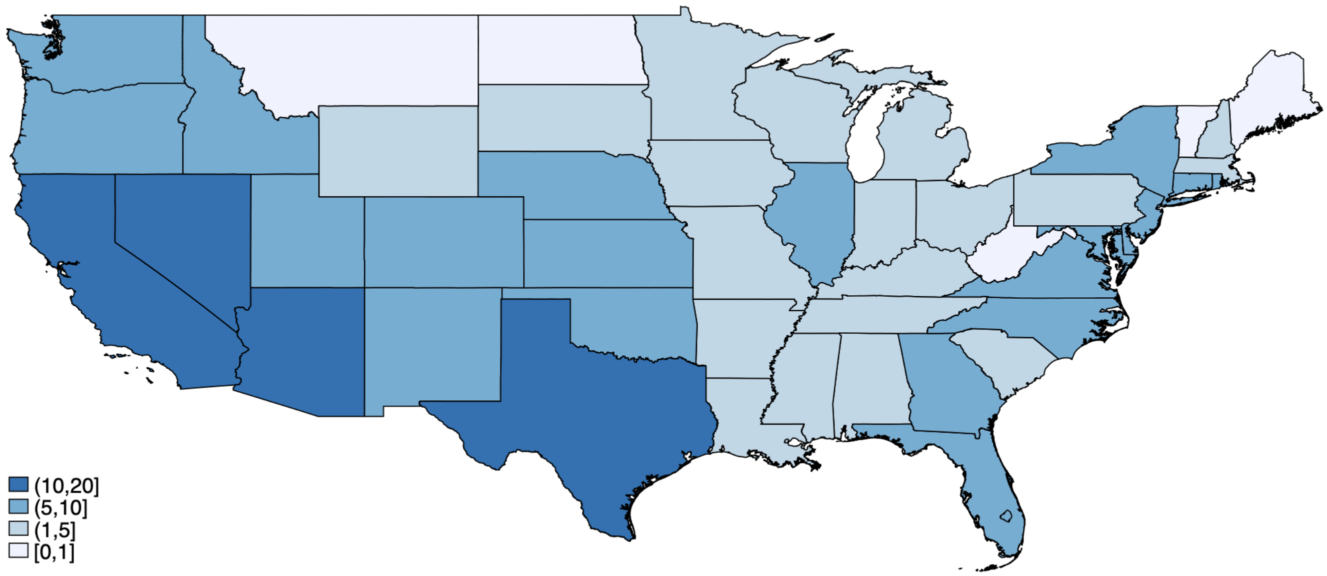
2.7 Figures and Tables

Figure 2.1: Estimated Undocumented Population, 1995-2016



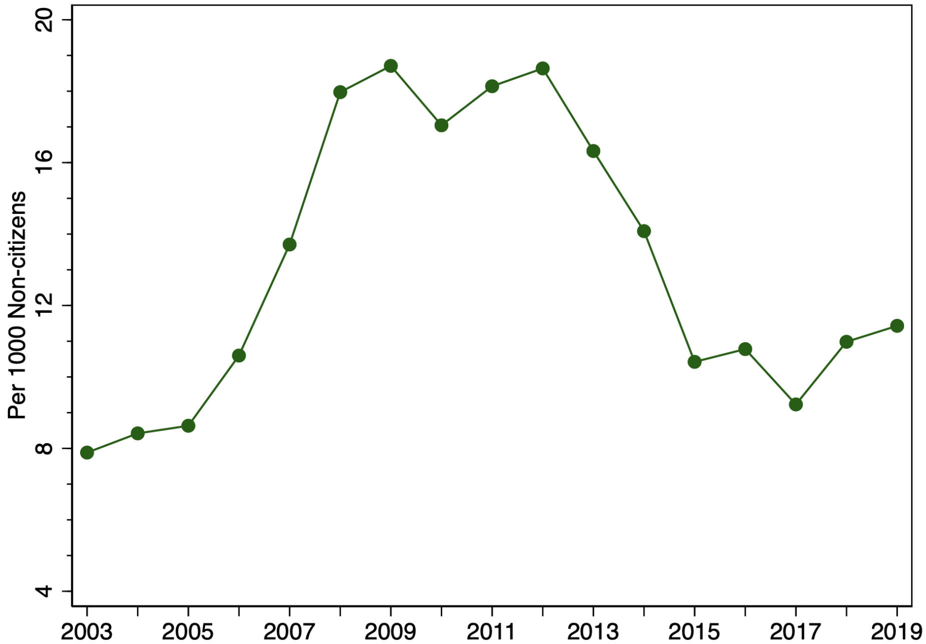
Note: Estimated undocumented population by time spent in the United States since entering the country.
Source: Author calculations using Passel and Cohn [1].

Figure 2.2: Percent of Children with Legal Status in Mixed-Status Household, 2010-2014



Note: This map shows the percent of children living with undocumented household members in each state. Children with legal status includes US born citizens or naturalized citizens. A mixed-status household contains people with legal residence in the US and those who are undocumented.
Source: Mathema [2].

Figure 2.3: Internal Deportations, 2003-2019



Note: Internal deportations, excluding border activity, are aggregated to a national level and reported as the annual deportations per 1000 non-citizens. Source: Author calculation using the Current Population Survey and the Transaction Records Access Clearinghouse, 2003-2019.

Table 2.1: Summary Statistics, Jan 2009 - Dec 2018

	US Born Mothers		Foreign Born Mothers		US Born
	All	Non-Latina	Latina	Select	Latina
Immigration Enforcement					
Deportations per 1000 non-citizens	14.20	12.48	18.44	21.13	20.30
	(35.84)	(38.01)	(27.55)	(27.00)	(25.82)
Enforcement Index	1.572	1.595	1.464	1.452	1.490
	(0.874)	(0.902)	(0.780)	(0.780)	(0.758)
Birth Outcomes					
WIC participation	0.584	0.548	0.723	0.727	0.712
	(0.234)	(0.243)	(0.172)	(0.180)	(0.173)
Medicaid payment	0.551	0.522	0.619	0.619	0.655
	(0.240)	(0.250)	(0.226)	(0.235)	(0.184)
Any Prenatal Care	0.983	0.984	0.968	0.964	0.979
	(0.0179)	(0.0168)	(0.0443)	(0.0488)	(0.0255)
Prompt Prenatal Care	0.936	0.939	0.888	0.881	0.925
	(0.0386)	(0.0369)	(0.0869)	(0.0931)	(0.0533)
Birth Weight	3214.6	3216.8	3226.5	3222.7	3206.8
	(80.37)	(87.65)	(85.92)	(97.96)	(84.10)
Healthy Birth Weight	0.913	0.910	0.929	0.930	0.921
	(0.0238)	(0.0260)	(0.0388)	(0.0468)	(0.0345)
Demographics					
Married	0.321	0.343	0.366	0.384	0.240
	(0.467)	(0.475)	(0.482)	(0.486)	(0.427)
Less than High School	0.186	0.165	0.401	0.429	0.261
	(0.389)	(0.371)	(0.490)	(0.495)	(0.439)
High School Graduate	0.371	0.369	0.365	0.364	0.378
	(0.483)	(0.483)	(0.482)	(0.481)	(0.485)
Some college, less than BA	0.443	0.465	0.234	0.207	0.361
	(0.497)	(0.499)	(0.423)	(0.405)	(0.480)
No. Births	5524353	4313512	778587	559804	1210841

Note: Summary statistics are derived from the National Center for Health Statistics Natality Files from 2009 to 2018. Immigration enforcement measures are constructed using data from the Transactional Records Access Clearing House, the Current Population Survey, Gelatt et al. [48], and the Surveillance, Epidemilogy, and End Results Program. Deportations per 1000 non-citizen is the cumulative deportation rate during a pregnancy. The enforcement index represents the number of immigration policies in a state at the time of childbirth. I limit the sample to first births to mothers with less than a 4-year college degree. In columns 1 and 2, I provide statistics for US born mothers and non-Latina US born mothers, my comparison groups. Columns 3 and 4 provide statistics for the two alternative groups of mothers used to proxy undocumented mothers, foreign born Latina mothers and foreign born mothers from select countries. The select countries are the most frequent countries of origin among undocumented people in the United States: Mexico, El Salvador, Guatemala, India, and Honduras. The last column reports summary statistics for US born Latina mothers.

Table 2.2: Effect of Deportations on Safety Net Program Participation

	(1)	(2)	(3)	(4)
Panel A: Prenatal WIC Participation				
log(Deportations)	0.0027	0.0021	0.0014	0.0013
	(0.008)	(0.009)	(0.008)	(0.008)
Undoc. Mother	0.1293***	0.1300***	0.1500***	0.1553***
	(0.006)	(0.006)	(0.008)	(0.010)
Interaction	-0.0135***	-0.0133***	0.0007	0.0009
	(0.003)	(0.003)	(0.004)	(0.004)
Mean	0.601	0.597	0.574	0.568
R-sqr	0.853	0.860	0.883	0.893
Panel B: Medicaid Payment				
log(Deportations)	-0.0012	-0.0008	0.0022	0.0031
	(0.007)	(0.007)	(0.007)	(0.007)
Undoc. Mother	0.1274***	0.1404***	0.1379***	0.1510***
	(0.021)	(0.024)	(0.022)	(0.025)
Interaction	-0.0499***	-0.0538***	-0.0406***	-0.0442***
	(0.012)	(0.012)	(0.012)	(0.013)
Mean	0.560	0.557	0.537	0.533
R-sqr	0.812	0.828	0.816	0.835
US born Mothers	All	All	Non-Latina	Non-Latina
Undoc. Mothers Proxy	Latina	Select	Latina	Select
Number of Births	6302940	6084157	5092099	4873316

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. Each column estimates the model using a different proxy of foreign born undocumented mothers and US born comparison group. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table 2.3: Effect of Deportations on Prenatal Care

	(1)	(2)	(3)	(4)
Panel A: Prompt Prenatal Care (Before 3rd Trimester)				
log(Deportations)	-0.0007	-0.0009	-0.0002	-0.0003
	(0.002)	(0.002)	(0.002)	(0.002)
Undoc. Mother	-0.0245***	-0.0252***	-0.0273***	-0.0283***
	(0.003)	(0.004)	(0.003)	(0.004)
Interaction	-0.0057***	-0.0068***	-0.0045**	-0.0055***
	(0.002)	(0.002)	(0.002)	(0.002)
Mean	0.930	0.931	0.931	0.932
R-sqr	0.625	0.639	0.605	0.620
Panel B: Any Prenatal Care				
log(Deportations)	0.0003	0.0001	0.0008	0.0005
	(0.001)	(0.001)	(0.001)	(0.001)
Undoc. Mother	-0.0004	-0.0019	-0.0008	-0.0025
	(0.002)	(0.002)	(0.002)	(0.002)
Interaction	-0.0046***	-0.0048***	-0.0043***	-0.0045***
	(0.001)	(0.001)	(0.001)	(0.001)
Mean	0.981	0.981	0.982	0.974
R-sqr	0.577	0.575	0.542	0.536
US born Mothers	US born	US born	Non-Latina	Non-Latina
Undoc. Mothers Proxy	Latina	Select	Latina	Select
Number of Births	6302940	6084157	5092099	4873316

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. Each column estimates the model using a different proxy of foreign born undocumented mothers and US born comparison group. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table 2.4: Effect of Deportations on Birth Weight

	(1)	(2)	(3)	(4)
Panel A: Healthy Birth Weight (Over 2500 grams)				
log(Deportations)	-0.0000	-0.0002	-0.0007	-0.0010
	(0.001)	(0.001)	(0.001)	(0.001)
Undoc. Mother	0.0162***	0.0140***	0.0179***	0.0164***
	(0.001)	(0.001)	(0.001)	(0.001)
Interaction	-0.0001	0.0005	0.0011***	0.0015***
	(0.000)	(0.000)	(0.000)	(0.000)
Mean	0.915	0.914	0.913	0.913
R-sqr	0.329	0.318	0.336	0.324
Panel B: Birth Weight				
log(Deportations)	-1.2831	-1.9391	-2.0328	-2.8654
	(2.982)	(3.092)	(2.847)	(2.978)
Undoc. Mother	10.5518***	-2.4026	9.6581***	-2.0815
	(3.215)	(3.700)	(3.215)	(3.733)
Interaction	5.2216***	8.0961***	5.9361***	8.3054***
	(1.440)	(1.595)	(1.405)	(1.550)
Mean	3216.089	3215.360	3218.296	3217.485
R-sqr	0.632	0.632	0.626	0.627
US born Mothers	All	All	Non-Latina	Non-Latina
Undoc. Mothers Proxy	Latina	Select	Latina	Select
Number of Births	6302940	6084157	5092099	4873316

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. Each column estimates the model using a different proxy of foreign born undocumented mothers and US born comparison group. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table 2.5: Effect of Deportations on Birth Rate

	(1)	(2)	(3)	(4)
log(Deportations)	-0.1949	-0.0079	-0.2263	0.0715
	(0.650)	(0.547)	(0.749)	(0.593)
Undoc. Mother	10.4201***	9.0747***	10.8157***	9.6298***
	(1.133)	(1.162)	(1.094)	(1.143)
Interaction	-0.8926*	-1.3963***	-0.7554*	-1.2653***
	(0.459)	(0.444)	(0.453)	(0.434)
US born Mothers	All	All	Non-Latina	Non-Latina
Undoc. Mothers Proxy	Latina	Select	Latina	Select
Mean	18.403	17.780	18.419	17.663
R-sqr	0.263	0.260	0.260	0.250
Number of Births	20845344	20026607	17321282	16502545

Note: Standard errors in parentheses are clustered by state-year. The birth rate for each group of mothers by marital status and educational attainment is estimated using all births to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files and the Current Population Survey from 2009 to 2018. The birth rate is weighted by the relevant number of births. Each column estimates the model using a different proxy of foreign born undocumented mothers and US born comparison group. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table 2.6: Effect of Deportations Among US Latina Mothers

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt	Any	Healthy	Average	Quarterly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(Deportations)	-0.0022	-0.0031	-0.0024	-0.0001	-0.0002	-1.2529	-0.1692
	(0.009)	(0.007)	(0.002)	(0.001)	(0.001)	(2.845)	(0.443)
Latina Mother	0.1174***	0.1004***	-0.0077***	0.0013**	0.0071***	-10.4568***	8.1698***
	(0.007)	(0.005)	(0.001)	(0.001)	(0.001)	(2.624)	(0.536)
Interaction	0.0093***	-0.0013	0.0037***	0.0001	0.0013***	3.6610***	-1.3724***
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.986)	(0.215)
Mean	0.584	0.551	0.936	0.983	0.913	3214.621	17.294
R-sqr	0.902	0.915	0.657	0.591	0.318	0.641	0.389
Number of Births	5524353	5524353	5524353	5524353	5524353	5524353	17348319

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of US born non-Latina mothers to US born Latina mothers. Columns 1-6 limit the sample to first time mothers, but Column 7 uses all births to estimate the quarterly birth rate for each subgroup. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table 2.7: Effect of Alternative Deportations Measures Among Likely Undocumented Mothers

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt	Any	Healthy	Average	Quarterly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Deportations per Non-Citizen in 2000							
log(Deportations)	0.0021	-0.0002	-0.0003	-0.0003	-0.0000	-2.3908	-0.1487
	(0.007)	(0.007)	(0.002)	(0.001)	(0.001)	(2.905)	(0.532)
Undoc. Mother	0.1671***	0.1786***	-0.0252***	-0.0006	0.0156***	-4.6927	9.7478***
	(0.011)	(0.026)	(0.004)	(0.002)	(0.001)	(3.733)	(1.156)
Interaction	-0.0043	-0.0515***	-0.0063***	-0.0048***	0.0017***	8.5695***	-1.1815***
	(0.003)	(0.009)	(0.002)	(0.001)	(0.000)	(1.226)	(0.379)
Mean	0.568	0.533	0.932	0.974	0.087	3217.485	17.663
R-sqr	0.893	0.840	0.621	0.539	0.324	0.627	0.250
Number of Births	4873316	4873316	4873316	1770645	4873316	4873316	16502545
Panel B: Policy Enforcement Index							
Enforcement Index	0.0051**	0.0072**	0.0011	0.0008	-0.0002	-1.3582	0.6364**
	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(1.116)	(0.249)
Undoc. Mother	0.2109***	0.1721***	-0.0116	0.0022	0.0168***	4.3524	10.6075***
	(0.017)	(0.051)	(0.008)	(0.005)	(0.002)	(6.897)	(1.377)
Interaction	-0.0340***	-0.0771***	-0.0186***	-0.0094***	0.0017*	7.6657**	-2.2086***
	(0.007)	(0.023)	(0.004)	(0.002)	(0.001)	(3.520)	(0.652)
Mean	0.577	0.535	0.933	0.974	0.087	3219.050	17.646
R-sqr	0.895	0.832	0.632	0.552	0.323	0.628	0.247
Number of Births	4416852	4416852	4416852	4416852	4416852	4416852	14853721

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of non-Latina US born mothers and foreign born mother from select countries. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. Panel A uses data from 2009-2018; Panel B only goes through 2017.

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

Table 2.8: Sub-Sample Analysis Among Likely Undocumented Mothers

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt	Any	Healthy	Average	Quarterly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 10 States with Highest Concentration of Undocumented People							
log(Deportations)	-0.0032	0.0010	-0.0004	-0.0010	-0.0030***	-6.8306	0.1814
	(0.011)	(0.013)	(0.003)	(0.002)	(0.001)	(5.113)	(0.727)
Undoc. Mother	0.2626***	0.3210***	0.0140*	0.0169***	0.0116***	-20.2647**	7.4423***
	(0.026)	(0.073)	(0.008)	(0.005)	(0.003)	(8.851)	(2.428)
Interaction	-0.0321***	-0.0972***	-0.0178***	-0.0100***	0.0027***	12.9743***	-0.3045
	(0.007)	(0.020)	(0.002)	(0.002)	(0.001)	(2.614)	(0.729)
Mean	0.583	0.543	0.917	0.973	0.086	3212.996	17.745
R-sqr	0.878	0.767	0.710	0.662	0.421	0.647	0.284
Number of Births	1786854	1786854	1786854	1786854	1786854	1786854	6093535
Panel B: 10 States with Lowest Concentration of Undocumented People							
log(Deportations)	-0.0062	0.0244***	0.0076*	0.0016	-0.0007	4.1529	0.3263
	(0.008)	(0.008)	(0.005)	(0.002)	(0.003)	(5.834)	(0.810)
Undoc. Mother	0.0239*	-0.2976***	-0.0663***	-0.0167***	0.0185***	5.1788	9.4375***
	(0.014)	(0.026)	(0.007)	(0.004)	(0.004)	(8.262)	(3.539)
Interaction	-0.0341	0.0141	-0.0384**	-0.0085	0.0005	-18.2286	3.0019
	(0.022)	(0.050)	(0.015)	(0.011)	(0.007)	(15.434)	(8.482)
Mean	0.569	0.533	0.945	0.989	0.090	3214.780	17.070
R-sqr	0.925	0.922	0.481	0.254	0.249	0.612	0.292
Number of Births	501150	501150	501150	501150	501150	501150	1637708

Standard errors in parentheses are clustered by state-year. The sample is restricted to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of non-Latina US born mothers and foreign born mother from select countries. Columns 1-6 limit the sample to first time mothers, but Column 7 uses all births to estimate the quarterly birth rate for each subgroup. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. Panel A includes the following states: Nevada, Texas, California, New Jersey, Maryland, Arizona, Florida, Massachusetts, the District of Columbia, and Georgia. Panel B includes the following states: Vermont, West Virginia, Montana, Maine, Mississippi, North Dakota, South Dakota, New Hampshire, and Ohio.

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

Table 2.9: Effect of Alternative Deportations Measures Among US Latina Mothers

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt	Any	Healthy	Average	Quarterly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Deportations per Non-Citizen in 2000							
log(Deportations)	-0.0006	-0.0055	-0.0027*	-0.0008	0.0005	-0.8958	-0.3916
	(0.007)	(0.007)	(0.001)	(0.001)	(0.001)	(2.662)	(0.383)
Latina Mother	0.1212***	0.1033***	-0.0072***	0.0013**	0.0070***	-11.0844***	8.1645***
	(0.008)	(0.005)	(0.001)	(0.001)	(0.001)	(2.545)	(0.537)
Interaction	0.0066***	-0.0024**	0.0031***	0.0001	0.0012***	3.5623***	-1.2232***
	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.885)	(0.195)
Mean	0.584	0.551	0.936	0.983	0.087	3214.621	17.294
R-sqr	0.902	0.915	0.656	0.591	0.318	0.641	0.389
Number of Births	5524353	5524353	5524353	5524353	5524353	5524353	17348319
Panel B: Policy Enforcement Index							
Enforcement Index	0.0032	0.0048	-0.0008	0.0005	-0.0006	-2.3754**	0.5224**
	(0.003)	(0.003)	(0.001)	(0.000)	(0.001)	(1.099)	(0.203)
Latina Mother	0.1525***	0.1175***	0.0029	0.0022**	0.0063***	-13.0581***	6.6001***
	(0.009)	(0.006)	(0.002)	(0.001)	(0.001)	(3.575)	(0.947)
Interaction	-0.0114***	-0.0135***	-0.0020*	-0.0004	0.0020***	5.7994***	-0.7825
	(0.004)	(0.003)	(0.001)	(0.001)	(0.001)	(2.220)	(0.541)
Mean	0.592	0.553	0.936	0.983	0.087	3215.873	17.236
R-sqr	0.902	0.916	0.661	0.604	0.318	0.646	0.389
Number of Births	4986378	4986378	4986378	4986378	4986378	4986378	15537103

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of US born non-Latina mothers to US born Latina mothers. Columns 1-6 limit the sample to first time mothers, but Column 7 uses all births to estimate the quarterly birth rate for each subgroup. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. Panel A uses data from 2009 - 2018; Panel B only goes through 2017.

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

Table 2.10: Sub-Sample Analysis Among US Born Latinas

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt	Any	Healthy	Average	Quarterly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 10 States with Highest Concentration of Undocumented People							
log(Deportations)	-0.0112	-0.0165	-0.0053*	-0.0026	0.0002	-2.0498	-0.3263
	(0.012)	(0.011)	(0.003)	(0.002)	(0.001)	(3.958)	(0.568)
Latina Mother	0.1623***	0.0952***	0.0022	0.0035***	0.0097***	-6.5750	8.4873***
	(0.021)	(0.013)	(0.002)	(0.001)	(0.002)	(4.553)	(0.898)
Interaction	-0.0020	0.0010	0.0013	-0.0003	0.0004	2.1956	-1.4434***
	(0.005)	(0.003)	(0.001)	(0.001)	(0.001)	(1.517)	(0.303)
Mean	0.611	0.576	0.927	0.976	0.085	3210.100	17.240
R-sqr	0.889	0.907	0.760	0.708	0.423	0.684	0.553
Number of Births	2268592	2268592	2268592	2268592	2268592	2268592	6770136
Panel B: 10 States with Lowest Concentration of Undocumented People							
log(Deportations)	-0.0081	0.0236***	0.0072	0.0014	-0.0016	3.4023	-0.1082
	(0.008)	(0.009)	(0.004)	(0.002)	(0.003)	(5.580)	(0.782)
Latina Mother	0.0672***	0.0749***	-0.0088***	0.0016	0.0080**	-4.2393	6.0446***
	(0.008)	(0.008)	(0.002)	(0.001)	(0.003)	(7.951)	(1.287)
Interaction	0.0059	0.0041	-0.0067*	-0.0025	-0.0031	-1.7535	3.9167**
	(0.010)	(0.008)	(0.004)	(0.002)	(0.004)	(7.849)	(1.491)
Mean	0.571	0.540	0.946	0.989	0.090	3214.873	16.999
R-sqr	0.923	0.932	0.456	0.269	0.225	0.579	0.331
Number of Births	509478	509478	509478	509478	509478	509478	1649441

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of US born non-Latina mothers to US born Latina mothers. Columns 1-6 limit the sample to first time mothers, but Column 7 uses all births to estimate the quarterly birth rate for each subgroup. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. Panel A includes the following states: Nevada, Texas, California, New Jersey, Maryland, Arizona, Florida, Massachusetts, the District of Columbia, and Georgia. Panel B includes the following states: Vermont, West Virginia, Montana, Maine, Mississippi, North Dakota, South Dakota, New Hampshire, and Ohio.

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

Chapter 3: Immigrating Out of the Nest? The Effect of DACA

3.1 Introduction

As discussed in chapter 2, the United States has a large undocumented population. One particular group of undocumented immigrants that has garnered public support are those who were brought to the United States as children. In a survey conducted in 2020, approximately three-quarters of Americans were in favor of providing undocumented immigrants brought as children with a permanent legal status [55]. Although permanent legal status has yet to be granted to Dreamers, undocumented childhood arrivals, President Obama began the Deferred Action for Childhood Arrivals program using an executive order on June 15, 2012. DACA recipients were granted temporary relief from deportation and were given a social security number and a work permit.

In this chapter, I use data from the American Community Survey (ACS) 2005-2019 to study the effect of DACA on living arrangements and housing of people between the ages of 22 and 35. I consider the following five mutually exclusive living arrangements: living with a parent, living with a partner but no parents, living alone, living with non-familial roommates, and living with other family members. I also examine measures of housing quality such as the household size, the number of bedrooms, ownership status of the house, and the cost of housing.

First I use a regression discontinuity model to estimate the effect of DACA. I limit the sample to non-citizen immigrants who meet all of the eligibility criteria except for their date of birth. I exploit the discontinuity in eligibility due to the fact that a person must be under 31 on June 2012 to qualify for DACA. I find DACA increase the probability of living with a parent in a rented house by 1.9 percentage points and lowers the likelihood of living with other family members. I find evidence that the benefit of DACA may have been dampened after the 2017 attempt to eliminate the program. When using data prior to 2017, I find DACA causes a $73 increase in the amount paid for housing among renters. When I use the full sample period through 2019, I do not find statistically significant changes in housing characteristics. As a second estimation strategy, I compare eligible and ineligible immigrants before and after the announcement of DACA in a difference-in-differences model. This strategy closely follows the methods of Pope [9] and Kuka et al. [11]. When using the difference-in-differences model, I am unable to establish credible estimates because the outcome for eligible and ineligible groups do not have parallel trends. Overall my findings suggest DACA does not increase beneficiaries' incidence of moving out of their family's house.

Prior studies on DACA have documented the improved labor market outcomes, rise in human capital accumulation, mental health benefits caused by DACA [4, 9, 10, 11, 43]. More closely related to this chapter, Christopher [56] finds a rise in home ownership among household with a DACA recipient. I build on and differentiate myself from the prior literature in two key ways. First and foremost, the focus of this chapter is the effect of DACA on living arrangements. This is a topic that has yet to be explore and there are multiple way deferred action could be altering the feasible options in terms of housing. I discuss this in more details in Section 2. My second contribution is the use of a regression discontinuity to estimate the effect of DACA. With this strategy, I estimate the causal effect of DACA using marginally eligible individuals. Unlike the difference-in-differences method, I do not have to rely finding an appropriate comparison group, which is challenging for the outcomes I am interested in studying.

The remainder of the chapter proceeds as follows. Section 2 provides institutional details regarding DACA, and I discuss the manner in which DACA influences living arrangements and housing. In Section 3 and Section 4, I discuss the data and empirical model. In Section 5, I present the findings and I conclude in Section 6.

3.2 Deferred Action for Childhood Arrivals

3.2.1 Institutional Background

On June 15, 2012 the Obama Administration signed an executive order enacting DACA. Prior to the executive order, there had been a building momentum pressuring Congress to pass an immigration reform targeting Dreamers since 2001. The term Dreamers originated from the legislation introduced to Congress in 2001, the Development, Relief, and Education for Alien Minors (DREAM) Act. The bill would have provided a pathway to citizenship and permanent relief from deportation for undocumented immigrants who arrived to the United States as children. This group of immigrants continues to be referred to as Dreamers since the bill's name aptly captures the barriers imposed by their legal status. In 2010, a version of the DREAM Act failed to pass by five votes, the closest historical margin, despite bipartisan support [57].

Unlike the DREAM Act, DACA does not provide a pathway to citizenship. Instead eligible undocumented immigrants would be granted deferred action, temporary relief from deportation. DACA beneficiaries receive a social security number and a legal work permit. With their social security number, people with DACA may also obtain a license from their home state. The deferred action status must be renewed every two years and the application costs $\$495$ [58]. Figure 3.1 and Appendix Figure B.1 depict the number of approved applications and the overall number of applications received over time. As of the 2019 fiscal year, 822,000 people had been granted deferred action at some point. In general, we should consider DACA recipients as having a very limited pathway to citizenship. Of the people who have ever received DACA, 80% continue to actively have deferred action status, 11% failed to renew their status, and the remaining 9% have become legal permanent residents as of the fiscal year 2019. The majority of those who became legal permanent residents did so through a citizen spouse or family member, and a smaller group of people received asylum or another special immigration status [59]. Even if a DACA recipient were to marry a US citizen, they can be barred from being in the United States for up to 10 years depending the age at which they applied for DACA and the number of years that they resided in the US without legal status [60].

An undocumented immigrant is eligible for DACA if they meet the following six conditions. First they must be under the age of 31 on June 15, 2012. Second, they must have remained continuously in the United States for five year when the policy was announced. In other words, they must have entered the United States before 2007. Thirdly, they must have arrived in the United States before turning 16 years old. I will be relying on these three eligibility requirements in my various identification strategies.

Additionally a person must be at least 15 years old when applying for deferred action. They must be currently enrolled in high school, completed high school, earned a GED, or have been honorably discharged from the military. Lastly, they cannot have any felony charges and less than three misdemeanors [58].

The Trump Administration attempted to remove DACA in 2017. Instead of granting a two-year deferred status, the period of reprieve was lowered to one year. This meant recipients had to apply for renewal more frequently and pay the application fee more often. The Trump Administration stopped accepting new DACA applications after September 2017. The decline in initial approval applications after 2017 can be seen in Figure 3.1. There was an attempt to halt application renewals as well but this effort was unsuccessful. Although DACA renewals did not cease, take-up of the program due to increasing mistrust may have fallen during this time. After court challenges, the United States Customs and Immigration Services began accepting first time DACA applications again on December 7, 2020. In January 2021, the Biden Administration reinstated DACA to its original state, increasing the deferred action status back to two years [61].

3.2.2 Conceptual Framework and Related Literature

To motivate the analysis, I consider how DACA may influence the decision to change living arrangements. I focus on DACA recipients between the ages of 22 and 35. At this point, these individuals have likely completed their schooling choices and are making decisions regarding their living arrangements. Housing is directly linked to economic well-being. Deciding to live with one's parents or other family members as an adult is likely driven by economic security. Pope [9] finds DACA lowers unemployment among DACA eligible individuals and increases income for those in the bottom of the income distribution. Amuedo-Dorantes and Antman [12] finds DACA reduces childhood poverty by 9 percentage points. The work permit and drivers license that DACA beneficiaries receive grant them greater mobility and potentially improve their job market matches. Their increased economic well-being may allow them to live without their parents, instead residing independently or with other roommates. Additionally their new legal status may cause them to relocate to access a better job now available to them.

Additionally life changes such as marriage and children may drive both living arrangements and the decision to rent compare to purchase a home. DACA lowers the future uncertainty by ensuring the beneficiary will not be deported. With the lowered uncertainty and improved economic well-being, DACA recipients may be perceived as more attractive partners in the dating and marriage-market. The formation of new relationships could cause a DACA recipient to cohabit with a partner.

Another benefit of DACA linked to home-ownership is the improved access to the credit market. The Migration Policy Institute estimates approximately 30% of undocumented immigrants own a home [62]. Prior to DACA, an undocumented immigrant could use an Individual Taxpayer Identification Number, ITIN, to acquire a private loan. Private loans tend to have higher interest rate than Federal Housing Administration (FHA) loans. With DACA, individuals could begin to build a credit score and they temporarily had access to lower interest, FHA loans. The Federal Housing Administration requires ”lawful residency” for a person to qualify for a loan but citizenship is not required. During the Obama Administration, the FHA was accepting deferred action as a ”lawful” resident status. In 2018, the Housing and Urban Development Department changed their internal policy and no longer allowed DACA recipients to obtain a FHA loan [63]. In 2021, the Biden Administration officially declared deferred action to be a lawful resident status.

In addition to the potential access to better loans, an undocumented person may be more willing to invest in such a large asset as a house given the lowered risk. The decision to purchase a home is significant and costly, and prior to DACA there was a possibility that the investment in a home would be lost if they were deported. Rugh and Hall [64] finds counties with agreements between local police officers and ICE have a 0.4 pp higher foreclosure rate among Hispanic households. The fear of losing a large investment such as a house is not trivial. Although DACA is not a permanent legal status, there is a lower chance of losing the capital investment. The lowered risk may influence DACA beneficiaries and their families decision to purchase as opposed to renting a home.

Little is known about the effects of DACA on living arrangement and home ownership. The established improvements in educational attainment, economic conditions, and access to credit caused by DACA may be important channels that further influence housing decisions. Christopher [56] examines the effect of immigration policies on home ownership using two different policies, DACA and the banking rule change of 2003 that allowed a person to use an ITIN to obtain a loan. First he uses the ACS between 2008-2018 at a household level to compare homes with immigrants with legal residence and homes with undocumented individuals. This household analysis finds some evidence of increased home ownership. In the second analysis, he uses mortgage application information and examines the effect the Department of Treasury allowing an ITIN to be used for loan applications. Allowing an ITIN to be used to obtain a loan increases Hispanic loan applications by 12%. I contribute to this sparse literature by analyzing the broader decision of living arrangements and housing quality for DACA beneficiaries.

3.3 Data

I use data from the American Community Survey (ACS) from 2005 through 2019 to examine living arrangement decisions by DACA eligible and ineligible immigrants [65]. My unit of analysis is the individual. Since the ACS is a repeated cross-section, I cannot observe whether a DACA recipient previously resided in the household or whether an immediate family member living outside the household has DACA. Given the potential movement of DACA recipients in and out of the household, I decide the individual is the appropriate unit of analysis. I use the IPUMS relationship variable to construct the following five mutually exclusive living arrangements: living with parents, living with a partner, living alone, living with roommates, and other living arrangements. A person is considered living with parents if either a mother or father figure is present. Under this living arrangement, other family members, including a spouse or partner, may be present in the household. A person is considered to be living with a partner if their spouse or unmarried partner is in the household and they do not live with their parent.[1] A person lives alone if no other person resides in the household. A person living with exclusively non-families members is categorized as living with roommates. Lastly, other living arrangements includes people living with at least one family member, such as a sibling, an uncle, an aunt, or a grandparent.

[1] I use the spouse location variable created by IPUMS to identify a partner. All married spouses will be accurately identified. In order to locate an unmarried partner, either the individual or his/her partner must be the head of the household.

I additionally examine the ownership status of the house and other household characteristics. I can identify whether the home in which an individual is residing is rented or owned by a person in the household. Unfortunately, this information is collected at the household level, therefore I cannot determine who is the primary homeowner. Given this data limitation, a person could be renting a room in a home that is owned. I would determine such a person to live in an owned home. I have information on the monthly rent and the home value conditional on living in a rented home and living in an owned home respectively. I also have the number of bedroom in the home, top-coded at five bedrooms. Using the number of people living in a household, I calculate the number of bedrooms per person. Overall these variables are meant to capture a measure of housing quality.

I limit the sample to immigrants with at least a high school diploma or GED. By limiting my sample in this manner, I ensure the whole sample automatically satisfies two of the DACA eligibility requirements. I determine an immigrant's DACA eligibility using information on their reported year of immigration, their citizenship status, and their current age. I refer to non-citizens as immigrants who do not have US born parents and are not naturalized citizens. From the non-citizen category, I further exclude immigrants who arrived to the United States before 1982 and Cubans who arrived prior to 2017. Undocumented immigrants who arrived before 1982 likely gained legal status during the Immigration Reform and Control Act of 1986. Before 2017, Cubans arriving to the United States were eligible for residency due to the Cuban Adjustment Act of 1966 [66]. I cannot observe the legal status of non-citizens, but I exclude these additional groups to increase my likelihood of observing undocumented immigrants. Passel and Cohn [1] estimates undocumented people account for approximately 42% of non-citizens in the

ACS. In the analysis, I primarily compare DACA eligible non-citizens with ineligible non-citizens. In certain models, I also use naturalized immigrants as a comparison group. When discussing each model in the empirical methods, I elaborate on the parameters defining the DACA eligible and ineligible groups.

3.4 Empirical Method

3.4.1 Regression Discontinuity

I first estimate the effect of DACA using a regression discontinuity model, which finds the average treatment effect based on the marginally eligible person. Using the ACS 2012-2019, I limit my sample to non-citizen with at least a high school education who arrived in the United States before 2007 and prior to the age of 16. The regression discontinuity exploits the cutoff in eligibility based on a person's age in June 2012. The ACS provides birth dates at a quarterly frequency. I estimate the following equation:

$$\begin{aligned} Y_{ist} = \alpha + \beta_1 After_i + \beta_2 g(CutoffQuarters_i) + \\ \beta_3 After_i \times g(CutoffQuarters_i) + \epsilon_{ist}, \end{aligned} \tag{3.1}$$

where Y_{ist} is the outcome of interest for person i living in state s in year t. $After_i$ is an indicator for being born after 1981Q2 and $g(CutoffQuarters_i)$ is a flexible birth date function centered at 1981Q2. I select the bandwidth based on a data driven procedure that chooses a mean-square-error optimal bandwidth using a local polynomial of degree one with triangular kernel [67, 68]. The optimal bandwidths for each living arrangement and housing outcome of interest range between 10 and 18 quarters (2.5-4.5 years). I also estimate the model using a tighter, consistent bandwidth of 10 birth quarters (2.5 years). I cluster the standard errors at the birth quarter.

Table 3.1 presents the summary statistics for the two groups, those born before and after 1981Q2. Figure 3.2 depicts a histogram with the quarterly birth date of the sample. The DACA application requires proof of a person's birth date. Supporting documents could be a birth certificate or passport to validate the birth date. Manipulation in the running variable, the birth quarter, is unlikely given the amount of supporting documented required in the DACA application. I use Calonico [69]'s nonparametric density estimator to test for manipulation. I do not find evidence of manipulation.[2]

I additionally estimate Equation 3.1 ending the sample in 2016, instead of 2019. The attempted removal of DACA during the Trump Administration could have changed the response to the policy. Misinformation could have caused individuals to lose their deferred action status if they believed they would be unable to renew the status. Fear of sharing information with the government could have prevented people from renewing even if they were aware that renewal applications were not halted. I expect the estimated effect during the sub-sample ending in 2016 to be stronger. I expect efforts to eliminate the program to have dampened or even nullified any initial response by beneficiaries.

3.4.2 Difference-in-Differences

As an alternative empirical strategy, I first use a difference-in-differences model to estimate the effect of DACA on living arrangements. Unlike the regression discontinuity, this model will be incorporating more than one eligibility component. The estimating

[2] Using an optimal bandwidth of ± 23 birth quarters, the resulting t-statistic is 1.47.

equation is as follows:

$$
\begin{aligned}
Y_{ist} = \alpha_1 Post_t + \alpha_2 Eligible_{it} + \alpha_3 Post_t \times Eligible_{it} \\
+\mathbf{X}_{ist}\beta + \delta_s + \gamma_t + \delta_s \times t + \epsilon_{ist},
\end{aligned}
\tag{3.2}
$$

where Y_{ist} is the living arrangement or housing outcome of interest. $Post_t$ takes the value 0 for the years 2005-2011 and the value 1 for the years 2012-2019. $Eligibility$ is an indicator for being DACA eligible. The parameter of interest is α_3, the coefficient on the interaction between DACA eligibility and when the policy was enacted. The vector X contains the following demographic characteristics: dummies for the year of arrival, current age, Hispanic ethnicity, some college, and 4-year college degree. I do not control for an individual's marital status or income. While these two variables are associated with living arrangements and home ownership, income and marital status may be changing in response to DACA. Other than educational attainment, I only control for time invariant characteristics. The model also includes state and year fixed effects and a linear state time trend. Standard errors are clustered at the state-year level.

I estimate Equation 3.2 using three different control and treatment groups. In the first model, I restrict the sample to non-citizens between the ages of 22 and 35. Following Pope [9], I define eligibility as if DACA was enacted in the prior survey year. This means that a non-citizen would be eligible if they arrived in the United States at least 5-years ago, arrived before they turned 16 years old, and they must be under the age of 31 in June of the prior survey year. These three conditions drive the variation in eligibility in the sample. A limitation of this strategy is that the accuracy of the eligibility variable declines over time. The requirements for DACA had two criteria that were tied to the 2012 announcement. An undocumented person had to be under the age of 31 when the policy was announced and must have resided continuously in the United States without leaving the country for 5-years. Another way to phrase these two criteria would be that a person had to be born after 1981q2 and must have arrived in the United States before 2007. Given the way eligibility is defined in the first difference-in-difference model, I accurately capture DACA eligible non-citizens in 2012, but my accuracy declines as the survey year increases. For instance, a non-citizen who arrived in 2008 but met all other eligibility criteria would appear to be eligible if they were surveyed in the 2013 survey or later. I refer to this model as the ”standard model” because it follows the prior literature’s manner of estimating the effect of DACA.

In my second model, I address the changing accuracy of the eligibility definition by estimating the effect of DACA on a cohort. I limit the sample to non-citizen who would be between the ages of 22 and 35 in 2012. As in model 1, a non-citizen will be consider DACA-eligible if they arrived to the United States prior to the age of 16, but now they must also have arrived before 2007 and have been under the age of 31 in 2012. These latter two criteria are directly tied to the policy. The variation in eligibility continues to come from three channels: age of arrival, the year of arrival, and the date of birth of the non-citizen. I observe this cohort’s living arrangements and housing choices as they age. In the earliest year of my sample, this cohort is between the ages of 15[3] and 28, and in the final year they are between the ages of 29 and 42. I present the baseline summary

[3]Since I limit the sample of non-citizen to those with at least a high school degree or GED, the youngest age of my sample is 18 years old even though the cohort would be younger.

characteristics of the samples used in the first and second difference-in-differences model in Table 3.2.

The third way I estimate Equation 3.1 follows methods by Kuka et al. [11]. I compare naturalized citizens to DACA eligible non-citizens. I restrict the sample to immigrants who arrived before the age of 16 and before 2007. I limit the sample to individuals who are between the ages of 22 and 31. This is a smaller age range than what is used in the other two models. Given these restrictions, all non-citizens in the sample would be DACA eligible and all naturalized citizen would be the control group. If I were to include people over the age of 31, I would not be able to define eligibility solely by using citizenship. Notice that this strategy does not rely on variation in the eligibility requirements. I no longer am comparing undocumented immigrants that are eligible for DACA to those who are ineligible. Now I am comparing undocumented immigrants who are eligible for DACA to immigrants with legal status. Table B.1 reports the baseline summary statistics for this treated and comparison group.

In all three of these treated and comparison group pairs, the identifying assumption is that absent the introduction of DACA, the trends in living arrangements and housing choices would be unaltered. I estimate the dynamic difference-in-differences model to first ensure that there is no systematic differences in the trends. In all of these samples, the point estimates I obtain will be attenuated for the following reason. My measure of non-citizen includes both undocumented people and non-citizen immigrants with legal status. If I could observe a person's true undocumented status, the results could be interpreted as intent-to-treat estimates. If I assume 42% of non-citizens in the survey are undocumented, then the intent-to-treat estimates could be 2.4 times larger than the difference-in-difference point estimates. In order to obtain a treatment on the treated effect, I must scale up the intent to treat estimate using the DACA take-up rate. According to Bruno [59], 822,000 undocumented youth were granted DACA and the Passel and Cohn [1] estimates the DACA eligible population to be 1,159,000. This implies a 70% DACA take-up rate. Thus the treatment on the treated effects could be 1.4 times larger than the intent-to-treat estimates.

3.5 Results

3.5.1 Regression Discontinuity Model

I first examine the effect of DACA on individual's living arrangements. DACA recipients can legally work, obtain a driver's license, and begin to build a credit score. Prior literature has already established that DACA has improved educational attainment and employment outcomes. Overall I expect the benefits of DACA to provide greater financial independence. Therefore I expect DACA eligible individuals to be less likely to live with their parents or in other family living arrangements after the policy is enacted. I present the results on living arrangements in Table 3.3, where Panel A reports the estimates using the optimal bandwidth and Panel B reports the estimates with a consistent bandwidth. Each column represents one of five mutually exclusive living arrangements: living with a parent, living with a partner regardless of marital status but with no parent, living alone, living with non-familial roommates, and living with family not described in a previous category.

I find that DACA does not effect any living arrangement category, expect for the likelihood of having other living arrangements. This living arrangement means the person is living without their parents or a partner but is living with some other family. Other living arrangement could include residing with extended family or a sibling. DACA lowers the likelihood of having an other living arrangement by 2.4 percentage points, which is about a 20% reduction. Figure 3.3 supports this finding graphically.[4] Appendix Figure B.2 show the continuity at the cutoff for the other living arrangement outcomes, supporting a null effect. The point estimates remain the same after controlling for age of arrival, educational attainment, Hispanic ethnicity, male, and fixed effect for the year and state (see Appendix Table B.2). Overall the regression discontinuity results indicate that DACA did not greatly change cohabitation but did lower the likelihood of living with some other extended family.

Next I combine the ownership status of the home and living arrangement to better understand housing decisions. I present the results in Table 3.4. In Columns 1-2, I examine the effect of DACA on living with a parent and living in a rented or owned. In Columns 3-4, I examine the effect of DACA on living without a parent and living in a rented or owned. I cannot identify the primary homeowner, only the ownership status of the home at the household level. I find DACA causes a 1.9 percentage point rise in living with a parent and living in a rented household. I do not find a statistically significant effect for any other outcome. Appendix Figure B.3 and Figure B.4 graphically illustrate the results. As seen in Appendix Table B.3 results are robust to the inclusion of covariates. Overall these results suggest DACA causes eligible individuals to move from living with

[4]Regression discontinuity figures were generated using the rdplot Stata command, using a global polynomial fit of degree 4 with a uniform kernel. [69].

other family members to living with their parents.

In an attempt to capture the quality of the housing situation, I examine the size of the household, the number of bedroom, and the value of the place. As seen in Table 3.5, I find a null effect on the number of people living in a household. I find some evidence that the number of bedrooms falls, but the bedrooms per person does not change. I do not find a statistically significant effect on the likelihood of living in a rented home as opposed to an owned home. Lastly, I estimate the effect of DACA on the cost of rent among renters and the value of the home among home owners. DACA does not appear to impact the home rental amount but does increase the value of a home by over $25,000. Appendix Figures B.5 -B.6 show the graphical representation of the estimated effects. In general, the measures of housing quality are not altered very much by DACA.

3.5.1.1 Sub-Sample Analysis: Excluding 2017 Onward

I re-estimate the effect of DACA using the American Community Survey from 2012 to 2016, instead of ending the sample in 2019. I report the results in Appendix Tables B.5-Tables B.7. Although I lose one-third of the sample, I now avoid any potential dampening effect from the attempted phase-out of DACA that occurred in 2017. I now find that DACA reduced the likelihood of having an other living arrangement by 2.2 percentage points and increased the likelihood of living with a parent by 3.2 percentage points. DACA increases the combination of living with a parent in a rented home by 3.6 percentage points. The monthly rental cost among people in a rented home rises by $73 dollars. The results on living arrangements are consistent with the findings when using the full sample period. When excluding any dampening or reversal caused by the Trump Administration, I am finding improvements in housing as reflected by a higher rental price. Assuming a higher rental price captures unit's and neighborhood's quality, DACA appears to have improved living conditions in the initial time period.

3.5.2 Difference-in-Difference Model

As an alternative estimation strategy in Table 3.6, I present the difference-in-difference results on living arrangements comparing eligible and ineligible non-citizens. Panel A reports results from the model defining DACA eligibility as if the policy was implemented in the year prior to the survey; I refer to this as the standard approach. Panel B reports results using the cohort approach with a sample of non-citizens ages 22-35 in 2012. Using the standard approach, I find DACA causes a 1 percentage point decline in the likelihood of living with a parent among eligible individuals. DACA causes an increase of 0.36 and 0.72 percentage points of living with roommates and in other family living arrangements. The cohort model results in consistent estimates in terms of the direction of the effect, but the point estimates are greater in magnitude and highly statistically significant. I find DACA causes an 11.4 percentage point decline in the likelihood of living with a parent and a 10.8 percentage point increase in living with a roommate. I now also find evidence of a decline in living with a spouse and a rise in living alone and living with other family members. Appendix Figure B.7 depict the pre-trends and dynamic effects. All of the results are driven by the pre-trend, thus results cannot be interpreted as casual effects. In the Appendix Table B.8, I present the living arrangement results comparing DACA-eligible immigrants to immigrants with legal status. I find a null effect on changes in living arrangements.

Next I examine the effect of DACA on the combination of living with a parent and living in a rented as opposed to an owned home. As seen in Table 3.7, I find that DACA increases the renting with a parent by 3.4 percentage points and lowers living in an owned home with parents by 4.5 percentage points when using the standard method. The cohort method results in a 11 percentage point decline in living with a parent in an owned home and a 10.2 percentage point increase in living without a parent in a rented home. When comparing DACA-eligible immigrants to immigrants with legal status, I find similar estimates to the standard method results. Appendix Figures **??**-**??** continue to demonstrate a violation to the parallel trends assumption of the model.

Lastly, I present the effect of DACA on housing characteristics using the difference-in-differences model in Table 3.8. I find DACA causes a decline the household size and the number of bedrooms in a home. DACA beneficiaries are more likely to rent, 4.1-9.8 percentage points. The amount paid for housing among renters falls by $60-$68, and the home value among owners also declines. As seen in Appendix Table B.8, similar results are found when comparing DACA-eligible immigrants to immigrants with legal status, but the estimate are not causal. Appendix Figures B.10-B.11 show the point estimates are likely cause by existing differences in the trend of the outcomes. While the difference-in-differences has been a popular method used to establish causal estimates of the effect of DACA, for the outcomes I am studying this method is not credible.

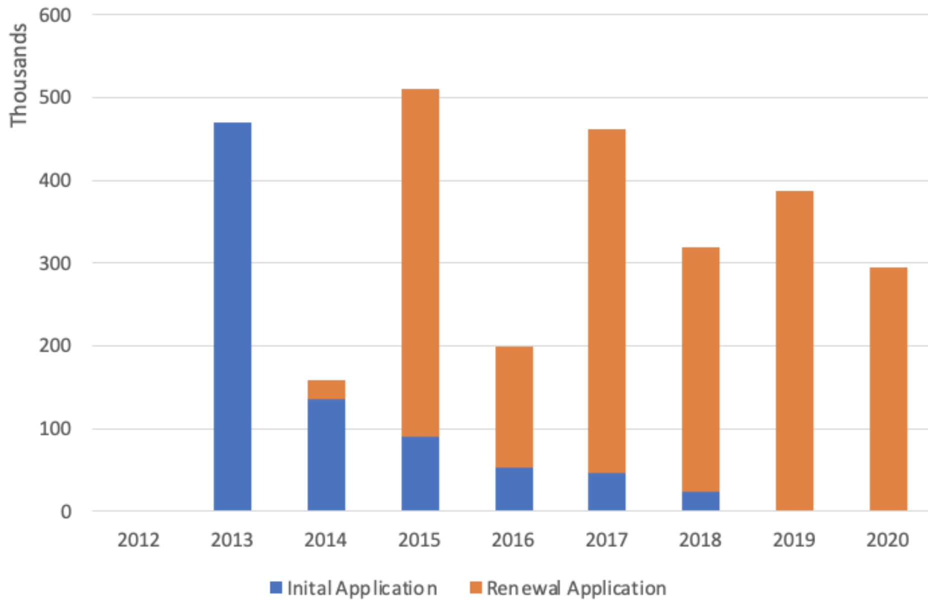
3.6 Conclusion

In this chapter, I study the effects of DACA on living arrangements and housing characteristics using the American Community Survey 2012-2019. I implement a regression discontinuity model comparing marginally eligible immigrants who arrived in the United States before 2007 and before turning 16 years old. The strategy exploits the DACA eligibility based on the person's date of birth, being under 31 on June 2012. I find DACA lowers the incidence of living with other family by 2.4 percentage points. I define living with other family as cohabiting with at least one other family member but cohabiting without a parent or partner. I find DACA causes a 1.9 percentage point rise in living with a parent in a rented home. I do not find significant change in the likelihood of establishing an independent household nor do I find an effect on other measures housing quality.

When I limit the sample to 2012-2016, I find consistent living arrangement results. I now also find that DACA causes a $73 increase in the amount paid for housing among renters. I interpret the rise in rent to reflect a higher quality housing unit or living in a better neighborhood. I found a null effect for this outcome when using the full sample. This suggest the attempted removal of DACA during the Trump Administration may have reversed improvements in housing conditions.

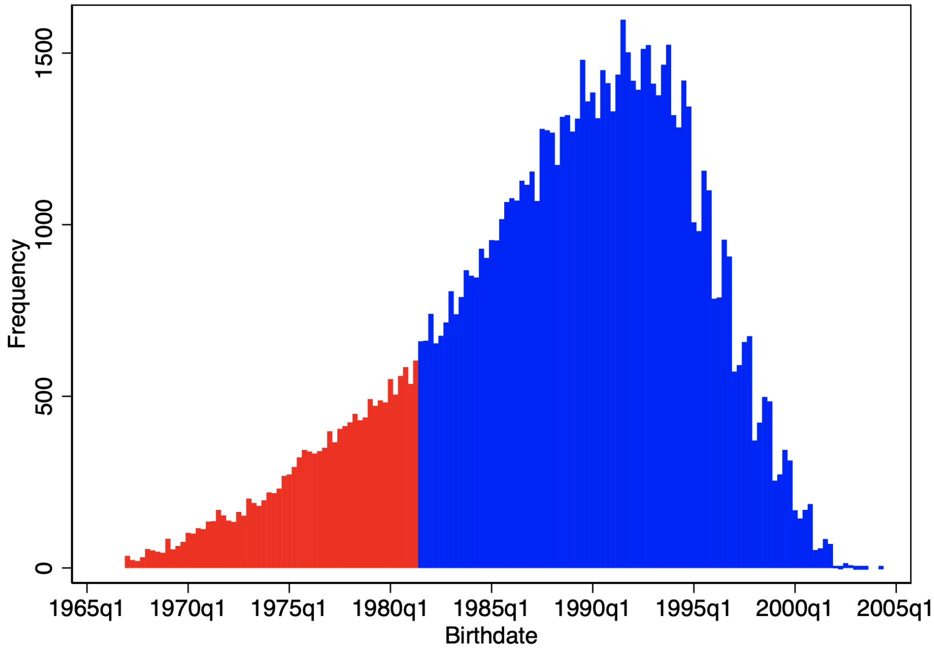
3.7 Figures and Tables

Figure 3.1: Approved DACA applications, 2012-2020



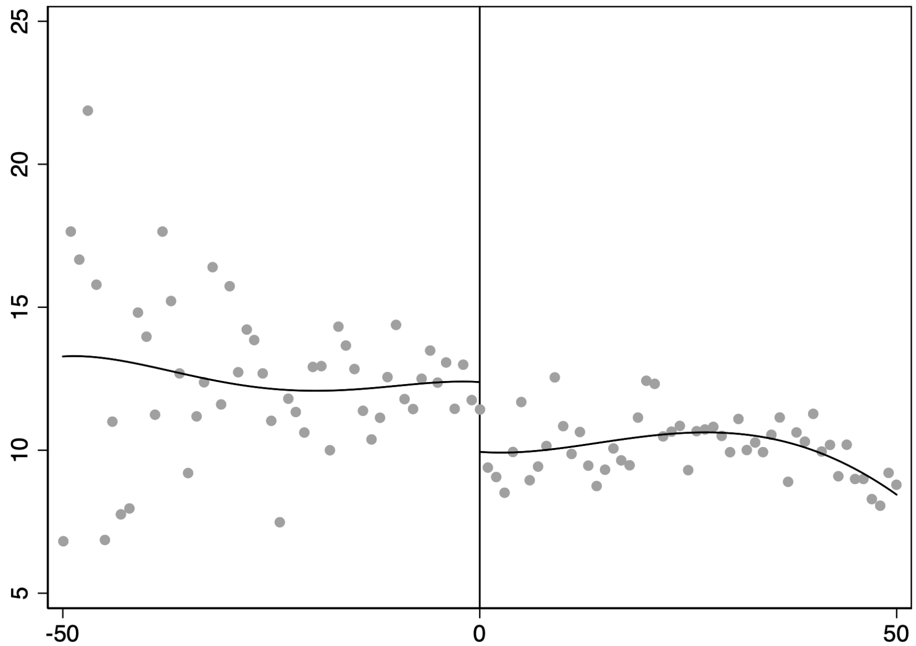
Note: Number of approved initial and renewal applications for DACA in each fiscal year.
Source: Bruno [59]

Figure 3.2: Date of Birth Among Non-citizens



Note: I restrict the sample to non-citizen immigrants who arrived in the United States by 2007 and were under the age of 16 when they entered the United States. To be eligible for DACA, an immigrant must be under the age of 31 on June 2012. This implies DACA eligible individuals were born after 1981Q2.
Source: American Community Survey, 2012 - 2019.

Figure 3.3: Regression Discontinuity: Effect of DACA on Other Living Arrangement



Note: The dependent variable is having an other living arrangement. This means that the person lives with family members, but they do not live with their parents or a partner. The running variable is birth date centered around 1981Q2. The regression discontinuity is estimated and plotted using a 4-degree polynomial, global fit with a uniform kernel.
Source: American Community Survey, 2012 - 2019.

Table 3.1: Summary Statistics of Non-citizen Immigrants, 2012 - 2019

	Born 1981Q2 or Before	Born 1981Q3 or After
Economic and Demographic Characteristics		
Working	0.781	0.714
Personal income	36264.8	20548.4
Household income	82343.2	72521.8
High school degree	0.489	0.469
Some college	0.325	0.391
4-year college degree	0.186	0.140
Hispanic	0.633	0.706
Married	0.575	0.299
Male	0.526	0.506
Metropolitan area	0.943	0.925
Living Arrangements and Housing Characteristics		
Living in Rented Home	49.07	59.47
Household size	4.235	4.312
Number of bedrooms	2.817	2.671
Living with Parents	14.83	45.34
Living with a Partner	60.85	31.97
Living Alone	5.493	3.830
Living with Roommates	6.500	9.420
Other Living Arrangement	12.32	9.443
Monthly Rent Among Renters	1124.6	1080.3
Home Value Among Owners	305690.9	253431.0
Observations	15092	77006

Note: I derive summary statistics among DACA eligible and ineligible non-citizen immigrants who arrived to the United States before 2007 and prior to the age of 16 using the 2012-2019 American Community Survey. DACA eligibility is defined by the person's birth date. To be eligible, a person must be under the age of 31 of June 2012. This sample is used in the regression discontinuity analysis.

Table 3.2: Baseline Statistics of Non-Citizens, 2005 - 2011

	Non-citizens Ages 22-35		Non-citizens Ages 22-35 in 2012	
	DACA Eligible	DACA Ineligible	DACA Eligible	DACA Ineligible
Economic and Demographic Characteristics				
Working	0.720	0.683	0.626	0.658
Personal income	20256.2	27290.7	13141.1	21986.9
Household income	65527.3	70516.5	62521.4	66061.4
High school degree	0.471	0.328	0.502	0.364
Some college	0.372	0.221	0.407	0.255
4-year college degree	0.158	0.451	0.0903	0.381
Hispanic	0.634	0.390	0.631	0.414
Married	0.347	0.613	0.202	0.503
Male	0.513	0.483	0.510	0.486
Metropolitan area	0.929	0.928	0.923	0.924
Living Arrangements				
Renting	52.81	63.90	52.83	68.03
Household size	4.267	3.509	4.521	3.583
Living with Parents	38.63	8.291	57.16	13.45
Living with a Partner	36.78	61.77	20.92	51.28
Living Alone	4.705	8.303	3.088	8.134
Living with Roommates	9.691	13.46	9.523	17.44
Other Living Arrangement	10.20	8.180	9.309	9.693
Observations	38068	206000	45332	153332

Note: I derive summary statistics among DACA eligible and ineligible non-citizens using the 2005-2011 American Community Survey. The first two columns define eligibility as if the policy was implemented the year prior to the survey. The latter columns restrict the sample to individuals ages 22-35 in 2012. Eligibility is based on the actual requirements, arrival in the US prior to 2007 and arriving prior to age 16. These samples are used in the difference-in-differences analysis.

Table 3.3: Effect of DACA on Living Arrangements

	Parents (1)	Partner (2)	Alone (3)	Roommates (4)	Other (5)
Panel A: Optimal Bandwidth Selection					
Born After 1981Q2	1.22	0.71	0.18	-0.68	-2.39***
	(1.02)	(1.23)	(0.76)	(0.80)	(0.61)
Obs.	18265	14300	13019	19625	20945
Bandwidth	±14	±11	±10	±15	±16
Panel B: Consistent Bandwidth					
Born After 1981Q2	2.33	0.74	0.18	-0.04	-3.22***
	(1.36)	(1.27)	(0.76)	(0.93)	(0.62)
Obs.	13019	13019	13019	13019	13019
Bandwidth	± 10	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2019 American Community Survey. I examine the effect of DACA on the following five mutually exclusive living arrangements: living with at lease one parent, living with a partner but without a parent, living alone, living with only non-family members, and other implies living with a different family member in a situation not previously described. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Panel A presents results using the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. Panel B reports results with a consistent bandwidth of 10 birth quarters.

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

Table 3.4: Effect of DACA on Housing Choices

	Living with a Parent		Living with no Parent	
	Rented Home	Owned Home	Rented Home	Owned Home
	(1)	(2)	(3)	(4)
Panel A: Optimal Bandwidth Selection				
Born After 1981Q2	1.94**	-0.25	-1.90	-0.52
	(0.83)	(0.89)	(1.90)	(1.05)
Obs.	14300	20945	14300	18265
Bandwidth	± 11	± 16	± 11	± 14
Panel B: Consistent Bandwidth				
Born After 1981Q2	2.12**	0.21	-2.31	-0.02
	(0.94)	(1.12)	(2.03)	(1.49)
Obs.	13019	13019	13019	13019
Bandwidth	$\pm$ 10	$\pm$ 10	$\pm$ 10	$\pm$ 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2019 American Community Survey. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Columns 1 and 2 present results on the effect of living with at least one parent in a rented and owned home. Columns 3 and 4 present results on the effect of living without any parents in a rented and owned home. The model in Panel A uses the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. The model in Panel B uses a consistent bandwidth of 10 birth quarters.

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

Table 3.5: Effect of DACA on Housing Characteristics

	Household size	Number of Bedrooms	Bedrooms per person	Renting	Monthly Rent	Home Value
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Optimal Bandwidth Selection						
Born After 1981Q2	-0.07	-0.09**	-0.00	0.64	30.82	24061.83*
	(0.06)	(0.04)	(0.02)	(1.39)	(23.42)	(11899.61)
Obs.	19625	19625	13019	19625	9103	10934
Bandwidth	±15	±15	±10	±15	±13	±18
Panel B: Consistent Bandwidth						
Born After 1981Q2	-0.04	-0.08	-0.00	-0.19	33.95	25068.97
	(0.08)	(0.05)	(0.02)	(2.01)	(26.15)	(19579.50)
Obs.	13019	13019	13019	13019	6989	6030
Bandwidth	± 10	± 10	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2019 American Community Survey. I further limit the sample in Column 5 to people living in a rented home and to people living in an owned home in Column 6. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Panel A presents results using the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. Panel B reports results with a consistent bandwidth of 10 birth quarters.

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

Table 3.6: Effect of DACA on Living Arrangements using Diff-in-Diff Model

	Parents (1)	Partner (2)	Alone (3)	Roommates (4)	Other (5)
Panel A: Standard Method with 22-35 year olds					
DACA × Post	-1.01***	-0.19	0.12	0.36*	0.72***
	(0.33)	(0.57)	(0.25)	(0.21)	(0.20)
DACA	12.07***	6.01***	-2.34***	-12.25***	-3.50***
	(0.49)	(0.64)	(0.23)	(0.72)	(0.32)
R-sqr	0.171	0.160	0.042	0.081	0.024
Obs	520973	520973	520973	520973	520973
Panel B: Cohort Method with 22-35 year olds in 2012					
DACA × Post	-11.38***	-4.11***	0.54**	10.78***	4.16***
	(0.91)	(0.62)	(0.22)	(0.82)	(0.27)
DACA	14.57***	5.77***	-1.51***	-13.99***	-4.83***
	(0.99)	(0.38)	(0.24)	(0.87)	(0.34)
R-sqr	0.240	0.190	0.040	0.069	0.026
Obs	493821	493821	493821	493821	493821

Note: Standard errors in parenthesis are cluster by state-year. I restrict the sample to non-citizen immigrants from the 2005-2019 American Community Survey. See Table 3.3 for a general description on the living arrangement definitions. Post is an indicator taking the value one for years 2012-2019. The specification includes controls for age, age of entry in the United States, educational attainment, Hispanic ethnicity and sex. The model includes year fixed effects, state fixed effects, and a state linear time trend. Panel A further restricts the sample to individuals ages 22-35, and DACA eligibility determined as if the policy was implemented in the year prior to the survey. Panel B restricts the sample to individuals who would be between 22 and 25 years old in June 2012. DACA eligibility is determined using the policies actual restriction, arrival in the US prior to 2007 and arriving prior to age 16.

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

Table 3.7: Effect of DACA on Housing Choices using Diff-in-Diff Model

	Living with a Parent		Living with no Parent	
	Rented Home	Owned Home	Rented Home	Owned Home
	(1)	(2)	(3)	(4)
Panel A: Standard Method with 22-35 year olds				
Daca $\times$ Post	3.44***	-4.45***	0.68	0.33
	(0.84)	(0.69)	(0.70)	(0.74)
DACA	2.85***	9.22***	-18.13***	6.06***
	(0.72)	(0.68)	(0.92)	(0.65)
R-sqr	0.083	0.095	0.084	0.083
Obs	520973	520973	520973	520973
Panel B: Cohort Method with 18-35 year olds in 2012				
Daca $\times$ Post	-0.40	-10.97***	10.16***	1.22**
	(0.60)	(0.79)	(0.64)	(0.52)
DACA	3.91***	10.66***	-16.11***	1.54***
	(0.72)	(1.31)	(1.09)	(0.46)
R-sqr	0.105	0.140	0.096	0.083
Obs	493821	493821	493821	493821

Note: Standard errors in parenthesis are cluster by state-year. I restrict the sample to non-citizen immigrants from the 2005-2019 American Community Survey. Post is an indicator taking the value one for years 2012-2019. The specification includes controls for age, age of entry in the United States, educational attainment, Hispanic ethnicity and sex. The model includes year fixed effects, state fixed effects, and a state linear time trend. Panel A further restricts the sample to individuals ages 22-35, and DACA eligibility determined as if the policy was implemented in the year prior to the survey. Panel B restricts the sample to individuals who would be between 22 and 25 years old in June 2012. DACA eligibility is determined using the policies actual restriction, arrival in the US prior to 2007 and arriving prior to age 16.

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

Table 3.8: Effect of DACA on Housing Characteristics using Diff-in-Diff Model

	Household size (1)	Number of Bedrooms (2)	Bedrooms per person (3)	Renting (4)	Monthly Rent (5)	Home Value (6)
Panel A: Standard Method with 22-35 year olds						
Daca $\times$ Post	-0.11***	-0.04**	0.01**	4.12***	-68.33**	-45117.49***
	(0.02)	(0.02)	(0.00)	(1.37)	(26.67)	(12890.62)
DACA	0.25***	0.25***	0.03***	-15.28***	69.75***	29520.31**
	(0.03)	(0.02)	(0.01)	(1.20)	(18.81)	(12199.31)
R-sqr	0.173	0.062	0.081	0.077	0.346	0.225
Obs	520973	520973	520973	520973	340251	180722
Panel B: Cohort Method with 18-35 year olds in 2012						
Daca $\times$ Post	-0.17***	-0.12***	-0.00	9.75***	-60.43***	-18041.57
	(0.02)	(0.01)	(0.00)	(0.69)	(22.03)	(13795.37)
DACA	0.21***	0.19***	0.02***	-12.20***	12.27*	-9446.80**
	(0.02)	(0.02)	(0.00)	(1.49)	(6.73)	(4244.21)
R-sqr	0.173	0.069	0.082	0.073	0.352	0.231
Obs	493821	493821	493821	493821	317593	176228

Note: Standard errors in parenthesis are cluster by state-year. I restrict the sample to non-citizen immigrants from the 2005-2019 American Community Survey. I further limit the sample in Column 5 to people living in a rented home and to people living in an owned home in Column 6. Post is an indicator taking the value one for years 2012-2019. The specification includes controls for age, age of entry in the United States, educational attainment, Hispanic ethnicity and sex. The model includes year fixed effects, state fixed effects, and a state linear time trend. Panel A further restricts the sample to individuals ages 22-35, and DACA eligibility determined as if the policy was implemented in the year prior to the survey. Panel B restricts the sample to individuals who would be between 22 and 25 years old in June 2012. DACA eligibility is determined using the policies actual restriction, arrival in the US prior to 2007 and arriving prior to age 16.

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

Chapter 4: Revisiting the Effect of NALs on Opioid Mortality

4.1 Introduction

Opioid addiction has caused detrimental consequences in the United States. In 2000, the opioid-related death rate was less than 5 per 100,000 people. By 2019, the opioid death rate increased four fold. Figure 4.1 shows that non-heroin substance abuse primarily drove the surge in opioid related mortality, but heroin mortality also rose steadily after 2010. The direct healthcare cost of treating opioid overdoses alone was $\$12.2$ billion in 2016 [70]. Considering the loss of productivity, the loss of lives, and other societal costs, the toll of the opioid crisis is far greater.

In response to the growing opioid crisis, states have implemented opioid mortality prevention policies meant to increase the availability of Naloxone, an opioid overdose reversal drug. In 2001, New Mexico became the first state to adopt a Naloxone Access Law (NAL). New Mexico granted healthcare professionals immunity from liability for prescribing Naloxone and enabled them to prescribe Naloxone to third parties. Typically, a prescription must only be written for the intended user. A third-party prescription would allow a person to obtain Naloxone with the intent of assisting another person experiencing an overdose. Other types of NALs include possession liability and standing orders. Possession liability provides immunity to people found with Naloxone without a prescription. A standing order would ensure emergency responders and trained community members could carry Naloxone at all times and easily refill their prescription. In addition to different NALs, a Good Samaritan Law (GSL) aims to reduce opioid mortality by encouraging bystanders to report an overdose to emergency responders. Typically GSLs provide bystanders with some level of protection from criminal liability for unlawful consumption or possession of alcohol and drugs if they contact first responders [71, 72]. As shown in Figure 4.2, by 2017 all fifty states had enacted a version of a NAL and by 2019 forty-six states had an accompanying GSL.

Although the purpose of these policies is to increase the availability of Naloxone in order to reduce opioid mortality, the theoretically implications are unclear. These policies increase access to Naloxone and encourage reporting of an overdose to emergency responders. Thus we may expect NALs and GSLs to lower opioid related mortality. Alternatively, lowering barriers to an overdose reversal could lead to moral hazard behavior. Individuals might increase their use of opiates because they know that they can rely on an overdose reversal if necessary. The current literature on opioid mortality and NALs has mixed results. In an attempt to understand the conflicting findings, I focus on two studies, both implementing a difference-in-differences estimation framework that exploit variation in the adoption of NAL and GSLs within a state across time. Rees et al. [14] found a reduction of 9% to 11% in opioid related deaths from a NAL and an insignificant effect from a GSL using the sample period 1999-2014. On the other hand, Doleac and Mukherjee [15] found NALs to have no effect on deaths using data from 2010-2015. Furthermore, they find NALs increases the opioid mortality rate by 14% in the Midwest. I assess differences in the studies design in terms of sample period and policy definitions to understand the source of the discrepancies.

I begin with the broader policy definition used by Rees et al. [14] that includes the adoption of any NAL. While I replicate the finding in Rees et al. [14] during their original sample period (1999-2104), I find a null effect of both policies once the sample is extended through 2019. Once I limit the sample to post-2010, I find evidence that NALs increase opioid mortality. The estimated treatment effect is highly sensitive to the sample period. This suggests heterogeneity in the treatment effect or the potential of a changing dynamic effect. Next, I adopt the policy definition used by Doleac and Mukherjee [15], which I refer to as supply side NALs. They focus only on NALs with a provision for a standing order or a third party prescription. These provision more directly address the supply of Naloxone availability. With this more limited policy definition, NALs no longer effect opioid mortality in either time period. From this exercise, I conclude the mixed results in the literature stem from both differences in the sample period and the definition of NAL.

Next I re-estimate the effect of opioid mortality prevention policies using methods developed by Callaway and Sant'Anna [16] and Sun and Abraham [17]. If the treatment effect is homogeneous across cohorts, there is no need to correct for the staggered policy adoption of NALs and GSLs. Given the sensitivity of the estimates to the sample period, a homogeneous treatment effect is unlikely. Additionally OxyContin was reformulated in 2010, making it more difficult to abuse synthetic opiates. The reformulation of OxyContin caused people to substitute from synthetic opiates to heroin [73]. The reformulation accelerated the opioid crisis and likely changed the effect of opioid mortality prevention policies over time. When correcting for the staggered policy adoption in the difference-in-differences model, I now find evidence that these prevention policies increased opioid mortality by 40%. I further contribute to the literature by examining measures of opioid activity. These measure of opioid use can inform us on the moral hazard of riskier opioid consumption. I proxy for opioid activity using pain medication misuse, admission for opioid substance abuse treatment, and self-reported measures of frequency of opioid use among individuals admitted for opioid abuse treatment. I find no statistically significant effect on these measures of opioid activity.

Although I have corrected for the variation in policy adoption, NALs and GSLs must be implemented exogenously without any anticipatory in order to interpret the results as causal estimate. I graph opioid mortality by policy adoption cohort. I find evidence that states do not pass opioid mortality prevention policies randomly. States experience a rise in mortality shortly prior to passing this legislation. The nonrandom adoption of these policies renders the causal interpretation from the difference-in-differences analysis inappropriate. I propose the following alternative estimation strategy to be considered in future work. While the policy adoption may be non-random at the state level, among county border-pairs the adoption may be exogenous at the county level. Thus causal estimates can be established comparing bordering counties that are on opposite sides of a state border, exploiting the variation in the state level adoption of the policies. The estimates will be attenuated due to spillovers across the county borderline, but the counties in states with the policies likely have greater Naloxone access, especially among first responders called during an overdose.

The remainder of this chapter proceeds as follows. Section 2 establishes a brief overview of Naloxone and Good Samaritan polices and findings from the prior literature in the United States. In Section 3, I discuss the data I compiled to examine opioid mortality and utilization. I present the empirical strategy and results in Section 4 and Section 5. In Section 6, I discuss the validity of a difference-in-differences framework in this setting. I propose an alternative empirical strategy to address the non-random adoption of opioid mortality prevention policies. In Section 7, I share conclusions from the analysis.

4.2 The Opioid Crisis in the United States

4.2.1 Naloxone Access Laws and Good Samaritan Laws

Across the county NALs and GSLs have become an increasingly common response to the opioid crisis. Prior to 2011 only five states had a type of NAL, but by 2017 all states had enacted at least one NAL. Naloxone legislation falls into four broad categories: standing order, prescriber immunity, possession liability, and third party prescription. A NAL standing order grants access to first responders and community organizations to obtain and dispense Naloxone. NAL prescriber immunity ensures that healthcare professionals and pharmacists are not liable for administering and prescribing Naloxone. NAL possession liability decriminalizes the possession of Naloxone without a prescription. NAL third party prescriptions allow both pharmacists and healthcare providers to give Naloxone to a person other than the intended user. The GSL complements the NALs by encouraging bystanders to call 911 and assist anyone experiencing an overdose. While the specific details and protection of the GSLs vary by state, they usually provides immunity to individuals who contact first responders even if they too were consuming illicit drugs or alcohol despite being under the legal drinking age [71, 72].

Although all states have some type of NAL, the policy remains controversial. When initially vetoing a Naloxone legislation in 2016, Governor Paul LePage of Maine stated, ”Naloxone does not truly save lives; it merely extends them until the next overdose, creating a situation where an addict has a heroin needle in one hand and a shot of Naloxone in the other produces a sense of normalcy and security around heroin use that serves only to perpetuate the cycle of addiction [74].” A 2019 report by the American Medical Association found some pharmacies do not provide Naloxone due to this fear that the drug enables further opioid abuse [75]. Not all medical professions are against NALs; national pharmacist chains such as CVS and Walgreens dispense Naloxone [75]. Proponents of Naloxone access believe the medication not only prevents an overdose death but even encourages pain medication users to be cautious [76]. The mixed opinions regarding NALs and GSLs in part reflect the theoretical ambiguity surrounding their effect due to potential for moral hazard.

4.2.2 Conceptual Framework

The theoretical implications of opioid mortality prevention policies are ambiguous. Greater access to Naloxone may prevent death by reversing an opioid overdose. The Good Samaritan Law may encourage bystanders to contact emergency services, who may then administer Naloxone. Through this mechanism, we would expect to see a reduction in mortality after states enacted NALs and GSLs. Yet these policies could also have unintended consequence due to moral hazard. The lowered risk of death may cause opioid users to consume greater quantities of opiates more frequently. Additionally, neither of these polices ensure longer term treatment for opioid addiction.

The current evidence about the effect of NALs and GSLs on opioid-related deaths is mixed. All of the previous papers used the same opioid mortality data and difference-in-differences identification strategy. With data from 1999-2014, Rees et al. [14] found a reduction of 9% to 11% in deaths from a NAL and an insignificant effect from a GSL. McClellan et al. [13] similarly concluded NALs and GSLs lower opioid deaths by 15% and 14% using data from 2000-2014. Examining a slightly longer period, 1999-2016, Atkins et al. [77] found a null effect on opioid related deaths from both policies. Similarly, Doleac and Mukherjee [15] found NALs to have no effect on deaths using data from 2010-2015. When examining heterogeneous effect among different geographic regions, they find NALs increase opioid mortality by 14% rise in the Midwest. Erfanian et al. [78] employs a spatial difference-in-differences model to account for spillovers in counties along the state border. Their analysis uses the sample period of 1999-2016, and they find NALs with certain immunity provisions increase opioid mortality by 11 per 100,000 population.

Given the mixed results, I examine the differences across the previous papers to determine if the mixed conclusions are caused by differences in the year span evaluated or the model estimated. I also redefine the policy treatment as either a type of NAL or a GSL since states largely enacted these policies together or shorter following each other. The concurrence of NALs and GSLs has not been addressed in prior research. My analysis of the prior papers and specification improvement by adjusting for the staggered policy adoption will allow us to better understand the true effect of opioid mortality prevention policies.

Few papers have examined the effects of NALs and GSLs beyond mortality. [13] examined self-reported non-medical opioid use from National Survey on Drug Use and Health from 2002–2014. They found NALs and GSLs do not change opioid use. Doleac and Mukherjee [15] used emergency room visits data from the Healthcare Cost Utilization Project spanning 2006-2015. In contrast to McClellan et al. [13], their estimates suggest NALs increase emergency room visits by 15%. I contribute to this sparse literature by examining novel proxies of opioid activity.

4.3 Data

4.3.1 Opioid Relevant Data

I collect information on state legislation related to Naloxone and Good Samaritan laws from 1999-2019. I rely on a reports published by the Legislative Analysis and Public Policy Association to compile legislative dates through 2019 [79]. Table 4.1 contains effective dates and legislative provisions of NALs and GSLs. In Appendix Table C.1, I list the NAL dates used by Rees et al. [14] and Doleac and Mukherjee [15]. The main difference in dates reflects the additional years of data I am using; the analysis by Rees et al. [14] ended in 2014 and by Doleac and Mukherjee [15] ended in 2015. Additional discrepancies reflect differences in categorization of the polices.

I obtain mortality information from the Center for Disease Control (CDC) Wonder tool [3]. I access the multiple-cause-of-death counts at a state-year level from 1999-2019. The CDC records the cause of death using an ICD-10 code, which is the international statistics classification of disease by the World Health Organization. The relevant ICD-10 codes are T40.0 (opium), T40.1 (heroin), T40.2 (other opioids), T40.3 (methadone), T40.4 (other synthetic narcotics), and T40.6 (unspecified narcotics). A cause of death listed broadly as ”opioid” would be classified as T40.6, thus excluding T40.6 would under-count total deaths. The multiple-cause-of-death records from the CDC are the standard measure for opioid mortality [13, 14, 15, 77]. I provide summary statistics for opioid mortality and other state characteristics in Table 4.2.

I use three novel proxies of opioid abuse and activity in order to understand the behavioral response of opioid users to the policies. The first proxy, pain medication misuse, in part reflects the non-heroin opioid availability and utilization. The National Survey of Drug Use and Health publishes state pain medication misuse per capita among people over the age of 18 [80]. Using the Interactive Tool, I obtain data on pain medication misuse from 2003 to 2018, excluding 2015. My next two measures of opioid abuse and activity come from the Treatment Episode Data (TED), 1999-2019 [81]. TED tracks all substance abuse admissions that occur at facilities receiving state or federal funding but does not identify unique individuals. A person admitted for treatment multiple times would be recorded separately for each instance. I calculate the number of admissions per state-year for opioid treatment, which acts as a proxy for opioid abuse. Lastly, I exploit the individual admission records to examine frequency of drug use reported by individuals. I restrict the sample to opioid related admission. The key outcomes of interest are daily opioid use and monthly opioid use. When admitted for treatment, individuals self-report the frequency of drug use as either daily use, monthly use, or infrequent. The self-reported measure of substance abuse may be under reported. Unless individuals systematically change their reporting accuracy after states enact NALs and GSLs, this should not bias my results. Table 4.3 contains the summary statistics for individuals admitted for opioid substance abuse treatment. The TED excludes certain states in specific years due to a lack of reporting. Assuming reporting compliance randomly occurs, this should not impact my analysis.[1]

4.3.2 Covariates

I collect the same covariates used by Rees et al. [14]. I first gather legislation information on two additional drug related policies, Medical Marijuana Laws (MMLs) and Prescription Drug Monitoring Programs (PDMPs). Sabia et al. [82] provide MMLs information from 1999-2013, and the Marijuana Policy Project supplement this source through 2019 [83]. Since medical marijuana is largely used for chronic pain, accessibility to this treatment could alter the use of pain medication. Pain medication use directly impacts opioid abuse, so it is important to control for MMLs [84]. Similarly, I must control for the implementation of Prescription Drug Monitoring Programs in states. PDMPs limit the over-prescription of pain medication containing opiates. If PDMPs coincided with NALs and GSLs, I would be unable to separate the effect PDMPs and the effect of NALs and GSLs on opioid-related deaths. Both MMLs and PDMPs have been associated with a reduction in opioid abuse [85, 86, 87]. I collect state PDMP information from the National Alliance for Model State Drug Laws [88] and Bao et al. [89].

I collect data related to economic conditions. I use the Current Population Survey

[1]Missing states include West Virginia 2000-2002; Arkansas 2004; Alaska and District of Columbia 2004-2006; Alaska 2006; Alabama 2007; District of Columbia and Mississippi 2009; South Carolina 2014-2015; Oregon 2015-2017; and Georgia 2016-2017.

via IPUMS to estimate the state college graduation rate [45]. I define a college graduate as a person over the age of 25 who completed four years of college. I use the Bureau of Economic Analysis Personal Income by State to obtain per capita income [90]. The Bureau of Labor Statistic provide the state unemployment rate [91]. The Department of Labor provide the minimum wage [92]. I include in the model college attainment, employment, and the minimum wage due to the positive association between opioid use and low economic conditions [93].

I collect additional state characteristics including the beer tax, the cigarette tax, the number of police officers, and state population. Beer and cigarettes taxes account for changes in opioid use caused by the cost of beer and cigarettes. This controls for the co-use or substitution between alcohol and tobacco, and opioids. The Tax Policy Center provide state excise tax rates for beer measured as dollars per gallon [94]. I obtain the cigarette tax measured as dollars per pack from the Tax Burden on Tobacco [95]. I use the Personal Consumption Expenditures: Chain-type Price Index to convert per capita income, the minimum wage, the beer tax, and the cigarette tax into 2014 dollars [96]. I construct a measure of police per 100,000 persons using the Justice Expenditure and Employment Extract Series (JEEPS), which was published through 2016 [97]. I use the Uniform Crime Reporting Program, which recorded police employee data for 2017 - 2019, to extend the police measure through 2019 [98]. Including the measure of police addresses the concern that changes in law enforcement could alter illicit opioid use and mortality. Lastly, the CDC Wonder tool provides annual state population [3]. All of these covariates account for time varying state characteristics that could impact opioid mortality.

4.4 Empirical Strategy

4.4.1 Effect on Opioid-Related Mortality

4.4.1.1 Replication: Standard Difference-in-Differences

I first reexamine the effect of NALs and GSLs on opioid-related mortality. I use the variation across states, over time to estimate the effect of Naloxone Access and Good Samaritan legislation on opioid mortality. I model the policies in a variety of ways using the following baseline equation:

$$y_{st} = \alpha + \sum_{p=1}^{P} \beta_p Policy_{pst} + \mathbf{X}_{st}\theta + \lambda_s + \gamma_t + \epsilon_{st}, \tag{4.1}$$

where y_{st} is the natural log of opioid related deaths per 100,000 persons in state s in year t. In the first specification, I model the NAL and GSL as two separate policies equal to the fraction of the year the policy was effective. This replicates the model by Rees et al. [14] with five additional years of data. I additionally estimate the model restricting the years to 2010-2019. This sample period coincides more closely with the time span used by Doleac and Mukherjee [15]. Prior to 2010, only three states had adopted a NAL. Additionally, the popular synthetic opioid, OxyContin was reformulated in 2010. The reformulation of the drug made it more challenging for people to abuse the drug. Despite the abuse deterrent technology, the OxyContin reformulation led people to switch from synthetic opioids to heroin [99]. The reformulation and the aftereffect likely altered the effectiveness of NALs. By restricting the sample to post-2010, I can better understand whether the estimated effect of the policies by Rees et al. [14] relies heavily on early adopting states, prior to the reformulation.

Both Rees et al. [14] and Doleac and Mukherjee [15] include NALs and GSLs separately in the model. Given the high collinearity between NALs and GSLs, in the second model I combine the NALs and GSLs into one policy variable that represents having either a NAL or GSL enacted. In a third variant, I impose a linear functional form of having an additional legislation. $Policy_{st}$ takes values 0 to 5 depending on the number of policies enacted - GSL, NAL possession, NAL prescriber liability, NAL standing order, and NAL third party prescription. In the final model, I estimate the effect of having low policy adoption compared to high policy adoption. Of the five possible NALs and GSL, I defined low adoption as one to three effective polices and high adoption as over three effective policies. Defining treatment in this way does not impose a linear function form of policy adoption while allowing the effect of the policies to vary depending on the level of policy adoption. For all of the model variations, the key identifying assumption of the difference-in-differences model requires that absent opioid mortality prevention policies, the trend in opioid-related deaths would be unaltered. Thus, no policy change impacting the opioid crisis should simultaneous occur with NALs and GSLs.

I include the same time varying controls as Rees et al. [14] in each model. I transform the following controls by the natural log: population, police per capita, beer tax, cigarette tax, college graduates, per capita income, the unemployment rate, and the minimum wage. I also include the presence of a MML and a PDMP as the fraction of the year the policy was effective. To address serial correlation in the error term, I cluster the standard errors by state. The observations are weighted by the state population.

I estimate the following non-parametric model to explore the dynamic effects of opioid mortality prevention policies and to examine the pre-trends:

$$y_{st} = \alpha + \sum_{y=-10}^{+8} \beta_y Policy_{sy} + \mathbf{X}_{st}\theta + \lambda_s + \gamma_t + \epsilon_{st}. \tag{4.2}$$

Equation 4.3 includes 10 years of lags and 8 years of leads, with the final lead indicating treatment for 8 or more years of exposure to a any GSL or NAL. The coefficient on the policy lags should be equal to zero because the future implementation of a NAL should not have an effect on contemporaneous opioid mortality. The sign on the leads of policy is ambiguous. If an opioid mortality prevention policy decreases opioid mortality and the effect builds over time, I would expect β_1 to β_{+8} to be negative and increase in magnitude. If time must pass after the policy before the law is fully implemented, I would expect the coefficient associated with the year in which the law was enacted to have a near-zero effect.

4.4.1.2 Correction for Time Varying Policy Adoption

There is a relatively new, growing literature on differences-in-differences with variation in the timing of the treatment [16, 17, 100, 101, 102]. A cohort is a group that receives the treatment at the same point in time; in this case, states who passed their first NAL in the same year are in the same cohort. If there is heterogeneity in the treatment effect by cohort, a two-way fixed effects model cannot recover average treatment effect. The two-way fixed effect model, Equation 4.4, results in a weighted sum of treatment effects. Although the weight sums to 1 across all cohorts and periods, the weights could be negative. If there are homogeneous treatment effects across cohorts, a causal effect is recovered from the two-way fixed effect regardless of the negative weights. If there is heterogeneity in the treatment effect by cohort, the estimated average treatment does not have a causal interpretation. Even if all of cohorts have a positive average treatment effect, the resulting estimate could be a negative average treatment effect due to the possibility of negative weights [100].

Given the long sample period of 1999-2019 and the reformulation of OxyContin, the effect of NALs across cohorts is unlikely to be homogeneous. I apply the approach developed by Callaway and Sant'Anna [16], which aggregates cohort-period average treatment effects.[2] The average treatment effect is estimated for each cohort in every period using not yet treated observations as the control group. An alternative control group could have been never treated observations, but all states adopt a NAL by 2017 thus I use not yet treated observations as the control group instead. In order to use the Callaway and Sant'Anna [16] approach, treatment cannot be reversed. This means once a NAL is enacted, the legislation must remain effective. Additionally there must be no anticipatory behavior and the treatment must be exogenous. I will return to this assumption in Section 4.6. Lastly, there must be parallel trends conditional on the covariates relative to the not-yet-treated group.

Next I use the Sun and Abraham [17] approach to estimate the dynamic effects of treatment with a correction for the variation in policy adoption. In this approach, the last treated cohort is the control group. An alternative control group could have been never

[2] In the most recent version of the working paper by Doleac and Mukherjee [15], there is robustness table presenting results adjusting for the time variation in policy adoption using methods developed by Goodman-Bacon [101].

treated observations but all states adopt a NAL by 2019 thus I use the final cohort who adopted a NAL in 2017. The following saturated model is estimated using data through 2016 to obtain a cohort-average treatment on the treated estimates.

$$y_{st} = \lambda_s + \gamma_t + \sum_{e \notin C} \sum_{l \notin -1} \delta_{e,l} 1\{E_s = e\} D^l_{s,t} + \epsilon_{st}. \tag{4.3}$$

The parameters of interest are the $\delta_{e,l}$; cohort e's effect during the relative period l. Each cohort-relative period pair has a parameter $\delta_{e,l}$ except the excluded control group, the last treated cohort C. The relative period l takes the value zero when the policy is introduced and is equal to the year minus the initial policy year. $D^l_{s,t}$ is an indicator that takes the value one when the observation is associated with the relative period l. For each relative period, the estimated $\hat{\delta_{e,l}}$ are combined weighted by the cohort's sample share in period l. As in Callaway and Sant'Anna [16], no anticipatory behavior must occur and treatment must be irreversible. The parallel trend assumption must hold relative to the last treated cohort instead of the not yet treated groups.

4.4.2 Effect on Measures of Opioid Activity

I estimate the effect of NALs and GSL on the following three measures of opioid activity: self-reported misuse of pain medication per 1000 persons, admission for opioid substance abuse treatment, and frequency of opioid abuse. This analysis is important because those who oppose NALs and GSLs typically cite concerns that greater Naloxone access encourages riskier opioid use. For these analyses I implement the corrected difference-in-difference model by Callaway and Sant'Anna [16] to estimate an average treatment effect and the Sun and Abraham [17] model to examine the dynamics of the treatment effect.

The first two measures of opioid activity, pain medication misuse and admission for treatment, are annual state level measures. The self-reported pain medication misuse reflects non-heroin opioid abuse, while changes in opioid treatment admissions do not reflect a specific type of opioid. A rise in the treatment admission rate could either reflect more opioid use or improved access to treatment from referrals after an overdose. I include in the model the same controls as in the mortality analysis. The frequency of opioid use more directly measures opioid activity. This is a self-reported measure among individuals admitted for opioid treatment. When a person is admitted for treatment, they report daily, monthly, or infrequent substance abuse. The outcome of interest is an indicator for at least monthly opioid use and daily opioid use. Conditional on admission for opioid treatment, I estimate the effect of a NAL or GSL on reported opioid use. I control for the state level implementation of MMLs and PDMPs, year fixed effect, and state fixed effects. I also control for the following demographic characteristics: whether the individual's treatment includes medication-assisted treatment, race, educational attainment as measured by a high school degree or less, and indicators for whether the individual is employed part-time and full-time. I cluster the standard errors by state.

4.5 Empirical Results

4.5.1 Replication: Opioid Mortality

I find that the effect of NALs and GSLs are sensitive both the policy definitions and the sample period examined. In Table 4.4, I report the effect of NALs and GSLs under five different ways to define the policy for the sample period of 1999-2019 in panel A and the sample period of 2010-2019 in panel B. Column 1 reports the estimated effect of NALs and GSLs as separate parameters following Rees et al. [14]. I find no statistically significant effect when using the full sample period and some evidence that NALs increase mortality when using the shorter time period. In the Appendix Table C.2, I estimate the effect using the same years as Rees et al. [14], 1999-2014. When restricting the time period to this earlier sample, I find that both NALs and GSLs reduces opioid related mortality by 19% and 13%, respectively.[3] The varying results suggest the treatment effect of NALs and GSLs are highly sensitive to the sample period.

In Columns 2 - 4, I present results that break from the prior literature and model NALs and GSLs as a combined treatment. The adoption of NALs and GSLs tending to occur simultaneous or within a short time span, which renders it infeasible to disentangle the effect of the two policies. In Column 2, I report the effect of having either a GSL or any NAL. I find having any opioid mortality prevention policy causes an 11% decline in opioid mortality when using the full sample period. To account for the variety in the number of policies a state enacts, I impose a homogeneous, linear treatment effect of each additional

[3] All models have a dependent log outcome and thus must be transformed using the following formula to meaningfully interpret the effects: $e^{\beta} - 1$.

policy. As seen in Column 3, I find an additional policy has no statistically significant effect on opioid mortality. As an alternative functional form, I model the intensity of policy adoption. I find that having low policy adoption reduces opioid mortality by 9% and high policy adoption reduces opioid mortality by 16% when using the full sample period. Panel B reports the results when using the 2010-2019 sample period. None of the three specifications with combined NALs and a GSL treatment find a statistically significant effect. While the estimated treatment effect continues to be sensitive to the sample period, I find no evidence of an increase in opioid related mortality from NALs and GSLs when using a model with a combined policy treatment.

Lastly, I limit the NAL to legislation with a provision for a third party prescription or standing order. Unlike the other provisions, these two aspects of a NAL more directly increase the supply and thus the availability of Naloxone. Doleac and Mukherjee [15] focuses on NALs with a supply enhancing provision. In Column 5, I present the estimated effect of having a supply NAL. I do not find a statistically significant effect of supply NALs on opioid mortality for any sample period examined, including 1999-2014 as seen in Table C.2. By comparing the results from Columns 1 and 5, it is evident that differences in the literature on the effect of NALs on opioid mortality are caused in part by the policy treatment definitions used by the authors.

Figure 4.3 depicts the dynamic time effects that correspond with Equation 4.3. I focus on my preferred method of policy modeling, the adoption of any NAL or a GSL. I refer to this as the adoption of an opioid mortality prevention policy. For the difference-in-differences specification to be valid, there must be no differential trend prior to the Naloxone access and Good Samaritan legislation. I do not find this to hold when using the full sample. The decline in mortality estimated in Column 2 of Table 4.4 is driven by a steady decline in mortality prior to the policy. This implies that we cannot take the estimated effect as a casual effect of treatment. Appendix Figure C.1 depicts the dynamic time effects of an opioid mortality prevention policy using the period 2010-2019. The graph shows that this later sample period does exhibit parallel trends. Prior to the policies, the pre-trend is close to zero with some noise. The effect of the legislation on mortality is positive immediately after adoption and then returns to zero after multiple years. This suggests that moral hazard occurs initially after the policy adoption but that there is no longer horizon effect of the policies on mortality.

4.5.2 Correction: Opioid Mortality

In Table 4.5, I report the estimated effect of the adoption of an opioid mortality prevention policy on mortality in Panel A. Panel B uses supply side policies instead of considering all the NALs regardless of provisions. These estimates account for the variation in the timing of adoption using methods developed by Callaway and Sant'Anna [16]. The control group is not yet treated observations thus the sample period goes from 1999 to 2016 since the last cohort adopted a prevention policy in 2017. In Panel A Column 1, I estimate the effect of the prevention policies on all opioid related deaths and find mortality increases by 39%. Figure 4.4 depicts the dynamic effects of opioid mortality prevention policies using the final cohort as a control group. Once adjusting for variation in the timing of treatment, the pre-trend is at zero. Opioid related deaths increase in the first couple years following the policy adoption and in years after the effect dampens. In

Columns 2 and 3 of Table 4.5, I report the effect on heroin and non-heroin opioid deaths. The rise in mortality is concentrated among non-heroin opioid deaths. I do not find a statistically significant effect on heroin mortality. Supply side policies increase all opioid deaths by 65% and non-heroin deaths by over 100%. Overall I find opioid mortality prevention policies increase opioid mortality once I account for the variation in policy adoption. In Section 4.6, I discuss the casual validity of these results.

4.5.3 Opioid Activity

In order to understand the potential adverse effects of opioid mortality prevention policies, I examine three measures of opioid activity and report the findings in Table 4.6. Column 1 reports the effect on pain medication misuse, which is meant to measure abuse of non-heroin opioids. I find the introduction of opioid mortality prevention policies do not effect pain medication misuse.

Columns 2 to 4 report the effect on opioid substance abuse treatment admissions. I find that opioid mortality prevention policies cause a 13% decline in heroin admission but do not cause a statistically significant effect on non-heroin admissions or on overall opioid admissions. However, Figure 4.5 shows the dynamic treatment effect of the policies on heroin treatment admissions. There is some evidence of changes in the trends of heroin treatment admissions prior to the policy implementation, violating the required parallel pre-trends. The dynamic treatment trends for opioid admissions and non-heroin admissions are depicted in Appendix Figure C.3 and Figure C.4. Furthermore changes in opioid abuse admissions could be reflective of the supply of treatment centers, the true need for treatment, behavioral changes in propensity to seek treatment, or even improved long-term substance abuse treatment outreach.

In a final attempt to capture a proxy for opioid activity, I examine the self-reported frequency of opioid use among individuals admitted for substance abuse treatment. Columns 5 and 6 estimate the effect on the probability of at least monthly opioid use and daily opioid use. I find no statistically significant effect of opioid mortality prevention policies on either measure of the frequency of opioid abuse among individuals admitted for opioid addiction treatment. These results suggest little evidence that opioid mortality prevention policies alter individual's behavior.

4.6 An Alternative Approach

A key assumption of the difference-in-differences empirical strategy is that treatment is exogenous. Treatment should not be determined by baseline characteristics. Figure 4.6 depicts the trends in opioid mortality by policy adoption. Appendix Figure C.5 and Figure C.6 illustrate the trends by non-heroin and heroin mortality. When examining the adoption of any opioid policy, there does not appear to be an increase in the opioid mortality rate preceding the policy implementation. If instead we examine supply side NALs, there is clear evidence that states with a rise in opioid mortality adopt legislation with provisions for standing orders or third party prescriptions. This finding holds when examining heroin and non-heroin opioid deaths separately. In general this suggests that states are responding to a rise in opioid related deaths by implementing these policies. Given the nonrandom adoption of NALs and GSLs by states, we cannot interpret the results discussed in Section 4.5 as causal evidence.

I propose an alternative estimation strategy for future work to help establish the causal effect of opioid mortality prevention policies inspired by Dube et al. [103]. Contiguous border counties with variation in their respective state's year of policy adoption provide a plausibly exogenous change in NALs and GSLs. Contiguous counties on the state border become increasingly comparable places relative to the comparability of states as a whole. If a county is in a state that adopted a NAL or GSL first, there will be a larger increase in Naloxone availability than in the neighboring county in a state without a policy. Both counties may benefit from one state's adoption of a NAL or GSL due broader availability in the entire area. For instance, people can visit local pharmacies in different states that supply Naloxone more readily. Despite the spillover effects, the county in the state that adopted the policy will have greater relative availability. In particular, emergency responders and police officers in counties located in the state with the policy are more likely to carry Naloxone when responding to an opioid overdose.

Using county level opioid mortality among contiguous counties on the state border, the following model can be estimated:

$$y_{cpt} = \alpha + \beta Policy_{ct} + \mathbf{X}_{ct}\theta + \lambda_c + \gamma_{pt} + \epsilon_{ct}. \tag{4.4}$$

The outcome of interest is opioid mortality in county c in year t. Notice the subscript p represents the cross-state county-border pair. A county will be in the sample multiple times depending on the number of cross state pairs. The parameter of interest is β. This model would include time varying county controls, county-pair time fixed effects, and county fixed effects. Standard errors should be two-way clustered by state and state border. Due to the necessary granular level of the data, I cannot estimate this model using the publicly available mortality data.

4.7 Conclusion

Naloxone Access and Good Samaritan Laws are contentious policies designed to increase the availability and use of Naloxone, an opioid overdose reversing medication. Opponents of these policies worry Naloxone lowers the risk of death which could encourage riskier opioid abuse. In this Chapter, I first replicate previous difference-in-differences models with various sample periods and policy treatment definitions. I then improve on these estimates by correcting for the staggered policy adoption. Lastly I discuss the violation of random adoption which inhibits a causal interpretation of the treatment effects. I propose an alternative empirical strategy future work can implement.

I analyze the effect of NALs and GSLs on opioid-related deaths using the multiple-cause-of-death data provided by CDC from 1999-2019. I use various sample periods and policy definitions to understand discrepancies in the literature about the effect on opioid mortality. I find the estimated treatment effect is highly sensitive to the sample period, particularly focusing on post-2010 as opposed to beginning in the earlier decade. I also determine restricting the policy treatment to NALs with supply side provisions results in a null effect on opioid related mortality. In certain sample periods, using all NALs regardless of the provision does produce a statistically significant, negative point estimate.

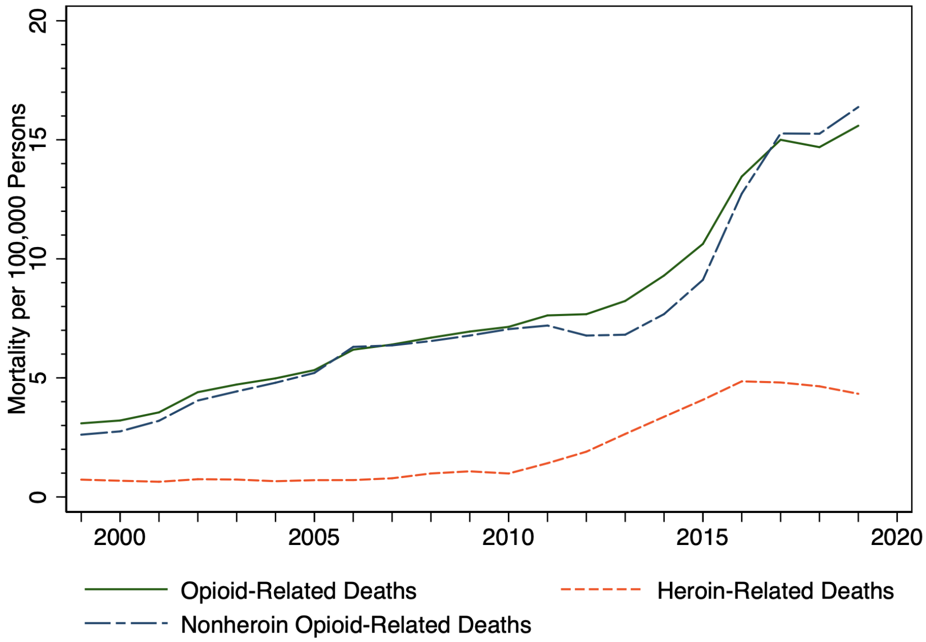
Once I correct for the variation in policy adoption, I find that having either a

NAL or a GSL increases opioid mortality by 39%. To further explore the potential of adverse consequences from NALs and GSLs, I use three measures of opioid activity: pain medication misuse, opioid substance abuse admissions, and the frequency of opioid use among individuals admitted for opioid abuse treatment. I find the adoption of these policies has no effect on my measures of opioid activity.

Lastly I examine the validity of the difference-in-differences model correcting for staggered policy adoption in this application. Since I find that states experience an increase in opioid mortality prior to adopting NALs and GSLs, the estimated treatment effects cannot be interpreted as causal. Future work should consider implementing a county-border pair strategy to recover causal estimates. While the point estimates will be attenuated due to spillovers across state borders, counties along the state borders plausibly experience exogenous changes in Naloxone availability due to the adoption of the policies at the state level.

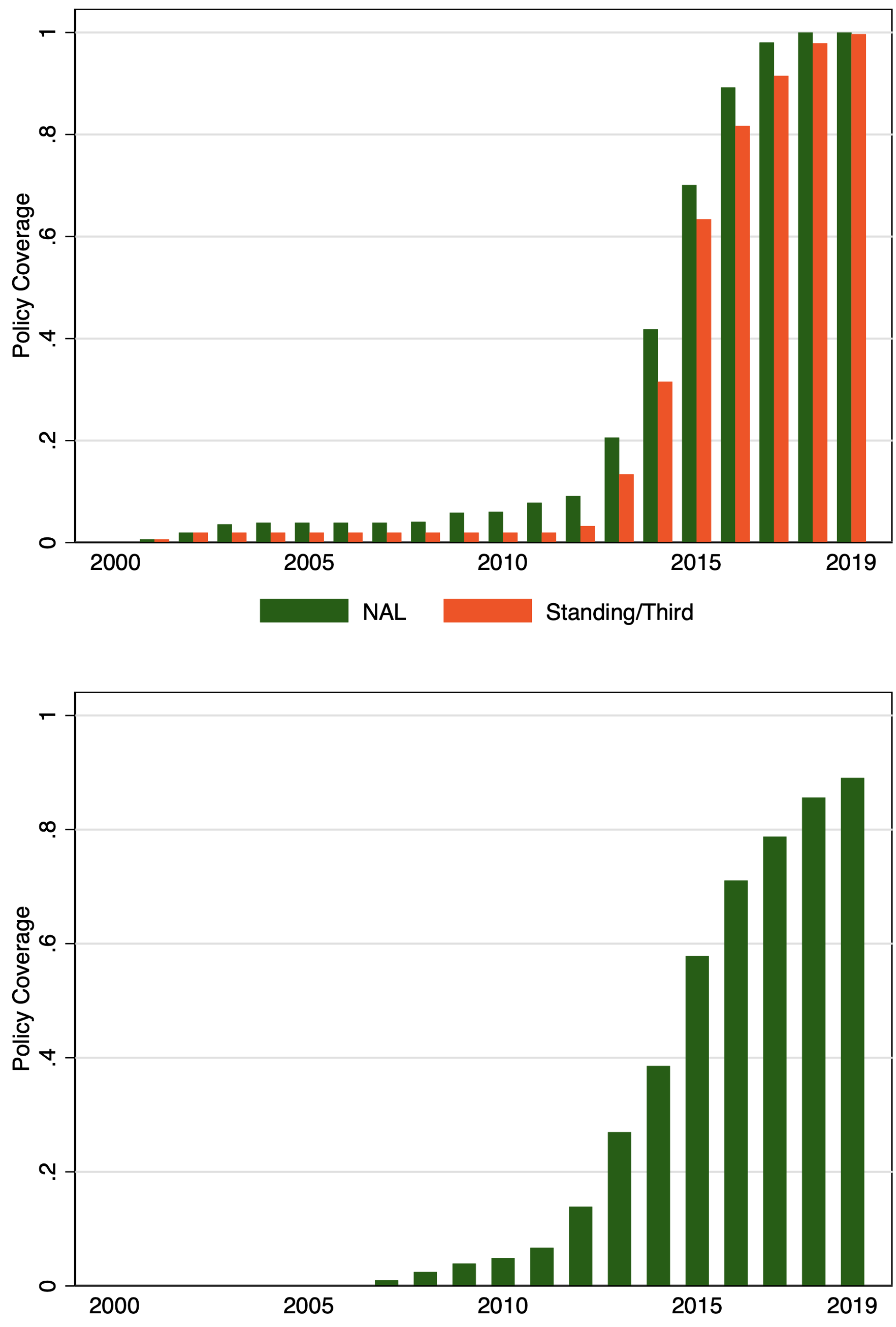
4.8 Figures and Tables

Figure 4.1: Opioid Related Deaths, 1999-2019



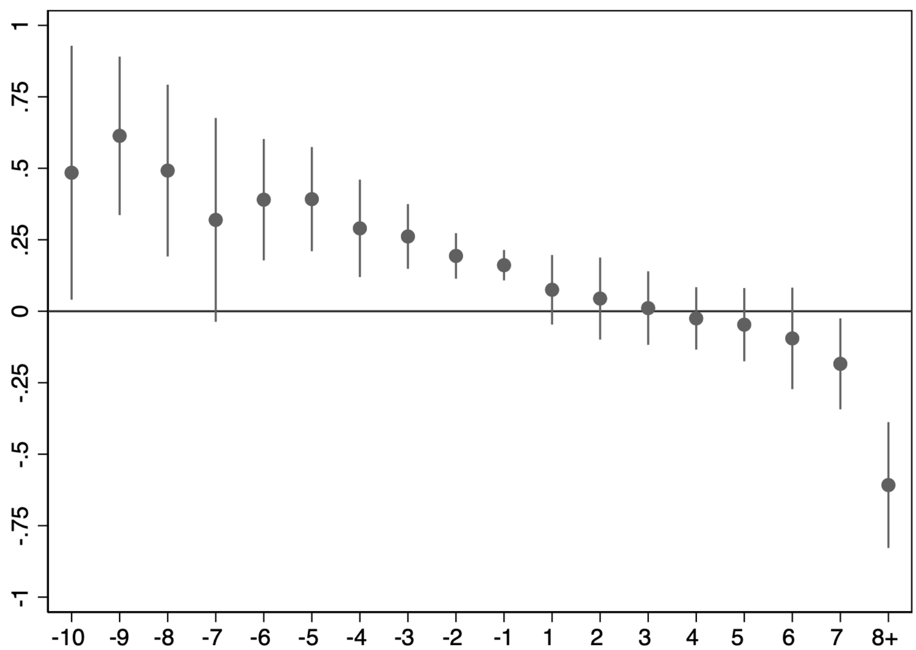
Source: Author's calculation using the Center for Disease Control Multiple Cause of Death via the Wonder Tool.

Figure 4.2: Policy Adoption Over Time: NALs and GSL



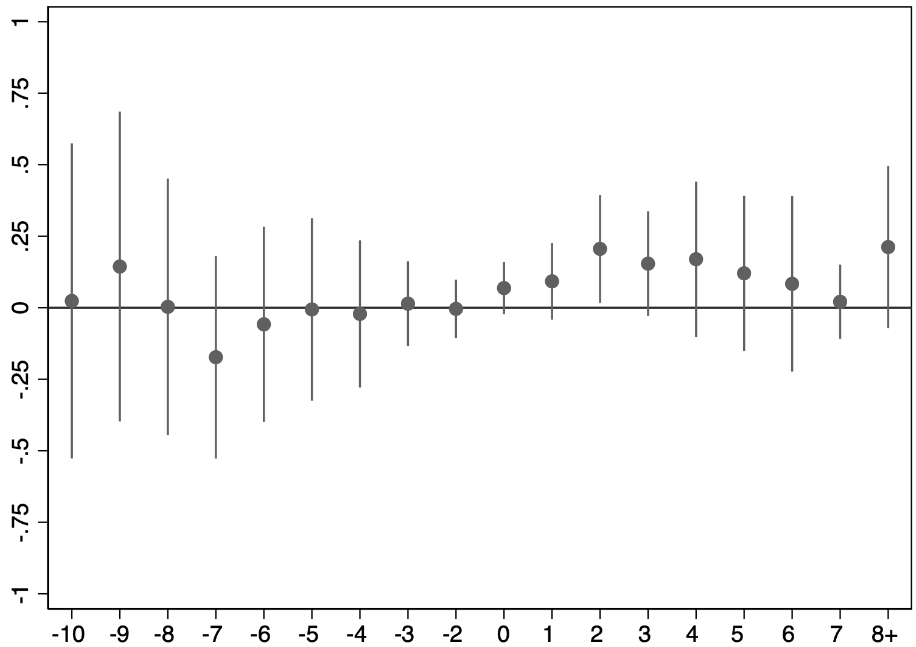
Note: The top panel show the fraction of states that have adopted any Naloxone Access Law (NAL) and adopted NALs with a provision for either a standing order or a third party prescription. Other NAL provisions include prescriber immunity and possession immunity. The bottom panel shows the fraction of states that have a Good Samaritan Law.
Source: Reports by the Legislative Analysis and Public Policy Association [79].

Figure 4.3: Effect on Opioid-Related Deaths - Unadjusted



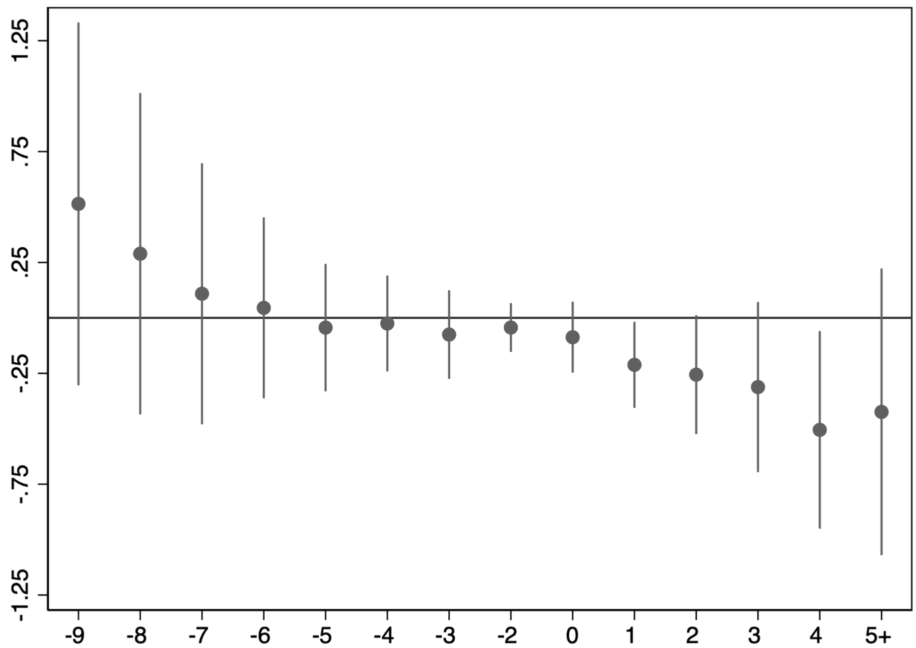
Note: This figure shows the dynamic timing effect of any opioid mortality prevention policies on the opioid mortality rate using data spanning 1999-2019. A state has an opioid mortality prevention policy if they have implemented any Naloxone Access Law or a Good Samaritan Law. The mortality rates were obtained from the Center for Disease Control Wonder Tool. The specification controls for Medical Marijuana Laws, Prescription Drug Monitoring Programs, population, police per capita, beer tax, cigarette tax, college graduates, per capita income, the unemployment rate, and the minimum wage. I cluster standard errors by state and weight observations by state population. The model does not adjust for the staggered policy adoption.

Figure 4.4: Effect on Opioid-Related Deaths - Adjusted



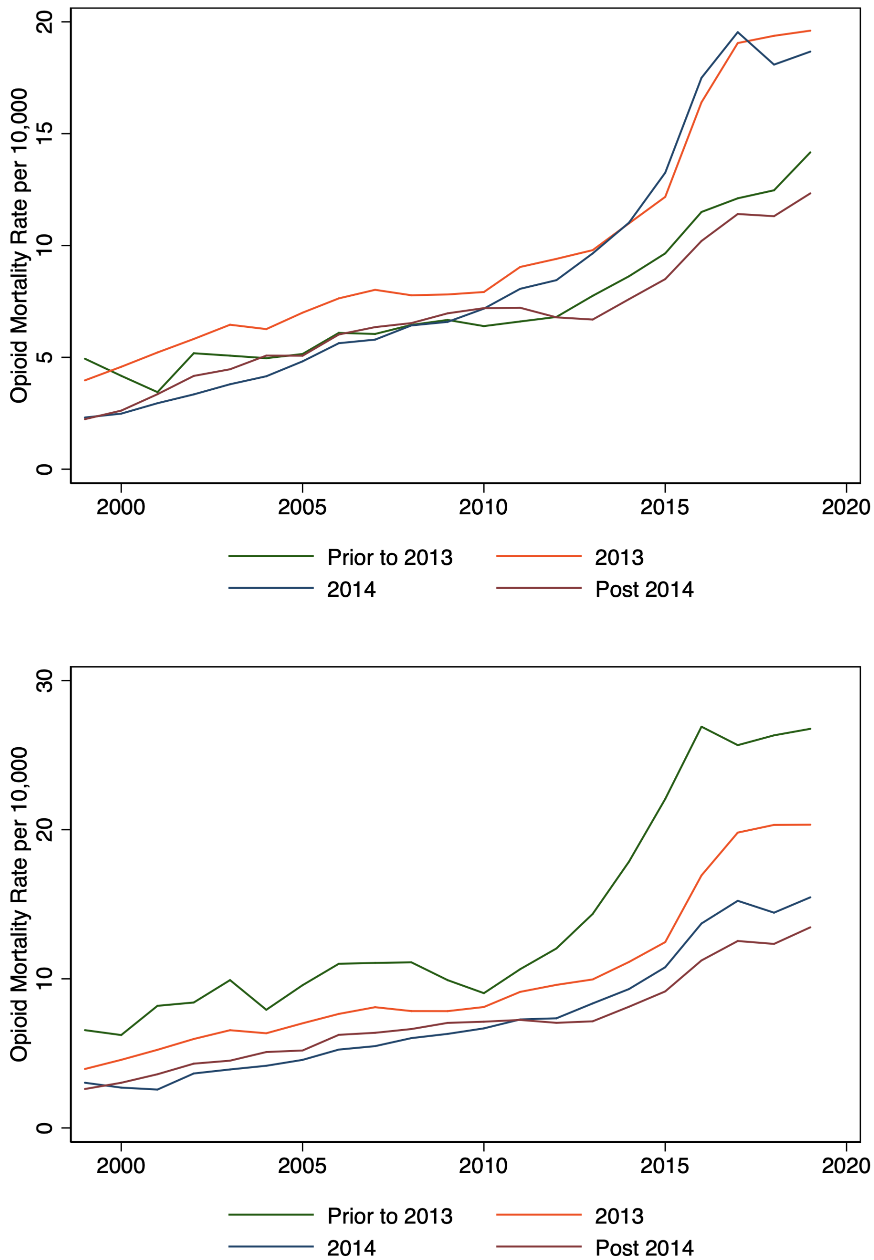
Note: See Figure 4.3 for a general description of the dynamic timing effects model and policy definition. In this figure, I estimate the effect of the policies on the opioid mortality rate with a model that corrects for the the variation in timing of the policy implementation. I use methods developed by Sun and Abraham [17].

Figure 4.5: Effect on Heroin Admissions - Adjusted



Note: This figure shows the dynamic timing effect of any opioid mortality prevention policies on heroin admission rate per 10,000 persons using data spanning 1999-2019. A state has an opioid mortality prevention policy if they have implemented any Naloxone Access Law or a Good Samaritan Law. Heroin admissions for substance abuse treatment come from the Treatment Episode Data. The specification controls for Medical Marijuana Laws, Prescription Drug Monitoring Programs, population, police per capita, beer tax, cigarette tax, college graduates, per capita income, the unemployment rate, and the minimum wage. I cluster standard errors by state and weight observations by state population. The model adjusts for the staggered policy adoption using methods by Sun and Abraham [17].

Figure 4.6: Opioid Related Deaths, by Policy Adoption



Note: The top panel depicts the opioid mortality rate per 10,000 persons by the adoption date of any opioid mortality prevention policy. Any policy includes Naloxone Access Laws (NALs) regardless of the provisions and Good Samaritan Laws. The bottom panel depicts the opioid mortality rate by the adoption date of NALs with a provision for third party prescriptions or a standing order.
Source: Author's calculation using the Center for Disease Control Multiple Cause of Death and reports by the Legislative Analysis and Public Policy Association [79].

Table 4.1: State Implementation of NALs and GSLs

State	Any	Standing	Possession	Third	Immunity	GSL
Alabama	Jun-15	Mar-18		Jun-15	Jun-15	Jun-15
Arkansas	Jul-15	Sep-17		Jul-15	Jul-15	Oct-14
Alaska	Mar-16	Mar-17	Mar-16	Mar-17	Mar-16	Apr-18
Arizona	Aug-16	Nov-18			Aug-16	Jul-15
California	Jan-08	Jan-14	Jan-11	Jan-14	Jan-08	Jan-13
Colorado	May-13	Oct-19		Oct-19	May-13	May-12
Connecticut	Oct-03	Oct-17		Jun-15	Oct-03	Oct-11
Delaware	Jun-14	Jun-14			Aug-14	Aug-13
District of Columbia	Mar-13	Dec-18	Mar-13	Feb-17	Feb-17	Mar-13
Florida	Jun-15	Jul-16		Jun-15	Jun-15	Oct-12
Georgia	Apr-14	Apr-14		Apr-14	Apr-14	Apr-14
Hawaii	Jun-16	Jun-16	Jun-16	Jun-16	Jun-16	Jul-15
Iowa	May-16	May-16			May-16	Jul-18
Idaho	Jul-15	Jul-15			Jul-15	Jun-12
Illinois	Jan-10	Sep-17	Jan-10	Sep-15	Sep-15	Jul-16
Indiana	Apr-15	Apr-15		Apr-15	Apr-15	May-18
Kansas	Jul-17	Jul-17			Jul-17	
Kentucky	Jun-13	Jun-13		Mar-15	Jun-13	Mar-15
Louisiana	Jun-15	Jun-16	Jun-16	Jun-16	Jun-15	Aug-14
Maine	Apr-14	Oct-15	Apr-14	Oct-15	Apr-14	
Maryland	Oct-13	Jun-19		Oct-13	Oct-13	Oct-14
Massachusetts	Aug-12	Jul-14	Aug-12	Aug-12	Jul-14	Aug-12
Michigan	Oct-14	Mar-17	Oct-14	Oct-14	Oct-14	Jan-17
Minnesota	May-14	May-14		May-14	May-14	Jul-14
Mississippi	Jul-15	Jul-17		Jul-15	Jul-15	Jul-15
Missouri	Aug-16	Aug-17			Aug-16	Aug-17
Montana	Oct-17	Oct-17		Oct-17	May-17	May-17
Nebraska	May-15	May-15		May-15	May-15	May-17
Nevada	Oct-15		Oct-15	Oct-15	Oct-15	Oct-15
New Hampshire	Jun-15	Jun-15		Jun-15	Jun-15	Sep-15
New Jersey	Jul-13	Jul-13	Jul-13	Jul-13	Jul-13	May-13
New Mexico	Apr-01	Apr-01	Mar-16	Apr-01	Apr-01	Jun-07
New York	Jul-14	Jul-14		Jul-14	Jul-14	Sep-11
North Carolina	Apr-13	Apr-13		Apr-13	Apr-13	Apr-13
North Dakota	Aug-15	Aug-15	Aug-15	Aug-15	Aug-15	Apr-15
Ohio	Mar-14	Jul-15	Apr-17	Mar-14	Mar-14	Sep-16
Oklahoma	Nov-13	Nov-14		Nov-13	Nov-18	
Oregon	Jun-13	Sep-13		Jun-13	Jun-13	May-15
Pennsylvania	Nov-14	Nov-14		Nov-14	Nov-14	Dec-14
Rhode Island	Oct-14	Oct-14	Jan-16	Oct-14	Oct-14	Jun-12
South Carolina	Jun-15	Jun-15		Jun-15	Jun-15	
South Dakota	Jun-15			Jun-15	Jun-15	Mar-17
Tennessee	Jul-14	Jul-14		Jul-14	Jul-14	Jul-15
Texas	Sep-15	Sep-15	Sep-15	Sep-15	Sep-15	
Utah	May-14	Dec-16		May-14	May-14	Mar-14
Vermont	Jul-13	Aug-16		Jul-13	Jul-13	Jun-13
Virginia	Jul-13	Apr-18		Jul-13	Jul-13	Jul-15
Washington	Jul-15	Aug-19	Jul-15	Jul-15	Jul-15	Jun-10
West Virginia	May-15	Nov-18	May-15	May-15	May-15	Jun-15
Wisconsin	Apr-14	Apr-14	Apr-14	Apr-14	Apr-14	Apr-14
Wyoming	Jul-17	Jul-17			Jul-17	

Note: This table provides Naloxone Access Law (NAL) and Good Samaritan Law (GSL) implementation dates in each state. I categorize NAL by the following legislation's provisions: standing order, possession liability, third party prescription, and prescriber immunity. Legislative information was provided by reports published by the Legislative Analysis and Public Policy Association.

Table 4.2: State-Level Descriptive Statistics

	Full Sample	Control	Treated
Opioid Related Deaths and Activities, per 10,000			
Opioid Deaths	8.029	5.642	12.06
	(5.981)	(3.216)	(7.276)
Prescription Misuse	455.3	479.7	421.6
	(72.52)	(72.34)	(57.80)
Treatment Admissions	18.62	14.89	24.94
	(20.12)	(15.33)	(25.12)
Heroin Admissions	6.426	5.377	8.203
	(6.152)	(5.409)	(6.892)
Non-Heroin Admissions	13.62	10.55	18.81
	(17.39)	(13.68)	(21.36)
Legislation			
Naloxone Access Law	0.319	0	0.857
	(0.456)	(0)	(0.312)
Standing Order	0.216	0	0.581
	(0.400)	(0)	(0.468)
Possession Immunity	0.140	0	0.376
	(0.342)	(0)	(0.475)
Third Party Prescription	0.248	0	0.667
	(0.422)	(0)	(0.446)
Prescriber Immunity	0.305	0	0.820
	(0.450)	(0)	(0.349)
Good Samaritan Law	0.257	0	0.691
	(0.428)	(0)	(0.439)
State Characteristics			
Prescription Drug Monitoring Program	0.380	0.169	0.734
	(0.485)	(0.375)	(0.442)
Police	3.237	3.301	3.128
	(1.053)	(1.211)	(0.698)
Medical Marijuana Law	0.330	0.179	0.586
	(0.471)	(0.384)	(0.493)
Beer Tax (2014$)	0.298	0.304	0.288
	(0.275)	(0.274)	(0.277)
Cigarette Tax (2014$)	1.278	1.018	1.716
	(0.949)	(0.709)	(1.126)
College Graduation	0.300	0.277	0.339
	(0.0567)	(0.0468)	(0.0503)
Per capital income (2014$)	45040.7	42011.6	50153.2
	(7622.7)	(5729.2)	(7697.7)
Unemployment Rate	5.855	5.868	5.832
	(2.074)	(1.987)	(2.215)
Minimum Wage (2014$)	7.584	7.223	8.192
	(1.041)	(0.754)	(1.168)
Obs.	1071	728	343

Note: This table reports summary statistics of key variables from 1999-2019. I weight the state-level observations by population. I derive the opioid mortality rate from the Center for Disease Multiple Cause of Death files (1999-2019) via the Wonder Tool. The National Survey of Drug Use and Health (2003-2018) provides data on prescription pain medication misuse and substance abuse admission come from the Treatment Episode Data (1999-2019). Legislation information is obtained from the Legislative Analysis and Public Policy Association. The first column reports summary statistics for all states. The second column, Control, reports statistics among states prior to adopting any Naloxone Access Law (NAL) or a Good Samaritan Law (GSL). The last column, Treated, reports statistics among states with a NAL or GSL effect for part of the year.

Table 4.3: Descriptive Statistics for Individuals Admitted for Opioid Abuse Treatment

	Full Sample	Control	Treated
Opioid Abuse Frequency			
Uses Monthly	0.0458	0.0531	0.0344
	(0.209)	(0.224)	(0.182)
Uses Daily	0.0288	0.0313	0.0249
	(0.167)	(0.174)	(0.156)
Legislation			
Naloxone Access Law	0.356	0	0.918
	(0.466)	(0)	(0.211)
Good Samaritan Law	0.294	0.0520	0.674
	(0.444)	(0.212)	(0.448)
State and Individual Characteristics			
Medical Marijuana Law	0.444	0.237	0.771
	(0.497)	(0.425)	(0.420)
Prescription Drug Monitoring Program	0.385	0.175	0.715
	(0.487)	(0.380)	(0.451)
Police	3.335	3.512	3.057
	(1.153)	(1.325)	(0.728)
Beer Tax	0.205	0.207	0.202
	(0.154)	(0.155)	(0.153)
Cigarette Tax	1.762	1.571	2.062
	(1.106)	(0.980)	(1.219)
Medication-Assistance Therapy	0.291	0.252	0.354
	(0.454)	(0.434)	(0.478)
White	0.705	0.691	0.727
	(0.456)	(0.462)	(0.445)
High School or Less	0.772	0.778	0.763
	(0.419)	(0.416)	(0.425)
Employed Part Time	0.0613	0.0579	0.0668
	(0.240)	(0.233)	(0.250)
Employed Full Time	0.127	0.133	0.116
	(0.333)	(0.340)	(0.321)
Obs.	8411992	5146691	3265301

Note: Summary statistics are derived from the Treatment Episode Data for individuals who were admitted for opioid substance abuse treatment between 1999 and 2019. The first column reports summary statistics for all individuals. The second column, Control, reports statistics among individuals admitted in a state without any Naloxone Access Law (NAL) or Good Samaritan Law (GSL). The last column, Treated, reports statistics among individuals admitted in a state with a NAL or GSL in effect for part of the year.

Table 4.4: Effect of NALs and GSL on Opioid Mortality

	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample, 1999-2019					
Naloxone Access Law	-0.0715				
	(0.059)				
Good Samaritan Law	-0.0607				
	(0.092)				
Any Opioid Policy		-0.1225**			
		(0.054)			
Opioid Policy			-0.0553		
			(0.033)		
Low Policy Adoption				-0.0983*	
				(0.049)	
High Policy Adoption				-0.1724*	
				(0.097)	
Supply NAL Policy					-0.0058
					(0.071)
R-sqr	0.838	0.838	0.839	0.838	0.837
Obs.	1071	1071	1071	1071	1071
Panel B: Subsample, 2010- 2019					
Naloxone Access Law	0.1665*				
	(0.088)				
Good Samaritan Law	0.0578				
	(0.065)				
Any Opioid Policy		0.1139			
		(0.084)			
Opioid Policy			0.0149		
			(0.022)		
Low Policy Adoption				0.0791	
				(0.076)	
High Policy Adoption				0.0986	
				(0.102)	
Supply NAL Policy					0.0208
					(0.070)
R-sqr	0.892	0.890	0.888	0.889	0.888
Obs.	510	510	510	510	510

Note: Standard errors in parenthesis are clustered by state and observations are weighted by the state population. I obtain the opioid mortality rate from the Center for Disease Control via the Wonder tool. Each column models the Naloxone Access Laws (NALs) and Good Samaritan Laws in distinct manners. Column 1 separately estimates the effect of having any NAL and a GSL. Column 2 models the policy as the presence of either a NAL or GSL. Column 3 imposes a linear functional form, using the count of provisions of the NALs and GSLs. Column 4 estimates the adoption of a high amount (four or more) and low amount (one to three) of NAL provisions and GSLs. The final column only includes NALs with provisions for a standing order and a third party prescription. The model controls for Medical Marijuana Laws, Prescription Drug Monitoring Programs, population, the number of police officers, the beer and cigarette tax rates, the college graduation rate, the average per capita income, the minimum wage, and the state unemployment rate. The model includes year and state fixed effects. I take the natural log of the outcome, the opioid mortality rate, therefore the coefficients should be interpreted as the $e^{\beta} - 1$ percent change.

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

Table 4.5: Effect on Opioid Mortality - Adjusted for Time Varying Adoption

	All (1)	Heroin (2)	Non-Heroin (3)
Panel A: Any Opioid Policy			
Any Opioid Policy	0.3304***	-0.0063	0.237***
	(0.077)	(0.059)	(0.072)
Panel B: Supply Side Policy			
Supply NAL Policy	0.5039***	0.0916	0.863***
	(0.191)	(0.177)	(0.273)

Note: Standard errors in parenthesis are clustered by state and observations are weighted by the state population. I obtain the opioid mortality rates from the Center for Disease Control via the Wonder tool. Column 1 uses all opioid related deaths, Column 2 uses only heroin related deaths, and Column 3 uses non-heroin related deaths. Panel A models the policy treatment as the presence of either a Naloxone Access Law (NAL) or Good Samaritan Law. Panel B models the policy treatment as the presence of a NAL with a provision for either a standing order or a third party prescription. The model controls for Medical Marijuana Laws, Prescription Drug Monitoring Programs, population, the number of police officers, the beer and cigarette tax rates, the college graduation rate, the average per capita income, the minimum wage, and the state unemployment rate. The model includes year and state fixed effects. I take the natural log of the outcome, the opioid mortality rate, therefore the coefficients should be interpreted as the $e^{\beta} - 1$ percent change. In this specification, I adjust for the staggered policy adoption applying methods developed by Callaway and Sant'Anna [16].

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

Table 4.6: Effect Opioid Activity - Adjusted for Time Varying Adoption

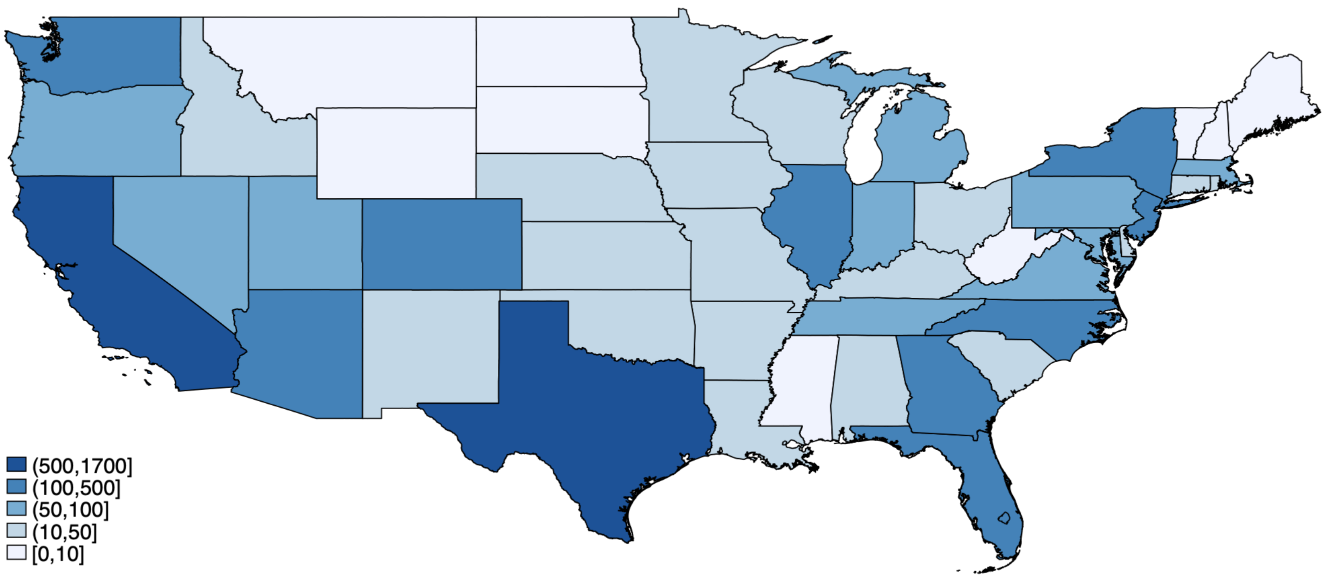
Outcome	Pain Med. Misuse	Treatment Admissions			Frequency of Use	
		Opioid	Non-Heroin	Heroin	Monthly	Daily
	(1)	(2)	(3)	(4)	(5)	(6)
Any Opioid Policy	-0.0458	0.0937	-0.1092	-.1354**	-0.0073	-.00048
	(0.048)	(0.070)	(0.092)	(0.056)	(0.042)	(0.039)

Note: Standard errors in parenthesis are clustered by state. I weight the observations by the state population in columns 1-4. Columns 5-6 are individual level outcomes and are unweighted observations. I obtain the pain medication misuse rate from the National Survey of Drug Use and Health for 2003-2018. I derive the substance abuse treatment rates and the frequency on opioid use from the Treatment Episode Data 1999-2019. I defined any opioid policy as the presence of either a Naloxone Access Law (NAL) or Good Samaritan Law. In columns 1-4, the model controls for Medical Marijuana Laws, Prescription Drug Monitoring Programs, population, the number of police officers, the beer and cigarette tax rates, the college graduation rate, the average per capita income, the minimum wage, and the state unemployment rate. The model includes year and state fixed effects. I take the natural log of outcome in Columns 1-4, therefore the coefficients should be interpreted as the $e^{\beta} - 1$ percent change. In columns 5-6, the model includes controls for Medical Marijuana Laws and Prescription Drug Monitoring Programs in the state. The model has the following individual covariates: whether the individual's treatment includes medication-assisted treatment, race, educational attainment as measured by a high school degree or less, and indicators for whether the individual is employed part-time and full-time. The model includes year and state fixed effects. In all the specifications, I adjust for the staggered policy adoption applying methods developed by Callaway and Sant'Anna [16].

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

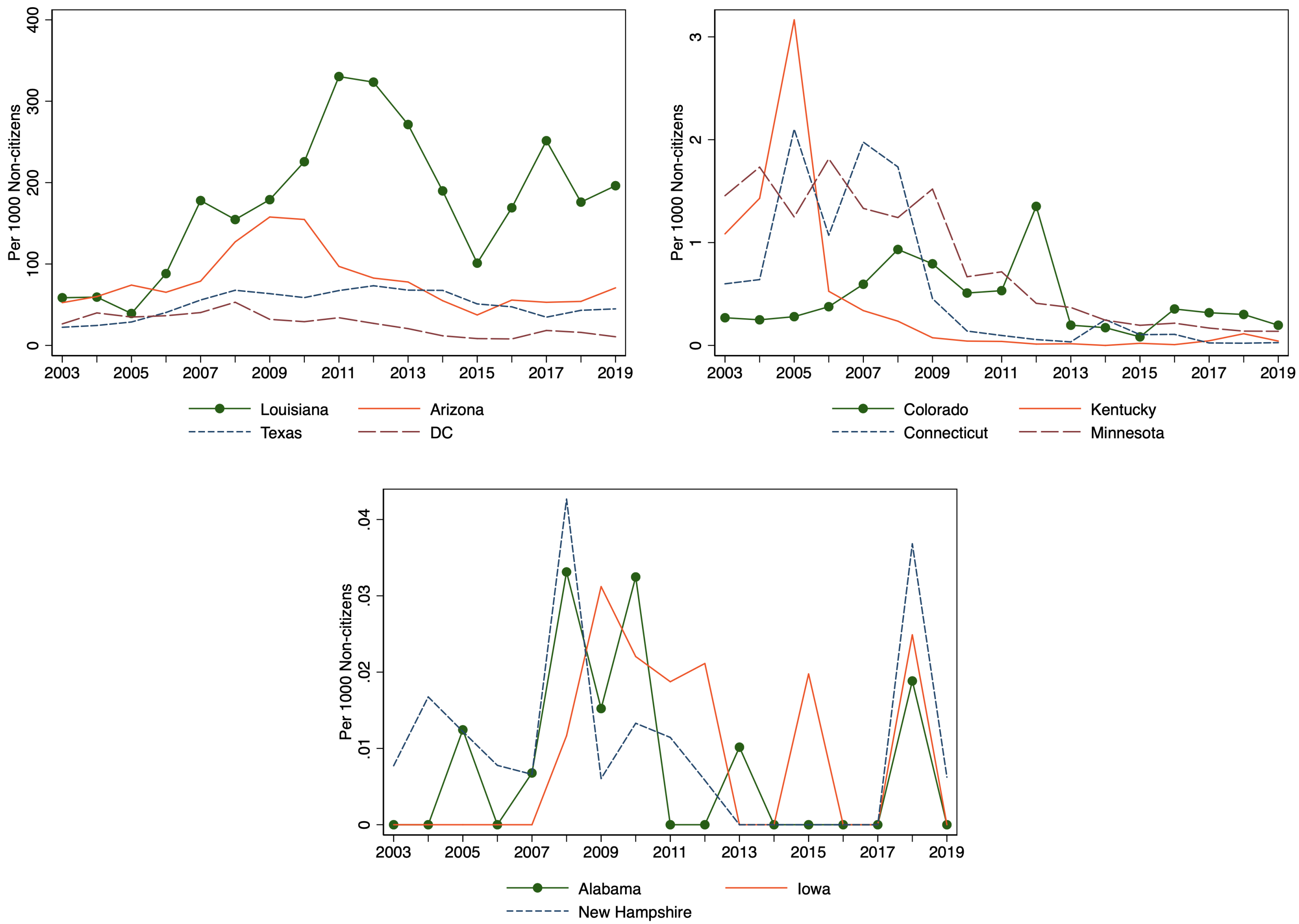
Appendix A: Appendix Figures and Tables

Figure A.1: Children with Legal Status in Mixed-Status Household, 2010-2014



Note: This map depicts the geographical distribution of the number of children living with undocumented household members. Children with legal status includes US born citizens or naturalized citizens. A mixed-status household contains people with legal residence in the US and those who are undocumented.
Source: Mathema [2].

Figure A.2: Variation in Deportations, Select States



Note: The deportation per 1000 non-citizens rate for high deportation, medium, and low deportation states is presented in the above figures.

Source: Author calculation using the Current Population Survey and the Transaction Records Access Clearinghouse, 2003-2019.

Table A.1: Effect of Immigration Policies Among Likely Undocumented Mothers

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt Prenatal	Any Prenatal	Healthy	Average	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Secure Community Program							
Secure Community	-0.0000	0.0083	0.0008	0.0010	-0.0012	-1.4342	2.5874***
	(0.008)	(0.012)	(0.002)	(0.001)	(0.001)	(3.330)	(0.629)
Undoc. Mother	0.1539***	0.0876	-0.0306***	-0.0085	0.0228***	25.5566***	18.2933***
	(0.026)	(0.065)	(0.011)	(0.005)	(0.003)	(8.129)	(2.999)
Interaction	0.0092	-0.0336	-0.0098	-0.0036	-0.0042	-12.2625	-13.1659***
	(0.030)	(0.073)	(0.011)	(0.006)	(0.003)	(9.038)	(3.074)
R-sqr	0.893	0.826	0.624	0.543	0.323	0.628	0.263
Panel B: Omnibus Acts							
Omnibus Act	0.0022	0.0007	0.0056**	0.0057***	0.0023	5.5395*	0.8377
	(0.007)	(0.009)	(0.002)	(0.002)	(0.002)	(3.227)	(1.116)
Undoc. Mother	0.1683***	0.0646**	-0.0350***	-0.0094***	0.0194***	15.5299***	7.5563***
	(0.011)	(0.032)	(0.004)	(0.002)	(0.001)	(3.787)	(0.782)
Interaction	-0.1111***	-0.0734	-0.0602***	-0.0338***	-0.0007	-0.5752	-3.3023
	(0.023)	(0.056)	(0.013)	(0.007)	(0.003)	(9.203)	(2.050)
R-sqr	0.894	0.827	0.631	0.554	0.323	0.627	0.244
Panel C: 287G Agreements							
287G Agreement	0.0068	0.0001	-0.0030	-0.0018	-0.0023*	-7.1499*	-0.7594
	(0.011)	(0.013)	(0.002)	(0.002)	(0.001)	(3.754)	(0.852)
Undoc. Mother	0.1574***	0.0953***	-0.0350***	-0.0081***	0.0177***	8.5419**	7.2156***
	(0.010)	(0.030)	(0.004)	(0.002)	(0.001)	(3.975)	(1.016)
Interaction	0.0147	-0.1265**	-0.0133	-0.0121**	0.0057**	24.9033***	0.3800
	(0.026)	(0.062)	(0.011)	(0.005)	(0.002)	(7.497)	(2.711)
R-sqr	0.893	0.828	0.625	0.545	0.323	0.628	0.244
Panel D: E-Verify Policies							
E-Verify	0.0120**	0.0203***	0.0033*	0.0011	0.0007	-1.3738	0.7136
	(0.005)	(0.006)	(0.002)	(0.001)	(0.001)	(1.899)	(0.488)
Undoc. Mother	0.1877***	0.1042***	-0.0295***	-0.0076**	0.0177***	9.4907**	7.1667***
	(0.012)	(0.039)	(0.005)	(0.003)	(0.001)	(4.579)	(0.889)
Interaction	-0.0896***	-0.1512***	-0.0315***	-0.0133***	0.0055***	20.5443***	0.4800
	(0.015)	(0.045)	(0.007)	(0.004)	(0.002)	(5.937)	(1.637)
Mean	0.577	0.535	0.933	0.974	0.087	3219.050	17.646
R-sqr	0.896	0.833	0.631	0.549	0.323	0.628	0.244
Number of Births	4416852	4416852	4416852	1600710	4416852	4416852	14853721

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2017. Columns 1-6 further restrict the sample to first time births, and Column 7 uses all births to examine the birth rate. I compare outcomes of non-Latina US born mothers and foreign born mother from select countries. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table A.2: Heterogeneity in Deportation Timing Among Likely Undocumented Mothers

Cumulative Deportations:	1^{st} Trimester and 3 months prior (1)	2^{nd} and 3^{rd} Trimester (2)	2 years before the birth (3)	3 years before the birth (4)	5 years before the birth (5)
Panel A: WIC Participation					
Interaction	-0.0005	-0.0009	0.0019	0.0027	0.0035
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
R-sqr	0.883	0.883	0.883	0.883	0.883
Panel B: Medicaid					
Interaction	-0.0515***	-0.0531***	-0.0314***	-0.0272***	-0.0229**
	(0.013)	(0.013)	(0.011)	(0.010)	(0.010)
R-sqr	0.817	0.818	0.814	0.814	0.813
Panel C: Prompt Prenatal Care					
Interaction	-0.0063***	-0.0065***	-0.0031*	-0.0025	-0.0021
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
R-sqr	0.606	0.606	0.605	0.604	0.604
Panel D: Any Prenatal Care					
Interaction	-0.0055***	-0.0056***	-0.0034***	-0.0030***	-0.0026***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
R-sqr	0.543	0.543	0.540	0.539	0.538
Panel E: Healthy Birth Weight					
Interaction	0.0013***	0.0014***	0.0009**	0.0008**	0.0006*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-sqr	0.336	0.336	0.336	0.336	0.336
Panel F: Average Birth Weight					
Interaction	7.1438***	7.3033***	4.9565***	4.4646***	3.8592***
	(1.609)	(1.581)	(1.266)	(1.203)	(1.161)
R-sqr	0.626	0.626	0.626	0.626	0.626

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of non-Latina US born mothers and foreign born mothers from select countries. Interaction is the interaction between Undocumented mother and log(Deportations). Each specification also include the log(Deportations) and indicator for being a likely undocumented mother. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. The cumulative measure of deportations per non-citizen vary based on the column heading.

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

Table A.3: Alternative Model of Timing of Deportation Among Undocumented Mothers

	Safety Net		Prenatal Care		Birth Weight	
	WIC	Medicaid	Prompt	Any	Healthy	Average
	(1)	(2)	(3)	(4)	(5)	(6)
log(Deportations$_{t+1}$)	0.0044	0.0058	0.0012	-0.0006	0.0002	-0.1967
	(0.007)	(0.016)	(0.003)	(0.002)	(0.001)	(2.797)
log(Deportations$_{t}$)	0.0045	-0.0007	-0.0028	-0.0005	-0.0003	-1.2510
	(0.008)	(0.019)	(0.003)	(0.002)	(0.001)	(2.939)
log(Deportations$_{t-1}$)	0.0088	-0.0004	0.0015	0.0009	0.0002	-0.6011
	(0.007)	(0.014)	(0.003)	(0.001)	(0.001)	(2.625)
log(Deportations$_{t-2}$)	0.0069	0.0034	-0.0026	-0.0015	-0.0006	-2.5862
	(0.007)	(0.013)	(0.002)	(0.001)	(0.001)	(2.423)
log(Deportations$_{t-3}$)	0.0279***	0.0101	-0.0030	-0.0027**	-0.0004	-3.0027
	(0.008)	(0.013)	(0.002)	(0.001)	(0.001)	(3.017)
Undoc. Mother	0.1454***	0.1260***	-0.0273***	-0.0013	0.0184***	11.5672***
	(0.009)	(0.024)	(0.003)	(0.002)	(0.001)	(3.292)
Interaction$_{t+1}$	-0.0264	-0.0882	-0.0108	-0.0062	0.0056**	16.4780
	(0.035)	(0.079)	(0.010)	(0.005)	(0.003)	(11.071)
Interaction$_{t}$	0.0146	-0.0006	0.0049	0.0007	-0.0038	-7.5339
	(0.046)	(0.096)	(0.012)	(0.007)	(0.004)	(15.937)
Interaction$_{t-1}$	-0.0137	0.0044	-0.0057	-0.0032	0.0003	4.3406
	(0.042)	(0.084)	(0.011)	(0.006)	(0.004)	(13.124)
Interaction$_{t-2}$	0.0129	-0.0052	0.0170	0.0043	0.0029	4.3150
	(0.046)	(0.086)	(0.012)	(0.006)	(0.004)	(12.066)
Interaction$_{t-3}$	0.0142	0.0506	-0.0103	0.0000	-0.0039	-11.9482
	(0.039)	(0.075)	(0.012)	(0.006)	(0.003)	(9.734)

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of non-Latina US born mothers and foreign born mothers from select countries. Interaction is the interaction between Undocumented mother and log(Deportations) at different year-long horizons. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table A.4: Effect of Deportations Using All Births

	Safety Net		Prenatal Care		Birth Weight	
	WIC	Medicaid	Prompt	Any	Healthy	Average
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Births of US Non-Latina and Foreign Born Latina Mothers						
log(Deportations)	-0.0005	0.0051	-0.0003	-0.0000	-0.0008	-1.7984
	(0.008)	(0.007)	(0.002)	(0.001)	(0.001)	(3.489)
Undoc. Mother	0.1708***	0.0883***	0.0000	0.0041***	0.0311***	72.8440***
	(0.007)	(0.020)	(0.002)	(0.001)	(0.001)	(4.010)
Interaction	0.0050	-0.0296**	-0.0020	-0.0015*	0.0007	4.2140**
	(0.003)	(0.012)	(0.001)	(0.001)	(0.000)	(1.673)
Mean	0.561	0.565	0.927	0.979	0.914	3249.198
R-sqr	0.871	0.772	0.677	0.595	0.635	0.771
Panel B: All Births of US Non-Latina and US Born Latina Mothers						
log(Deportations)	-0.0016	-0.0022	-0.0016	0.0000	-0.0008	-1.5919
	(0.008)	(0.008)	(0.001)	(0.001)	(0.001)	(3.391)
Undoc. Mother	0.1138***	0.0803***	0.0030***	0.0050***	0.0103***	4.0309
	(0.006)	(0.004)	(0.001)	(0.000)	(0.001)	(2.927)
Interaction	0.0099***	-0.0001	0.0022***	-0.0003	0.0019***	4.8841***
	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.889)
Mean	0.546	0.567	0.930	0.980	0.911	3237.317
R-sqr	0.896	0.917	0.777	0.703	0.617	0.819

Note: Standard errors in parentheses are clustered by state-year. The sample is includes all mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. Panel A compares US born non-Latina mothers to foreign born Latina mothers. Panel B compares US born non-Latina mothers to US born Latina mothers.

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

Table A.5: Effect of Immigration Policies Among US Latina Mothers

	Safety Net		Prenatal Care		Birth Weight		Birth Rate
	WIC	Medicaid	Prompt Prenatal	Any Prenatal	Healthy	Average	Birth Rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Secure Community Program							
Secure Community	-0.0059	0.0036	-0.0014	0.0006	-0.0020*	-3.2468	1.3643***
	(0.008)	(0.007)	(0.002)	(0.001)	(0.001)	(3.040)	(0.431)
Latina Mother	0.1104***	0.0869***	-0.0051	-0.0006	0.0069***	-7.9010	9.8090***
	(0.013)	(0.009)	(0.003)	(0.001)	(0.002)	(4.866)	(0.989)
Interaction	0.0296*	0.0123	0.0058	0.0025*	0.0027	4.0821	-5.0993***
	(0.016)	(0.011)	(0.003)	(0.001)	(0.002)	(5.232)	(1.011)
R-sqr	0.902	0.916	0.661	0.605	0.318	0.645	0.392
Panel B: Omnibus Acts							
Omnibus Act	-0.0042	-0.0035	0.0022	0.0039***	0.0009	3.6966	1.7394*
	(0.007)	(0.009)	(0.002)	(0.001)	(0.002)	(3.137)	(0.940)
Latina Mother	0.1383***	0.0983***	0.0008	0.0018***	0.0090***	-5.2270***	5.4130***
	(0.006)	(0.004)	(0.001)	(0.000)	(0.001)	(1.563)	(0.409)
Interaction	-0.0497***	-0.0169*	-0.0177***	-0.0046***	0.0043	14.8028*	0.3688
	(0.011)	(0.009)	(0.002)	(0.001)	(0.003)	(7.851)	(2.911)
R-sqr	0.902	0.916	0.662	0.605	0.318	0.645	0.389
Panel C: 287G Agreements							
287G Agreement	0.0110	0.0025	-0.0055***	-0.0019	-0.0012	-4.6624	-0.1411
	(0.012)	(0.012)	(0.002)	(0.001)	(0.001)	(3.667)	(0.658)
Latina Mother	0.1323***	0.1074***	-0.0022*	0.0019***	0.0091***	-4.5855**	5.8704***
	(0.008)	(0.004)	(0.001)	(0.001)	(0.001)	(2.145)	(0.495)
Interaction	0.0122	-0.0365***	0.0075***	-0.0014	0.0004	0.6046	-1.6453
	(0.016)	(0.009)	(0.003)	(0.001)	(0.002)	(4.979)	(1.115)
R-sqr	0.902	0.916	0.661	0.605	0.318	0.645	0.389
Panel D: Everify Policies							
E-Verify	0.0086	0.0114**	0.0006	0.0006	-0.0004	-3.7036*	0.3081
	(0.005)	(0.005)	(0.001)	(0.001)	(0.001)	(2.027)	(0.354)
Latina Mother	0.1487***	0.1053***	0.0023*	0.0017***	0.0082***	-8.1198***	5.2288***
	(0.007)	(0.004)	(0.001)	(0.001)	(0.001)	(1.805)	(0.524)
Interaction	-0.0423***	-0.0254***	-0.0081***	-0.0006	0.0036***	11.9439***	0.6497
	(0.010)	(0.006)	(0.002)	(0.001)	(0.001)	(3.053)	(0.781)
Mean	0.592	0.553	0.936	0.983	0.087	3215.873	17.236
R-sqr	0.903	0.916	0.662	0.604	0.318	0.646	0.389
Number of Births	4986378	4986378	4986378	4986378	4986378	4986378	15537103

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2017. Columns 1-6 further restrict the sample to first time births, and Column 7 uses all births to examine the birth rate. I compare outcomes of US born non-Latina mothers and US born Latina mothers. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

Table A.6: Heterogeneity in Deportation Timing Among US Born Latinas

Cumulative Deportations:	1^{st} Trimester and 3 months prior	2^{nd} and 3^{rd} Trimester	2 years before the birth	3 years before the birth	5 years before the birth
	(1)	(2)	(3)	(4)	(5)
Panel A: WIC Participation					
Interaction	0.0103***	0.0100***	0.0090***	0.0091***	0.0095***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
R-sqr	0.902	0.902	0.902	0.902	0.902
Panel B: Medicaid					
Interaction	-0.0017	-0.0020	-0.0005	0.0000	0.0007
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
R-sqr	0.915	0.915	0.915	0.915	0.915
Panel C: Prompt Prenatal Care					
Interaction	0.0041***	0.0042***	0.0034***	0.0032***	0.0032***
	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
R-sqr	0.656	0.657	0.657	0.657	0.657
Panel D: Any Prenatal Care					
Interaction	0.0001	0.0001	0.0001	0.0002	0.0002
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-sqr	0.591	0.591	0.591	0.591	0.591
Panel E: Healthy Birth Weight					
Interaction	0.0015***	0.0015***	0.0011***	0.0010***	0.0008***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-sqr	0.318	0.318	0.318	0.318	0.318
Panel F: Average Birth Weight					
Interaction	4.2674***	4.3546***	3.0607***	2.6964***	2.2890***
	(1.158)	(1.173)	(0.858)	(0.802)	(0.760)
R-sqr	0.641	0.641	0.641	0.641	0.641

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of non-Latina US born mothers and US born Latina mothers. Interaction is the interaction between being a Latina mother and log(Deportations). Each specification also include the log(Deportations) and indicator for being a Latina mother. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend. The cumulative measure of deportations per non-citizen vary based on the column heading.

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

Table A.7: Alternative Model of Timing of Deportation Among US Latina Mothers

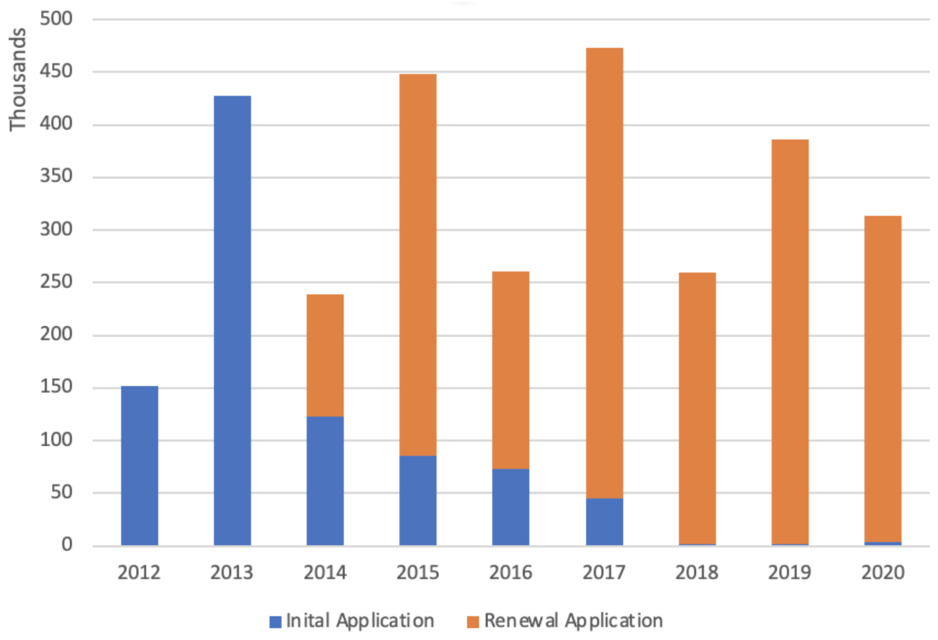
	Safety Net		Prenatal Care		Birth Weight	
	WIC	Medicaid	Prompt	Any	Healthy	Average
	(1)	(2)	(3)	(4)	(5)	(6)
log(Deportations$_{t+1}$)	0.0032	-0.0021	-0.0021	-0.0020*	0.0005	0.5662
	(0.006)	(0.006)	(0.002)	(0.001)	(0.001)	(2.209)
log(Deportations$_{t}$)	0.0040	0.0021	-0.0025	-0.0001	-0.0000	-0.9217
	(0.007)	(0.007)	(0.002)	(0.001)	(0.001)	(2.801)
log(Deportations$_{t-1}$)	0.0079	0.0031	0.0014	0.0011	0.0002	-0.7928
	(0.007)	(0.006)	(0.002)	(0.001)	(0.001)	(2.996)
log(Deportations$_{t-2}$)	0.0074	0.0075	-0.0013	-0.0005	-0.0009	-3.5193
	(0.007)	(0.007)	(0.002)	(0.001)	(0.001)	(2.678)
log(Deportations$_{t-3}$)	0.0246***	0.0163**	-0.0037***	-0.0027***	-0.0005	-4.3878
	(0.007)	(0.007)	(0.001)	(0.001)	(0.001)	(2.681)
Latina Mother	0.1106***	0.0947***	-0.0078***	0.0010**	0.0073***	-8.9269***
	(0.007)	(0.005)	(0.001)	(0.000)	(0.001)	(2.767)
Interaction$_{t+1}$	-0.0171	-0.0182*	0.0015	0.0005	0.0032*	11.5588**
	(0.016)	(0.011)	(0.003)	(0.001)	(0.002)	(4.595)
Interaction$_{t}$	0.0003	-0.0036	0.0028	0.0003	-0.0015	-3.6730
	(0.021)	(0.012)	(0.003)	(0.001)	(0.002)	(5.353)
Interaction$_{t-1}$	0.0010	0.0017	-0.0023	-0.0027**	0.0011	2.2204
	(0.021)	(0.009)	(0.002)	(0.001)	(0.002)	(4.314)
Interaction$_{t-2}$	0.0025	-0.0034	0.0017	0.0006	-0.0011	0.0387
	(0.026)	(0.013)	(0.003)	(0.001)	(0.002)	(5.081)
Interaction$_{t-3}$	0.0240	0.0234**	0.0001	0.0015	-0.0004	-6.6581
	(0.021)	(0.011)	(0.003)	(0.001)	(0.001)	(4.176)

Note: Standard errors in parentheses are clustered by state-year. The sample is restricted to first time mothers with less than a bachelor's degree from the National Center for Health Statistics Natality Files from 2009 to 2018. I compare outcomes of US born non-Latina mothers and US born Latina mothers. Interaction is the interaction between Undocumented mother and log(Deportations) at different year-long horizons. The model includes controls for a mother being married, obtaining less than high school education, and earning a high school degree. All specifications include state, year, and birth quarter fixed effects. Additionally the model controls for a state specific linear time trend.

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

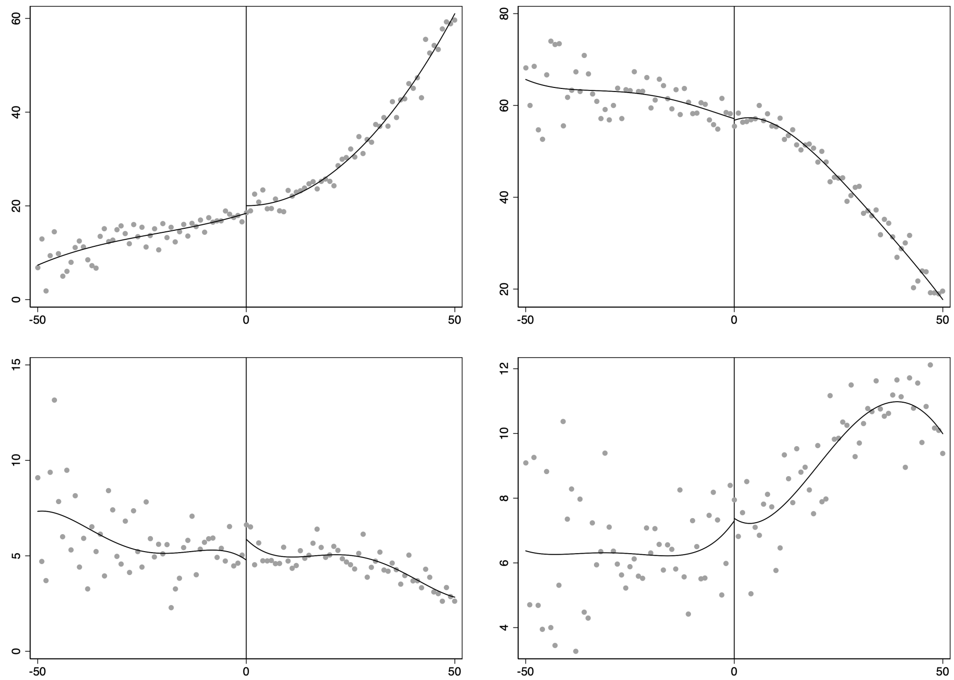
Appendix B: Appendix Figures and Tables

Figure B.1: Applications for DACA, 2012-2020



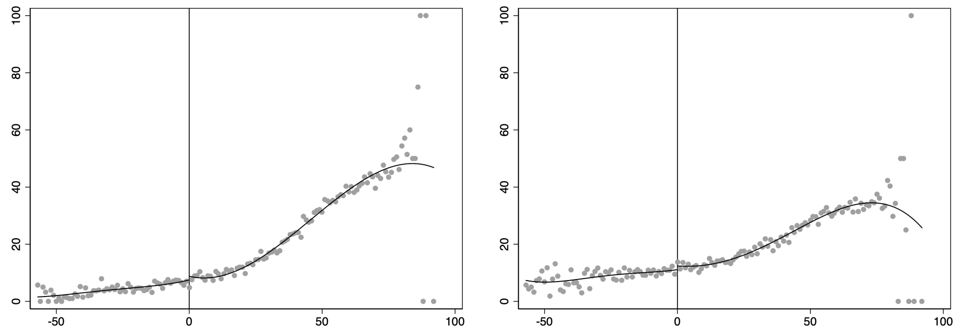
Note: Number of initial and renewal applications for DACA in a fiscal year.
Source: Bruno [59]

Figure B.2: Effect of DACA on Living Arrangements



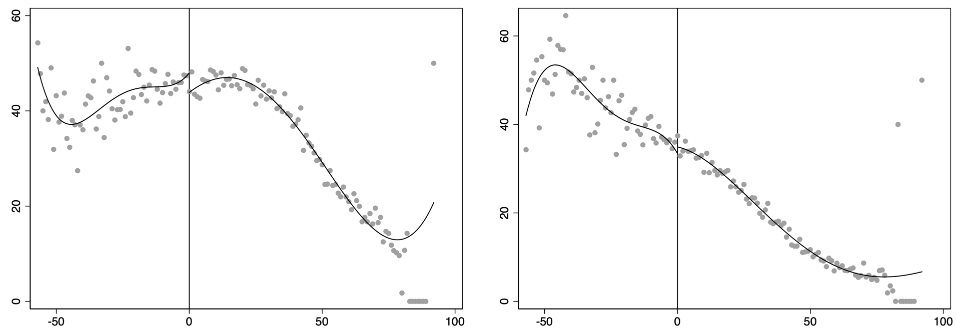
Note: This figure shows the effect of DACA on living arrangements. I defined five mutually exclusive living arrangements. The top left panel depicts the effect of DACA on living with a parent. The top right panel depicts the effect of DACA on living with a partner but without a parent. The bottom left and right panels depict the effect of DACA on living alone and living with non-family members. The effect on the fifth category, living with family members not described in the prior situations, can be found in Figure 3.3. The running variable is birth date centered around 1981Q2. The regression discontinuity is estimated and plotted using a 4-degree polynomial, global fit with a uniform kernel.
Source: American Community Survey, 2012 - 2019.

Figure B.3: Effect of DACA on Housing Choices and Living with a Parent



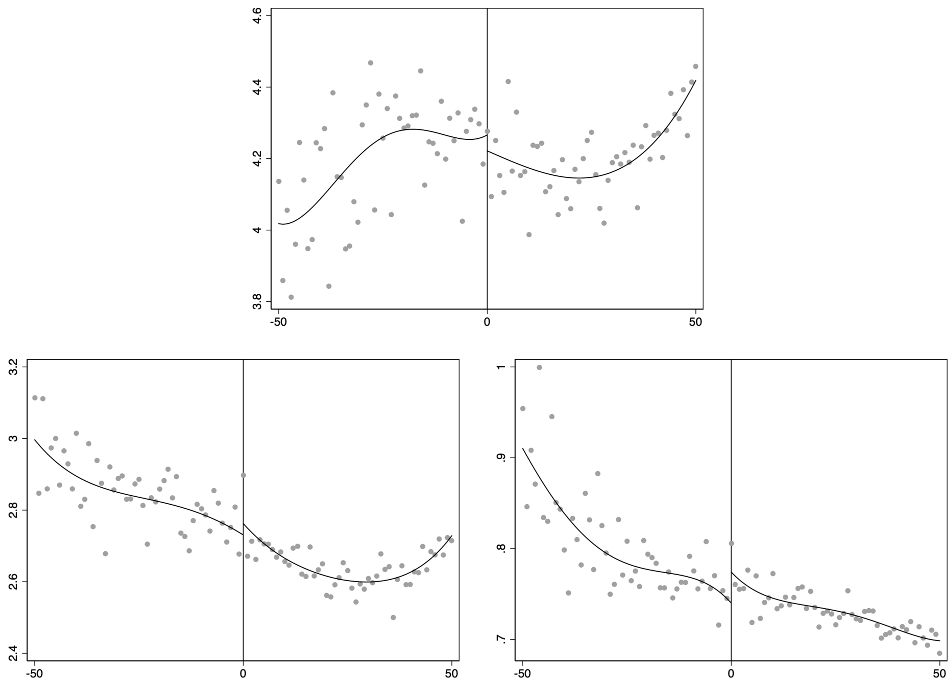
Note: This figure shows the effect of DACA on living with a parent and the status of the home. The left panel depicts the effect on living with a parent in a rented home. The right panel depicts the effect of living with a parent in an owned home. Since I observe the home ownership status at the household level, I cannot determine who in the household owns the home. The running variable is birth date centered around 1981Q2. The regression discontinuity is estimated and plotted using a 4-degree polynomial, global fit with a uniform kernel.
Source: American Community Survey, 2012 - 2019.

Figure B.4: Effect of DACA on Housing Choices and Living without a Parent



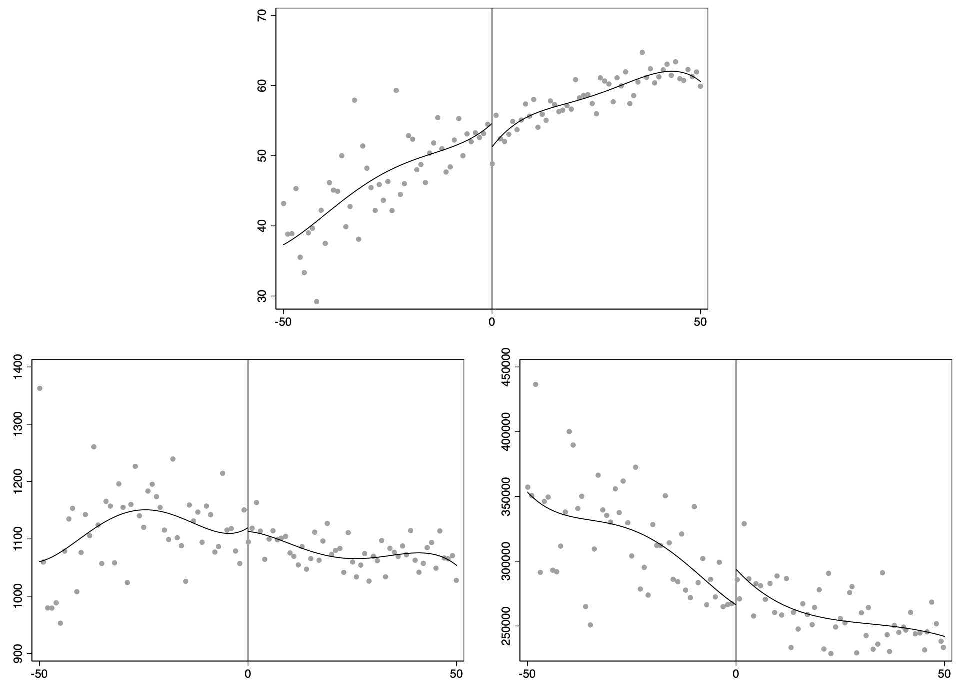
Note: This figure shows the effect of DACA on living without parent and the status of the home. The left panel depicts the effect on living without a parent in a rented home. The right panel depicts the effect of living without a parent in an owned home. Since I observe the home ownership status at the household level, I cannot determine who in the household owns the home. The running variable is birth date centered around 1981Q2. The regression discontinuity is estimated and plotted using a 4-degree polynomial, global fit with a uniform kernel.
Source: American Community Survey, 2012 - 2019.

Figure B.5: Effect of DACA on Housing Characteristics and Size



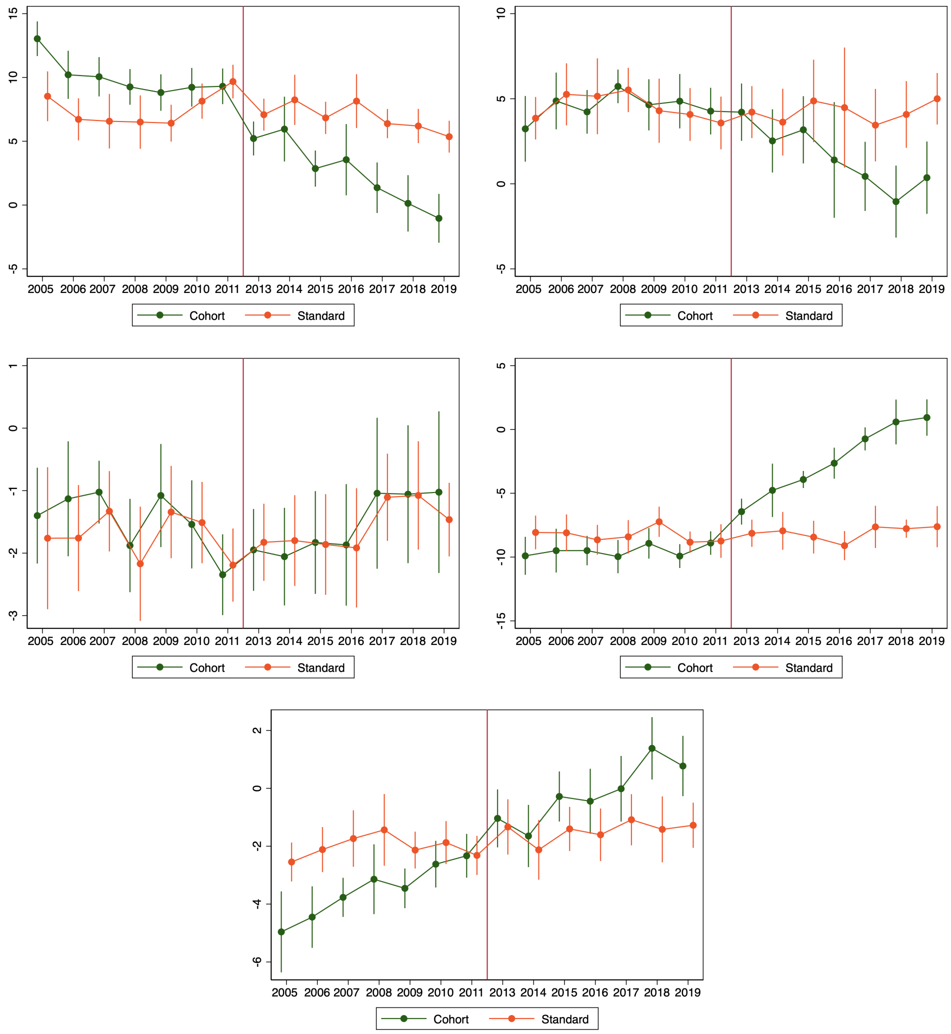
Note: The top panel show the effect of DACA on the number of people in the household. The bottom left and bottom right panels show the effect of DACA of the number of bedroom and the bedrooms per person in the house. The running variable is birth date centered around 1981Q2. The regression discontinuity is estimated and plotted using a 4-degree polynomial, global fit with a uniform kernel.
Source: American Community Survey, 2012 - 2019.

Figure B.6: Effect of DACA on Housing Ownership and Value



Note: The top panel show the effect of DACA on the likelihood the home is rented instead of owned. Since I observe the home ownership status at the household level, I cannot determine who in the household owns the home. The bottom left panels show the effect of DACA among renters on the monthly rent. The bottom right panel shows the effect of DACA among home owners on the home value. The running variable is birth date centered around 1981Q2. The regression discontinuity is estimated and plotted using a 4-degree polynomial, global fit with a uniform kernel.
Source: American Community Survey, 2012 - 2019.

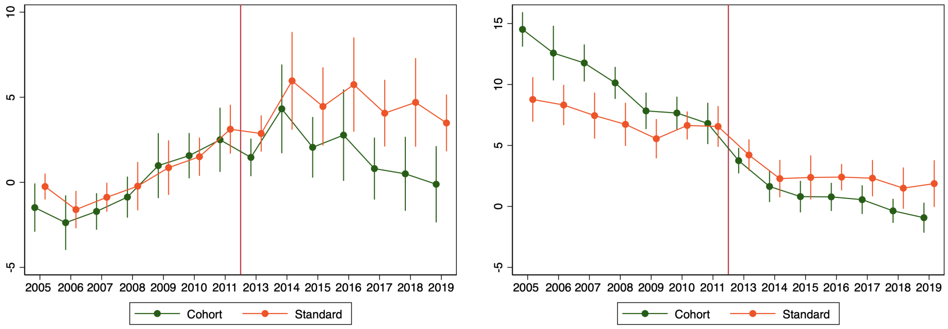
Figure B.7: Effect of DACA on Living Arrangements using a Difference-in-Differences Model



Note: This figure shows the dynamic timing effect of DACA on five mutually exclusive living arrangements: living with a parent (top left), living with a partner but no parent (top right), living alone (middle left), living with non-family members (middle left), and living with family in another arrangement (bottom). The standard method compares eligible and ineligible non-citizens between the ages of 22-35 as if DACA was enacted in the year prior to the survey. The cohort method compares eligible and ineligible non-citizens between the ages of 22-25 in 2012. The model additionally controls for age, age of entry in the United States, educational attainment, Hispanic ethnicity and sex. The model includes year fixed effects, state fixed effects, and a state linear time trend.

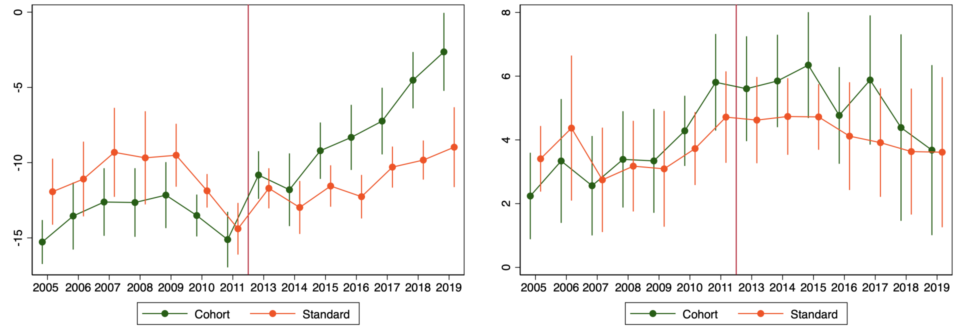
Source: American Community Survey, 2005 - 2019.

Figure B.8: Effect of DACA on Housing Choices and Living with a Parent using a Difference-in-Differences Model



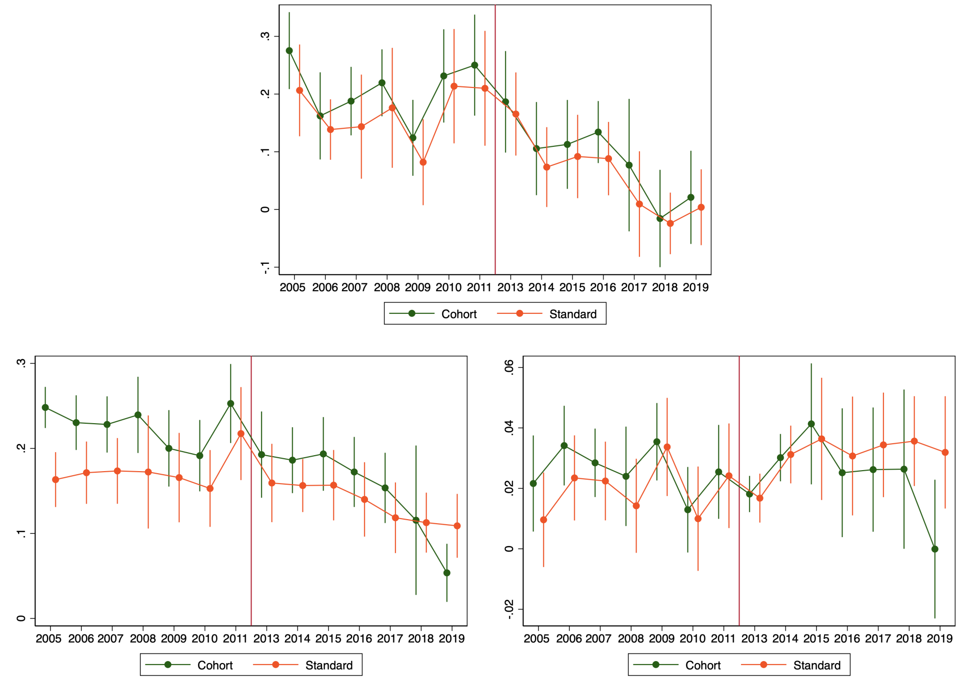
Note: This figure shows the effect of DACA on living with a parent and the status of the home. The left panel depicts the effect on living with a parent in a rented home. The right panel depicts the effect of living with a parent in an owned home. Since I observe the home ownership status at the household level, I cannot determine who in the household owns the home. See Figure B.7 for a general description of the sample and models.
Source: American Community Survey, 2005 - 2019.

Figure B.9: Effect of DACA on Housing Choices and Living without a Parent using a Difference-in-Differences Model



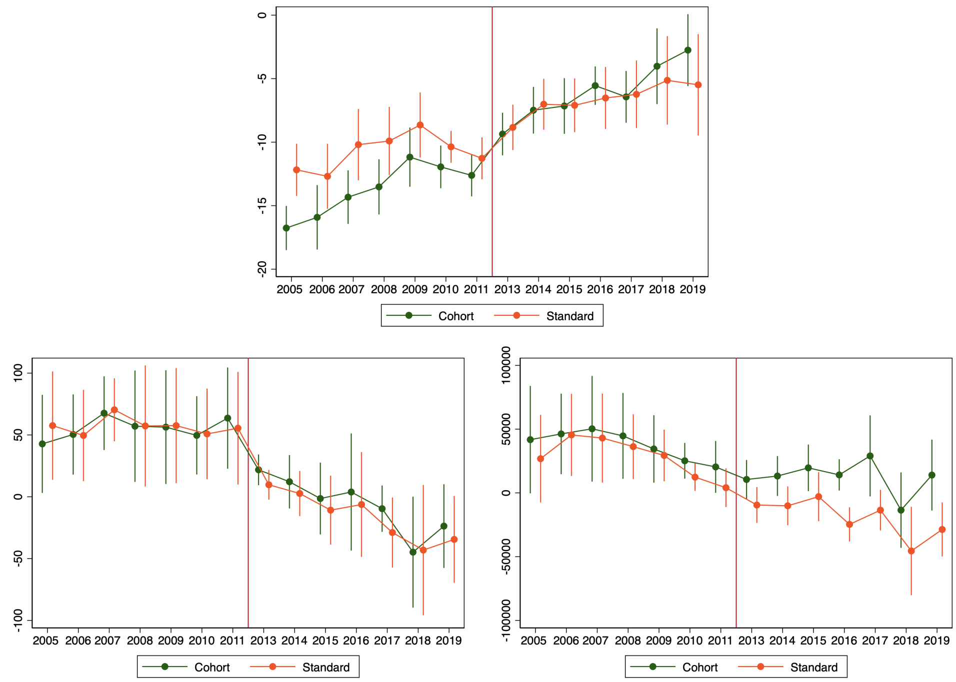
Note: This figure shows the effect of DACA on living without a parent and the status of the home. The left panel depicts the effect on living without a parent in a rented home. The right panel depicts the effect of living without a parent in an owned home. Since I observe the home ownership status at the household level, I cannot determine who in the household owns the home. See Figure B.7 for a general description of the sample and models.
Source: American Community Survey, 2005 - 2019.

Figure B.10: Effect of DACA on Housing Characteristics and Size using a Difference-in-Differences Model



Note: The top panel show the effect of DACA on the number of people in the household. The bottom left and bottom right panels show the effect of DACA of the number of bedroom and the bedrooms per person in the house. See Figure B.7 for a general description of the sample and models.
Source: American Community Survey, 2005 - 2019.

Figure B.11: Effect of DACA on Housing Ownership and Value using a Difference-in-Differences Model



Note: The top panel show the effect of DACA on the likelihood the home is rented instead of owned. Since I observe the home ownership status at the household level, I cannot determine who in the household owns the home. The bottom left panels show the effect of DACA among renters on the monthly rent. The bottom right panel shows the effect of DACA among home owners on the home value. See Figure B.7 for a general description of the sample and models.
Source: American Community Survey, 2005 - 2019.

Table B.1: Baseline Statistics of Immigrants, 2005 - 2011

	Eligible	Ineligible
Working	0.727	0.786
Personal income	21571.1	36304.2
Household income	66168.0	91586.8
High school degree	0.472	0.191
Some college	0.367	0.355
4-year college degree	0.162	0.454
Hispanic	0.632	0.318
Married	0.373	0.451
Male	0.516	0.459
Metropolitan area	0.929	0.951
Living Arrangements		
Renting	51.82	35.44
Household size	4.265	3.628
Living with Parents	36.00	34.24
Living with a Partner	39.48	44.28
Living Alone	4.872	8.653
Living with Roommates	9.324	6.233
Other Living Arrangement	10.32	6.593
Observations	42998	87984

Note: I derive summary statistics among DACA eligible and ineligible immigrants using the 2005-2011 American Community Survey. I restrict the sample to immigration who arrived before the age of 16 and before 2007. The sample is limited to immigrants ages 22-31. With these restrictions, DACA eligibility is only based on citizenship. The ineligible group are naturalized immigrants and the eligible group are non-citizen immigrants. I use this sample in an alternative sample the difference-in-differences analysis.

Table B.2: Effect of DACA on Living Arrangements in Model with Controls

	Parents	Partner	Alone	Roommates	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Optimal Bandwidth Selection					
Born After 1981Q2	1.08	0.74	0.02	-0.88	-2.09***
	(1.04)	(1.29)	(0.79)	(0.80)	(0.63)
Obs.	18265	14300	13019	19625	20945
Bandwidth	±14	±11	±10	±15	±16
	(1)	(2)	(3)	(4)	(5)
	Parents	Partner	Alone	Roommates	Other
Panel B: Consistent Bandwidth					
Born After 1981Q2	2.32	0.68	0.02	-0.11	-2.91***
	(1.37)	(1.32)	(0.79)	(0.97)	(0.58)
Obs.	13019	13019	13019	13019	13019
Bandwidth	± 10	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2019 American Community Survey. I examine the effect of DACA on the following five mutually exclusive living arrangements: living with at lease one parent, living with a partner but without a parent, living alone, living with only non-family members, and other implies living with a different family member in a situation not previously described. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). The model includes dummies for being male, age of entry into the United States, obtaining a high school degree, having some college but less than a four year degree, and Hispanic ethnicity. The specification also has year and state fixed effects. Panel A presents results using the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. Panel B reports results with a consistent bandwidth of 10 birth quarters.

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

Table B.3: Effect of DACA on Housing Choices in Model with Controls

	Living with a Parent		Living with no Parent	
	Rented Home	Owned Home	Rented Home	Owned Home
	(1)	(2)	(3)	(4)
Panel A: Optimal Bandwidth Selection				
Born After 1981Q2	1.89**	-0.40	-1.89	-0.22
	(0.86)	(0.91)	(1.88)	(1.12)
Obs.	14300	20945	14300	18265
Bandwidth	± 11	± 16	± 11	± 14
Panel B: Consistent Bandwidth				
Born After 1981Q2	2.12**	0.19	-2.32	0.00
	(0.97)	(1.14)	(2.00)	(1.50)
Obs.	13019	13019	13019	13019
Bandwidth	$\pm$ 10	$\pm$ 10	$\pm$ 10	$\pm$ 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2019 American Community Survey. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). The model includes dummies for being male, age of entry into the United States, obtaining a high school degree, having some college but less than a four year degree, and Hispanic ethnicity. The specification also has year and state fixed effects. Columns 1 and 2 present results on the effect of living with at least one parent in a rented and owned home. Columns 3 and 4 present results on the effect of living without any parents in a rented and owned home. The model in Panel A uses the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. The model in Panel B uses a consistent bandwidth of 10 birth quarters.

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

Table B.4: RD Results: Effect of DACA on Housing Characteristics

	Household size (1)	Number of Bedrooms (2)	Bedrooms per person (3)	Renting (4)	Monthly Rent (5)	Home Value (6)
Panel A: Optimal Bandwidth Selection						
Born After 1981Q2	-0.06	-0.08*	-0.01	0.47	11.12	12242.10
	(0.06)	(0.04)	(0.02)	(1.45)	(21.95)	(11166.95)
Obs.	19625	19625	13019	19625	9103	10934
Bandwidth	±15	±15	±10	±15	±13	±18
Panel B: Consistent Bandwidth						
Born After 1981Q2	-0.03	-0.07	-0.01	-0.20	21.98	14325.44
	(0.08)	(0.05)	(0.02)	(2.03)	(25.30)	(16779.84)
Obs.	13019	13019	13019	13019	6989	6030
Bandwidth	± 10	± 10	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2019 American Community Survey. I further limit the sample in Column 5 to people living in a rented home and to people living in an owned home in Column 6. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Panel A presents results using the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. Panel B reports results with a consistent bandwidth of 10 birth quarters.

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

Table B.5: Effect of DACA on Living Arrangements, Sub-Sample

	Parents	Partner	Alone	Roommates	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Optimal Bandwidth Selection					
Born After 1981Q2	3.20**	-0.16	-0.50	-0.69	-2.18**
	(1.36)	(1.51)	(0.65)	(1.14)	(0.90)
Obs.	9642	9642	10504	10504	12294
Bandwidth	±11	±11	±12	±12	±14
	(1)	(2)	(3)	(4)	(5)
	Parents	Partner	Alone	Roommates	Other
Panel B: Consistent Bandwidth					
Born After 1981Q2	3.33**	0.01	-0.79	-0.10	-2.45**
	(1.49)	(1.56)	(0.73)	(1.12)	(1.09)
Obs.	8780	8780	8780	8780	8780
Bandwidth	± 10	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2016 American Community Survey. I examine the effect of DACA on the following five mutually exclusive living arrangements: living with at lease one parent, living with a partner but without a parent, living alone, living with only non-family members, and other implies living with a different family member in a situation not previously described. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Panel A presents results using the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. Panel B reports results with a consistent bandwidth of 10 birth quarters.

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

Table B.6: RD Results: Effect of DACA on Housing Choices

	Living with a Parent		Living with no Parent	
	Rented Home	Owned Home	Rented Home	Owned Home
	(1)	(2)	(3)	(4)
Panel A: Optimal Bandwidth Selection				
Born After 1981Q2	3.56***	-0.54	0.08	-1.41
	(0.99)	(0.80)	(1.54)	(1.26)
Obs.	7060	11424	12294	14147
Bandwidth	±8	±13	±14	±16
Panel B: Consistent Bandwidth				
Born After 1981Q2	2.72**	0.61	-1.42	-1.92
	(1.06)	(0.86)	(2.01)	(1.61)
Obs.	8780	8780	8780	8780
Bandwidth	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2016 American Community Survey. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Columns 1 and 2 present results on the effect of living with at least one parent in a rented and owned home. Columns 3 and 4 present results on the effect of living without any parents in a rented and owned home. The model in Panel A uses the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. The model in Panel B uses a consistent bandwidth of 10 birth quarters.

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

Table B.7: RD Results: Effect of DACA on Housing Characteristics

	Household size (1)	Number of Bedrooms (2)	Bedrooms per person (3)	Renting (4)	Monthly Rent (5)	Home Value (6)
Panel A: Optimal Bandwidth Selection						
Born After 1981Q2	-0.09	-0.07	0.00	1.86	72.96***	33713.77
	(0.06)	(0.06)	(0.02)	(1.53)	(22.71)	(20499.00)
Obs.	11424	10504	11424	10504	6870	5808
Bandwidth	±13	±12	±13	±12	±14	±15
Panel B: Consistent Bandwidth						
Born After 1981Q2	-0.02	-0.06	-0.00	1.30	63.71**	40656.89
	(0.07)	(0.07)	(0.02)	(1.78)	(24.73)	(28684.16)
Obs.	8780	8780	8780	8780	4899	3881
Bandwidth	± 10	± 10	± 10	± 10	± 10	± 10

Note: Standard errors in parenthesis are cluster by birth quarter. I restrict the sample to non-citizen immigrants who arrived to the United States before 2007 and before the age of 16 from the 2012-2016 American Community Survey. I further limit the sample in Column 5 to people living in a rented home and to people living in an owned home in Column 6. The running variable is the number of birth quarters before or after 1981Q2, (centered at zero). Panel A presents results using the optimal bandwidth selected using a polynomial of degree one and a triangular kernel. Panel B reports results with a consistent bandwidth of 10 birth quarters.

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

Table B.8: Effect of DACA using Difference-in-Differences Model with Alternative sample

Panel A:	Parents (1)	Partner (2)	Alone (3)	Roommates (4)	Other (5)
DACA × Post	-0.38	0.16	0.29*	-0.21	0.15
	(0.43)	(0.52)	(0.16)	(0.25)	(0.15)
DACA	-6.15***	1.25***	-0.27*	3.17***	2.01***
	(0.52)	(0.22)	(0.16)	(0.26)	(0.17)
R-sqr	0.106	0.119	0.038	0.019	0.014
Obs	225350	225350	225350	225350	225350

		Living with a Parent		Living without a Parent	
Panel B:	Renting (1)	Renting (2)	Home Owner (3)	Renting (4)	Home Owner (5)
DACA × Post	2.13**	2.57***	-2.95***	-0.44	0.82
	(1.01)	(0.72)	(0.51)	(0.42)	(0.60)
DACA	11.66***	2.75***	-8.90***	8.91***	-2.76***
	(0.61)	(0.38)	(0.61)	(0.56)	(0.36)
R-sqr	0.052	0.074	0.066	0.026	0.088
Obs	225350	225350	225350	225350	225350

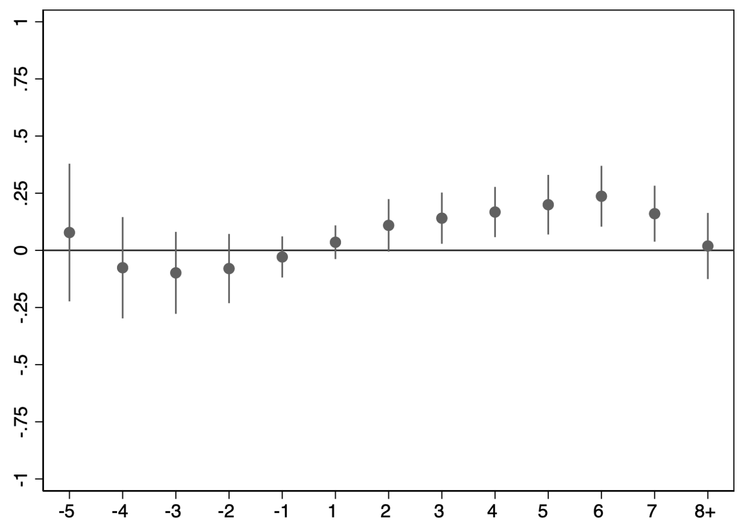
Panel C:	Household size (1)	Number of Bedrooms (2)	Bedrooms per person (3)	Monthly Rent (4)	Home Value (5)
DACA × Post	-0.10***	-0.02**	0.02***	-44.87***	-12043.40**
	(0.02)	(0.01)	(0.00)	(8.60)	(4564.53)
DACA	0.16***	-0.13***	-0.07***	17.65**	-6019.77**
	(0.03)	(0.02)	(0.01)	(7.24)	(2658.68)
R-sqr	0.111	0.024	0.107	0.266	0.200
Obs	225350	225350	225350	108005	117345

Note: Standard errors in parenthesis are cluster by state-year. I restrict the sample to immigrants between the ages of 22 and 31 who arrived to the United States prior to 2007 and before the age of 16 from the 2005-2019 American Community Survey. I compare non-citizen and citizen immigrants to estimate the effect of DACA. Panel A presents the effect of DACA on living arrangements, panel B on housing choices, and panel C on housing characteristics

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

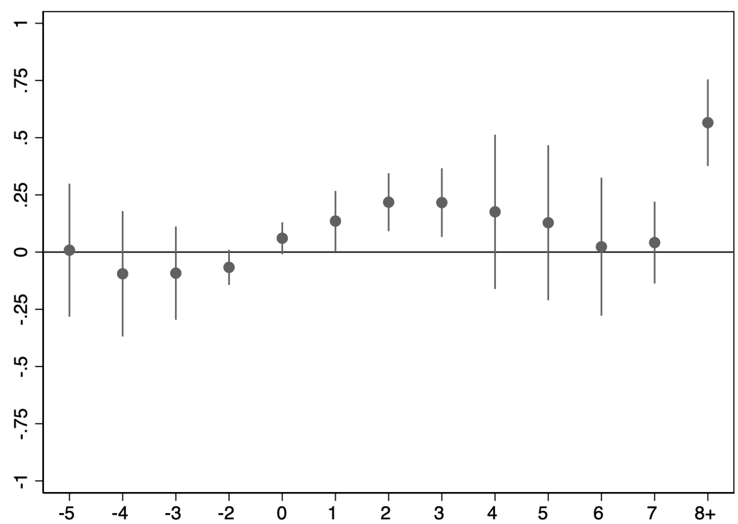
Appendix C: Appendix Figures and Tables

Figure C.1: Effect on Opioid-Related Deaths - Unadjusted with Subsample



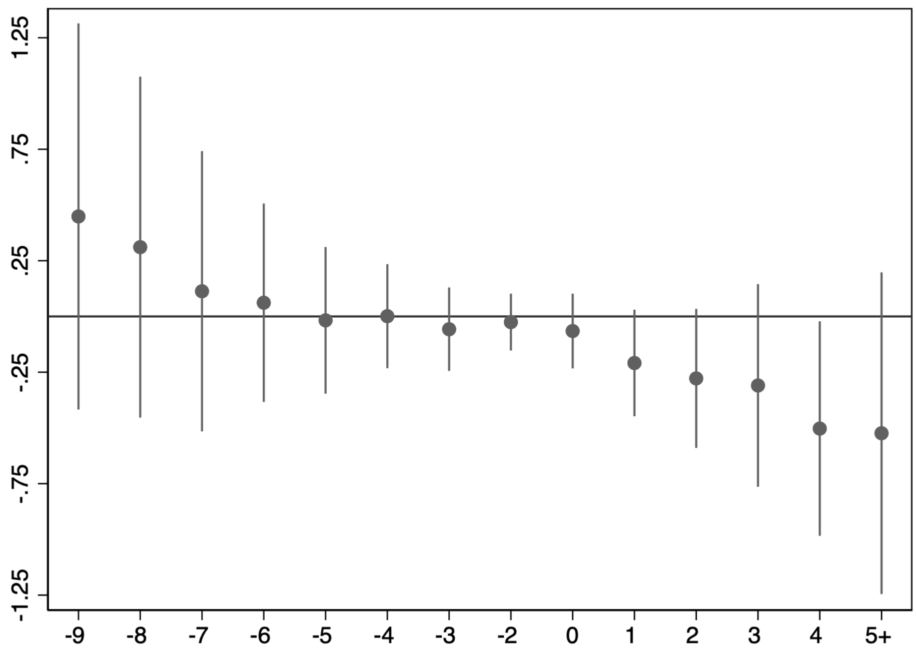
Note: See Figure 4.3 for a general description of the dynamic timing effects model and policy definition. In this figure, I estimate the effect of the policies on the opioid mortality rate using data from 2010-2019. This model does not correct for the the variation in timing of the policy implementation.

Figure C.2: Effect on Opioid-Related Deaths - Adjusted with Subsample



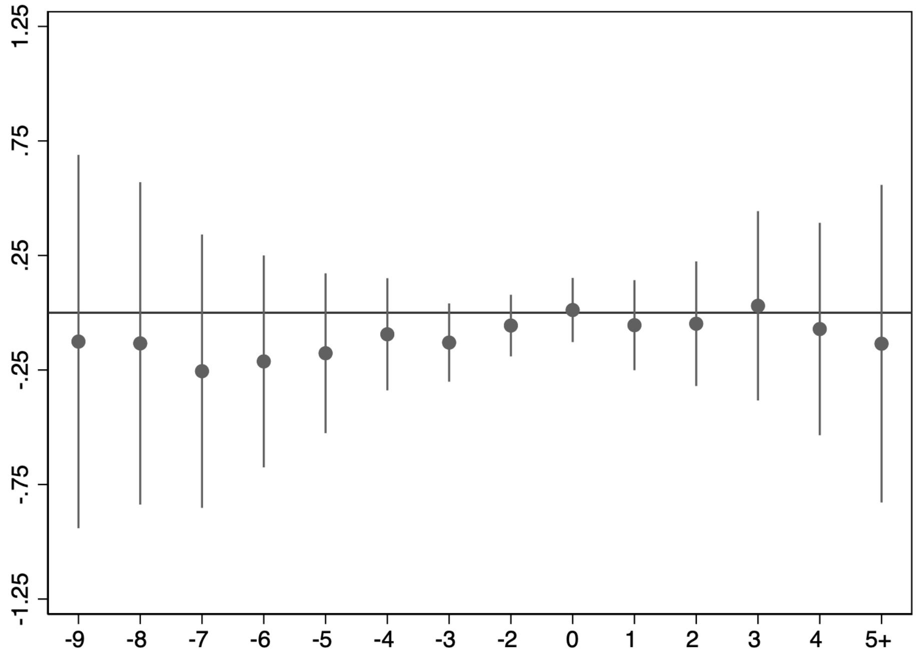
Note: See Figure 4.3 for a general description of the dynamic timing effects model and policy definition. In this figure, I report the estimated effect of the policies on the opioid mortality rate using data from 2010-2019. This model corrects for the the variation in timing of the policy implementation using method developed by Sun and Abraham [17].

Figure C.3: Effect on Opioid Admissions - Adjusted



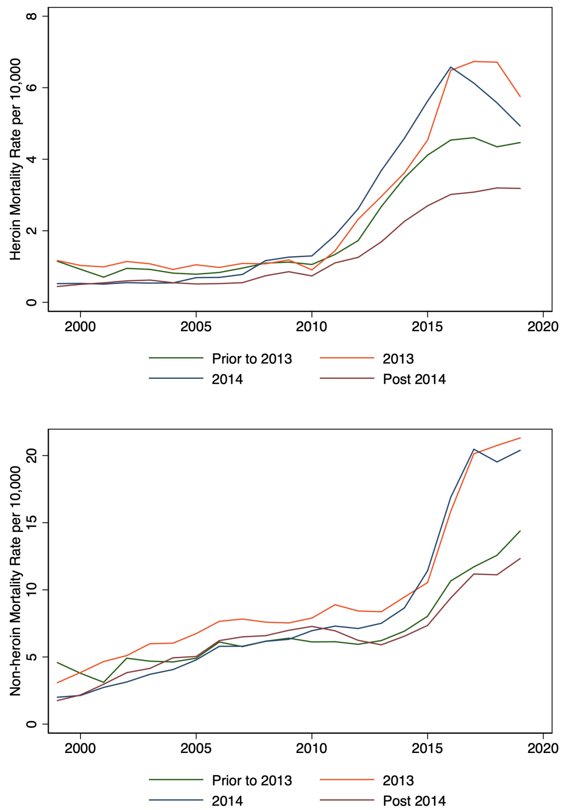
Note: See Figure 4.5 for a general description of the dynamic timing effects model and policy definitions. In this figure, I report the estimated effect of the policies on the rate of admission for opioid substance abuse treatment using data from 1999-2019. This model corrects for the the variation in timing of the policy implementation using method developed by Sun and Abraham [17].

Figure C.4: Effect on Non-Heroin Admissions - Adjusted



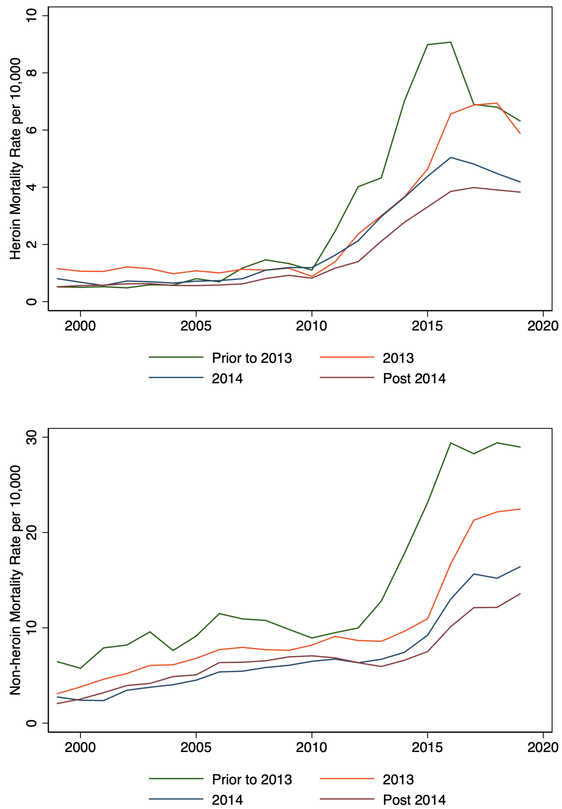
Note: See Figure 4.5 for a general description of the dynamic timing effects model and policy definitions. In this figure, I report the estimated effect of the policies on the rate of admission for non-heroin opioid substance abuse treatment using data from 1999-2019. This model corrects for the the variation in timing of the policy implementation using method developed by Sun and Abraham [17].

Figure C.5: Heroin and Non-Heroin Opioid Mortality by Policy Adoption



Note: The top and bottom panel show heroin mortality and non-heroin mortality rate per 10,000 persons by the adoption date of any opioid mortality prevention policy. Any policy includes Naloxone Access Laws (NALs) regardless of the provisions and Good Samaritan Laws.
Source: Author's calculation using the Center for Disease Control Multiple Cause of Death and reports by the Legislative Analysis and Public Policy Association [79].

Figure C.6: Heroin and Non-Heroin Opioid Mortality by Supply-Side NAL Adoption



Note: The top and bottom panel show heroin mortality and non-heroin mortality rate per 10,000 persons by the adoption date of a supply-side Naloxone Access Law (NAL). The supply-side NAL adoption is limited to only NALs with a provision for either a standing order or a third party prescription.
Source: Author's calculation using the Center for Disease Control Multiple Cause of Death and reports by the Legislative Analysis and Public Policy Association [79].

Table C.1: State Implementation of NALs and GSLs in Literature

State	Supply Side	Standing	Possession	Third	Immunity	GSL
Alabama	Jun-15					
Arkansas	Jul-15					
Alaska						Oct 2014
Arizona						
California	Jan-14	Jan 2014	Jan 2011	Jan 2014	Jan 2008	Jan 2013
Colorado	Apr-15				May 2013	May 2012
Connecticut	Jun-15				Oct 2003	Oct 2011
Delaware	Jun-14	Aug 2014			Aug 2014	Aug 2013
District of Columbia	Mar-13		Mar 2013			Mar 2013
Florida	Jun-15					Oct 2012
Georgia	Apr-14	Apr 2014		Apr 2014	Apr 2014	Apr 2014
Hawaii						
Iowa						
Idaho	Jul-15					
Illinois	Sep-15	Jan 2010	Jan 2010	Jan 2010		Jun 2012
Indiana	Apr-15					
Kansas						
Kentucky	Mar-15	Jun 2013				
Louisiana	Jun-16					
Maine	Oct-15			Apr 2014		
Maryland	Oct-15			Oct 2013		Oct 2014
Massachusetts	Aug-12	July 2014	Aug 2012	Aug 2012		Aug 2012
Michigan	Oct-14			Oct 2014		
Minnesota	May-14	May 2014	May 2014		May 2014	Jul 2014
Mississippi	Jul-15					
Missouri						
Montana						
Nebraska	May-15					
Nevada	Oct-15					
New Hampshire	Jun-15					
New Jersey	Jul-13	Jul 2013	Jul 2013	Jul 2013	Jul 2013	May 2013
New Mexico	Apr-01			Apr 2001	Apr 2001	Jun 2007
New York	Jun-14	Jun 2014	Jul 2014	Jun 2014	Jul 2014	Sep 2011
North Carolina	Apr-13	Apr 2013		Apr 2013	Apr 2013	Apr 2013
North Dakota	Aug-15					
Ohio	Mar-14			Mar 2014	Mar 2014	
Oklahoma	Nov-13			Nov 2013		
Oregon	Jun-13	Sep 2013		Jun 2013		
Pennsylvania	Nov-14	Nov 2014		Nov 2014	Nov 2014	Dec 2014
Rhode Island	Oct-14	Mar 2014	Jun 2012	Mar 2014		Jun 2012
South Carolina	Jun-15					
South Dakota						
Tennessee	Jul-14	Jul 2014		Jul 2014		
Texas	Sep-15					
Utah	May-14			May 2014	May 2014	Mar 2014
Vermont	Jul-13	Jul 2013	Jul 2013	Jul 2013	Jul 2013	Jun 2013
Virginia	Apr-15			Jul 2013		
Washington	Jul-15		Jun 2010	Jun 2010		Jun 2010
West Virginia	May-15					
Wisconsin	Apr-14	Apr 2014	Apr 2014	Apr 2014	Apr 2014	Apr 2014
Wyoming						

Note: This table lists the policy dates used in the papers by Doleac and Mukherjee [15] in the first column and Rees et al. [14] in the remaining columns.

Table C.2: Replication: Effect of NALs and GSL on Opioid Mortality

	(1)	(2)	(3)	(4)	(5)
Naloxone Access Law	-0.2149***				
	(0.068)				
Good Samaritan Law	-0.1474*				
	(0.077)				
Any Opioid Policy		-0.2405***			
		(0.062)			
Opioid Policy			-0.0972***		
			(0.033)		
Low Policy Adoption				-0.1822***	
				(0.056)	
High Policy Adoption				-0.3762***	
				(0.109)	
Supply NAL Policy					-0.0060
					(0.080)
R-sqr	0.805	0.805	0.803	0.804	0.798
Obs.	816	816	816	816	816

Note: Standard errors in parenthesis are clustered by state and observations are weighted by the state population. I obtain the opioid mortality rate from the Center for Disease Control via the Wonder tool. The sample spans 1999 to 2014, the same sample period used by Rees et al. [14]. See Table 4.4 for a general description of the different policy treatment definitions used in each column. The model controls for Medical Marijuana Laws, Prescription Drug Monitoring Programs, population, the number of police officers, the beer and cigarette tax rates, the college graduation rate, the average per capita income, the minimum wage, and the state unemployment rate. The model includes year and state fixed effects. I take the natural log of the outcome, the opioid mortality rate, therefore the coefficients should be interpreted as the $e^{\beta} - 1$ percent change.

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

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