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Essays on Immigration and Policy

DISSERTATION

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in Economics

by

Derek Christopher

Dissertation Committee:
Professor Matthew Freedman, Chair
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ABSTRACT OF THE DISSERTATION

Essays on Immigration and Policy

By

Derek Christopher

Doctor of Philosophy in Economics

University of California, Irvine, 2022

Professor Matthew Freedman, Chair

I analyze the relationship between immigrant status and an array of economic outcomes with a special focus on undocumented status and housing. I evaluate the role public policy has played in influencing the observed relationships. In the first chapter, I provide evidence that fear of deportation results in search frictions among undocumented immigrants in the rental housing market, leading undocumented immigrants to pay a rent premium and devote a greater share of their income to housing. Making use of a triple differences empirical strategy, I show that sanctuary city policies work to reduce or even eliminate this premium.

In the second chapter, using both difference-in-differences and synthetic control methods, I show that undocumented status is a barrier to homeownership. The Deferred Action for Childhood Arrivals policy (DACA) reduced the existing homeownership gap between undocumented and legal resident immigrants. Additionally, I find that a clarification made by the U.S. Treasury Department in 2003 that expanded the availability of certain financial services to undocumented immigrants led to an increase in the relative number of Hispanic home loan applications in counties with the highest concentrations of Hispanic undocumented immigrants, providing evidence that limitations on access to credit have been at least one factor responsible for the homeownership gap.

In the final chapter, I make use of a fuzzy differences-in-discontinuities empirical strat-

egy to evaluate the consequences of providing Medicaid to low-income immigrants. I find evidence that provision of public health insurance reduces the uninsured rate among low-income immigrants but also crowds out spending on private insurance. Importantly, the reduction in private insurance is driven by immigrants substituting away from purchased (non-group) insurance. I do not find evidence of a similar reduction in employer-sponsored health insurance. Consistent with this finding, I also do not detect reductions in labor supply in response to Medicaid.

Chapter 1

Seeking Sanctuary: Housing Undocumented Immigrants

Seeking Sanctuary:

Housing Undocumented Immigrants

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This paper studies housing market outcomes of undocumented immigrants in the U.S. and explores the mechanisms behind the differential prices such immigrants pay for shelter. I show that undocumented renters pay a premium for housing relative to observably similar, documented, immigrant renters occupying similar housing. Building on theory and suggestive evidence that the premium is the result of search frictions, driven by fear of deportation, I employ a triple-differences strategy to evaluate the impacts of sanctuary city policies on housing market outcomes of undocumented immigrants. I find that sanctuary city policies, which limit immigration enforcement, reduce housing costs of undocumented renters, suggesting such policies mitigate search frictions for this group.

Keywords: Rental housing, undocumented immigrants, sanctuary cities

JEL Classification: R3, K4, H7, J1

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1.1. Introduction

More than 1 of every 35 people living in the United States is an undocumented immigrant. Most estimates place the number around 11 million, about 10% greater than the population of the state of Michigan. Regardless of how policymakers weigh the personal welfare of individuals often described as "criminal" because of their immigration status, the presence of a group this size impacts the economy in a manner that has important welfare implications for everyone who calls the United States "home."

This paper studies the housing costs of immigrants in the United States and illustrates how such costs depend on legal status and local immigration enforcement policy. The findings suggest that the housing market faced by undocumented immigrants is one characterized by a pervasive fear of deportation. I show that undocumented immigrants pay a premium for rental housing, amounting to hundreds of dollars for the average household each year. Further, I find evidence that suggests the premium is largely attributable to search frictions that arise when undocumented immigrants fear deportation.

It is an established fact that immigration, in general, influences the cost of housing.[1] However, no study to date has isolated the influence of undocumented status on housing costs. If undocumented immigrants - who comprise nearly half of the non-citizen, immigrant population in the U.S. - navigate the housing market differently than other immigrants, then inference on housing market responses to immigration should account for this important heterogeneity in the immigrant population.

There is reason to believe that such heterogeneity plays a role in the housing market faced by immigrants. First, strict enforcement of current immigration policy may create search frictions for undocumented individuals as they navigate the housing market. Second,

[1] See, for instance, Saiz (2003), Saiz (2007), and Saiz and Wachter (2011).

it is possible that landlords engage in price discrimination or implement their own policies (e.g. requiring background checks) that lead to the creation of separate housing markets for undocumented immigrants. In either of these cases, the general equilibrium implication is higher prices for housing (at least for undocumented immigrants) and a socially inefficient allocation of the housing stock.

A primary focus of this study is to identify the existence of heterogeneity in rents by immigration status and shed light on the mechanisms responsible.[2] Making use of household level data from the American Community Survey and an imputation procedure to predict undocumented status, I show that undocumented immigrants pay higher rents than similar legal resident immigrants, and I provide evidence that search frictions are a driving force behind the observed premiums. If, as I propose, fear of deportation or formal participation in the housing market restricts search, then policies that alleviate such fears should work to mitigate the search frictions, resulting in a reduction in the rent premium. Exploiting geographic and temporal variation in the implementation of sanctuary city policies (that reduce fear of deportation among undocumented immigrants), I find that such policies work to equalize the rents of immigrants in multi-unit housing and reduce the fraction of income undocumented immigrants devote to rent by about 3.5%.

Finding that sanctuary city policies have such an impact on the housing market, I emphasize the importance of considering the housing market implications of immigration policy and enforcement. Immigration policy is frequently implemented to address concerns about crime, employment, and wages, and characterizations of the effectiveness of such policies often focus on these outcomes. My findings suggest that policymakers should also care-

[2]I focus exclusively on rents and not house prices for two primary reasons. First, due in part to their lack of access to home financing options, undocumented immigrants are nearly twice as likely to be renters as they are to live in owner-occupied housing (about 65% of undocumented immigrants are renters). Second, the period for which I have data is relatively short (6 years), and the analysis considers policy that, for most households, took effect no more than 3 years prior to when I observe them. Because renters move (re-optimize their housing consumption) more frequently, a short sample may reasonably capture policy effects on rents. The same cannot be said for home prices.

fully consider the impacts of immigration policy on markets beyond those more traditionally addressed in studies of unauthorized immigration.

The paper is organized as follows. Section 1.2 establishes context and notes work relevant for the motivation of this study. Section 1.3 describes the data and procedures used to achieve a sample of the likely undocumented population. Section 1.4 presents descriptive findings of the relationship between undocumented status and rents. Section 1.5 makes use of sanctuary city policies in a triple-differences framework to provide quasi-experimental evidence for the undocumented status rent premium. Section 1.6 addresses identifying assumptions and tests the robustness of the findings. Section 1.7 concludes.

1.2. Background and Conceptual Framework

1.2.1. Immigration, Policy, and Welfare

An oft-neglected variable in broad analyses of the welfare implications of immigration and immigration policy (especially compared to the attention given to employment or income) is housing. Saiz (2007) articulates this point well. He finds that an immigration inflow equal to 1% of the initial metropolitan area population is associated with increases in rents and housing values by roughly 1%. In his study, he notes that the impact of immigration on purchasing power through its effects on rents is “an order of magnitude bigger” than its effect through the labor market. Any discussion about the welfare implications of immigration that limits its focus to wages and crime (as is common today) neglects important other channels. Failure to consider impacts on the housing market would appear to be an especially consequential omission.

I contribute to the developing literature on the economic implications of unauthorized immigration by investigating and providing an initial characterization of the rental housing market faced by undocumented immigrants. Saiz faced data limitations that prevented him from thoroughly analyzing whether undocumented immigrants differentially influenced his estimate of immigration on area rents and home values. This is a possibility that warrants some attention. Are his results generalizable to all immigrant groups?

Borjas (2002) makes a point that national origin and residential location choices made by different immigrant groups are understudied but important explanatory variables in explaining housing market outcomes. If the broader (perhaps oversimplified) conclusion is simply that different immigrant groups navigate housing markets in different ways, then we should expect undocumented immigrants to have unique housing market outcomes. This seems especially likely when one considers what "residential location choice" means for individuals who are constrained by a lack of documentation. If undocumented immigrants are forced into less desirable, more costly housing, their immobility creates an inefficient allocation of the housing stock in the same way search frictions disrupt optimal labor market outcomes.[3] Amuedo-Dorantes, Bansak and Raphael (2007) concluded that the changes in immigration status (gaining documentation) through the Immigration Reform and Control Act (IRCA) of 1986 may have had positive effects on labor market efficiency by spurring wage growth and eliminating search frictions that impeded job mobility. I provide evidence that undocumented status obstructs similar potential improvements in the housing market.

Bohn, Lofstrom and Raphael (2014) find that the Legal Arizona Workers Act - an immigration policy intended to prevent the employment of undocumented immigrants - resulted in the displacement of Hispanic noncitizens with characteristics strongly associated with undocumented status. Relatedly, Hoekstra and Orozco-Aleman (2017) find that the announcement of Arizona's SB 1070 law (which would criminalize applying for or holding a job

[3]Even if all of the immediate welfare loss from this allocation comes at the expense of undocumented immigrants, there is an abundance of evidence (though not always conclusive) that neighborhood effects, location, and housing affordability have highly consequential implications for aggregate welfare (Bezin and Moizeau (2017) and Chetty and Hendren (2018)).

without legal status and drastically increase the power and responsibilities of law enforcement officers who encounter individuals suspected of lacking documentation) significantly reduced the number of undocumented migrants destined for Arizona. Miles and Cox (2014) evaluated the effect of ICE's Secure Communities policy (strengthening the relationship between ICE and local law enforcement to aid deportation efforts) on crime as it rolled out and found that it is, at best, only effective in inducing small reductions in the rates of burglary and motor vehicle theft and has no effect on violent crime. My evaluation of changes in rents in response to sanctuary city policies (generally, a locality's decision not to cooperate with ICE or the Secure Communities program) serves as another measurement of a potential consequence of such immigration policies.[4]

1.2.2. Search Frictions as a Mechanism

In a model of housing search, a prospective tenant's optimal strategy is characterized both by the expected match value of the available property and the cost of search.[5] If undocumented immigrants expect that, upon visiting a property, there is some non-negligible probability that they will be unable to rent the unit (e.g. because the landlord requires documentation that they do not possess and cannot obtain because of their status), making the realized value of the match 0, their expected return (in terms of improved utility from a successful match) to visiting the property is reduced and the likelihood that visiting the unit is their optimal decision falls.In addition, higher costs of search will reduce the number of housing units visited under utility-maximizing behavior. Importantly, if the cost of search is higher for one type of renter (undocumented), then the optimal search behavior for that type

[4]Increased immigration enforcement has been shown to have consequences for poverty rates of children with likely undocumented parents (Amuedo-Dorantes, Arenas-Arroyo and Sevilla (2018)), consumption (Dustmann, Fasani and Speciale (2017)), voter registration and civic engagement (Amuedo-Dorantes and Lopez (2017)), education (Kuka, Shenhav and Shih (2020)), and others (see, for example, Kubrin (2014)).

[5]See Carrillo (2012). While his model is designed to explain outcomes for owner-occupied housing, I argue that his findings are valid, at least qualitatively, for explaining rental market outcomes as well. He estimates large, non-pecuniary "visiting costs" in the search for housing. While the magnitude of this cost may be lower in the rental market, it is hard to argue that *no* visiting costs exist in rental markets.

will be different. In particular, if undocumented renters face higher search costs because, for example, they risk exposing their status in the process,[6] then the expected value of visiting the property must be sufficiently high to compensate for the additional cost imposed.[7]

In summary, if search costs are heterogeneous by immigration status, then search decisions are heterogeneous by immigration status. If expected match value is a function of the probability that landlords ask for documentation, then search decisions are heterogeneous by immigration status. In either case, because of their status, undocumented immigrants end up restricted to sub-optimal housing units.

1.2.3. Related Studies and Mechanisms

In one of few early applications of search models to the housing market, Courant (1978) develops a model that accounts for racial prejudice and demonstrates that if even some white sellers are unwilling to sell housing to black buyers, equilibria where black buyers pay more for housing are sustainable. I argue that, in the same way, if some landlords refuse to rent to undocumented immigrants (or prohibit them from renting, even unintentionally, through the documentation they require), an equilibrium may arise where undocumented immigrants pay more for housing than similar legal residents.

Several audits and correspondence studies have also confirmed the existence of search

[6]Notably, undocumented immigrants commonly drive unlicensed (often because they lack the documentation necessary to obtain a license). Therefore, any search (visit) that involves driving to a property risks a traffic stop that could be especially consequential for undocumented immigrants (i.e. they may be detained and held until deported). Recently, an increasing number of states have passed laws to allow undocumented immigrants to obtain driver's licenses. See Amuedo-Dorantes, Arenas-Arroyo and Sevilla (2020) for an evaluation of the labor market effects of such laws.

[7]Note that Lach (2007) argues that recent immigrants from the former Soviet Union to Israel have lower search costs and, thus, their immigration reduced the price of various products. In his study's context, the immigrants had authorization, were largely unemployed or out of the labor force (allowing more time for search), and possibly unaccustomed to price dispersion or variety in brands. In the present study, the immigrant group of focus lacks documentation, has high employment rates, and is composed mostly of individuals from countries with similar (capitalistic) market structures to the U.S.

frictions in the housing market, empirically.[8] Often, these studies conclude that racial discrimination leads landlords to show fewer available units to prospective minority tenants, resulting in a restricted supply of available housing to these groups. Because prospective black tenants have fewer units made available to them[9] and receive fewer serious responses to their housing inquiries,[10] they must search much harder than prospective white tenants to find equivalent housing (Yinger (1986)). Additional search costs may yield an optimal stopping rule that leads minority tenants to settle for sub-optimal (lower-quality or higher-cost) housing. If undocumented immigrants face higher search costs, then they would similarly settle for sub-optimal housing.

An audit study conducted by Hanson, Hawley and Taylor (2011) investigates landlord discrimination in a more modern setting. One result of their study is that differential response to housing inquiries is especially pronounced for landlords of apartments and minimal for landlords offering single family homes. The discrimination that results in their finding in the context of race may also be a barrier to search in the context of immigration status. Moreover, if single-unit housing is considered to be a more "informal" segment of the housing market, undocumented immigrants may be more likely to seek out single family homes in the same way they are more likely to participate in informal segments of the labor market.

The studies discussed above present examples of search frictions (or "barriers") in the housing market and illustrate the potential consequences of such inefficiencies. If search frictions are present or if the supply of available housing to undocumented immigrants is less than that of legal residents, then the market may be characterized by undocumented renters competing over a restricted supply of the housing stock, driving up prices paid and preventing sorting into preferred housing units. Conditioning on characteristics for housing

[8]See Yinger (1986) and Page (1995) for audit studies. See Hanson et al. (2016)'s correspondence study for response to owner-occupied housing inquiries. See Phillips (2019) for a recent evaluation of relevant correspondence studies and their measured effects.

[9]See Yinger (1986).

[10]Hanson et al. (2016) show this for requests for information regarding loans for owner-occupied housing, at least.

quality, heterogeneous search frictions will manifest as premiums paid by the group subjected to them.

1.3. Data

The unique circumstances that burden undocumented immigrants in their daily lives also present unique challenges for the researchers who would seek to inform the ongoing debate over the welfare implications of unauthorized immigration in the United States. Some of the earliest contributions made to the literature on estimating the undocumented population come from Robert Warren. Warren has published his methodology in some detail (Warren (2014)). Most widely accepted estimates of the size and characteristics of the undocumented population are based, at least loosely, on the general procedure proposed by Warren and Passel (1987). This includes the Migration Policy Institute (MPI), Pew Research Center, the Center for the Study of Immigrant Integration (CSII) at USC, and even the Department of Homeland Security (DHS). Broadly, the process is to start by creating three categories: citizen, legal permanent resident (LPR), and undocumented. Citizen status is assigned to any individual born in the United States.[11] The rest of the process is dedicated to sorting the remaining individuals into the LPR or undocumented category. All sources listed above begin with a procedure sometimes referred to as "logical edits," though, exactly what the logical editing procedure entails varies by researcher and by data available. I apply logical edits that closely resemble those Borjas (2017) applies to CPS data.[12]

[11] Usually, naturalized citizens are grouped together with LPR's. The analysis in the text of this paper excludes naturalized citizens from any immigrant category, but following a conversation with Emily Owens, who pointed out that non-citizen survey respondents may reasonably believe that they are naturalized citizens (perhaps, based on misconceptions of the process), this categorization is included in Appendix A.6 as a robustness test. Results under this categorization are discussed in the appendix, but the main findings are similar regardless of categorization choice.

[12] I also add a logical edit to account for H-1B visa recipients. Borjas and Cassidy (2019) add such an edit in their more recent paper incorporating an imputation for undocumented status. Thus, the imputation procedure I implement may be more closely related to Borjas and Cassidy (2019) than Borjas (2017) where it is initially implemented.

To ensure a sample large enough to capture undocumented immigrants, I make use of data from the American Community Survey (ACS) provided by IPUMS. The goal of this editing procedure is to "rule out" immigrants as undocumented by examining characteristics that individuals could only have if they were legal residents. Any individual satisfying at least one of the following conditions (and not already assigned citizen status) is classified as a legal permanent resident (LPR):

- Arrived in the United States before 1980[13]
- Is a veteran or currently serving in the U.S. military
- Received public health insurance, Medicaid, Medicare, or VA insurance
- Received any welfare payment, SSI, or Social Security Benefits
- Works in government or in an occupation that requires licensing
- Born in Cuba[14]
- Received food stamps/SNAP[15]
- Arrived in the U.S. as an adult and currently enrolled in undergraduate, graduate, or professional school[16]
- Works in a computer-related occupation, possesses at least a bachelor's degree, *and* has been in the U.S. for no more than 6 years[17]
- Spouse is classified as LPR or citizen

After applying these edits to the 2017 ACS data, my estimate of the undocumented population stands at roughly 11.1 million. By comparison, Pew's 2017 estimate is 10.5 million, the MPI's 2016 estimate is 11.3 million, and the Center for Migration Studies' (Robert Warren) estimate for 2017 is just over 10.6 million. I allow for this relatively small overestimate of the undocumented population. Borjas (2017) also elects to go no further

[13]These individuals are assumed to have achieved legal status through IRCA 1982.

[14]Individuals born in Cuba are likely to be refugees.

[15]Since undocumented parents of U.S. citizens may be eligible for food stamps on behalf of their children, the only time I apply this edit is if the indicator for whether someone in the household received food stamps is true *and* there is only one individual in the household.

[16]This is to account for student visa holders (Pastor and Scoggins (2016)).

[17]This is to account for individuals on H-1B visas.

than the logical edits.[18]

I run the same algorithm on the ACS data from 2012 to 2016.[19] Additional details on how estimates from the imputation procedure I implement compare with other estimates of the undocumented population are presented in Appendix A.1. Broadly, my estimates, as expected, indicate a slight overestimate of the undocumented population. Overestimates are not substantially troublesome as they would indicate that my results understate the true effects of undocumented status (i.e. the "treatment" group is contaminated). Thus, if overestimation is an issue and the subset of individuals I have classified as "undocumented" contains some legal resident immigrants, the estimated effects of undocumented status are biased towards zero and should be interpreted as lower bounds.[20]

At this point, all individuals have been assigned a status. Because my outcome of interest is the amount a household pays for rent, I aggregate a number of personal characteristics to the household level and reduce my sample so that the unit of observation is a household. In the choice specifications, I categorize a household as an "undocumented household" if the

[18]The list of edits I apply differ from Borjas (2017) in three ways. First, since Borjas makes use of CPS data, he has access to a variable indicating whether an individual resides in public housing or receives rental subsidies. The ACS does not contain this information, so I am unable to make a logical edit based on receipt of housing assistance. Second, Borjas does not use receipt of food stamps as a logical edit. I do so, conservatively. Lastly, I attempt to rule out student visa holders. Borjas applies no such restriction. Additionally, Borjas (2017) applies no edit to account for H-1B recipients. In a more recent paper, Borjas and Cassidy (2019) add a similar logical edit and discuss the consequences of its exclusion from the 2017 study.

[19]Some geographic boundaries change from the 2011 data to the 2012 data. Therefore, I primarily rely on data from 2012 and later for the purpose of greater geographic precision. Additionally, the Secure Communities program that established the connections and federal oversight that sanctuary city policies are often designed to restrict only finished rolling out by the beginning of 2013, meaning there were very few sanctuary city policies in existence (or even conceived of as necessary) prior to this time period. Nonetheless, robustness tests (with additional years of data and less geographic precision) are presented in Appendix A.10. 2017 is chosen as the final year due to a decision by ICE in early 2017 to cease reporting jurisdictions that restrict cooperation with the agency (information that I rely on later for the identification of sanctuary cities).

[20]Note that this interpretation only holds if failures of the imputation procedure occur at random. In results not shown, I assess whether the imputation procedure introduces a mechanical bias in my estimates by running a comparable procedure on the sample of citizens (who, of course, may be assigned undocumented status by the imputation but are never truly undocumented). I find no consistent evidence of such bias. If anything, the results would suggest that estimates of the effect I find of undocumented status on rents are biased towards zero.

household head is undocumented. Robustness tests left to the appendix include alternative sample restrictions and alternative definitions of "undocumented household."[21] I then impose a number of sample restrictions with the primary goal of minimizing the frequency with which legal residents are categorized as undocumented.

To ensure that the counterfactual immigrant group (LPR's) shares similar characteristics with undocumented immigrants and to account for the fact that undocumented immigrants cannot have a *years in U.S.* term greater than 37,[22] I drop any immigrant who has lived in the U.S. for more than 37 years. I also exclude any immigrant who has lived in the U.S. for less than 1 year. This exclusion ensures that any visitors or very temporary residents do not drive results.[23] I also exclude individuals with more than a bachelor's degree (to address the abnormal number of post-secondary teachers and scientists classified as undocumented likely because they are missed by the imputation procedure) and anyone currently enrolled in school (living circumstances of typical college students are arguably quite distinct from renters, more broadly). Lastly, I exclude all immigrants from a handful of countries where it is exceptionally difficult to determine immigration status. Singapore and Chile have an agreement with the U.S. that guarantees at least 6,800 H-1B visas (5,400 and 1,400, respectively) are available exclusively to individuals from these countries each year. Burma (Myanmar), Bhutan, Democratic Republic of the Congo, Iraq, and Syria had extremely high numbers of refugees relative to the number of total immigrants in the period of analysis. Therefore, all immigrants from these 7 countries are excluded from the sample. After all edits are applied, the sample is restricted to counties that are identified in the ACS microdata and in which at least 25 undocumented households are observed each year.[24][25]

[21] Namely, see Appendices A.2, A.5, and A.6.

[22] See the logical edit regarding IRCA.

[23] Additionally, many questions in the survey ask respondents for information about the previous year (e.g. individuals are asked what their total income was in the past 12 months). Immigrants who have just moved to the U.S. will then, be offering responses based on their behavior outside of the U.S.

[24] This is to minimize the effect of erroneous assignment of any single household's immigration status and support the asymptotic assumptions of OLS estimators at the county level, where sanctuary city policies tend to go into effect. The inclusion of these counties, though, does not meaningfully change results.

[25] The only relatively large counties excluded by these restrictions are those in the Denver area. In this

Descriptive statistics by immigrant status are presented in Table 1.1. Additional descriptive statistics are left to Appendix A.1. Altogether, I am left with just over one million observations of renter households in 77 counties over the period of 6 years.[26]

The other data source I use is information published by ICE on sanctuary city policies. The data from ICE is a list of localities that have made public statements or enacted policies affirming an unwillingness to cooperate with ICE in at least some circumstances. ICE ceased their updates to the report in early 2017, and the final report contains information that was current as of February 2017. Since the ACS data I use is for the period of 2012-2017, the report covers policies enacted for every year in my sample (excluding a few months in 2017, but as I describe later, policies enacted in the latter half of the year may not have observable effects on rents until the following year, anyway). I have included the first page of the list of uncooperative jurisdictions and a reference to the full report in Appendix A.7.

1.4. The Undocumented Status Rent Premium

In Section 1.4, I first establish that immigrant renters with undocumented status pay more for housing than comparable legal residents. Supplementary analyses shed light on the mechanisms that may be responsible for the observed premium. Building on these descriptive results, in Section 1.5 I provide evidence of the existence of the undocumented status rent premium using a quasi-experimental, triple-differences empirical strategy.

area, PUMA's (Public Use Microdata Areas) frequently cross county lines, and since the PUMA (and state) of residence is the only geographic identifier initially provided in the data, it is often impossible to know whether a household in one of these PUMA's lives in "county A" or "county B."

[26] I do make one assumption about Miami-Dade county. There is a PUMA that crosses into Broward county to the west. Given that Broward county has an estimated population of under 80,000, I have assumed that any observed household in this PUMA that crosses into Broward county is a household that is in the Miami-Dade portion of the PUMA.

1.4.1. Empirical Framework

I run regressions on the ACS data to determine whether undocumented immigrants pay more than legal residents do for similar housing. To mitigate concerns that an observed premium is the result of discrimination against or differential behavior among immigrants in general, restrict my sample of renters to non-citizens. Thus, legal resident immigrants (LPR's) serve as the comparison group.[27] The specification is described by equation (1.1).

$$Rent_{ipt} = \beta_1 undocumented_i + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{1.1}$$

$Rent_{ipt}$ is gross monthly rent for household i in PUMA p in year t.[28] $Undocumented_i$ is an indicator that takes value 1 if the householder is undocumented and 0 otherwise. PUMA (public use microdata area, the lowest level of geography publicly available in the household-level data) and year fixed effects are represented by α_p and γ_t, respectively. X_i is a vector of household-level controls that includes age of householder, age squared, marital status, gender, household income, number of workers in household, number of people in household, number of bedrooms, number of rooms, and dummies for year built (intervalled) and time in residence (intervalled). Importantly, X_i also includes controls for whether the household is living in multi-unit housing ($multi\text{-}unit_i$) and how many years the householder has spent in the U.S. ($years\ in\ U.S._i$).

If there are no factors correlated with undocumented status that also independently affect rent beyond the vector of controls (X_i) and PUMA and year fixed effects, then β_1 can be interpreted as the causal effect of undocumented status on rent. A positive β_1 indicates that undocumented immigrants pay a premium for rental housing. A premium is consistent with the story that high search costs, lower expected returns to searching, and restricted

[27] In Appendix A.2, I run similar regressions on the full sample.

[28] All dollar amounts have been adjusted to 2010 dollars.

supply of housing available to undocumented immigrants limit their ability to sort into optimal housing, causing them to pay more for housing than they would if they had legal resident status.

Next, to support the argument that search frictions are present and driving the observed premium, I include a variable for the interaction of years in the U.S. with undocumented status as well as an indicator for whether the household resides in multi-unit housing and has undocumented status. This is specification (1.2) and the choice specification for this section of the paper.

$$\begin{aligned} Rent_{ipt} = {} & \beta_1 undocumented_i + \beta_2 years\ in\ U.S._i + \beta_3 multi\text{-}unit_i \\ & + \beta_4 (undocumented_i \times years\ in\ U.S._i) \\ & + \beta_5 (undocumented_i \times multi\text{-}unit_i) \\ & + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \end{aligned} \tag{1.2}$$

If search frictions are at work, we might expect to see higher premiums for undocumented renters of multi-unit housing. First, just as informal participation in the labor market may appear safer to undocumented immigrants, less formal participation in the housing market (e.g. negotiating with a single landlord who owns a couple of homes instead of dealing with an apartment complex) may appear safer, increasing the willingness of undocumented renters to search for optimal single-unit housing relative to multi-unit housing or leading them to restrict their choice set of potential rental housing to exclude units in apartment complexes. Second, if undocumented renters view the market for single-unit housing as less formal, they may expect landlords of these units to be more flexible about what documentation they require, decreasing the probability that visiting the property or inquiring further about the unit is futile and increasing their expected return to seeking more optimal housing of this kind.

Third, if apartment complexes are more likely to ask for formal documentation or run background checks,[29] then there is a very real supply restriction that undocumented renters face for these units, specifically. A restriction on the supply of rental housing raises rents paid.[30] Units that are subject to greater supply restrictions should have higher observed premiums.

In specification (1.2), then, we would expect a positive coefficient on the interaction of the indicator for undocumented status and the indicator for multi-unit housing.[31] In other words, if no friction like I have described exists, then the premium that undocumented renters pay for multi-unit housing should be no different than the premium they pay for single-unit homes (i.e. $\beta_5 = 0$).

Additionally, search frictions might be expected to result in higher premiums for undocumented immigrants who are least equipped to navigate the housing market and have had the least amount of time (fewest chances) to engage in any amount of search for housing. As undocumented immigrants adjust to living with their status, they learn of the housing available to them and are able to sort into more appropriate units. If this is the case, we should expect to see the premium fall as undocumented renters spend longer in the U.S. Then, the term in specification (1.2) that captures the effect of the interaction of undocumented status and years in the U.S. (β_4) would be negative (the premium, or effect of undocumented status, diminishes over time).[32]

[29]Rental law requires that any documentation demanded of one applicant must also be demanded of all applicants. The implication is that apartment complexes may be more likely to have in place a standard procedure (standard set of required documents) for determining applicant eligibility because if they don't, they risk violating the Fair Housing Act.

[30]Depending on the elasticity of demand for these units, housing quantity/quality should also decrease if supply is reduced. The covariates in the regression account for housing characteristics, though, so the regression results answer the question of, "how much more do undocumented renters pay for housing with the same characteristics (compared to similar, documented renters)?"

[31]The multi-unit housing variable essentially captures whether the household lives in an apartment or home.

[32]One drawback, though, is that a variable that captures years spent in the United States may be capturing more than just experience in the U.S. housing market (e.g. it will also be correlated with changing immigrant characteristics over time). If this is the case, then *years in U.S.* remains an important control variable, but the interpretation of its effect (and the effect of its interaction with undocumented status) becomes less clear.

Other than the (uninteracted) *years in U.S.*$_i$ and *multi-unit*$_i$ variables (which are now more explicitly included in the specification), all other controls remain the same as in specification (1.1). Note that the coefficient on the *years in U.S.* term (β_2) accounts for the trend in what an immigrant (of either status) pays for rent the longer they stay in the U.S. A negative coefficient is consistent with the story that immigrants need time to adjust to a new housing market before being able to locate more affordable housing. A negative coefficient on the (uninteracted) indicator for multi-unit housing (β_3) simply illustrates that renting an apartment unit is cheaper than renting a home. The additional parameters of interest in specification (1.2) are β_4 and β_5.

1.4.2. Results

Results from specifications (1.1) and (1.2) are presented in Table 1.2. Column 1 includes no controls (this effectively shows the raw, average difference in rents paid by immigrants of different statuses if one ignores omitted variable bias). Column 2 adds fixed effects (to show rent differences after accounting for year and location). Column 3 includes all controls, corresponding exactly to specification (1.1). Column 4 includes the interaction terms and is the choice specification, corresponding exactly to equation (1.2).

As expected, column 3 provides evidence that undocumented immigrants pay a premium for rental housing. In column 4, the positive coefficient on the *multi-unit*$_i$ interaction with undocumented status (β_5) indicates that multi-unit housing, especially, is more expensive for undocumented households. This is consistent with the idea that apartment complexes are more likely to ask for documentation or conduct background checks, restricting the supply of apartments that undocumented immigrants have access to. The negative coefficient on the interaction of years in the U.S. with undocumented status indicates that the premium undocumented immigrants pay decreases as they spend more time in the U.S., consistent with the story that the premium is the result of search frictions that diminish over time. However, the economic significance of this term is debatable (a 62 cent reduction in monthly rent for every year spent in the U.S. is an effect size dwarfed by the observed effect of multi-unit housing, for example).

The magnitude of the coefficients in Column 4 of Table 1.2 suggests that the premium paid by undocumented immigrants is primarily driven by a premium for multi-unit housing. Column 4, the choice specification, indicates that undocumented renters pay a baseline premium of around $14 per month for housing. The more significant finding (both economically and statistically), is that there is an additional premium of $47 per month for undocumented renters of multi-unit housing. In other words, undocumented renters of multi-unit housing spend an additional $700 on housing per year because of their undocumented status. If the theory that this premium is the result of search frictions effectively restricting the supply of housing (especially, multi-unit housing) to undocumented immigrants is correct, then the results of alleviating the search friction should be especially evident for renters in multi-unit housing. The next section provides quasi-experimental evidence from triple-differences specifications to assess this possibility.

1.5. Sanctuary Cities

I now turn my focus to the effects of sanctuary city policies. Section 1.5.1 provides context and motivates the use of sanctuary city policies to further investigate the relationship between undocumented status and rents. Section 1.5.2 formalizes the empirical strategy. Section 1.5.3 presents baseline results for the effect of sanctuary city policies on the rent premium. Section 1.5.4 illustrates that sanctuary city policies may affect rents through more than one channel. To address this, I show results for the effects of sanctuary city policies on both rents and rent as a fraction of income to account for systematically different incomes of undocumented immigrants in sanctuary cities.[33] Section 1.6 validates the parallel trends assumption necessary to interpret the results as causal and discusses other robustness tests.

1.5.1. Background and Conceptual Framework

Between 2008 and 2013, Immigration and Customs Enforcement (ICE) gradually implemented a program called Secure Communities, creating a direct connection between the agency and local law enforcement who may come into contact with undocumented immigrants. With these connections in place, ICE would, in principle, know the whereabouts of any undocumented person booked for any crime anywhere in the United States (so long as they were being detained). Beyond information sharing between ICE and local law enforcement, ICE could also issue "detainers," which are orders (or requests, depending on legal interpretation) for local jails to detain individuals who ICE believed may be unauthorized immigrants, allowing the agency time to question and deport the individuals.[34]

Following the roll out of Secure Communities, local governments, police departments, and jails began enacting policies that restricted or prevented compliance with the program. Such areas have become colloquially known as "sanctuary cities." Sanctuary cities are not well-defined ("sanctuary city" is not a federally recognized designation, but sanctuary jurisdictions are characterized by varying degrees of non-compliance with ICE). For the purpose of this study, a sanctuary city is any jurisdiction that appears on ICE's list of jurisdictions that have enacted policies which restrict cooperation with the agency. Such policies range in scope from a sheriff's public statement of noncompliance with ICE detainers to a change in

[33] Appendix A.3 presents results for effects on movement, and Appendix A.9 presents results for the policies' effects on select other outcomes.

[34] Secure Communities was technically suspended in November, 2014. However, within 2 months, it was replaced with the Priority Enforcement Program (PEP), which was functionally almost identical to Secure Communities. For a more detailed history of the Secure Communities program, how it operates, and a summary of the effects of immigration policy like Secure Communities, see Kubrin (2014).

a jail's policy about continuing to hold arrested individuals beyond a specified time period (regardless of ICE's demands) to local law prohibiting ICE's detention orders from being honored at all.

It is difficult to objectively measure the relative "intensity" of any one sanctuary policy, but importantly, all such policies were public demonstrations of local authorities' refusals to use local law enforcement to help ICE in deporting undocumented immigrants. Even a policy that has no demonstrable effect on actual deportation rates may affect behavior of undocumented immigrants through a change in their *perceived* safety, especially given the public nature and media attention to many of these policies.

By reducing the likelihood (or even just the believed likelihood) that interaction with law enforcement would result in deportation, sanctuary city policies can reduce search frictions. For example, undocumented immigrants may fear that the application process for a new apartment will reveal their status. Anything from going through a background check to driving (usually unlicensed) to view available units imposes an additional cost on undocumented immigrants in the form of deportation risk. In the absence of a sanctuary city policy, the costly risk incurred in the search for new housing may preclude undocumented immigrants from optimizing their housing consumption, resulting in the rent premium they pay. In sanctuary cities, the probability of deportation as a result of minor infractions or having one's status revealed is reduced. In this way, sanctuary cities reduce the expected cost of increased participation in the housing market.

If fear of deportation raises rents by creating search frictions (as the previous section suggests), then mitigating that fear should alleviate the search frictions and reduce the observed premium. Therefore, following the enactment of sanctuary city policies, we would expect to see a reduction in the premium paid by undocumented immigrants through this channel. In this section, I seek to answer two questions. First, are the results from Section 1.4 supported by evidence from a quasi-experimental research design? Second, what effect have sanctuary city policies had on local rents? The triple-differences specification I employ can provide meaningful insight into the impact of immigration policies and, at the same time, serve as a stronger test of the hypothesis that costly searches for housing created by a locality's response to undocumented immigration drive housing market outcomes.

1.5.2. Triple Differences Formulation

I have digitized the most recent file made available by ICE, listing localities that have enacted policies or made statements restricting cooperation with the agency. This file covers all policies and statements made through February 2017.[35] The file includes the month, year, and location of each policy. Since I have both the month and the year in which each policy was enacted but households in the ACS data are observed only annually (i.e. a response in the 2017 data may have been recorded at any time during 2017, but only the year is observable in the public data), I adjust the year of policy enactment to the next year if the policy was enacted in the months of July through December.[36] [37] Equation (1.3)

[35]Fortunately, all but a handful of these policies are enacted at the county (or state) level, and those that are enacted at the city level occur in cities that are identified by my subsample from the ACS. Therefore, I can identify which individuals within my set of identified counties live within the "treated" area. Ultimately, the main sample for this section consists of 70 unique counties, 27 of which ever become sanctuary jurisdictions during the period of analysis and 3 of which contain some residents that are treated because a partially overlapping city became a sanctuary jurisdiction.

[36]For example, King County, Washington enacted an ordinance in September 2014. Since the enactment occurred in September, the policy year is coded as 2015.

[37]The statewide policy enacted by California seems to merely give *permission* to decline detainers issued by ICE. Arguably, any jurisdiction was already able to decline detainers at their discretion (there remains legal debate regarding this point). Since many California counties enact their own, separate policies around the same time and since the state-level policy arguably changed very little, I only assign "treated" status to counties in California that enact their own policy in addition to the state's policy. Thus, while some counties in California could be considered "treated" because of the statewide policy, they are considered untreated in my sample unless they enact a policy or make a statement of their own as well. Note that Appendix A.11 presents results where California is excluded from the analysis. The results are robust to the state's exclusion.

incorporates the policies in a triple-differences framework.

$$
\begin{aligned}
Rent_{ipt} = \; & \beta_1 undocumented_i + \beta_2 treat_{pt} + \beta_3 (treat_{pt} \times undocumented_i) \\
& + \beta_4 (PUMA_p \times undocumented_i) + \beta_5 (Year_t \times undocumented_i) \\
& + X_i \theta + \alpha_p + \gamma_t + \varepsilon_{ipt}
\end{aligned}
\tag{1.3}
$$

$Undocumented_i$ is an indicator for whether individual i is undocumented or not. $Treat_{pt}$ takes value 1 if PUMA p has a sanctuary city policy active in year t. Also included are PUMA by immigration status and year by status fixed effects to account for baseline differences in the premium by PUMA and national trends in the premium over time, respectively. Note that because I have included the $PUMA \times undocumented$ fixed effects, β_1 only captures the premium from the excluded category (PUMA) in the fixed effects. Thus, this specification allows for a different baseline premium from undocumented status in each PUMA. Therefore, the effect of the treatment is interpreted as the average change in premiums across all PUMA's when they experience the treatment.[38] Lastly, the PUMA and year fixed effects and the household-level controls from Section 1.4 are included as well.

The parameter of interest is β_3, which captures the difference that undocumented immigrants pay for rent following the enactment of a sanctuary city policy. In other words, it captures the difference in rents of undocumented immigrants (v.s. legal resident immigrants) in locations that enact sanctuary policies (v.s. locations that don't or haven't yet) after (v.s. before) they are enacted. In this way, $treat_{pt}$ can be thought of as the more standard difference-in-differences term of interest as it captures the difference in rents (for all immigrants) between pre and post periods in locations that get a sanctuary city policy v.s. those that do not. The interaction with undocumented status adds the third difference.

[38]Recall, Public Use Microdata Areas are (usually) smaller geographic boundaries that generally fit entirely within counties with at least 100,000 people. Therefore, when a policy goes into effect at the county level, I can conclude that the PUMA's that comprise the county experience treatment. IPUMS does this identification when preparing their ACS data. Thus, PUMA fixed effects serve as a more geographically precise alternative to county fixed effects, *and* treatment can still be determined at the PUMA level.

A negative and significant β_3 would indicate that the premium paid by undocumented renters is reduced following the enactment of a sanctuary city policy, consistent with the story of alleviated search frictions. A positive β_3 may indicate an increased rental price paid as a result of increased demand for housing among undocumented people in these locations or some other factor.[39]

Equation (1.4) adds an interaction between the treatment variable and the indicator for whether the household lives in multi-unit housing and includes its interaction with the indicator for undocumented status. Thus, β_7 represents a heterogeneous treatment effect in the triple-differences specification. It captures the effect treatment (the sanctuary city policy) has on rents, specifically through the channel of its effect on multi-unit housing, specifically for undocumented immigrants. A negative β_7 is consistent with the idea that undocumented immigrants, on average, pay more for multi-unit housing like apartments *because* the restricted availability (or perception that such units are less available to undocumented immigrants) makes the costly search for housing in these units prohibitively high. Then, sanctuary city policies that reduce those search costs work to reduce the resulting premium specific to multi-unit housing. In other words, undocumented immigrants in sanctuary cities may be more inclined to approach potential landlords of multi-unit complexes now that the

[39]One might argue that locations with sizable undocumented populations will experience lower rents as low-skilled immigration can reduce prices through reduced labor costs (Cortes (2008)). This possibility is unlikely to affect my empirical design. First, the triple-differences design derives its validity from exploiting *differences* in rents. Any effects of a location's initial, existing undocumented population on rents will be captured by fixed effects. Second, if sanctuary city policies are, in fact, implemented in a way that is correlated with changes in the undocumented population and the undocumented population affects rents because their labor supply reduces production costs for new housing, the effect on rents through this channel would apply to all immigrants in the location (meaning such an effect would be captured by the baseline treatment term (β_2), not the parameter of interest where treatment status is interacted with undocumented status (β_3)), and such an effect would have to manifest in the short time span (in terms of housing supply adjustments) between the implementation of the policy and the end of my sample (generally, no more than 3 years).

cost of formally interacting with anyone who might ask about documentation is reduced.

$$
\begin{aligned}
Rent_{ipt} =& \beta_1 undocumented_i + \beta_2 treat_{pt} + \beta_3 (treat_{pt} \times undocumented_i) \\
&+ \beta_4 (PUMA_p \times undocumented_i) + \beta_5 (Year_t \times undocumented_i) \\
&+ \beta_6 (treat_{pt} \times multi\text{-}unit_i) \\
&+ \beta_7 (treat_{pt} \times multi\text{-}unit_i \times undocumented_i) \\
&+ X_i \theta + \alpha_p + \gamma_t + \varepsilon_{ipt}
\end{aligned}
\tag{1.4}
$$

The multi-unit interaction term ($treat_{pt} \times multi\text{-}unit_i \times undocumented_i$) is important for another reason. Sanctuary cities are expected to offset or eliminate existing rent premiums faced by undocumented immigrants. Therefore, β_3 will capture the extent to which sanctuary city policies reduce the premium for all housing units (about \$14 per month if the descriptive results from Section 1.4 are to be believed), and β_7 will capture the extent to which the policies further reduce the premium, specifically for multi-unit housing (about \$47 per month by Section 1.4). However, if sanctuary cities affect undocumented immigrants' rents through a generalized shift in housing demand (e.g. through increased incomes) or a similar channel, that effect should be captured entirely by β_3 unless the other hypothetical channel through which sanctuary cities affect rents is one that also differentially affects multi-unit housing. Thus, in the case that sanctuary cities also shift housing demand of undocumented immigrants, generally, only the measurement of β_3 would be contaminated by such an effect. So, the observed β_3 would be interpreted as the result of the combination of both effects (generally increased housing demand and alleviation of a search friction), but β_7 would continue to be interpreted as the result of alleviated search frictions alone.[40]

[40]As a thought experiment, suppose that sanctuary cities increase undocumented immigrant income (which is positively correlated with rents) and that this is the *only* channel through which sanctuary cities affect rents. In this case, β_3 will be positive, and unless, for some reason, the policy causes income to change differentially depending on what kind of unit one lives in, β_7 will be zero. Thus, while an income effect would bias β_3, β_7 will be free of such bias.

1.5.3. Baseline Results for Rents

Results from equations (1.3) and (1.4) are presented in Table 1.3. The first 3 columns exclude the $(PUMA_p \times undocumented_i)$ and $(Year_t \times undocumented_i)$ fixed effects to allow for a meaningful interpretation of the first-order effect of undocumented status (β_1). Columns 4 through 6 include the interacted fixed effects to account for cross-PUMA and cross-year variation in the baseline (pre-treatment) rent premium to undocumented status.[41] These are the preferred specifications for this section, but results are consistent across columns.

Columns 1 and 4 do not allow for any heterogeneous effects by undocumented status. Column 2 (5) illustrates (again) that first order effects of undocumented status on rent appear to be driven by renters of multi-unit housing. Columns 2 and 5 correspond to equation (1.3). Columns 3 and 6 correspond to equation (1.4) and allow for heterogeneous effects of the treatment on what undocumented immigrants pay for renting multi-unit housing, specifically.

In column 6 (the choice specification for this section), the coefficient on *undocumented*$\times$ *multi-unit* suggests undocumented immigrants pay a \$40 (monthly) premium for multi-unit housing, consistent with the findings in Section 1.4.2. The coefficient on *treat*$\times$*multi-unit*$\times$ *undocumented* indicates that once a sanctuary city policy is in place, though, undocumented immigrants pay \$45 less for multi-unit housing. In other words, the rent premium specific to multi-unit housing is eliminated in sanctuary cities.[42] Also, note that the coefficient on *treat* $\times$ *multi-unit* is insignificant as would be expected, since these policies should have no

[41]Inspection of the data reveals a number of PUMA's where the difference in average rents of undocumented immigrants and legal residents is substantially higher (or lower) than the average premium. To account for these outliers and capture the true change in the baseline premium following the enactment of a sanctuary city policy, it is appropriate to allow different first-order effects (different baseline premiums) for each PUMA. Then, treatment will capture the average change across all treated PUMA's relative to untreated.

[42]With a p-value over 0.72, the hypothesis that undocumented renters of multi-unit housing in sanctuary cities pay the same as their legal resident counterparts (i.e. that the -45.20 and 40.63 simply offset each other and these undocumented renters aren't actually paying *less* for multi-unit housing, now) cannot be rejected at any conventional levels.

effect (at least, directly) on the rents legal residents pay for multi-unit housing, specifically. The null effect observed on the baseline treatment indicator (the effect of a sanctuary city policy on rents on immigrants of any status) is similarly unsurprising.[43]

The consistently positive coefficient on the triple difference term ($treat \times undocumented$), however, would suggest that rents of undocumented immigrants, in general (i.e. for any type of housing - single-unit homes or multi-unit apartments), rise following the enactment of a sanctuary city policy. In fact, it would appear that regressions that disallow treatment to vary by whether one lives in multi-unit housing mask the heterogeneity in the effect of treatment. While columns 1-2 and 4-5 show a smaller (and insignificant in 4-5) effect of treatment on rents, columns 3 and 6 suggest that such an effect arises from offsetting forces; undocumented renters in sanctuary cities pay more for housing in general, but they no longer pay a premium specific to multi-unit housing.

If sanctuary cities eliminate rent premiums specific to multi-unit housing, as we would expect if the policies reduce search frictions that had restricted the effective supply[44] of such housing to undocumented immigrants, why do undocumented immigrants pay more for rent in sanctuary cities? As I discuss in the remainder of this section, sanctuary cities may alleviate search frictions in both the housing market and the labor market. Alleviated frictions in the housing market would result in the reduction of rent premiums (as evidenced by a negative β_7). Alleviated frictions in the labor market could raise incomes, raising demand for housing (as evidenced by a positive β_3).

[43] *A priori*, the direction of the effect captured by this coefficient is unclear. A baseline increase in rents among all immigrants may be reasonable if these policies induce additional demand for the same units legal residents rent. On the other hand, a baseline decrease in rents among all immigrants could arise from general equilibrium effects if these policies increase the efficiency of the housing market. There may also be no effect of these policies on baseline rents of all immigrants if sanctuary city policies truly only matter for the outcomes of undocumented immigrants.

[44] The "effective supply," in this case, is the stock of units for which undocumented immigrants are willing to search, given the additional search costs imposed because of their status.

1.5.4. Effects of Sanctuary Cities Through Other Channels

It is important to consider other implications of sanctuary city policies and how those effects may impact the analysis of the policies' effects on rents. As previously mentioned, Amuedo-Dorantes, Bansak and Raphael (2007) found that awarding documented status to immigrants can alleviate frictions in the labor market and increase income. While sanctuary cities do not award legal resident status to undocumented immigrants, it may be reasonable to think that, if they reduce search frictions in the housing market, they would also reduce search frictions in the labor market. If incomes of undocumented households in sanctuary cities rise, these renters may seek out higher quality, more expensive housing to satisfy their new, expanded budget.[45]

Empirically, it is possible to eschew any effect of increased income on rents by redefining the outcome variable. The appropriate outcome of interest to capture the effects of sanctuary cities on rental housing (net of the effects through increased income) may not be gross rents, but rather, rent as a fraction of household income. This outcome implicitly accounts for any shifts in demand for rental housing driven by changes in income and may be more consistent with the story that undocumented status forces immigrants into suboptimal housing units (i.e. they must allocate more of their income to rent than they otherwise would if they had lower search costs or access to the same set of units that other residents can access).[46]

For the analysis that follows, I must add further restrictions to the sample of renters. First, as household income will be in the denominator of the "rent as a fraction of income" ratio, I exclude any household with zero reported household income. Second, to address

[45] Additionally, Dustmann, Fasani and Speciale (2017) find that undocumented immigrants in Italy have lower levels of consumption than authorized immigrants, even conditional on income (and notably, housing is the good with the largest observed difference in expenditures). They also find that a higher probability of deportation significantly lowers consumption. These results suggest that a policy that reduces deportation risk could increase the (housing) consumption levels of affected undocumented immigrants.

[46] Note also that if sanctuary cities affect income and income affects rent, then income is a bad control in the rent regressions in Section 1.5.3. The result is a positively biased β_3. There is no such bad control when the dependent variable is, instead, rent as a fraction of income.

extreme outliers, I exclude any household that has reported gross rent or household income below the 1st percentile or above the 99th percentile. Then, for simplicity and to limit the scope of my analysis to more standard rental households, I exclude any household that spends more than 100 percent of its household income on rent.[47]

Finally, to test the robustness of my findings and ensure they are driven by renters who are truly undocumented (further testing the reliability of the imputation procedure), I construct a number of additional subsamples on which I repeat all of the analysis. I present 3 separate subsamples (in addition to the "unrestricted" sample just described).[48] The first subsample attempts to address the issue of inordinately high incomes of some (often classified as undocumented, perhaps erroneously) immigrants more directly. For each household, I determine the breadwinner and the income of that individual. I then exclude households where the breadwinner's income is above the 90th percentile or below the 10th percentile. The second subsample restricts to Hispanic households only, which addresses anomalies in the number of undocumented immigrants from European or some Asian countries, for example. The final subsample presented within the text restricts to renters where the household head has no more than a high school diploma or GED, which should account for remaining immigrants on H-1B visas.[49]

[47] There are a number of explanations for why a household's rent expenditure may exceed its income. First, the ACS survey asks individuals to report their total income over the last 12 months. If individuals have recently taken a job that pays more or if more people in the household only recently began working, then their income over the last year would understate what their true monthly income is and will be. Second, households may be breaking into savings or using loans to assist with housing payments. Third, some households may be recipients of aid for housing expenditures, or some other unobserved (unreported) source of income may exist.

[48] Analysis on 3 other subsamples (in addition to the 4 presented in the text) can be found in Appendix A.12.

[49] After imposing restrictions, I again ensure that all counties included in the sample contain at least 25 undocumented renter households each year.

Effect on Rent and Evidence of an Income Effect

First, Table 1.4 presents results from running the regressions given by equations (1.3) and (1.4) on the new samples to confirm that the findings in Table 1.3 are robust to the additional sample restrictions that will ultimately be necessary to evaluate sanctuary cities' impact on rent as a fraction of income. "Unr" (unrestricted) refers to the sample that only excludes households with zero income, rent as a fraction of income greater than 1, or rent or income below the 1st or above the 99th percentile. "Inc" refers to the first subsample (restricted on breadwinner income), "Hisp" refers to the second (only Hispanic households), and "Educ" refers to the third (high school diploma/GED or less).

The results in Table 1.4 paint a familiar picture. In each sample, I find no evidence that (baseline) rents for undocumented immigrants are reduced following the implementation of a sanctuary city policy. However, in the even numbered columns, note the coefficients on *undocumented* $\times$ *multi-unit* and *treat* $\times$ *multi-unit* $\times$ *undocumented*. Regressions on each subsample come to the same conclusion. Undocumented immigrants pay a premium specific to multi-unit housing, but that premium disappears if the household resides in a sanctuary city.

It is possible, however, that incomes of undocumented immigrants are systematically different in sanctuary cities. In fact, running regressions similar to the one specified by equation (1.3) (where gross rent is replaced by monthly household income as the outcome of interest) provides evidence of a positive correlation between sanctuary city policies and income of undocumented immigrants (see Table 1.5). These results illustrate that, in the rent regressions, household income is, econometrically, a bad control, meaning β_3 (the effect of *treat* $\times$ *undocumented*) is not an unbiased estimator of the effect of sanctuary city policies on rent of undocumented households net of income. In fact, β_3 captures the combined effect of sanctuary city policies on rent through both alleviated search frictions and differences in income.[50]

Effect on Rent as a Fraction of Income

If sanctuary city policies are associated with systematically higher household incomes of undocumented immigrants, then income is a bad control in the regressions for gross rent because the treatment affects income, which in turn, affects gross rent. Therefore, one plausible explanation for the positive effect of treatment (for undocumented immigrants) on baseline rents observed in Table 1.3 is that the effect of the policy on rents through its effect on income dominates its effect through alleviated frictions.

If sanctuary cities raise incomes but do not otherwise relieve housing search frictions, then rent as a fraction of income should remain constant (if rising income induces a proportional increase in rent, on average). However, if search frictions that resulted in undocumented immigrants paying premiums for housing are alleviated at the same time, then we would expect re-optimization to induce a reduction in the fraction of income undocumented renters devote to rent.

Table 1.6 presents results for effects on the fraction of a household's income that is spent on rent.[51] Results are obtained by estimating equation (1.5), which is identical to equation (1.3) except that the dependent variable is now rent as a fraction of income and the vector of controls, X_i, no longer includes income. Note that findings are, again, quite consistent across subsamples, bolstering the argument that they are not an artifact of misidentified

[50] A more formal analysis of the effect of these policies on labor market outcomes would (among other considerations) examine individual incomes (not household incomes), expand the sample to include immigrants in owner-occupied housing, and select covariates more carefully than I have. Until such work has been done, I caution the reader against interpreting these regression results as robust evidence of the effect of sanctuary city policies on incomes. However, these regressions do show that incomes of undocumented immigrant renter households are systematically different once a sanctuary city policy has taken effect, which may explain the positive effect of treatment on rents of undocumented households.

[51] For completeness, I also include Table 1.7, which presents results from regressions like those in Section 1.4 but where the outcome is replaced with rent as a fraction of income. Results are, at least, qualitatively similar to those in Table 1.2.

immigration status. If search frictions restricted the supply of housing available to undocumented immigrants, forcing them to devote more of their incomes to rent than they would absent these frictions, then sanctuary city policies that reduce fear (search costs) should allow undocumented immigrants to sort into more ideal housing and reduce the amount of their income they allocate to rent, holding other characteristics constant.

$$
\begin{aligned}
(\frac{Rent}{Income}_{ipt}) &= \rho_1 undocumented_i + \rho_2 treat_{pt} + \rho_3 (treat_{pt} \times undocumented_i) \\
&+ \rho_4 (PUMA_p \times undocumented_i) + \rho_5 (Year_t \times undocumented_i) \\
&+ X_i \theta + \alpha_p + \gamma_t + \varepsilon_{ipt}
\end{aligned} \tag{1.5}
$$

The results presented in Table 1.6 imply that, following the enactment of a sanctuary city policy, the fraction of income undocumented renters devoted to rent, compared to similar documented immigrants, was approximately 1.5 percentage points lower, working to reduce the existing rent premium. In other words, despite rising rents, undocumented tenants' rent as a fraction of income *fell* by roughly 3.5 percent, depending on the choice of sample.

1.6. Identifying Assumptions and Robustness

I present evidence that the parallel trends assumption holds in Section 1.6.1 and evidence that results are not influenced by DACA, which went into effect shortly before most of the households in the sample experience treatment, in Section 1.6.2. In Appendix A.2 I show that the descriptive rent premium is not an artifact of excluding citizens from the analysis. In Appendix A.5 I impose a stricter condition for defining "undocumented households," assuming a household is "undocumented" only if all adults in the household are (as opposed to using the status of the household head). In Appendix A.6 I include naturalized citizens in the group of legal resident immigrants. In Appendix A.11 I systematically remove each of the 4 states with the largest undocumented populations from the sample.[52] Results are, qualitatively, consistent in all cases (even though magnitude and statistical significance do not always perfectly mimic results from choice specifications). Further discussion of these robustness tests is left to the appendix.

1.6.1. Verifying the Parallel Trends Assumption

To rule out the possibility of pre-trends driving the effect on rent as a fraction of income or the observed effects on rents, I run "event-study-style" regressions corresponding to equations (1.6)-(1.8), which are simply extensions of equations (1.3)-(1.5) (respectively), and plot point estimates in Figures 1.1 through 1.3.[53]

$$\begin{aligned} Rent_{ipt} = \alpha_p + \gamma_t + \beta_1 undocumented_i \\ + \beta_2^k (event\ time)_{pt} + \beta_3^k (undocumented_i \times event\ time_{pt}) \\ + \beta_4 (PUMA_p \times undocumented_i) + \beta_5 (Year_t \times undocumented_i) \\ + X_i \theta + \varepsilon_{ipt} \end{aligned} \tag{1.6}$$

[52]The robustness of results upon systematically excluding certain states offers reassurance that the effects of sanctuary city policies are not driven by a single state (the results hold for sanctuary cities all over the U.S.). Additionally, it suggests that the findings are also not the result of some possible state-level change that could have occurred around the same time period (at least for the states of California, Texas, Florida, and New York).

[53]In Appendix A.10, I add data from years prior to 2012 and rerun regressions on the new, extended samples. Event study plots based on this extended sample also produce no apparent pre-trends.

$$
\begin{aligned}
Rent_{ipt} = {} & \alpha_p + \gamma_t + \beta_1 undocumented_i \\
& + \beta_2^k(event\ time)_{pt} + \beta_3^k(undocumented_i \times event\ time_{pt}) \\
& + \beta_4(PUMA_p \times undocumented_i) + \beta_5(Year_t \times undocumented_i) \\
& + \beta_6^k(event\ time_{pt} \times multi\text{-}unit_i) \\
& + \beta_7^k(event\ time_{pt} \times multi\text{-}unit_i \times undocumented_i) \\
& + X_i\theta + \varepsilon_{ipt}
\end{aligned} \tag{1.7}
$$

$$
\begin{aligned}
(\frac{Rent}{Income})_{ipt} = {} & \alpha_p + \gamma_t + \rho_1 undocumented_i \\
& + \rho_2^k(event\ time)_{pt} + \rho_3^k(undocumented_i \times event\ time_{pt}) \\
& + \rho_4(PUMA_p \times undocumented_i) + \rho_5(Year_t \times undocumented_i) \\
& + X_i\theta + \varepsilon_{ipt}
\end{aligned} \tag{1.8}
$$

Event time is defined as $year - policy\ year$, meaning treatment begins at $event\ time = 0$ for all households that experience treatment. The k superscript indicates that a separate estimate is generated in each time period, k. X is the same vector of controls used in the regressions in Section 1.5.2 for equations (1.6) and (1.7) and Section 1.5.4 for equation (1.8) (where income is no longer included as a control).[54] Estimates of β_3^k from equation (1.6) are plotted in Figure 1.1. Estimates of β_3^k and β_7^k from equation (1.7), where treatment effects may vary both by undocumented status and whether one lives in multi-unit housing, are plotted in Figure 1.2.[55]

[54]Effects measured in Figures 1.1 and A.1 should resemble the (aggregated) estimated effects of *treat* and *treat* × *undocumented* in column 1 of Table 1.4. Similarly, effects in Figures 1.2, A.2, and A.3 can be compared to column 2 of Table 1.4, and effects in Figures 1.3 and A.4 can be compared to column 5 of Table 1.6.

[55]In Appendix A.4, I provide figures that also plot the effects of baseline treatment and treatment interacted with multi-unit status (i.e. β_2^k and β_6^k) as further validation of the parallel trends assumption. These figures suggest that the parallel trends assumption also holds for the effect of *treat* (even though this is not the primary term of interest).

No estimate in any pre-treatment period in any event study figure differs significantly from zero, and estimates exhibit no apparent pre-trends that would bias treatment effects. Consistent with findings in Section 1.5.3, Figure 1.2 shows diverging effects of treatment on rent. For undocumented immigrants, rent is rising for all units, but the rent paid specifically for multi-unit housing is falling.[56] Note that, across figures, estimated effects in the earliest periods and latest periods have the largest confidence intervals and may vary greatly in magnitude. This is a result of the specification's reliance on fewer and fewer observations to estimate treatment effects.[57] These outliers do not drive the effects of treatment.[58]

Finally, Figure 1.3 plots estimates of ρ_3^k from equation (1.8).[59] Point estimates in the pre-period exhibit no apparent upward or downward trend and never deviate significantly from zero. In the post period, estimated effects of undocumented status are negative, consistent with regression results in Section 1.5.4.

1.6.2. Exclusion of Households with DACA-eligible Residents

One threat to identification in standard difference-in-differences designs is the possibility that another event occurs around the same time as the treatment and therefore, may influence regression estimates in unobserved ways. Deferred Action for Childhood Arrivals (DACA), a major policy affecting the legal status of hundreds of thousands of young undocumented immigrants took effect in late 2012. DACA certainly affected undocumented immigrants differently than legal residents, and it took effect close to (slightly before) the time many of these sanctuary city policies did. However, because the triple-differences design

[56] Also consistent with Section 1.5.3's findings, Figure A.2 shows that rents of legal residents exhibit no such divergence.

[57] For example, the only households that ever experience 5 periods of treatment are those that are observed in 2017 in locations that had active policies in 2012, whereas effects in period 2 are comprised of effects in 2017 of policies that took effect in 2015, effects in 2016 of policies that took effect in 2014, and so on.

[58] Results are robust to the exclusion of any household that is treated before 2014 or after 2015 (households treated in these two years comprise nearly 80% of households that are "ever treated").

[59] Appendix A.4 provides a figure that also plots ρ_2^k.

I implement makes use of geographic variation (in addition to time and immigration status), DACA is only a threat to identification if it differentially affected undocumented immigrants who were in sanctuary cities relative to those who were not. This seems unlikely as DACA is a federal program available to individuals who meet the eligibility criteria regardless of their location in the country. However, in the case that DACA impacted undocumented immigrants in sanctuary cities differently than it did elsewhere (perhaps jurisdictions that would become sanctuary cities were also better at facilitating DACA take-up), results could not be attributed solely to sanctuary city policies. To address this possibility, I exclude any household in which at least one member meets the (observable) eligibility criteria for DACA. That is, households are dropped if at least one undocumented resident was no older than 31 as of 2012, was no older than 16 when they arrived in the U.S., and arrived in the U.S. no later than 2007. I then rerun all regressions on this new sample. Results are presented in Tables 1.8 through 1.12 and are consistent with previous findings.

1.7. Conclusion

The implications of unauthorized immigration to the United States is a subject of extensive debate. Researchers are presented with a unique challenge in analyzing this particular subset of the population. Undocumented immigrants actively try to avoid detection and lack formal connections to the economy. Thus, there is a rather large segment of the immigrant population with unique characteristics that is often neglected in studies of immigration due to data limitations. Building on a method laid out by Borjas (2017), I applied an adapted imputation procedure to determine undocumented status of individuals in the ACS public-use microdata. Once achieved, I used these estimates to provide empirical support for the theory that search frictions drive undocumented immigrants to pay a premium for rental housing. First, this is a contribution to our knowledge of how undocumented immigrants participate in the market for rental housing. Second, it suggests that studies of immigration and housing markets that fail to account for undocumented immigrants may neglect important heterogeneity.

To provide quasi-experimental evidence of the existence of the premium, I made use of recent sanctuary city policies as sources of variation in fear of deportation among undocumented immigrants. I conclude that these policies alleviate rent premiums faced by undocumented immigrants in multi-unit housing, supporting the notion that sanctuary cities work to equalize rents among immigrants of different statuses. At the same time, sanctuary cities appear to increase housing consumption, at least through higher incomes and increased demand. I show that any induced increase in baseline rents is more than offset by increases in household incomes of undocumented immigrants. My interpretation of this finding is that sanctuary cities allow undocumented households to reassess their housing consumption choices. On one hand, the policies may expand the supply of rental housing that undocumented immigrants believe is available to them and are willing to search for (evidenced by equalizing rents in multi-unit housing). At the same time, the policies may result in higher incomes - another factor to consider when reassessing housing consumption choices. If the policies drive higher baseline rents, then this reassessment story seems most plausible.

There are many avenues for future research. In this paper, I have provided evidence for the existence of barriers (search frictions) that differentially burden undocumented immigrants in the housing market, and I have shown that policy plays a role in how consequential these barriers can be. Future work should further investigate the market consequences of the unique barriers and heterogeneity among immigrants in the long-run and perhaps on a more aggregate scale. For a thorough assessment of the welfare ramifications of the presence of 11 million undocumented people in the United States, studies must also determine if similar barriers exist in other markets, what the consequences of such barriers are, and how policy may influence their existence or consequences.

1.8. Tables and Figures

	LPR	Undocumented	Citizen
monthly gross rent	1064	1080	1126
multi-unit	0.7024	0.7053	0.6841
years in us	16.19	14.73	NA
age	44.36	40.05	46.34
male	0.512	0.5857	0.4386
monthly household income	3440	3649	4360
workers in household	1.418	1.629	1.108
people in household	3.583	3.422	2.254
time in residence*	5.654	4.945	5.519
beds	1.926	1.938	1.948
rooms	3.97	3.98	4.288
married	0.6203	0.4464	0.2863
new housing*	0.2066	0.2074	0.2626
high school diploma	0.5613	0.5485	0.8802
bachelor's degree	0.1624	0.1457	0.2147

Notes: New housing is an indicator for whether the building in which the household lives was built in 1990 or later (the source variable is a broad indicator variable for, roughly, in which decade the building was constructed). The variable for time in residence is an intervalled indicator variable (e.g. less than 1 year, 1-2 years, 2-4 years). I have recoded it as a linear interpolation of these various ranges. The linear interpolation is used to produce the means here, but the original coding as an indicator variable is used in all regressions. Note that, while undocumented households appear to have higher incomes, they also have more workers in the residence contributing to that total. So, while total household income is higher for undocumented renters, the average undocumented worker's income is lower than the average legal resident worker's.

Table 1.1: Means of each variable by immigration status.

	Model 1	Model 2	Model 3	Model 4
undocumented	11.60^{***}	51.06^{***}	38.44^{***}	14.42^{*}
	(3.51)	(5.06)	(3.76)	(7.89)
years in U.S.			-1.25^{***}	-0.97^{***}
			(0.23)	(0.29)
multi-unit			-107.35^{***}	-135.73^{***}
			(5.03)	(6.65)
undocumented × years in U.S.				-0.62^{**}
				(0.32)
undocumented × multi-unit				47.13^{***}
				(6.65)
Year fixed effects	yes	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
Controls	no	no	yes	yes
Adj. R^2	0.01	0.33	0.55	0.55
Num. obs.	111713	111713	111713	111713

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Restricted to non-citizen immigrants. Robust standard errors clustered at the PUMA level. All regressions (in all tables) are weighted using the household weight variable provided in the ACS data.

Table 1.2: Descriptive effect on monthly gross rent.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	29.66^{***}	8.59	2.39			
	(3.78)	(7.96)	(8.16)			
treat	-7.27	-6.32	-1.99	2.02	1.84	3.28
	(6.33)	(6.33)	(9.39)	(7.76)	(7.76)	(10.50)
treat × undocumented	26.23^{***}	24.19^{***}	52.81^{***}	10.83	11.05	45.35^{***}
	(6.40)	(6.34)	(11.92)	(9.18)	(9.19)	(14.01)
years in U.S.	-1.26^{***}	-0.94^{***}	-0.94^{***}	-1.26^{***}	-1.01^{***}	-1.01^{***}
	(0.23)	(0.28)	(0.29)	(0.23)	(0.28)	(0.28)
multi-unit	-107.49^{***}	-134.81^{***}	-133.25^{***}	-107.33^{***}	-125.15^{***}	-124.49^{***}
	(5.03)	(6.60)	(6.88)	(4.98)	(6.23)	(6.60)
undocumented × years in U.S.		-0.69^{**}	-0.71^{**}		-0.51	-0.51
		(0.32)	(0.32)		(0.33)	(0.33)
undocumented × multi-unit		45.42^{***}	54.72^{***}		29.73^{***}	40.63^{***}
		(6.55)	(6.79)		(6.76)	(7.12)
treat × multi-unit			-5.46			-1.90
			(10.30)			(10.20)
treat × multi-unit × undocumented			-38.16^{***}			-45.20^{***}
			(13.50)			(13.75)
PUMA × undocumented fe	No	No	No	Yes	Yes	Yes
Year × undocumented fe	No	No	No	Yes	Yes	Yes
Adj. R^2	0.55	0.55	0.55	0.55	0.55	0.56
Num. obs.	111713	111713	111713	111713	111713	111713

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Robust standard errors clustered at the PUMA level.

Table 1.3: Effect on monthly gross rent from regressions that incorporate sanctuary city policies.

	Unr	Unr	Inc	Inc	Hisp	Hisp	Educ	Educ
treat	8.28	3.03	8.09	−1.20	−3.02	1.16	5.38	5.51
	(7.72)	(10.16)	(7.54)	(10.44)	(8.14)	(10.98)	(7.89)	(10.97)
treat × undocumented	−1.75	31.38**	−8.67	21.51	3.11	25.50*	3.19	24.90*
	(8.54)	(12.78)	(8.71)	(13.37)	(9.18)	(13.47)	(9.07)	(13.40)
undocumented × multi-unit	28.49***	39.89***	24.04***	35.37***	31.57***	38.73***	32.08***	39.63***
	(6.23)	(6.60)	(6.36)	(7.01)	(6.41)	(6.79)	(6.67)	(7.20)
treat × multi-unit		6.99		12.59		−6.18		−0.19
		(9.75)		(9.96)		(10.45)		(10.76)
treat × multi-unit × undocumented		−43.93***		−40.30***		−31.43**		−29.88**
		(12.35)		(12.70)		(13.49)		(13.01)
Adj. R^2	0.57	0.57	0.55	0.55	0.59	0.59	0.57	0.57
Num. obs.	93776	93776	72167	72167	61481	61481	60169	60169

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Standard errors clustered at the PUMA level. First two columns present estimates from the "unrestricted" sample. Columns 3 and 4 restrict the sample based on the breadwinner's income. Columns 5 and 6 restrict the sample to Hispanic immigrants. Columns 7 and 8 restrict the sample to immigrants with high school diplomas or less (GED included).

Table 1.4: Effect on monthly gross rent.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	−218.51***	−134.82***	−99.00***	−58.49***				
	(28.66)	(19.29)	(21.75)	(20.74)				
treat	−179.80***	−107.99***	−127.21***	−97.28***	−81.84	−5.77	−80.80*	−55.45
	(41.78)	(30.43)	(38.29)	(37.34)	(50.09)	(39.98)	(48.61)	(46.81)
treat × undocumented	240.22***	181.15***	188.39***	190.32***	108.44*	37.33	128.70**	132.06**
	(47.31)	(29.81)	(35.82)	(35.27)	(64.52)	(49.28)	(58.69)	(56.59)
PUMA × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Year × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.32	0.39	0.40	0.41	0.33	0.40	0.41	0.41
Num. obs.	93776	72167	61481	60169	93776	72167	61481	60169

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Controls include the same fixed effects as described in equation (1.3). Additional controls are age, age squared, years in the U.S., marital status, gender, number of workers in household, number of people in household, and length of stay in current residence.

Table 1.5: Effect on monthly household income.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	0.0103^{***}	0.0110^{***}	0.0064^{***}	0.0043^{*}				
	(0.0022)	(0.0020)	(0.0023)	(0.0024)				
treat	0.0094^{***}	0.0064^{*}	0.0068	0.0091^{**}	0.0070	0.0048	0.0060	0.0103^{*}
	(0.0035)	(0.0035)	(0.0044)	(0.0043)	(0.0045)	(0.0047)	(0.0059)	(0.0057)
treat × undocumented	-0.0185^{***}	-0.0177^{***}	-0.0161^{***}	-0.0175^{***}	-0.0150^{***}	-0.0154^{***}	-0.0152^{***}	-0.0188^{***}
	(0.0033)	(0.0030)	(0.0037)	(0.0038)	(0.0054)	(0.0054)	(0.0066)	(0.0065)
Sample Mean	0.38	0.38	0.40	0.40	0.38	0.38	0.40	0.40
Sample Median	0.33	0.34	0.35	0.36	0.33	0.34	0.35	0.36
PUMA × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Year × undocumented fe	No	No	No	No	Yes	Yes	Yes	Yes
Adj. R^2	0.2143	0.2697	0.2494	0.2469	0.2193	0.2738	0.2531	0.2508
Num. obs.	93776	72167	61481	60169	93776	72167	61481	60169

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Specifications are identical to the one given by equation (1.3) with the exceptions that the outcome is now rent as a fraction of income, income is no longer included as a control (because it is part of the outcome of interest), and terms for heterogeneous effects of undocumented status have been removed (so that the treatment effects apply to all kinds of housing and all effects of undocumented status are completely captured by the baseline indicator for status and the treatment interacted with status). Columns 4 through 8 include fixed effects interacted with undocumented status.

Table 1.6: Effect on (monthly) rent as a fraction of (monthly) income.

	Model 1	Model 2	Model 3	Model 4
undocumented	-0.0188^{***}	-0.0098^{***}	0.0041^{**}	0.0075^{*}
	(0.0016)	(0.0021)	(0.0019)	(0.0044)
years in U.S.			-0.0004^{***}	-0.0002
			(0.0001)	(0.0002)
multi-unit			-0.0135^{***}	-0.0161^{***}
			(0.0022)	(0.0028)
undocumented × years in U.S.				-0.0004^{**}
				(0.0002)
undocumented × multi-unit				0.0042
				(0.0033)
Year fixed effects	yes	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
Controls	no	no	yes	yes
Adj. R^2	0.0022	0.0389	0.2139	0.2140
Num. obs.	93776	93776	93776	93776

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Compare to Table 1.2.

Table 1.7: Descriptive effect on rent as a fraction of income.

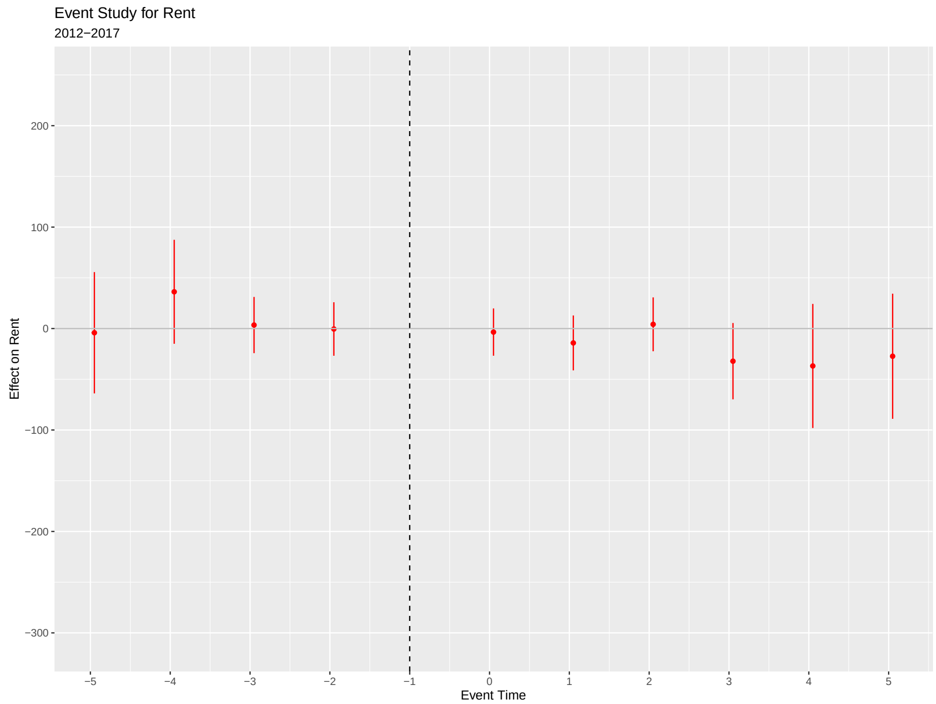

Figure 1.1: Event study plot based on equation (1.6). Plots just β_3 estimates (the effect unique to undocumented households).

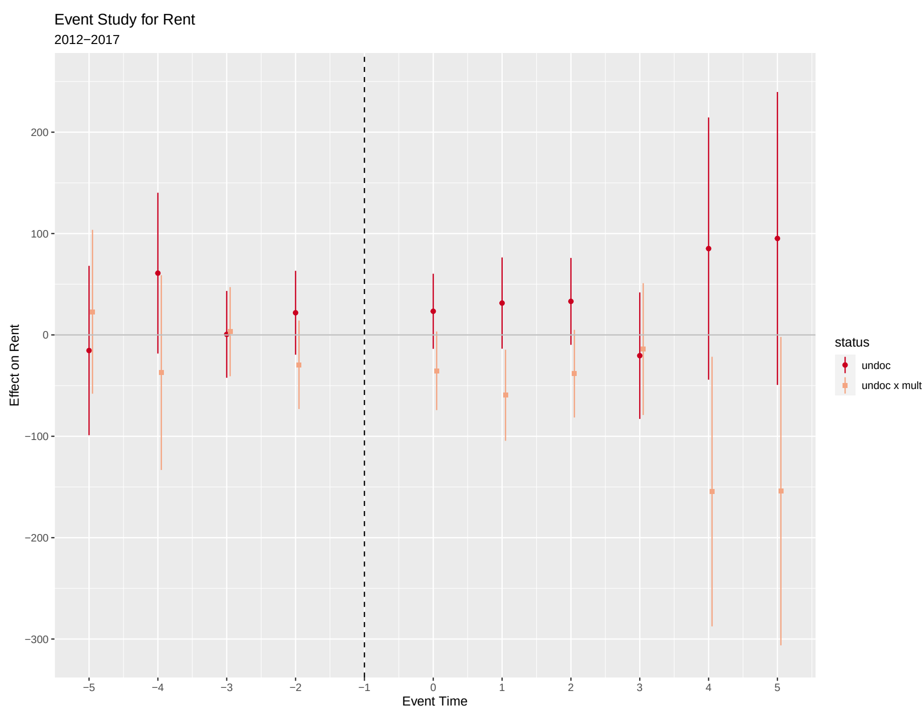

Figure 1.2: Event study plot based on equation (1.7). Plots β_3 (red) and β_7 (orange) estimates.

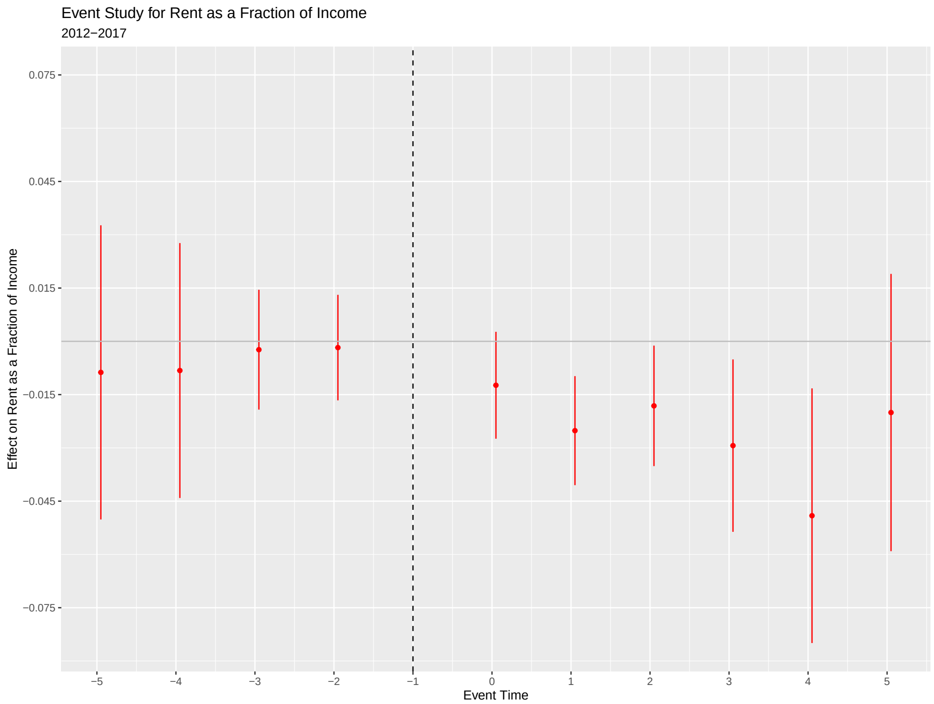

Figure 1.3: Event study plot based on equation (1.8). Plots just ρ_3 estimates (effect unique to undocumented households).

	Model 1	Model 2	Model 3	Model 4
undocumented	8.67^{**}	47.66^{***}	37.03^{***}	1.86
	(3.56)	(5.17)	(3.82)	(7.97)
years in U.S.			-0.74^{***}	-0.68^{**}
			(0.23)	(0.29)
multi-unit			-103.62^{***}	-135.24^{***}
			(5.09)	(6.76)
undocumented $\times$ years in U.S.				-0.21
				(0.32)
undocumented $\times$ multi-unit				53.87^{***}
				(6.75)
Adj. R^2	0.01	0.32	0.54	0.54
Num. obs.	103782	103782	103782	103782

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Compare to Table 1.2.

Table 1.8: Descriptive effect on rent, excluding households with DACA-eligible residents.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	29.62^{***}	-2.85	-8.79			
	(3.83)	(8.04)	(8.33)			
treat	-3.69	-2.33	0.78	3.73	3.67	5.87
	(6.27)	(6.28)	(9.47)	(7.64)	(7.64)	(10.56)
treat × undocumented	22.15^{***}	19.51^{***}	46.35^{***}	10.71	10.84	40.04^{***}
	(6.53)	(6.48)	(11.98)	(9.26)	(9.26)	(14.10)
years in U.S.	-0.75^{***}	-0.66^{**}	-0.66^{**}	-0.74^{***}	-0.74^{**}	-0.74^{**}
	(0.24)	(0.29)	(0.29)	(0.23)	(0.29)	(0.29)
multi-unit	-103.71^{***}	-134.56^{***}	-133.46^{***}	-103.32^{***}	-125.20^{***}	-124.19^{***}
	(5.09)	(6.70)	(7.04)	(5.03)	(6.35)	(6.79)
undocumented × years in U.S.		-0.26	-0.28		-0.04	-0.05
		(0.33)	(0.33)		(0.33)	(0.33)
undocumented × multi-unit		52.58^{***}	61.52^{***}		37.45^{***}	46.77^{***}
		(6.65)	(6.99)		(6.83)	(7.31)
treat × multi-unit			-3.87			-2.89
			(10.30)			(10.21)
treat × multi-unit × undocumented			-35.83^{***}			-38.56^{***}
			(13.64)			(13.76)
Adj. R^2	0.54	0.54	0.54	0.54	0.54	0.54
Num. obs.	103782	103782	103782	103782	103782	103782

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Compare to Table 1.3.

Table 1.9: Effect on rent, excluding households with DACA-eligible residents.

	Unr	Unr	Inc	Inc	Hisp	Hisp	Educ	Educ
treat	7.71	2.84	9.27	-0.01	-2.24	0.68	7.81	7.62
	(7.62)	(10.05)	(7.52)	(10.60)	(8.23)	(11.07)	(8.00)	(10.98)
treat × undocumented	-2.30	25.29^{*}	-9.45	15.40	1.31	20.30	0.43	18.21
	(8.51)	(13.06)	(8.79)	(13.63)	(9.22)	(13.73)	(9.24)	(13.68)
undocumented × multi-unit	32.85^{***}	42.68^{***}	24.07^{***}	33.76^{***}	31.08^{***}	37.61^{***}	29.44^{***}	35.97^{***}
	(6.38)	(6.95)	(6.49)	(7.20)	(6.70)	(7.12)	(6.80)	(7.47)
treat × multi-unit		6.48		12.55		-4.33		0.26
		(9.67)		(10.12)		(10.50)		(10.66)
treat × multi-unit × undocumented		-36.64^{***}		-33.25^{***}		-26.89^{*}		-24.62^{*}
		(12.47)		(12.85)		(13.75)		(13.19)
Adj. R^2	0.56	0.56	0.56	0.56	0.58	0.58	0.56	0.56
Num. obs.	86345	86345	65926	65926	57431	57431	56176	56176

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Compare to Table 1.4.

Table 1.10: Effect on rent, excluding households with DACA-eligible residents.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	-186.18^{***}	-87.82^{***}	-80.97^{***}	-45.82^{**}				
	(28.40)	(18.40)	(22.12)	(20.76)				
treat	-172.46^{***}	-83.32^{***}	-113.75^{***}	-84.59^{**}	-64.63	5.84	-62.77	-49.78
	(41.51)	(30.07)	(38.53)	(37.85)	(48.44)	(38.94)	(48.38)	(46.64)
treat × undocumented	215.22^{***}	142.56^{***}	164.77^{***}	166.25^{***}	67.51	16.66	100.15^{*}	122.31^{**}
	(46.01)	(29.26)	(36.52)	(35.95)	(63.50)	(48.84)	(59.69)	(57.73)
Adj. R^2	0.32	0.40	0.40	0.41	0.33	0.41	0.41	0.42
Num. obs.	86345	65926	57431	56176	86345	65926	57431	56176

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Compare to Table 1.5.

Table 1.11: Effect on income, excluding households with DACA-eligible residents.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	0.0086^{***}	0.0085^{***}	0.0050^{**}	0.0033				
	(0.0022)	(0.0020)	(0.0024)	(0.0024)				
treat	0.0086^{**}	0.0049	0.0057	0.0087^{*}	0.0055	0.0042	0.0054	0.0103^{*}
	(0.0036)	(0.0036)	(0.0045)	(0.0044)	(0.0046)	(0.0049)	(0.0060)	(0.0058)
treat × undocumented	-0.0172^{***}	-0.0153^{***}	-0.0141^{***}	-0.0159^{***}	-0.0124^{**}	-0.0143^{**}	-0.0142^{**}	-0.0184^{***}
	(0.0034)	(0.0032)	(0.0038)	(0.0039)	(0.0057)	(0.0057)	(0.0068)	(0.0068)
Adj. R^2	0.2161	0.2744	0.2502	0.2461	0.2209	0.2781	0.2538	0.2496
Num. obs.	86345	65926	57431	56176	86345	65926	57431	56176

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Compare to Table 1.6.

Table 1.12: Effect on rent as a fraction of income, excluding households with DACA-eligible residents.

Chapter 2

Homeownership in the Undocumented Population and the Consequences of Credit Constraints

Homeownership in the Undocumented Population and the Consequences of Credit Constraints

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I study the relationship between undocumented status and homeownership among immigrants in the U.S. Finding that undocumented immigrants are less likely to own their homes (even conditional on observable characteristics), I assess whether policy has affected the relationship between legal status and homeownership and explore potential mechanisms behind differences in housing tenure outcomes of otherwise similar immigrant groups. I use the 2012 Deferred Action for Childhood Arrivals policy to provide quasi-experimental evidence of the homeownership gap and estimate the impact of the recent immigration policy on housing market outcomes. I supplement the analysis with an evaluation of the legal clarification made in the 2003 changes to Treasury Department rules, explicitly allowing the use of individual taxpayer identification numbers in lieu of social security numbers to establish bank accounts. Comparing the effects of these changes in policy allows for further discussion of the factors that drive the homeownership gap between undocumented immigrants and those with legal status.

Keywords: Undocumented immigrants, homeownership, housing tenure, DACA

JEL Classification: R2, J1, K4, R3

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2.1. Introduction

While the relationship between unauthorized immigration and outcomes such as crime, employment, and earnings dominate much of the current immigration discourse, considerably less attention is given to other important economic implications of undocumented status. This paper sheds light on the role undocumented status plays in the market for owner-occupied housing. Despite housing's role as the biggest contributor to Americans' net worth,[1] evidence of the relationship between undocumented status (a characteristic of 11 million people in the country) and homeownership is limited. A number of sources provide descriptive estimates of the homeownership rate among undocumented immigrants.[2] Fewer provide estimates that adjust for differences in characteristics of undocumented immigrants that may be correlated with homeownership. Without this adjustment, a much different (larger) homeownership gap is observed.[3]

In this study, I first compute nationwide, regression-adjusted estimates of the homeownership gap between undocumented immigrants and legal residents, providing an estimate of how undocumented status influences the probability of homeownership. The results suggest that some, but not all, of the homeownership gap can be explained by differences in characteristics associated with lower homeownership rates. Second, I make use of the timing and variation in the impact of the Deferred Action for Childhood Arrivals (DACA) program to establish that the link between undocumented status and lower homeownership propensities is causal. In other words, there is something unique about undocumented status, itself, that is responsible for differences in homeownership propensities, and the homeownership

[1] See https://www.census.gov/content/dam/Census/library/publications/2019/demo/P70BR-164.pdf.

[2] The Migration Policy Institute (MPI) provides an estimate of the number of undocumented immigrants in owner-occupied housing among other characteristics of undocumented immigrants. Studies, such as Cort (2011), McConnell and Marcelli (2007), and Hall and Greenman (2013) also estimate homeownership rates among undocumented immigrants.

[3] E.g. of the four sources mentioned in the previous footnote, only the last two estimate differences in homeownership rates that account for individual characteristics.

gap can only be partially explained by undocumented status' correlation with characteristics associated with lower homeownership rates.

Finally, to shed light on possible mechanisms responsible for the homeownership gap, I assess the impact of the 2003 change in Treasury Department rules that clarified existing policy and allowed individuals without social security numbers to open bank accounts (using individual taxpayer identification numbers), greatly expanding the number of financial institutions offering mortgages to undocumented immigrants.[4] Though DACA's effect on homeownership could be driven by several factors (increased housing search, higher incomes, etc.), the effect of the Treasury Department's rule change should be driven solely by increased access to credit markets. Thus, making use of these two changes in policy, which are quite distinct from one another in their scope and intent, allows for a more comprehensive analysis of the housing tenure of undocumented immigrants and the factors responsible for their lower rates of homeownership.

I employ 3 variations of a difference-in-differences strategy to holistically investigate the immigrant status homeownership gap and channels through which it arises.[5] The first and second of these variations rely on the implementation of DACA and its impact on undocumented immigrants (substantial for some), relative to legal residents (minimal). As a contribution to the existing literature on the economic implications of the program,[6] I illustrate DACA's impact on the housing tenure of undocumented households and add to the even less prolific literature on the effects of immigration policy on housing market outcomes.[7]

The final empirical strategy makes use of the Treasury Department's 2003 decision to

[4]For more details on the rule change and subsequent changes in lending practices, see Appendix B.6.

[5]For the main outcomes of interest, where the data allow for it, I also run synthetic control. Thus, many of the results are actually supported by 2 different (but related) empirical designs.

[6]See studies by Kuka, Shenhav and Shih (2020), Pope (2016), Amuedo-Dorantes and Antman (2017), and Hsin and Ortega (2018).

[7]Bohn, Lofstrom and Raphael (2014) measure residential location responses of Hispanic noncitizens to the Legal Arizona Workers Act. Christopher (2022) examines changes in rents of likely undocumented households in response to sanctuary city policies.

explicitly permit the opening of bank accounts with an individual taxpayer identification number (ITIN) in lieu of a social security number (and by extension, allow more institutions to offer home loans to individuals who lack social security numbers). Prior to this rule change and clarification of policy on what constitutes valid identification for the purpose of opening bank accounts, undocumented immigrants would only be able to purchase a home if they did so without a formal loan or if they successfully originated a loan with a fraudulent social security number.[8] Therefore, this rule change, while not directly intended to benefit undocumented immigrants,[9] removed a barrier that previously impeded the ability of many to enter the market for owner-occupied housing. The estimated effects of this policy change serve both as robustness tests to the estimated effects of DACA and as evidence that constrained credit access is at least one important mechanism through which the homeownership gap manifests. Moreover, the findings are an empirical demonstration that differential access to credit can be a driving force behind homeownership gaps.

The paper proceeds as follows. Section 2.2 provides background information about homeownership in the context of immigrant populations and the role of credit constraints in the market for owner-occupied housing. Section 2.3 presents descriptive estimates of the relationship between undocumented status and homeownership. In section 2.4, I provide quasi-experimental evidence of the homeownership gap between undocumented immigrants and legal residents by evaluating the effect of DACA on the homeownership propensities and home loan applications. Section 2.5 addresses the effects of the 2003 Treasury Department rule change using similar data and a comparable empirical strategy. Section 1.7 concludes.

[8]There were a few exceptions as, pre-2003, some smaller financial institutions used the ambiguity in the existing legal code as justification to offer financial services to individuals without Social Security numbers. For more information, see Appendix B.6.

[9]In fact, it was a part of the PATRIOT Act.

2.2. Background

2.2.1. Homeownership and the Undocumented Population

Currently, the popular narratives about undocumented immigrants in the U.S. revolve around their propensities to commit crime and the impact they have on their local labor markets. As such, other unique characteristics of this population (such as housing choices and constraints) and the economic ramifications of these differences remain under-explored. Prior studies of the housing tenure of the undocumented population essentially fall into one of two categories. In the first category, there are a number of immigration-focused policy institutions that produce estimates of the undocumented population and present information on their characteristics in a comprehensible manner. These institutions aspire to provide summary information in a format catered to informing the general public. In the second category are a handful of studies (often in the sociology literature) that qualitatively investigate the housing conditions of undocumented immigrants and provide statistics to support their analysis. To my knowledge, only one study presents nationwide, regression-adjusted estimates of the homeownership gap between immigrants of different legal statuses. After adding controls for a handful of household characteristics, Hall and Greenman (2013) estimate that documented immigrants have odds of homeownership more than 100% higher than those of undocumented immigrants.

Descriptive statistics on immigration status and housing tend to show large homeownership gaps between undocumented immigrants and legal residents. As illustrated by McConnell and Marcelli (2007), who measure the homeownership gap between immigrants of different statuses in Los Angeles, this gap shrinks once other characteristics have been accounted for, though, supporting the conclusion that a substantial portion of the homeownership gap is not due to undocumented status directly, but rather, is driven by the fact that undocumented immigrants are disproportionately likely to have traits correlated with lower homeownership rates (e.g. they tend to be younger and lower-income). This is consistent with existing literature on racial disparities in homeownership which finds that the majority of the difference in homeownership rates between whites and racial and ethnic minorities is explained by characteristics correlated with both race and homeownership.[10] Within this literature, Charles and Hurst (2002) also find that most of the remaining racial gap in homeownership is explained by differences in propensities to apply for a home loan among otherwise similar individuals, which may be driven by an understanding (or expectation) that their loan applications are more likely to be denied. Motivated in part by this result, I devote extensive focus to changes in propensities to apply for home loans.

It is important to confirm the results of the Hall and Greenman (2013) study using a larger and more recent sample of immigrants and work to uncover the mechanisms behind the homeownership gap. Barriers to homeownership restrict an individual's residential and broader economic mobility. Eliminating such barriers has the potential to improve welfare and economic equality through several channels. First, homeownership is an important mechanism for wealth accumulation. The differential ability of some residents to accumulate wealth presents additional challenges in circumstances where wealth (even conditional on income) is an important factor (e.g. retirement decisions and, ironically, future housing tenure). Second, Harding and Rosenthal (2017) highlight the relationship between homeownership and self-employment. Home equity provides a line of credit that may be used to finance endeavors that result in business creation and promote occupational mobility, and, as noted by Harding and Rosenthal, self-employment can serve as a replacement for

[10]See Haurin, Herbert and Rosenthal (2007) for a review of evidence on racial homeownership gaps. Gabriel and Rosenthal (2005) find that household characteristics are responsible for about two-thirds of the white-minority homeownership gap that existed in the 80s and 90s. Charles and Hurst (2002) find that the black-white gap in mortgage applications is reduced once characteristics are controlled for. Munnell et al. (1996) find that the racial disparity in denials of mortgage applications is reduced by more than half after applicant and property characteristics have been accounted for. Painter, Gabriel and Myers (2001) find that differences in income, education, and immigration status explain the white-Latino homeownership gap in Los Angeles. Borjas (2002) finds that most of the native-immigrant homeownership gap can be explained by immigrant country of birth, residential location, and other socioeconomic characteristics.

wage-work when such work is unavailable.[11] Given their limited access to the formal labor market, self-employment may be especially appealing to undocumented immigrants who find it feasible. Also, to the extent that lacking access to formal employment leads undocumented immigrants to turn to income-generating crime (e.g. theft, drug sale, prostitution) as a substitute for formal labor market participation,[12] the ability to self-employ may serve to reduce crime. In light of such findings, those who are concerned that undocumented immigrants are "bringing crime" or "taking American jobs" may be especially eager to increase undocumented immigrants' homeownership rates, given homeownership's effects on self-employment.

Third, and related to the above, increasing access to owner-occupied housing can reduce crime rates and improve occupational mobility. Disney et al. (2021) show that the Right to Buy scheme in the United Kingdom, which allowed public housing tenants to become owners of their current homes (and facilitated the purchasing process), generated both short run and long run reductions in property crime and robberies. Their findings provide a glimpse of what might happen if policymakers facilitated homeownership among undocumented immigrants in a similar manner. The Right to Buy scheme targeted individuals who had already been living in their current residence for several (at least 3) years and provided residents access to owner-occupied housing that was previously unavailable to them. U.S. immigration policies proposed to address issues related to undocumented immigrants commonly favor those who have lived in the country longer, and as demonstrated by Disney et al. (2021), even absent changes in residential location, allowing current residents to transition to owner-occupied housing can reduce crime.[13]

[11] It is worth mentioning that Harding and Rosenthal (2017) note the link between homeownership and self-employment but the focus of their study is the effect of housing capital gains on entry into self-employment. Increasing access to owner-occupied housing for the 11 million undocumented immigrants would induce an increase in demand for such housing, resulting in higher home values (capital gains), all else equal. Thus, whether it is homeownership (on the extensive margin) or housing capital gains (on the intensive margin) that drives additional self-employment, increased access to owner-occupied housing in this study's setting should be expected to facilitate self-employment.

[12] See Freedman, Owens and Bohn (2018).

[13] A reduction in crime is just one of many potential spillover effects of increased homeownership. Related

Fourth, access to owner-occupied housing promises to improve residential mobility. Numerous studies have addressed the Moving to Opportunity experiment conducted in the 90's and outlined the benefits of improved access to housing and neighborhoods.[14] Stated more broadly, reduced residential mobility has important implications for neighborhood composition, the effects of which are well-documented.[15] Notably, the neighborhood effects literature often finds that the consequences of one's residential environment are most pronounced for children. In a country where more than 75% of children who have at least one undocumented parent are U.S. citizens,[16] welfare loss that results from restricted residential and economic mobility will be borne, in large part, by already disadvantaged U.S. citizen children.

Beyond the possibility of exacerbated inequality, barriers to accessing owner-occupied housing may have efficiency implications. First, prior to the Treasury Department's legal clarification in 2003, institutions may have been willing to lend to qualified undocumented immigrants at rates undocumented immigrants would have accepted. However, the inability to use an ITIN (or other means) to access credit markets posed a demand-side entry barrier, preventing undocumented immigrants from accessing the market for owner-occupied housing. Such a barrier prevents matches between undocumented immigrants and lenders (and sellers of owner-occupied housing) that would occur in an efficient market. Second, if sub-optimal borrowing constraints (i.e. those imposed externally, not borrowing constraints that result from optimal lending behavior of institutions towards a potentially more risky borrower) exist, then undocumented immigrants are inefficiently confined to the rental housing market, resulting in higher demand for rental housing and lower demand for owner-occupied housing,

studies noted in Disney et al. (2021) include DiPasquale and Glaeser (1999) and the literature review by Haurin, Dietz and Weinberg (2002).

[14]See Chetty, Hendren and Katz (2016) and Chyn (2018) and references therein. See also Chetty and Hendren (2018) for related work emphasizing the importance of neighborhood effects on intergenerational mobility.

[15]See, for example, studies by Raj Chetty, including Chetty and Hendren (2018).

[16]See Migration Policy Institute and Pew Research estimates at https://www.migrationpolicy.org/research/profile-us-children-unauthorized-immigrant-parents and https://www.pewresearch.org/hispanic/2018/11/27/most-unauthorized-immigrants-live-with-family-members/, respectively.

the upshot of which is higher equilibrium rents and lower home values.[17]

2.2.2. Consequences of Credit Constraints

No recent policy has been implemented with the explicit goal of expanding homeownership in the undocumented population, but a couple of policies may have inadvertently done so. In 2012, DACA granted temporary legal permission for undocumented immigrants who arrived in the U.S. as children and satisfied a number of other requirements to live and work in the U.S. The documentation possessed by DACA recipients increased access to (more favorable) home loans.[18] Because DACA affected the income, education, and security of its recipients[19] (in addition to their credit access), the policy presents several avenues through which it may cause homeownership rates of undocumented immigrants to rise.[20]

I will show that DACA increased homeownership in the undocumented population, but this finding alone should not be interpreted as the result of a corrected housing or credit market inefficiency. However, I will argue that, taken together with the results from my analysis of the change in Treasury Department rules, relieved borrowing constraints are likely responsible for at least part of DACA's effect on homeownership. Additionally, not to be diminished, a policy that reduces the homeownership gap has notable equity implications even if it achieves the reduction only through its effects on individual attributes correlated

[17] Gete and Reher (2018) provide an example of this, empirically demonstrating that the contraction of mortgage credit supply after the Great Recession was responsible for rising rents.

[18] There has been widespread confusion about whether DACA recipients can receive FHA backed loans. Media outlets and HUD Secretary Ben Carson have suggested that, historically, DACA recipients have been eligible for FHA loans (see, for example, https://www.buzzfeednews.com/article/nidhiprakash/trump-daca-housing-ben-carson). However, HUD's official policy was clarified in 2019 to state that DACA recipients are not (and never have been) eligible for FHA backed loans. DACA recipients remain eligible for conventional loans from the institutions willing to serve them, however, and may still receive better terms than other undocumented immigrants because, for example, they possess a social security number and can prove that they are (at least temporarily) not at risk of deportation.

[19] See Kuka, Shenhav and Shih (2020), Pope (2016), and Amuedo-Dorantes and Antman (2017).

[20] Additionally, Ballis (2021) finds evidence of spillover effects in educational achievement among students with more DACA-recipient peers. I cannot rule out the possibility that similar spillover effects occur in the housing market.

with homeownership.

While finding that DACA positively affects homeownership is meaningful in its contribution to existing knowledge of immigration policy and housing tenure of undocumented immigrants, it offers little insight into what mechanisms are responsible for the effect. The literature on homeownership gaps (usually black-white) details several channels through which such gaps may arise. Relevant to this study, it is commonly found that wealth constraints or borrowing constraints (that, like wealth, limit an individual's ability to make a down payment) are more binding than income constraints and are more responsible for existing racial disparities in homeownership rates. Duca and Rosenthal (1994) compare simulated estimates of preferences for homeownership with actual rates of owner-occupancy and find evidence that borrowing constraints significantly reduce homeownership rates and have a disproportionate effect on non-white families.

Gabriel and Rosenthal (2005) again find a minority-white homeownership gap but argue that credit barriers are responsible for a relatively small fraction of the gap. Importantly, they note the difficulty of empirically identifying the effect of credit barriers. They point out that, "it requires that one identify, a priori, a group of households that are not credit constrained, and then use the behavior of that group to infer how others would have behaved in the absence of binding credit limits, ceteris paribus." In my setting, legal residents serve as a group that, while not totally unconstrained (as would be the ideal experiment), faces constraints that should be constant or relatively unchanged by the policy of interest (DACA or the 2003 Treasury rule change). Thus, a difference-in-differences formulation can use the change in behavior of undocumented immigrants relative to the change in behavior of this group to infer how undocumented immigrants behave in the absence (or differential alleviation) of binding credit constraints. In this way, this study's setting offers a unique opportunity to evaluate the influence of credit barriers.

Linneman and Wachter (1989) and Haurin, Hendershott and Wachter (1997) both find that borrowing constraints reduce homeownership propensities and that wealth's impact on homeownership is greater than income's.[21] Building on these findings, Gyourko, Linneman and Wachter (1999) find that, in the absence of wealth constraints, white and minority households experience no difference in homeownership rates. If the tenure outcomes of undocumented immigrants (who are a disproportionately low-wealth group) are as sensitive to borrowing constraints as the tenure outcomes of racial minorities, we might expect that the removal of such constraints would generate large increases in homeownership propensities.

It is worth noting that many studies that investigate the determinants of homeownership gaps examine application *rejection* rates, which are, by definition, conditional on *application* rates. Munnell et al. (1996) acknowledge this fact, stating that their estimates of the role of race in mortgage lending may be understated if differential treatment occurs at other stages in the lending process. Charles and Hurst (2002) consider potential determinants of the black/white homeownership gap (that remains even after observable characteristics have been controlled for) on several margins. They find that the homeownership gap is not driven by discrimination in lending terms (the most "intensive" margin), is driven to some extent by discrimination in lender decisions to originate loans (a more extensive margin), but is driven most by the *ex ante* decision to apply for a home loan. Using a panel of renter households, they find that, despite having observably similar characteristics or qualifications, white households were much more likely than black households to transition to homeownership. While black households were significantly more likely to be rejected conditional on applying for a mortgage, this discrimination in application decisions accounted for a relatively small portion of the homeownership gap. By contrast, the fact that black renters were nearly twenty percentage points less likely to apply for a mortgage explained 93% of the homeownership gap. The authors also found strong evidence to suggest that black households had difficulty coming up with a down payment, which may be able to

[21] Acolin et al. (2016) also find (in the context of the Great Recession) that tightened borrowing constraints significantly reduce the probability of transitioning to homeownership.

explain the differential propensity to bother applying for a home mortgage (other examples of what they call a "discouragement effect" are also presented, such as the anticipation of discrimination in lenders' application acceptance/rejection decisions).

Taken together, the findings of these previous studies suggest that wealth constraints that limit an individual's ability to cover a down payment should be considered the primary barrier to homeownership among prospective homeowners (especially once demographic characteristics have been accounted for), and home loan applications can be thought of as the first-order outcome of interest when theoretical predictions indicate a policy may affect a homeownership gap. Brueckner (1986) presents a model that allows for theoretical predictions of the optimal tenure choice of a given household when down payment constraints may be present. One prediction of the model is that an individual's probability of becoming a homeowner is decreasing in the price of a home and the fraction of that price required as a down payment. By extension, this means that if the down payment percentage is heterogeneous by immigration status, then the tenure outcome for otherwise identical immigrants considering an identical home is heterogeneous by immigration status (when the down payment constraint binds).[22] If the results of the Charles and Hurst study apply to the homeownership gap by immigrant status, then a policy change (DACA or the Treasury Department rule change) that loosens down payment constraints should induce increased homeownership through an increase in applications and, possibly, a reduction in rejected applications.

[22]One way to think about this study's setting in the context of Brueckner's model is that, prior to the 2003 legal clarification, the down payment percentage for undocumented immigrants is (except in a few select cases) effectively equal to 100% as undocumented immigrants lacked other financing options without relying on successfully using a fake social security number to originate a loan.

2.3. Household-Level Data and Analysis

2.3.1. Data

There are three parts to the analysis. Each involves a distinct data set, derived from a handful of sources. The first data set is annual, household-level microdata from the American Community Survey (ACS) from 2008 to 2018. The ACS surveys about 1% of the U.S. population every year, asking questions related to employment, income, housing, and demographics. The goal in this part of the analysis is to ascertain the effect of undocumented status on the probability of owning one's home. The ACS asks about homeownership, but it does not ask about legal status. For this reason, I must rely on an imputation procedure that uses individual characteristics to predict the legal status of individuals in the ACS microdata. The procedure employed is based on that proposed by Borjas and Cassidy (2019).[23]

First, anyone who was born in the U.S. or claimed U.S. citizenship is assigned "citizen" as their status. All remaining individuals are assumed to be undocumented until "proven" otherwise by the remaining steps in the imputation procedure.[24] While there is no way to conclusively determine that an individual is undocumented, there are several cases where it can be concluded that an individual is *not* undocumented. More specifically, if an individual in the ACS satisfies one of the following conditions, which generally cannot be satisfied by anyone lacking legal status, then they are assigned legal resident status.

- Arrived in the United States before 1980[25]

[23]Variations on the procedure are used by other institutions (such as the Department of Homeland Security) to estimate the undocumented population in the U.S. The procedure used in this study is identical to the one used in Christopher (2022).

[24]This assumption results in an overestimate of the undocumented population, leaving some legal residents to be assigned undocumented status. Assuming the imputation procedure fails at random, an overestimate is more desirable than an underestimate as it will bias estimates of the effect of undocumented status towards zero in the same way estimates of "treated" status, more generally, are biased towards zero when the treatment group is contaminated with observations from the control group (but not vice-versa).

[25]These individuals are assumed to have achieved legal status through IRCA 1982.

- Is a veteran or currently serving in the U.S. military
- Received public health insurance, Medicaid, Medicare, or VA insurance
- Received any welfare payment, SSI, or Social Security Benefits
- Works in government or in an occupation that requires licensing
- Born in Cuba[26]
- Received food stamps/SNAP[27]
- Arrived in the U.S. as an adult and currently enrolled in undergraduate, graduate, or professional school [28]
- Works in a computer-related occupation, possesses at least a bachelor's degree, *and* has been in the U.S. for no more than six years[29]
- Spouse is classified as a legal resident or citizen

All individuals who are not assigned "citizen" or "legal resident" status at this stage are assigned "undocumented" status in the data. Table 2.1 presents my estimates of the undocumented population by state in 2015 and 2017 alongside estimates from other sources for the same time period. My estimates of between 11 and 12 million undocumented immigrants nationwide are largely in line with estimates produced by other institutions. The data are then aggregated to the household level with the characteristics of the household head retained.[30]

Before moving to regressions, I impose five sample restrictions. First, I drop all households assigned "citizen" status as this leaves only legal resident immigrants as the comparison group in the analysis and may help account for unobserved differences between non-citizen immigrants (documented or not) and citizens. Second, because the imputation procedure defines all immigrants who arrived in the U.S. before 1980 as legal residents and because

[26]Individuals born in Cuba are overwhelmingly likely to be refugees.

[27]Since undocumented parents of U.S. citizens may be eligible for food stamps on behalf of their children, the only time I apply this edit is if the indicator for whether someone in the household received food stamps is true *and* there is only one individual in the household.

[28]This is to account for student visa holders (Pastor and Scoggins, 2016).

[29]This is to account for immigrants on H-1B visas. H-1B visas are generally renewable up to six years. In 2012, 61% of H-1B visa applications approved were for workers in computer-related occupations, and 99% of approved petitions were for workers with at least a bachelor's degree.

[30]For example, an "undocumented household" will be defined as a household where the head is assigned undocumented status.

"years in the U.S." will be an important control variable, I drop any household where "years in the U.S." (defined as year of interview minus year of arrival in the U.S.) is greater than 38 (the maximum possible value for those assigned undocumented status because the sample ends in 2018). I also drop any household that has been in the U.S. for less than one year largely because the ACS asks respondents about many characteristics that are defined by the previous year (e.g. income is defined as income over the past 12 months), meaning responses would likely not be representative of these individuals' current characteristics. Third, because the imputation still produces an inordinately high number of undocumented immigrants with advanced degrees, I restrict the sample to household heads with a bachelor's degree or less education. Fourth, because individuals who are currently enrolled in schooling face unique housing circumstances, any household with a head that is currently enrolled in school (generally, post-secondary students pursuing bachelor's or associate's degrees) is dropped from the data. Finally, any household head reporting a birthplace of Singapore, Chile, Burma (Myanmar), Iraq, Democratic Republic of the Congo, Bhutan, or Somalia is excluded from the analysis. Singapore and Chile have unique agreements with the U.S. regarding the availability of H-1B visas, meaning immigrants from these countries are disproportionately likely to be legally present on H-1B visas (and therefore, disproportionately difficult to classify correctly as legal residents). The remaining countries in the list are those with disproportionately high numbers of refugees relative to total immigrants during the period covered by the data. Without information about refugee status in the microdata, the imputation procedure is more likely to incorrectly assign undocumented status to refugees.

The final microdata sample consists of 468,960 households over 11 years. Table 2.2 presents descriptive statistics by immigration status.

2.3.2. Descriptive Regressions

The primary outcome of interest is the indicator variable for whether the housing unit is owner-occupied. Specification (2.1) yields regression-adjusted estimates of the relationship between undocumented status and homeownership. Results from specification (2.1) are presented in Table 2.3.

$$owned_{ipt} = \beta_1 undoc_i + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{2.1}$$

$Owned_{ipt}$ is an indicator variable that takes value 1 if the housing unit is owner-occupied (by household i in PUMA p in year t). $Undoc_i$ is an indicator that takes value 1 if the household head's status is undocumented and 0 otherwise.[31] X_i is a vector of controls that includes number of people in the household, number of workers, number of children, the log of household income, the gender of the household head, marital status, and quadratics in age and years in the U.S. Also included are year and PUMA fixed effects. PUMA's are the most precise geographical unit provided in the public-use ACS data. Since PUMA boundaries change between the 2011 and 2012 ACS files, I present results both from the 2012-2018 sample (where PUMA fixed effects are used) and the full sample where CPUMA (a variable provided by IPUMS that defines consistent PUMA's across pre and post 2012 files) fixed effects are used. In short, CPUMA's sacrifice geographic precision in exchange for four additional years of data before 2012. As shown in Table 2.3, the results are qualitatively identical despite the difference in geographic fixed effects.

Estimates in columns 3 and 6 of Table 2.3 account for differences in observable characteristics of undocumented immigrants (e.g. the fact that undocumented immigrants tend to

[31] In Appendix B.2, I address the possibility that the imputation procedure mechanically generates a negative relationship between individuals classified as undocumented and probability of homeownership. I run a comparable imputation and similar regressions on the sample of citizens who were excluded from the present analysis. I find no evidence that the imputation procedure is responsible for the negative relationship observed between undocumented status and homeownership.

be younger and younger people are less likely to be homeowners) by including them as controls. The results in Table 2.3 imply that undocumented immigrants are about 7 percentage points (or about 19%) less likely to own their homes than legal resident immigrants. Once demographic characteristics are controlled for, the difference falls to about 1.5 percentage points (or about 4%) but remains highly statistically significant. Note that specification (2.1) is only an ordinary least squares linear probability model. Unless the included controls eliminate all omitted variable bias otherwise present, these estimates should not be interpreted as a statement about the causal relationship between undocumented status and homeownership.

2.4. Evidence from DACA

2.4.1. Household-Level Difference-in-differences

Next, I employ a quasi-experimental difference in differences strategy to 1) provide additional evidence of the negative relationship between undocumented status and homeownership and 2) offer evidence that Deferred Action for Childhood Arrivals (DACA) impacted undocumented immigrants and the market for owner-occupied housing. In specification (2.2), the parameter of interest is β_2, which captures the effect of undocumented status on homeownership after DACA has gone into effect ($post_t$ is an indicator that takes value 1 in 2013 and later years[32]). Note that the baseline $post_t$ is not explicitly included in the specification because it is subsumed by the year fixed effects (γ_t).

$$owned_{ipt} = \beta_1 undoc_i + \beta_2 (undoc_i \times post_t) + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{2.2}$$

[32]While DACA technically took effect in June of 2012, take-up was low until 2013. Additionally, those who did take it up in 2012 likely did not move to owner-occupied housing in the 6 months left in the year.

In specification (2.2), β_1 captures the effect of undocumented status across all time periods. β_2 captures any additional effect of undocumented status specific to the period after DACA is in effect. Thus, β_2 captures just the change (from before DACA was in place to after DACA's implementation) in the effect of undocumented status. While, in the previous section, the coefficient on undocumented status may be biased due to some unobservable characteristics (omitted variables) correlated with both undocumented status and homeownership propensities (e.g. cultural differences between immigrants of different statuses in their preferences for homeownership), the estimate of β_2 will only be biased if those unobservables are also changing over the sample period. In other words, since β_2 captures only the change in the effect of undocumented status, it will only be biased by unobservables correlated with undocumented status if those unobservables *also change* in the post period. So long as the parallel trends assumption holds (evidence for the absence of pre-existing trends is presented in the next subsection), β_2, or the change in the effect of undocumented status on homeownership propensities, can only be biased by a corresponding change in unobservables correlated with both undocumented status and homeownership. If there are unobservables that are correlated with both undocumented status and homeownership *and* that are changing around the same time that DACA takes effect, then the estimates of β_2 are biased. It is hard to think of any such unobservables.

Results from specification (2.2) are presented in Table 2.4. Column 1 excludes controls. Column 2 includes all controls. Column 3 includes all controls except log income. Results are consistent with the hypothesis that DACA increased the rate at which undocumented immigrants reside in owner-occupied housing, reducing the gap between them and observably similar legal resident immigrants. However, it is impossible to say whether DACA's effect operates solely through the channel of increased household income[33] or if DACA lifted other constraints (e.g. increased loan eligibility) that drove the change.[34] Despite being unable

[33] Pope (2016) finds that DACA led to increases in income for recipients lower in the income distribution.

[34] This point is highlighted by the difference between column 2 and column 3 in Table 2.4. If DACA raised household income, then household income is a bad control and contaminates the estimate of interest in column

to determine the specific channel through which DACA affected homeownership, the results in Table 2.4 confirm that it had an effect and present quasi-experimental evidence for the existence of a link between undocumented status and homeownership with the caveat that the relationship could be driven mostly by differences in incomes.

Parallel Trends

A necessary condition for the causal interpretation of difference-in-differences estimates is parallel pre-trends in the dependent variable across treatment and control groups. Figure 2.1 plots mean (unadjusted) homeownership rates by immigration status over the sample period. A similar downward trend in homeownership rates is observed for both immigrant groups.

A more formal test of the parallel trends assumption relies on an event study. Formally, the event study equation is:

$$owned_{ipt} = \beta_1 undoc_i + period_t + \beta_{2t}(undoc_i \times period_t) + X_i\theta + \alpha_p + \varepsilon_{ipt}$$

where perfect multicollinearity causes the effect of undocumented status in period -1 (immediately before treatment) to serve as the reference point for the relative effect of undocumented status in other periods. X_i is a vector of controls as defined in section 2.4.1.

If there are no pre-existing trends in the relationship between undocumented status and homeownership, then the effect of $undoc_i$ on $owned_{ipt}$ should be the same in every period prior to treatment (i.e. $...\beta_{2,t=-3} = \beta_{2,t=-2} = \beta_{2,t=-1}$). Equivalently, in the absence of pre-trends, the *relative* effect of undocumented status in any period prior to treatment should be zero (when compared to the effect in the reference period). In other words, since the $\beta_{2,t}$'s

2. If income is excluded from the list of controls (column 3), then the estimated effect of DACA is not biased by the bad control, but it is interpreted as the effect of DACA on homeownership both independently *and* through DACA's effect on income.

capture the effect of undocumented status in each time period, t, *relative to the effect in the reference period* ($\beta_{2,t=-1}$), there are no pre-trends when $\beta_{2t} = 0$ for all $t < -1$. Figures 2.2 through 2.4 plot β_{2t} for all other periods t.

Point estimates of the effect of undocumented status in periods prior to DACA should be stable and indistinguishable from zero. Any upward trend in point estimates leading up to the implementation of DACA indicates that the diff-in-diff estimates in section 2.4.1 are likely to be upward biased. Figures 2.2 through 2.4 plot point estimates from event studies. The controls included in the regressions used to generate the event study plots in figures 2.2 through 2.4 are the same sets of controls indicated by columns 1 through 3, respectively, of Table 2.4.

The event study plots suggest that the parallel trends assumption is satisfied once controls are added (columns 2 and 3 of Table 2.4). Thus, the preferred estimates of the effect of DACA on homeownership rates of undocumented immigrants are those presented in columns 2 and 3, indicating an increase of 0.65 or 0.9 percentage points. In other words, DACA increased homeownership rates of undocumented immigrants by roughly 2% and eliminated more than one quarter of the regression-adjusted homeownership gap between undocumented immigrants and legal residents.

Also worth noting is that point estimates in the event study plots do not consistently appear above zero until 2 years (periods) after DACA takes effect, consistent with the story that adjusting to DACA and transitioning to homeownership would take time (more than one year). This "adjustment period" will be a consistent feature of the analysis of DACA's effects on homeownership, appearing in the event studies in section 2.4.2 as well.

2.4.2. County-Level Difference-in-differences

County-Level Data

One concern about the results from the microdata analysis is that a change in sample composition may be responsible for more undocumented immigrants in owner-occupied housing. While unlikely, DACA may have led more undocumented immigrants to live in owner-occupied housing without actually causing them to buy more homes. For example, following DACA, if undocumented renters were more likely to move in with family members in owner-occupied housing or if undocumented renters were more likely to be deported (skewing the composition of the remaining pool of undocumented immigrants towards homeowners), then undocumented immigrants may appear in owner-occupied housing more often, even though their home-purchasing behavior hasn't changed. If the findings of the previous section are a mere artifact of changing sample composition, then we should expect to see no evidence of a mortgage market response to DACA. However, the analysis in this section illustrates that DACA did affect home mortgage applications.[35] The findings complement those of the previous subsection and introduce data and regression specifications similar to those that will be used in section 2.5.

The data for this section come primarily from the Home Mortgage Disclosure Act (HMDA). The HMDA files contain annual, application-level data on mortgage applications from all institutions that are required to report.[36] I restrict the data to applications where the loan was for a home purchase, where the loan was for housing that is owner-occupied as a principal dwelling, and where the application was either denied, approved but not accepted,

[35]Additionally, each year of ACS microdata constitutes a sample of roughly 1% of the U.S. population. The HMDA data used in the is section contains the near universe of U.S. home loan applications, increasing the likelihood that effects of DACA are detected.

[36]All depository institutions that offer home loans, have at least one office located within an MSA, and have assets greater than $44 million are required to report. All mortgage and consumer finance companies with greater than $10 million in assets or that extend 100 or more home purchase or home refinancing loans in a year are required to report.

or resulted in a loan being originated. I include applications that did not result in loan origination for two primary reasons. First, I would expect DACA's first-order effect to be one that drives more individuals to apply for housing. It may also be true that DACA increased the probability of a mortgage application's acceptance. A positive effect of DACA on loan origination rates alone may indicate that undocumented immigrants' behavior did not change but the likelihood of loan origination for those who were already applying for mortgages increased. A significant change in applications, instead, captures a behavioral response to DACA. Both are interesting, but for now, the focus will be on applications. Section 4.4 considers effects on home loan approval rates. Second, Charles and Hurst (2002) find that racial homeownership gaps are driven most by differences in propensities to apply for home loans. If the same is true for the homeownership gap between immigrants of different statuses, then the most important effect of the policy is, arguably, its effect on applications.

Information on applicants is limited to sex, income, and race/ethnicity. Thus, it would be impossible to reliably determine immigrant status at the application level. Instead, applications by ethnicity (Hispanic/non-Hispanic) are aggregated to the county level. Since nearly 90% of DACA recipients were born in Mexico or one of the Northern Triangle countries, the effect of DACA should be concentrated among Hispanics.

I use data on the number of active DACA recipients by CBSA[37] from USCIS. Since 2017, USCIS has provided information on the number of DACA recipients by CBSA in any CBSA with at least 1,000 active cases. I use the most recent (2019) report to assign counts of DACA cases to the CBSA's within the data.[38] Then, using population data from the 2010 census, I create a "DACA Take-up" variable defined as the number of active DACA cases per Hispanic population. I apply the (CBSA-level) DACA Take-up measure to all counties

[37] Core Based Statistical Areas can simply be thought of as collections of counties for my purposes.

[38] I also add five CBSA's that appeared in the first (2017) report but are missing from the 2019 report, presumably because they fell below 1,000 active cases.

within each CBSA (as if DACA recipients in a CBSA are distributed across counties within a CBSA the same way Hispanics are).

Finally, I use 1-year American Community Survey (ACS) summary files to add time-varying, county-level information on population by ethnicity. This data is only available for counties with 65,000 people or more. To ensure a consistent sample of counties across the 2010-2017 period of interest, I drop all counties that do not appear every year, effectively restricting the sample to counties that had at least 65,000 people in 2010.[39] [40]

County-Level Specifications

The choice specification for this section is given by equation (2.3).

$$\begin{aligned}\frac{Hisp\ Apps}{Total\ Apps}_{ct} &= \alpha_c + \gamma_t + \beta_1 \frac{Hisp\ Pop}{Total\ Pop}_{ct} + \beta_2(DACA\ Takeup\ MED_c \times post_t) \\ &\quad + \beta_3(DACA\ Takeup\ HI_c \times post_t) + \varepsilon_{ct}\end{aligned} \tag{2.3}$$

Large differences in populations across counties suggests the outcome should be a scaled measure (i.e. instead of simply the number of Hispanic applications) so that relatively large effects in small counties are not obscured by spurious variation in the number of applications in large counties like Los Angeles over time.[41] Thus, the outcome of interest is the number of mortgage applications made by Hispanic applicants relative to the number of mortgage applications made by all applicants in a county c and year t. A control for Hispanic population relative to total population is included as well as county and year fixed effects. $DACA\ Takeup_c$ (not included in specification (2.3) but used to define the independent vari-

[39] 2017 is chosen as the final year because that was the last year for which HMDA data was available. 2010 is chosen as the first year to avoid effects of the 2008 recession and because IPUMS NHGIS did not have ACS summary files available for years before 2010.

[40] Additionally, to address inconsistencies in county definitions/boundaries over time, I drop all counties in Alaska; Clifton Forge City and Alleghany counties in Virginia; South Boston City and Halifax counties in Virginia; and Yellowstone National Park, Gallatin, and Park counties in Montana.

[41] For example, a 200-unit change in applications could be attributed to mere noise in Los Angeles but would represent a roughly 100% change in applications in Elkhart county, Indiana.

ables of interest) is defined as $\frac{CBSA\ DACA\ cases}{CBSA\ Hispanic\ Population\ in\ 2010}$. $DACA\ Takeup\ MED_c = 1$ if county c is in a CBSA with nonzero $DACA\ Takeup_c$ but below median $DACA\ Takeup_c$ among CBSA's with nonzero $DACA\ Takeup_c$. $DACA\ Takeup\ HI_c = 1$ if county c is in a CBSA with nonzero $DACA\ Takeup_c$ and above median $DACA\ Takeup_c$ among CBSA's with nonzero $DACA\ Takeup_c$. The excluded category is $DACA\ Takeup\ LOW_c$, which captures all counties in CBSA's with fewer than 1,000 active DACA cases.

The parameter of interest is β_3, which is the estimated change in the fraction of mortgage applications made by Hispanic applicants in the counties where DACA Take-up was highest. A positive β_3 indicates that counties where larger fractions of the Hispanic population are DACA recipients saw larger increases in the fraction of mortgage applications made by Hispanic applicants after DACA took effect, implying DACA caused more Hispanic home loan applications. The same hypothesis may predict a positive β_2 if the counties in CBSA's with fewer than 1,000 active DACA recipients have, in reality, low DACA Take-up.[42]

The results from specification (2.3) are presented in Table 2.5. The second column applies weights based on county population. The final column also drops California from the data.[43] Depending on the weighting and sample choice, the results imply a .33 to

[42]CBSA's that have fewer than 1,000 DACA cases are defined as having 0 cases in the continuous takeup measure (because USCIS does not report the number of cases in CBSA's that have fewer than 1,000). Thus, there is a possibility that, for example, a CBSA with a Hispanic population of 2,000 may have 500 DACA cases (and therefore, have a "true" DACA Takeup of 0.25) and show up as having 0 in the data. I'm inclined to believe that extreme cases like this example don't happen frequently since undocumented immigrants tend to be concentrated in a handful of immigrant enclaves and most (about 85%) of the nation's DACA cases already do fall into one of the roughly 80 identified CBSA's (about 15% are scattered among the remaining 800+ CBSA's, which each have fewer than 1,000 cases).

[43]It's not clear whether applying weights is appropriate in this context. On the one hand, the analysis is designed to determine the effect of DACA, at the county level, on Hispanic applications scaled by total applications to make counties more comparable (bounding the outcome between 0 and 1 so that *relatively* large variation in applications in smaller counties is not incidentally attributed to noise). This would suggest an equal weighting of all counties. On the other hand, DACA affected *people*, not just counties. So, weighting may yield estimates more indicative of the policy's nationwide impact. Additionally, with information on DACA Take-up being more limited in small CBSA's (recall, the DACA Take-up measure is unknown in any CBSA with under 1,000 active DACA cases, and all counties in these CBSA's are placed in the LOW Take-up category) and with population estimates potentially being less reliable in smaller counties, attributing more weight to larger counties may make estimates slightly more reliable as they are based, to a greater extent, on more precise data. From a policy perspective, though, results from weighted regressions may be less appealing to those outside of California (or highly populated counties in other states) as it could be argued

.54 percentage point increase in the percent of mortgage applications made by Hispanic applicants in the high take-up counties. Off of a sample mean of just around 7%, this amounts to a 4.5 to 8 percent change in the relative number of Hispanic mortgage applications as a result of DACA.

2.4.3. Robustness

Robustness to Age-dependent Population Controls

One might argue that the observed effect is the result of counties with higher DACA take-up being the same counties that simply have (relatively) more young adult Hispanics (as the eligibility requirements of DACA lead most of its recipients to be in their 20's, currently) and that young adult Hispanics drive Hispanic mortgage applications, not DACA. First, if this were the case, we would expect an upward trend in the relative number of Hispanic mortgage applications in the high take-up counties across the sample period. Figures 2.5 and 2.6 provide evidence that this is not the case and support the parallel trends assumption necessary for diff-in-diff estimates to be interpreted as causal.

Second, at the risk of over-fitting the data and making use of population estimates that may less reliably capture Hispanic population changes, I can run regressions like specification (2.3) but where the $\frac{Hisp.\ Pop}{Total\ Pop}$ control is replaced by relative Hispanic populations by age group. The new controls are $\frac{Hisp.\ Children}{Total\ Children}$, $\frac{Hisp.\ Youth}{Total\ Youth}$, and $\frac{Hisp.\ Adults}{Total\ Adults}$, where children are defined as anyone under the age of 18, youth are defined as those between ages 18 and 30, and adults are those over the age of 30. Since the ACS 1-year files do not provide this population information by age and ethnicity for all counties in the sample, I substitute all population estimates based on the ACS files with population estimates from the Surveillance, Epidemiology, and End

that results are simply driven by the largest counties and may not be applicable elsewhere. For this reason, I also present the weighted estimates from the subsample of counties not in California.

Results Program (SEER) files, which provide updated, annual population estimates from the Census Bureau (based largely on the most recent decennial census).

Results are presented in Table 2.6. The first 3 columns repeat the regressions in Table 2.5, using population measures from the SEER data instead of the ACS. The last 3 columns mimic the first 3, but replace the original control for relative Hispanic population with the controls based on age group. Results are robust to the usage of SEER data. When using controls by age group, magnitudes of the estimates of interest fall somewhat, but the direction of the effect is consistent and statistical significance is retained in two of the three regressions. It's not clear that these controls based on age group are appropriate (especially if there are concerns about the accuracy of such precise estimates in small counties), and it's not clear that the age group categories as I have (somewhat arbitrarily) defined them constitute the best set of controls. However, the robustness of the estimates to at least one breakdown of the control variable in conjunction with the trends shown in Figure 2.6 should alleviate concerns that the findings are only an artifact of potential differences in the age composition of Hispanics in places with higher DACA take-up.

Effect of "Treatment on the Treated"

A broader concern may be that it is possible that some characteristic(s) (including age) about those who are eligible for DACA drives the observed effect, meaning the DACA Take-up measures merely serve as proxies for DACA eligibility and the associated characteristics. Again, this argument does not hold if you believe the parallel trends assumption is satisfied. To my knowledge, no one has produced county-level estimates of the DACA-eligible population. The Migration Policy Institute (MPI) provides state-level and nationwide estimates that imply that over half of Hispanics who are eligible for DACA have taken it up.[44]

[44]See https://www.migrationpolicy.org/programs/data-hub/deferred-action-childhood-arrivals-daca-profiles.

Thus, we might expect the effect of *eligibility* on the relative number of Hispanic home loan applications to be a little more than half that of the effect of take-up if the observed effect (measured by the take-up variable) is driven only by the effect of DACA on those who take it up. In other words, I argue that the measured effect is the effect of "treatment on the treated," and if the observed positive effect is driven by treatment and not by characteristics associated with eligibility for treatment, then the effect of eligibility would be just over 50% of the effect of take-up and not more.

Unfortunately, there are no readily available estimates of DACA eligibility at the county level that I can rely on. Nonetheless, I am able to generate an approximation using the microdata from section 2.3. I start with the estimates of the undocumented population generated by the imputation procedure. From there, individuals are classified as "DACA eligible" if they satisfy the age and time of immigration requirements for DACA eligibility.[45] Since the list of identified counties in the microdata changes between 2011 and 2012, I use estimates only from 2008-2011. For each county, I take the median of the estimated number of DACA eligible Hispanics over the 2008-2011 period and divide by the median of the estimated number of Hispanics over the same period.[46] As with the take-up measure, the eligibility measure is split into 3 categories. Counties where $DACA\ Eligible = \frac{\#\ DACA\ eligible}{Hispanic\ Population}$ is zero comprise the (excluded) "DACA Eligible LOW" category. Those with an above-median measure among the non-zero counties are classified as high eligibility, and those with below-median eligibility measures among the non-zero counties are classified as medium eligibility.

After creating the eligibility measures, I rerun regressions corresponding to equation 3

[45]Specifically, I categorize them as eligible if they were born in 1981 or later, have been in the U.S. since at least 2007, and arrived in the U.S. when they were no older than 16.

[46]Estimating the Hispanic population from the microdata instead of using more reliable estimates (such as those from the 2010 census) leads the eligibility measure (a fraction of two values generated by ACS sampling and weighting) to implicitly account for artificially high or low counts of the DACA eligible that are driven by the sampling or weighting procedure in the ACS. In other words, if the count of Hispanic DACA eligible is inflated because Hispanics received too much weight in that county, then the count of the Hispanic population should be similarly inflated. Dividing to arrive at the chosen eligibility measure, then, can correct for this inflation as the scaled numerator is associated with a similarly scaled denominator.

except "DACA Takeup" is replaced with "DACA Eligible." Since I am only able to construct eligibility measures for the subset of counties identified in the ACS microdata files, I restrict the sample to these counties and also rerun specification 3 for the take-up measure on this restricted sample for a better comparison of the effect of eligibility with the effect of take-up. Results are presented in Table 2.7. The coefficients on DACA <u>Eligible</u> HI × Post are positive but smaller in magnitude (and statistically insignificant in two of three columns) than the corresponding coefficients on DACA <u>Takeup</u> HI × Post, consistent with the story that it is the *take-up* of DACA that is responsible for the measured effect.

Robustness to Inclusion of Small Counties

Finally, since population data from SEER is available for all counties, specification 3 can be run for all counties in the U.S., not just those with populations greater than 65,000 in 2010 (what is available when using ACS data). These are not the choice specifications, however. First, population data from SEER is based largely on birth and death rates and may be less sensitive to migration (especially migration of undocumented immigrants) and may risk understating births or deaths of minorities and immigrants. These population estimates are based on the census and undergo a sort of scaling that incorporates some information from the ACS. However, any kind of systematic scaling can be expected to be largely captured by county fixed effects already included in the regressions. Population estimates from the ACS, while more volatile, provide a more current picture of the county's population (i.e. changes less easily captured by fixed effects). Second, the addition of nearly 1,000 counties with small populations and where population estimates and DACA Takeup are likely less accurate makes the use of weights in regressions much more important.

I run regressions corresponding to specification (2.3), using all counties in CBSA's[47]

[47] A number of small counties in the U.S. do not belong to any CBSA. These are excluded. Note that Alaska and the other counties that were dropped earlier are still excluded.

and present results in Table 2.8. The first 3 columns are analogous to Table 2.5. The last 3 columns aggregate counties to the CBSA level and run regressions where the unit of observation is a CBSA-year, instead of a county-year (such that the subscript c in equation (2.3) can be thought of as indicating a CBSA instead of a county). In the unweighted regressions, significance is lost, but the expected sign remains. Once weights are applied, the magnitude and significance of each estimate is consistent with those in the choice specifications.

2.4.4. Related Outcomes

Results for Loan Approval Rates

The analysis so far has found that DACA increased the Hispanic home loan application rate, indicative of a behavioral, demand-side response to the policy. Note that an increase in applications (observed in section 2.4.2) will lead to an increase in homeownership rates (observed in section 2.3) even if the home loan approval rate remains constant. However, there is reason to believe that the approval rate may also change in response to DACA. On the one hand, if the applicants DACA induces to enter the market for owner-occupied housing are less qualified than other Hispanic applicants, overall Hispanic home loan approval rates would fall as the pool of applicants becomes less qualified. On the other hand, if DACA raises incomes or otherwise improves an applicant's qualifications, then Hispanic approval rates would rise as the average applicant is more qualified.

To assess the impact of DACA on home loan approval rates, I run regressions similar to specification (2.3).

$$\begin{aligned}\left.\frac{Hisp\ Approvals}{Hisp\ Apps}\right._{ct} &= \alpha_c + \gamma_t + \beta_1 \left.\frac{Nonhisp\ Approvals}{Nonhisp\ Apps}\right._{ct} \\ &+ \beta_2(DACA\ Takeup\ MED_c \times post_t) \\ &+ \beta_3(DACA\ Takeup\ HI_c \times post_t) + \varepsilon_{ct}\end{aligned} \tag{2.4}$$

The outcome is now the Hispanic home loan approval rate defined as $\frac{Hispanic\ Approvals}{Hispanic\ Applications}$. In place of a control for relative Hispanic population is a control for the non-Hispanic home loan approval rate, which serves to account for possible localized, time-variant factors (such as fluctuations in local credit markets), which would not be captured by county and year fixed effects.

Results are presented in Table 2.9. The first 3 columns are identical to Table 2.5 and are included for reference. Columns 4 through 6 consistently find that, relative to counties with fewer than 1,000 DACA recipients, counties in both "DACA takeup" groups saw increases in Hispanic home loan approval rates of around 1.5 percentage points. These results are consistent with the story that DACA resulted in more home loan applicants *and* that applicants were more qualified than the average Hispanic applicant prior to DACA.[48] It might be the case that individuals who would become eligible for DACA were already strong applicants whose statuses prevented them from approval and DACA allowed these already highly qualified applicants to receive loans. However, as suggested in section 2.3, rising homeownership rates among the undocumented were at least partially attributable to rising incomes in the same group. Therefore, an at least equally plausible explanation for higher approval rates is that DACA created more home loan applicants but also *more qualified* home loan applicants. Altogether, Table 2.9 offers more support for the hypothesis that DACA increased homeownership rates and at least some of its effect is attributable to

[48]In percent terms, DACA's effect on the relative number of home loan *applications* (an increase of at least 4.5%) is roughly 3 times its effect on approval rates for counties in the highest DACA takeup category.

its impact on the incomes of undocumented immigrants.

Results for Loan Amount

Relatedly, if the undocumented immigrants who had applied for home loans prior to DACA (e.g. through the use of an ITIN) had to accept unfavorable terms, they may have been constrained to smaller or lower-quality housing to compensate. If DACA increased eligibility not just for home loans, but for larger or more favorable loans, undocumented immigrants would have a new opportunity to improve the quantity or quality of their housing.[49] Another similar possibility is that DACA recipients are more qualified than the average Hispanic home loan applicant or desire larger or higher quality housing, making them able or willing (respectively) to secure larger loans. In any of these cases, DACA would increase average loan amounts.

Alternatively, DACA recipients may still lack the qualifications to secure larger home loans, or DACA recipients may desire smaller housing or be more willing to accept lower quality housing. In this case, DACA could reduce average loan amounts. So, *a priori*, it is unclear what kind of impact (if any) DACA should have on the size of loans applied for or approved. However, if an effect is detected, it can shed further light on the ways in which the immigration policy affected undocumented immigrants' housing decisions.

As with applications and approvals, I aggregate the HMDA data on loan amounts to the county-year level. I adjust for inflation[50] and compute the natural logarithm of the average loan amount for each county-year. These averages are broken out by ethnicity and loan

[49]One way to think about this is to treat DACA as a policy that relieved the constraint (that may have been binding) imposed by reduced eligibility for loans. One may also think about DACA as a policy that lowered the price of home loans, which, regardless of whether pre-existing credit constraints were binding, induces both income and substitution effects.

[50]Results are in 2010 dollars.

decision (e.g. approved v.s. denied). I then run regressions using the following specification:

$$\begin{aligned} log(Hisp\ Loan\ Amt)_{ct} = \alpha_c + \gamma_t + \beta_1 log(Nonhisp\ Loan\ Amt)_{ct} \\ + \beta_2(DACA\ Takeup\ MED_c \times post_t) \\ + \beta_3(DACA\ Takeup\ HI_c \times post_t) + \varepsilon_{ct} \end{aligned} \tag{2.5}$$

Results are presented in Tables 2.10 and 2.11. In Table 2.10, $log(Hisp\ Loan\ Amt)$ refers to the average loan amount among Hispanic home loan *applications*. In Table 2.11, $log(Hisp\ Loan\ Amt)$ refers to the average loan amount among Hispanic home loans that were *approved*. The estimates suggest that the counties with the highest take-up rates of DACA saw a roughly 3% increase in the loan amount applied for by Hispanic applicants following the policy. For the subset of loans that were approved, the increase is more modest (roughly 2%) and only marginally statistically significant in just one of the three specifications but still consistently positive.

These results suggest that Hispanic applicants were at least *trying* to secure larger home loans post-DACA. The smaller estimates in the case of approved loans indicates not all were successful, and the lack of statistical significance at conventional levels in at least two of the specifications makes it difficult to conclude that the amount actually offered changed significantly. Results are consistent with the idea that undocumented immigrants wished to use the benefits of DACA (which may include improved credit access, higher incomes, lengthier expected duration of stay, etc.) to make improvements to the quantity or quality of their housing. However, the extent to which they were successful is unclear, consistent with the idea that financial or lending conditions still pose barriers to undocumented immigrants' abilities to achieve their desired quantity (or quality) of housing.

2.5. Treasury Department's New Rule

2.5.1. The Legal Clarification

The previous sections present results that imply that DACA increased home loan applications and homeownership rates in the undocumented population. However, the mechanism through which DACA affected these outcomes remains unclear. Since DACA is a policy that raised incomes of undocumented immigrants, it is possible that the entirety of the effect of the policy is driven by changing incomes. If this is the case, then one could argue that lack of legal status is not a barrier to homeownership as long as incomes are equal across immigrants of different statuses.

DACA's effect on income certainly plays some role in its effect on housing tenure, but is it the only important factor? For example, DACA may have reduced fear among the immigrant population and led to increased housing search. DACA may have reduced uncertainty about immigrants' expected duration of stay in their location, increasing their demand for owner-occupied housing. Also, DACA allowed immigrants who were previously excluded from favorable loan terms to access home loans that required much lower down payments. This last point may be especially important as prior work has noted that it is down payment constraints (insufficient wealth to cover down payments) that are most responsible (i.e. versus income constraints) for the inaccessibility of owner-occupied housing among renters who would choose it.

Previous studies most often assume a homogeneous borrowing constraint across individuals. Within the context of immigrants, I consider the possibility that the accessibility of home loans is heterogeneous, leading the allocation of owner-occupied housing to be heterogeneous. In other words, differences in loan accessibility can drive differences in homeownership rates. In this section, I will provide evidence that home loan accessibility can vary by immigration status and that this variation is responsible for some of the difference in undocumented immigrants' propensities to apply for home loans.

Unlike DACA, the 2003 Treasury rule change explicitly allowing the use of ITIN's in lieu of SSN's to open bank accounts and, by extension, access the mortgage market, did not impact undocumented immigrants' incomes or legal status. In fact, its purpose was not to disproportionately affect undocumented immigrants at all. Regardless of the new rule's intent, allowing the use of ITIN's to open bank accounts had a disproportionate impact on undocumented immigrants whose only other avenues to owner-occupied housing were paying the home price in full, successfully originating a loan under a fraudulent Social Security Number, or accessing credit through unregulated or less formal channels.[51] Therefore, if inaccessible credit was a binding constraint for undocumented immigrants, we would expect to see a disproportionate increase in home loan applications among likely undocumented immigrants following the rule change in 2003.

2.5.2. Data

As with the previous section, this section makes use of Home Mortgage Disclosure Act (HMDA) data. Since the period of analysis will be the mid-1990's through the mid-2000's, ACS data is unavailable for annual population estimates. Therefore, I use the estimates from SEER described in section 2.4.3. Since no county-level estimates of the undocumented population exist for years during the sample period, I rely on state-level estimates from Pew Research[52] in combination with estimates from the imputation procedure described in section 2.3.1. Specifically, if it is assumed that county undocumented populations within

[51] Included in such channels are the limited instances in which small financial institutions lent to individuals without Social Security numbers prior to 2003, the legality of which was, at best, ambiguous. For more information, see Appendix B.6.

[52] See https://www.pewresearch.org/hispanic/2018/11/27/unauthorized-immigration-estimate-appendix-c-additional-tables/.

a state grow at the same rate as the state undocumented population, then the following equality holds:

$$county\ undocumented_{2000} = county\ undocumented_{2010} \times \left(\frac{state\ undocumented_{2000}}{state\ undocumented_{2010}}\right)$$

To ensure estimates of the county-level undocumented population aren't driven by noise in one single year, I use the median of the estimated (via the imputation procedure) undocumented population in a county over the 2008-2011 period[53] as a proxy for the county's 2010 undocumented population.[54] I then scale this measure by the county's state's undocumented population growth rate derived from Pew's 2000 and 2010 state-level estimates of the undocumented population to arrive at the county's estimated undocumented population in 2000. The estimated undocumented population in 2000 is then divided by the county's Hispanic population in 2000 (the vast majority of undocumented immigrants during this time period are from Mexico and Central America) to arrive at a measure of the percent of the county's Hispanic population that is undocumented ($Hisp.\ Undoc\ Percentage = \frac{Estimated\ Hisp.\ Undoc\ Pop_{2000}}{Hisp.\ Pop_{2000}}$). I then generate an indicator variable for high undocumented population that takes value 1 if undocumented immigrants are over-represented among Hispanics in the county ($Undoc\ HI = 1$ if $Hisp.\ Undoc\ Percentage >$ median $Hisp.\ Undoc\ Percentage$). This indicator will be the independent variable of interest.

[53]Recall, the list of identifiable counties changes in 2012.

[54]Note that the sample of undocumented immigrants is restricted to just Hispanic undocumented immigrants here.

2.5.3. Specifications and Results

Difference-in-differences

Specifications resemble those of section 2.4.2, but here, $post_t$ refers to post-2003 (i.e. $post_t = 1$ if year $\geq$ 2004). The choice specification is equation (2.6).

$$\frac{Hisp\ Apps}{Total\ Apps}_{ct} = \alpha_c + \gamma_t + \beta_1 \frac{Hisp\ Pop}{Total\ Pop}_{ct} + \beta_2 (Undoc\ HI_c \times post_t) + \varepsilon_{ct} \tag{2.6}$$

Alternatively, the indicator $Undoc\ HI_c$ may be replaced by the continuous measure that approximated the percent of the Hispanic population in the county that was undocumented. To avoid potential spurious results attributable to error in the measurement of the precise number of undocumented immigrants, I focus on the specification as it is presented in equation (2.6), but Table 2.12 presents results from both formulations.

The choice specification is column 1. Columns 2 and 5 add weights based on county population. Columns 3 and 6 also drop California. Column 1 implies that the ruling change led to a 1.34 percentage point change in the percent of mortgage applications made by Hispanics. Off of a mean of about 7.8 percent, this equates to an effect of roughly 17%. While large, I should note that the parallel trends identifying assumption of the diff-in-diff design likely does not perfectly hold in this case. See figures 2.7 and 2.8. It's not clear that the data exhibit a sizable and definitive upward trend that would be expected to continue in the absence of the Treasury department's rule change (or why they might), but in light of the observed point estimates, it is reasonable to believe that such a trend may exist. In the case of an upward trend, the results are biased upward to some degree. Nonetheless, the event study (presented in figure 2.8) that may raise concerns about differential pre-trends also illustrates a stark increase post-2003. Thus, depending on your belief about how well the parallel trends assumption holds, the point estimates in Table 2.12 should be considered upper bounds on the true effect of the ruling change.

Synthetic Control

Because the county-level data are structured as a panel (with the same units observed over time), it is possible to implement synthetic control as an alternative empirical strategy. An advantage to synthetic control is that pre-trends are, by design, parallel.[55] The synthetic control procedure is described further in Appendix B.1,[56] specifically B.1.5. For completeness and as tests of the identifying assumptions of the difference-in-differences designs, Appendix B.1 also presents event studies and synthetic control estimates for all other county-level outcomes for each period of analysis (DACA in 2012 and the Treasury rule change in 2003).

In summary, the estimated effect from the synthetic control estimation strategy is, as expected, smaller in magnitude.[57] It remains positive and statistically significant, indicating a 0.95 percentage point (or roughly 12%) increase in the relative number of Hispanic home loan applications, rather than the 1.34 percentage point (or roughly 17%) change indicated by the (biased) difference-in-differences results.

[55]Synthetic control is still far from being universally superior to difference-in-differences, however. In addition to being unable to use the design in most experiments where data is cross-sectional, there is no single, objective way to run synthetic control and produce p-values to assess statistical significance. Allowing for a data-driven approach to choosing predictor weights, while relatively free from researcher discretion, is a kind of "black box" method (it is not clear *why* the synthetic control is an appropriate counterfactual, except that, mathematically, it generates a close approximation of the treated unit's pre-treatment observed values). Synthetic control is also susceptible to problems with over-fitting, and its reliance on optimization algorithms can make it computationally costly. Nonetheless, with appropriate data, transparency, and minimization of problems with over-fitting, synthetic control proves to be an improvement over difference-in-differences in parts of this study.

[56]As noted in Appendix B.1, the interested reader may find the procedures described in even greater detail (additional tests for significance, addressing cases of over-fitting and poor pre-period match quality, further interpretation of results, etc.) in Appendix B.4.

[57]If synthetic control eliminates the bias from upward pre-trends, the estimated effect will be smaller than the corresponding difference-in-differences estimate that suffers from the bias.

2.5.4. Results for Loan Approval Rates

Section 2.4.4 finds that DACA raised home loan approval rates as well as home loan applications. Thus, DACA induced more qualified applicants to apply for home loans. If DACA's effect on approval rates is the result of its effect on incomes, then there is no reason to expect the same approval rate response to the Treasury rule change. As in section 2.4.4, I run regressions like those in equation (2.6), replacing the outcome with Hispanic approval rate and the control variable for relative Hispanic population with the control for non-Hispanic approval rate. Results are presented in Table 2.13.

Unlike DACA, the Hispanic home loan approval rate exhibits no discernible response to the 2003 Treasury rule change. Taken together with the evidence from DACA's effects on loan applications and approvals, the results support the hypothesis that DACA increased homeownership through its effects on application propensities *and* through its effect on applicant qualifications but the effect of the change in Treasury rules operated only through its effect on application propensities. Thus, Table 2.13 is further evidence that the analysis of the Treasury rule change is able to isolate the effect of credit constraints on homeownership in a way that the analysis of DACA's impact cannot. With zero change in approval rates (as indicated by columns 4 through 6 of Table 2.13), more applications (as indicated by columns 1 through 3 of Table 2.13) will still result in higher homeownership rates.

2.5.5. Results for Loan Amount

Finally, as with DACA, one might consider the effects of the 2003 change on the size of loans applied for or approved. Section 2.4.4 found that DACA increased the average size of loans Hispanic applicants applied for (and possibly, the average size of loans that were actually approved). Should we expect the change in Treasury rules to induce a similar change?

Broadly, there are two potential explanations for the positive effect of DACA on loan amounts. 1) undocumented immigrants have preferences for larger loans, possibly because they are more tolerant of debt or possibly because they prefer larger or higher quality housing. When DACA expands loan eligibility, some undocumented immigrants enter the pool of loan applicants, increasing the size of the average loan (due to their preferences for larger loans). 2) something about DACA changes the preferences of undocumented immigrants, leading them to demand more housing. For example, it could be that DACA increases the expected length of stay in the United States, increasing the value of owner-occupied housing, or it may simply be that DACA's effect on incomes raises budget constraints to accommodate a consumption bundle that includes a greater quantity (or quality) of housing. Because the first explanation is dependent on a change in credit access, finding a similar effect of the Treasury rule change on loan amounts would support that explanation (as long as undocumented immigrants have similar preferences in 2003 as they do 9 years later in 2012). The absence of an effect of the Treasury rule change on loan amounts would suggest that the new mortgage applicants (post-2003) have similar preferences to the existing population of Hispanic home loan applicants, and a negative effect would suggest they are unwilling to take on as much debt or are interested in smaller or lower quality housing.[58]

The regression specification is comparable to equation (2.5).

$$\begin{aligned} log(Hisp\ Loan\ Amt)_{ct} = \alpha_c + \gamma_t + \beta_1 log(Nonhisp\ Loan\ Amt)_{ct} \\ + \beta_2 (Undoc\ HI_c \times post_t) + \varepsilon_{ct} \end{aligned} \tag{2.7}$$

Results are presented in Tables 2.14 and 2.15. As before, in Table 2.14, $log(Hisp\ Loan\ Amt)$ refers to the average loan amount among Hispanic home loan *applications*. In Table 2.15,

[58]Note that an unwillingness to apply for larger loans may be driven by a belief that they would not be approved for such a loan, anyway. This would be comparable to what Charles and Hurst (2002) refer to as a "discouragement effect."

log(*Hisp Loan Amt*) refers to the average loan amount among Hispanic home loans that were *approved*. The estimated effect on the size of the loan applied for is not statistically distinguishable from zero, and the estimated effect on the size of approved loans is only marginally significant (at the 90% level) and only in regressions where weights are applied. Where it is significant at the 90% confidence level, the estimate would imply a roughly 2% reduction in the size of approved loans. No regression yields results that would suggest a non-zero and positive effect.[59] Therefore, if anything, applicants induced to apply by the expanded credit access following the Treasury rule change have preferences for *smaller* loans and are *less* qualified than the average Hispanic applicant. So, unless housing (and/or debt) preferences of undocumented immigrants changed prior to DACA's implementation (i.e. unless undocumented immigrants started to prefer larger loans relative to all Hispanics), these results do not support the explanation that DACA increased average loan amounts because undocumented immigrants prefer larger loans or more housing.

The findings are consistent with only one of the explanations for DACA's effect on loan amounts - DACA did something to change housing and/or borrowing preferences. Therefore, while other specific mechanisms are possible (e.g. DACA extended expected length of stay in the U.S.), the results are once again consistent with the theory that DACA's effect on income was an important part of the policy's effects on housing decisions. On the other hand, results from section 2.5.3 support the theory that credit access effects of DACA also played a role. A more optimistic interpretation of the findings is that policymakers may have several options available to close homeownership gaps. A more pessimistic view is that action must be taken on several different fronts (credit, income, security) to achieve housing equality. From this study, we know expanding credit access has worked. We have strong evidence that increasing income has worked as well. However, the results also indicate a persistence in the homeownership gap that has not been closed by policy and remains today.

[59] The largest effect any 95% confidence interval would include would still be an effect smaller than 3% (the estimated effect of DACA in comparable regressions).

2.6. Conclusion

The analysis of this paper provides insight into largely neglected segments of the literatures on immigration and homeownership. I provide among the first estimates of the homeownership gap between undocumented and legal resident immigrants. A simple difference in means indicates a massive (overstated, depending on the context) gap, suggesting a roughly 20% difference in homeownership rates, but even conditional on observable characteristics, undocumented immigrants are around 4% less likely to own their homes. To establish the existence of a causal link between lower homeownership rates and undocumented status and to explore the mechanisms through which undocumented status may reduce homeownership, I start by looking into DACA's effects on the relationship. I find that DACA led to a 25-30% reduction in the existing homeownership gap between undocumented and legal resident immigrants and increased the relative number of Hispanic home mortgage applications by roughly 5%. Finally, exploiting the differential impact of the 2003 Treasury Department decision to explicitly allow the use of ITIN's to open bank accounts, I provide evidence that undocumented immigrants have been constrained by restricted access to credit markets, finding that the rule change led to a 12% increase in relative Hispanic home loan applications in the areas with the greatest concentrations of undocumented immigrants.

These findings paint a broad picture of the market for owner-occupied housing faced by undocumented immigrants in the U.S. over that last 20 years, but they are individually important to the immigration, wealth inequality, and housing literatures, as well. Restricted access to owner-occupied housing and credit has consequences for the wealth and welfare of not only the 11 million undocumented people in the U.S., but also the millions of U.S. citizens who are their children. Even a social planner who allocates zero weight to the welfare of undocumented immigrants must acknowledge the disparate impact of restrictions on wealth accumulation and economic and residential mobility that result from inaccessible housing markets. In terms of the children whose parents are most impacted, the economic consequences disproportionately fall on those who come from among the most disadvantaged backgrounds. Thus, policymakers ought to consider the regressivity or progressivity of housing policy.

Additionally, constrained access to housing has efficiency implications. Theoretically, there exists a set of optimal matches between some subset of the undocumented population and mortgage lenders (and sellers of owner-occupied housing). The barriers that prevent matches between undocumented immigrants and mortgage lenders (lack of access to bank accounts/financial institutions, search frictions, etc.) prevent the optimal allocation of the housing stock. Also, recall that the first-order effect of fewer homeowners among the undocumented population is higher rents and lower home values.

Lastly, thinking beyond the scope of the immigrant population for a moment, these results highlight the significant role borrowing constraints play in perpetuating wealth gaps in the U.S. The findings of much of this paper arise because of one group's differential access to home financing. There is a causal link between differential access to credit and home loans. Prior work has found that homeownership gaps persist, in large part, because one group does not apply for home loans to the same extent as another. Some may interpret this fact as the former group's revealed preference for rental housing. However, my findings suggest that the same outcome would arise if the former group's access to financing is more restricted than the latter's. Better understanding the mechanisms behind the persistence of homeownership gaps is an important step in addressing the persistence of wealth inequality in the United States.

2.7. Tables and Figures

	State	2015 Imputed	2017 Imputed	2017 Pew	2017 CMS	2016 MPI	2015 DHS
1	California	2340	2095	2000	2400	3100	2900
2	Texas	1796	1844	1600	1800	1600	1900
3	Florida	901	900	825	766	656	810
4	New York	848	777	650	753	940	590
5	New Jersey	512	505	450	452	526	440
6	Illinois	510	465	425	460	487	450
7	Georgia	410	388	375	335	351	390
8	North Carolina	334	329	325	300	321	390
9	Virginia	304	295	275	243	269	310
10	Washington	251	260	250	251	229	
11	Arizona	255	249	275	252	226	
12	Maryland	275	247	250	224	247	
	Total (millions)	11.5	11.1	10.5	10.7	11.3	12.0

Notes: For comparison, estimates from Pew, CMS, DHS, and the Migration Policy Institute are provided.

Table 2.1: Estimates of the undocumented population by state of residence (in thousands).

	Legal Resident	Undocumented	Citizen
owned	0.4217	0.3469	0.7219
age	45.92	40.8	54.17
male	0.5564	0.6106	0.5155
married	0.7089	0.5318	0.5281
years in us	17.61	14.7	NA
monthly income (2010 dollars)	4489	4206	6075
people in household	3.631	3.594	2.408
workers in household	1.49	1.66	1.135
children in household	1.249	1.267	0.5196

Table 2.2: Summary statistics for the household-level microdata sample.

	owned	owned	owned	owned	owned	owned
(Intercept)	0.3513***			0.3735***		
	(0.0065)			(0.0099)		
undoc	−0.0524***	−0.0733***	−0.0156***	−0.0710***	−0.0928***	−0.0206***
	(0.0042)	(0.0029)	(0.0026)	(0.0054)	(0.0043)	(0.0029)
years in U.S.			0.0110***			0.0141***
			(0.0005)			(0.0007)
(years in U.S.)^2			−0.0001***			−0.0002***
			(0.0000)			(0.0000)
age			0.0138***			0.0163***
			(0.0005)			(0.0005)
(age)^2			−0.0001***			−0.0001***
			(0.0000)			(0.0000)
log(income)			0.0369***			0.0413***
			(0.0010)			(0.0013)
never married			−0.1274***			−0.1407***
			(0.0031)			(0.0034)
female			0.0189***			0.0196***
			(0.0020)			(0.0021)
number workers			−0.0061***			−0.0040**
			(0.0016)			(0.0016)
number people			0.0247***			0.0203***
			(0.0015)			(0.0022)
number kids			−0.0277***			−0.0192***
			(0.0018)			(0.0019)
Fixed Effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Adj. R^2	0.0030	0.1276	0.2321	0.0054	0.0945	0.2152
Num. obs.	292816	292816	292816	468960	468960	468960
N Clusters	2350	2350	2350	1077	1077	1077

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Columns 1-3 use the shorter sample period but have more precise geographic fixed effects. Robust standard errors clustered at the smallest possible geographic unit for the sample. All regressions use household weights provided by the ACS.

Table 2.3: Linear probability models for housing tenure (owned = 1).

	owned	owned	owned
undoc	-0.1154^{***}	-0.0242^{***}	-0.0293^{***}
	(0.0050)	(0.0033)	(0.0034)
undoc $\times$ post	0.0413^{***}	0.0065^{*}	0.0090^{***}
	(0.0039)	(0.0035)	(0.0035)
years in U.S.		0.0141^{***}	0.0144^{***}
		(0.0007)	(0.0007)
(years in U.S.)^2		-0.0002^{***}	-0.0002^{***}
		(0.0000)	(0.0000)
age		0.0163^{***}	0.0173^{***}
		(0.0005)	(0.0006)
(age)^2		-0.0001^{***}	-0.0001^{***}
		(0.0000)	(0.0000)
log(income)		0.0413^{***}	
		(0.0013)	
never married		-0.1407^{***}	-0.1572^{***}
		(0.0034)	(0.0036)
female		0.0196^{***}	0.0101^{***}
		(0.0021)	(0.0020)
number workers		-0.0040^{**}	0.0240^{***}
		(0.0016)	(0.0016)
number people		0.0203^{***}	0.0167^{***}
		(0.0022)	(0.0022)
number kids		-0.0192^{***}	-0.0174^{***}
		(0.0019)	(0.0019)
Fixed Effects	Yes	Yes	Yes*
Controls	No	Yes	Yes*
Adj. R^2	0.0949	0.2152	0.2037
Num. obs.	468960	468960	468960
N Clusters	1077	1077	1077

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Robust standard errors clustered at the smallest possible geographic unit for the sample. All regressions use household weights provided by the ACS.

Table 2.4: Linear probability models for housing tenure (owned = 1).

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$
$\frac{Hisp.\ Pop}{Total\ Pop}$	0.9992^{***}	1.3197^{***}	1.2704^{***}
	(0.1099)	(0.2152)	(0.1086)
DACA Takeup MED × Post	0.0016	−0.0050	0.0024
	(0.0020)	(0.0048)	(0.0024)
DACA Takeup HI × Post	0.0033^{***}	0.0054^{***}	0.0052^{**}
	(0.0010)	(0.0020)	(0.0020)
Weights	No	Yes	Yes
Excludes CA	No	No	Yes
Adj. R^2	0.9918	0.9938	0.9949
Num. obs.	6376	6376	6056
N Clusters	476	476	446
Outcome Mean	0.0711	0.0711	0.0633
Outcome Median	0.0360	0.0360	0.0341

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Outcome is $\frac{Hisp.\ Apps}{Total Apps}$. All specifications contain county and year fixed effects. All columns present robust standard errors clustered at the CBSA level. Column 1 applies no weighting. Column 2 weights counties by total population. Column 3 weights by population but drops all counties in California.

Table 2.5: Choice specifications for county-level DACA analysis, corresponding to equation 3.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$
$\frac{Hisp.\ Pop}{Total\ Pop}$	1.2841^{***}	1.4678^{***}	1.4475^{***}			
	(0.1177)	(0.2359)	(0.1250)			
$\frac{Hisp.\ Children}{Total\ Children}$				0.4523^{***}	0.7772^{***}	0.3931^{***}
				(0.0865)	(0.2396)	(0.1273)
$\frac{Hisp.\ Youth}{Total\ Youth}$				0.1014^{*}	0.1128	0.0719
				(0.0517)	(0.0977)	(0.0948)
$\frac{Hisp.\ Adults}{Total\ Adults}$				0.6753^{***}	0.4553^{*}	0.9650^{***}
				(0.1483)	(0.2597)	(0.2238)
DACA Takeup MED × Post	0.0003	−0.0059	0.0014	−0.0001	−0.0044	−0.0002
	(0.0019)	(0.0048)	(0.0021)	(0.0020)	(0.0032)	(0.0017)
DACA Takeup HI × Post	0.0031^{***}	0.0047^{**}	0.0043^{**}	0.0025^{**}	0.0039^{*}	0.0027
	(0.0011)	(0.0021)	(0.0021)	(0.0010)	(0.0022)	(0.0021)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9923	0.9939	0.9951	0.9922	0.9940	0.9952
Num. obs.	6376	6376	6056	6376	6376	6056
N Clusters	476	476	446	476	476	446
Outcome Mean	0.0711	0.0711	0.0633	0.0711	0.0711	0.0633
Outcome Median	0.0360	0.0360	0.0341	0.0360	0.0360	0.0341

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: First 3 columns replicate Table 2.5 results using population data from SEER instead of the ACS. The last 3 columns replace the single control for fraction of population that is Hispanic with similar controls based on age. Children, Youth, and Adults are described as those under 18, those between 18 and 30, and those over 30, respectively. All specifications contain county and year fixed effects. All columns present robust standard errors clustered at the CBSA level. Columns 1 and 4 apply no weighting. Columns 2 and 5 weight counties by total population. Columns 3 and 6 weight by population and drop all counties in California.

Table 2.6: First set of robustness tests for county-level DACA analysis.

	Eligibility	Take-up	Eligibility	Take-up	Eligibility	Take-up
$\frac{Hisp.\ Pop}{Total\ Pop}$	1.1874***	1.2135***	1.3233***	1.4593***	1.2938***	1.3682***
	(0.1129)	(0.1200)	(0.2072)	(0.2732)	(0.1269)	(0.1300)
DACA Eligible MED × Post	−0.0020		−0.0083		0.0004	
	(0.0016)		(0.0056)		(0.0023)	
DACA Eligible HI × Post	0.0013		0.0025		0.0045***	
	(0.0016)		(0.0020)		(0.0017)	
DACA Takeup MED × Post		−0.0012		−0.0064		0.0015
		(0.0020)		(0.0052)		(0.0027)
DACA Takeup HI × Post		0.0035**		0.0070***		0.0065**
		(0.0016)		(0.0027)		(0.0026)
Weights	No	No	Yes	Yes	Yes	Yes
Excludes CA	No	No	No	No	Yes	Yes
Adj. R^2	0.9948	0.9948	0.9942	0.9943	0.9956	0.9956
Num. obs.	3008	3008	3008	3008	2736	2736
N Clusters	376	234	376	234	342	209
Outcome Mean	0.0937	0.0937	0.0937	0.0937	0.0808	0.0808
Outcome Median	0.0481	0.0481	0.0481	0.0481	0.0442	0.0442

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Each pair of columns corresponds to a column in Table 2.5, where the first in the pair presents results from regressions where the take-up measure is replaced with the eligibility measure. Note the smaller sample size that results from restricting to counties where the DACA eligibility measure can be defined. All specifications contain county and year fixed effects. All columns present robust standard errors. In the odd-numbered columns, standard errors are clustered at the county level. In the even-numbered columns, standard errors are clustered at the CBSA level ("Eligible" is a variable estimated at the county level, whereas "Takeup" is estimated at the CBSA level). The first pair of columns apply no weighting. The second pair weight counties by total population. The final pair weight by population and drop all counties in California.

Table 2.7: Second set of robustness tests for county-level DACA analysis.

	County	County	County	CBSA	CBSA	CBSA
$\frac{Hisp.\ Pop}{Total\ Pop}$	1.1592***	1.4475***	1.4297***	1.1507***	1.4318***	1.3789***
	(0.1089)	(0.2160)	(0.1176)	(0.1200)	(0.3049)	(0.1095)
DACA Takeup MED × Post	0.0033	−0.0057	0.0016	−0.0046	−0.0055	0.0021
	(0.0022)	(0.0047)	(0.0021)	(0.0033)	(0.0054)	(0.0016)
DACA Takeup HI × Post	0.0012	0.0044**	0.0042**	0.0018	0.0042**	0.0039**
	(0.0009)	(0.0019)	(0.0020)	(0.0015)	(0.0019)	(0.0019)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9713	0.9928	0.9935	0.9864	0.9937	0.9950
Num. obs.	14368	14368	14008	7296	7296	7024
N Clusters	912	912	878	912	912	878
Outcome Mean	0.0616	0.0616	0.0578	0.0759	0.0759	0.0700
Outcome Median	0.0262	0.0262	0.0252	0.0295	0.0295	0.0279

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: The first 3 columns are identical to Table 2.5 except that regressions are run on the expanded set of counties identifiable in the SEER population data. The last 3 columns, similarly, include all counties, but here, the spacial unit of analysis is a CBSA (i.e. population characteristics are aggregated to the CBSA level and each CBSA-year is treated as one observation instead of each county-year). The first (last) 3 columns contain county (CBSA) and year fixed effects. All columns present robust standard errors, clustered at the CBSA level. Columns 1 and 4 apply no weighting. Columns 2 and 5 weight counties by total population. Columns 3 and 6 weight by population and drop all counties in California.

Table 2.8: Third set of robustness tests for county-level DACA analysis.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$
$\frac{HispPop}{TotalPop}$	0.9992***	1.3197***	1.2704***			
	(0.1099)	(0.2152)	(0.1086)			
$\frac{NonhispApprovals}{NonhispApps}$				0.6926***	0.8179***	0.8200***
				(0.0888)	(0.0539)	(0.0560)
DACA Takeup MED × Post	0.0016	−0.0050	0.0024	0.0149***	0.0180***	0.0196***
	(0.0020)	(0.0048)	(0.0024)	(0.0052)	(0.0039)	(0.0045)
DACA Takeup HI × Post	0.0033***	0.0054***	0.0052**	0.0133**	0.0132***	0.0132***
	(0.0010)	(0.0020)	(0.0020)	(0.0067)	(0.0040)	(0.0041)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9918	0.9938	0.9949	0.3914	0.5384	0.5202
Num. obs.	6376	6376	6056	6369	6369	6049
N Clusters	476	476	446	476	476	446
Outcome Mean	0.0711	0.0711	0.0633	0.7939	0.7939	0.7918
Outcome Median	0.0360	0.0360	0.0341	0.8097	0.8097	0.8065

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Columns 1 through 3 replicate Table 2.5 and are provided for reference.

Table 2.9: Effect on relative number of Hispanic applications and on Hispanic approval rate.

	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	log(hisp loan amt)	log(hisp loan amt)	log(hisp loan amt)
$\frac{HispPop}{TotalPop}$	0.9992***	1.3197***	1.2704***			
	(0.1099)	(0.2152)	(0.1086)			
log(nonhisp loan amt)				0.9461***	1.0132***	1.0072***
				(0.0541)	(0.0442)	(0.0597)
DACA Takeup MED × Post	0.0016	−0.0050	0.0024	0.0247**	0.0152	0.0143
	(0.0020)	(0.0048)	(0.0024)	(0.0126)	(0.0120)	(0.0142)
DACA Takeup HI × Post	0.0033***	0.0054***	0.0052**	0.0341***	0.0268**	0.0278**
	(0.0010)	(0.0020)	(0.0020)	(0.0129)	(0.0122)	(0.0128)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9918	0.9938	0.9949	0.8695	0.9556	0.9425
Num. obs.	6376	6376	6056	6314	6314	6010
N Clusters	476	476	446	476	476	446
Outcome Mean	0.0711	0.0711	0.0633	4.9349	4.9349	4.9117
Outcome Median	0.0360	0.0360	0.0341	4.9091	4.9091	4.8943

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1 through 3 replicate Table 2.5 and are provided for reference.

Table 2.10: Effect on relative number of Hispanic applications and on log average Hispanic applications loan amount.

	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	log(hisp loan amt)	log(hisp loan amt)	log(hisp loan amt)
$\frac{NonhispApprovals}{NonhispApps}$	0.6926***	0.8179***	0.8200***			
	(0.0888)	(0.0539)	(0.0560)			
log(nonhisp loan amt)				1.0044***	1.0638***	1.0572***
				(0.0578)	(0.0494)	(0.0711)
DACA Takeup MED × Post	0.0149***	0.0180***	0.0196***	0.0211*	0.0137	0.0130
	(0.0052)	(0.0039)	(0.0045)	(0.0124)	(0.0113)	(0.0134)
DACA Takeup HI × Post	0.0133**	0.0132***	0.0132***	0.0227*	0.0152	0.0157
	(0.0067)	(0.0040)	(0.0041)	(0.0124)	(0.0122)	(0.0129)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.3914	0.5384	0.5202	0.8563	0.9527	0.9389
Num. obs.	6369	6369	6049	6306	6306	6002
N Clusters	476	476	446	476	476	446
Outcome Mean	0.7939	0.7939	0.7918	4.9789	4.9789	4.9569
Outcome Median	0.8097	0.8097	0.8065	4.9598	4.9598	4.9451

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1 through 3 replicate the second half of Table 2.9 and are provided for reference.

Table 2.11: Effect on Hispanic approval rate and on log average Hispanic approved loan amount.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$
$\frac{HispPop}{TotalPop}$	1.1520***	1.0638***	1.1267***	1.1665***	1.1246***	1.1158***
	(0.0836)	(0.1243)	(0.1204)	(0.0843)	(0.1142)	(0.1181)
Hisp. Undoc HI × Post	0.0134***	0.0262***	0.0104**			
	(0.0036)	(0.0079)	(0.0045)			
Hisp. Undoc Percentage × Post				0.0351**	0.0390*	0.0557***
				(0.0139)	(0.0229)	(0.0191)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9738	0.9728	0.9811	0.9735	0.9713	0.9814
Num. obs.	4849	4849	4407	4849	4849	4407
N Clusters	373	373	339	373	373	339
Outcome Mean	0.0778	0.0778	0.0646	0.0778	0.0778	0.0646
Outcome Median	0.0328	0.0328	0.0280	0.0328	0.0328	0.0280

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: The first 3 columns correspond to equation (2.6). The last 3 replace the indicator for high (relative) undocumented population with the continuous measure used to generate the indicator. Outcome is $\frac{Hisp.\ Apps}{Total\ Apps}$. All specifications contain county and year fixed effects. All columns present robust standard errors clustered at the county level. Columns 1 and 4 apply no weighting. Columns 2 and 5 weight counties by total population. Columns 3 and 6 weight by population but drop all counties in California.

Table 2.12: Results from the Treasury ruling analysis.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$
$\frac{HispPop}{TotalPop}$	1.1520***	1.0638***	1.1267***			
	(0.0836)	(0.1243)	(0.1204)			
$\frac{NonhispApprovals}{NonhispApps}$				0.9156***	0.9701***	0.9661***
				(0.0349)	(0.0343)	(0.0381)
Hisp. Undoc HI × Post	0.0134***	0.0262***	0.0104**	−0.0001	−0.0012	−0.0028
	(0.0036)	(0.0079)	(0.0045)	(0.0058)	(0.0044)	(0.0051)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9738	0.9728	0.9811	0.6083	0.6852	0.6807
Num. obs.	4849	4849	4407	4841	4841	4399
N Clusters	373	373	339	373	373	339
Outcome Mean	0.0778	0.0778	0.0646	0.7771	0.7771	0.7733
Outcome Median	0.0328	0.0328	0.0280	0.8000	0.8000	0.7971

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Notes: Columns 1 through 3 replicate the first half of Table 2.12 and are provided for reference.

Table 2.13: Effect on relative number of Hispanic applications and on Hispanic approval rate.

	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	log(hisp loan amt)	log(hisp loan amt)	log(hisp loan amt)
$\frac{HispPop}{TotalPop}$	1.1520***	1.0638***	1.1267***			
	(0.0836)	(0.1243)	(0.1204)			
log(nonhisp loan amt)				0.8100***	0.9196***	0.9003***
				(0.0940)	(0.0868)	(0.1050)
Hisp. Undoc HI × Post	0.0134***	0.0262***	0.0104**	0.0079	−0.0115	−0.0179
	(0.0036)	(0.0079)	(0.0045)	(0.0107)	(0.0108)	(0.0124)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.9738	0.9728	0.9811	0.8631	0.9402	0.9200
Num. obs.	4849	4849	4407	4831	4831	4392
N Clusters	373	373	339	373	373	339
Outcome Mean	0.0778	0.0778	0.0646	4.7846	4.7846	4.7395
Outcome Median	0.0328	0.0328	0.0280	4.7632	4.7632	4.7287

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1 through 3 replicate the first half of Table 2.12 and are provided for reference.

Table 2.14: Effect on relative number of Hispanic applications and on log average Hispanic applications loan amount.

	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	log(hisp loan amt)	log(hisp loan amt)	log(hisp loan amt)
$\frac{NonhispApprovals}{NonhispApps}$	0.9156^{***}	0.9701^{***}	0.9661^{***}			
	(0.0349)	(0.0343)	(0.0381)			
log(nonhisp loan amt)				0.7981^{***}	0.9215^{***}	0.9058^{***}
				(0.1027)	(0.0889)	(0.1095)
Hisp. Undoc HI × Post	-0.0001	-0.0012	-0.0028	0.0002	-0.0196^{*}	-0.0247^{*}
	(0.0058)	(0.0044)	(0.0051)	(0.0108)	(0.0113)	(0.0126)
Weights	No	Yes	Yes	No	Yes	Yes
Excludes CA	No	No	Yes	No	No	Yes
Adj. R^2	0.6083	0.6852	0.6807	0.8545	0.9365	0.9147
Num. obs.	4841	4841	4399	4826	4826	4387
N Clusters	373	373	339	373	373	339
Outcome Mean	0.7771	0.7771	0.7733	4.8412	4.8412	4.8004
Outcome Median	0.8000	0.8000	0.7971	4.8190	4.8190	4.7886

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1 through 3 replicate the second half of Table 2.13 and are provided for reference.

Table 2.15: Effect on Hispanic approval rate and on log average Hispanic approved loan amount.

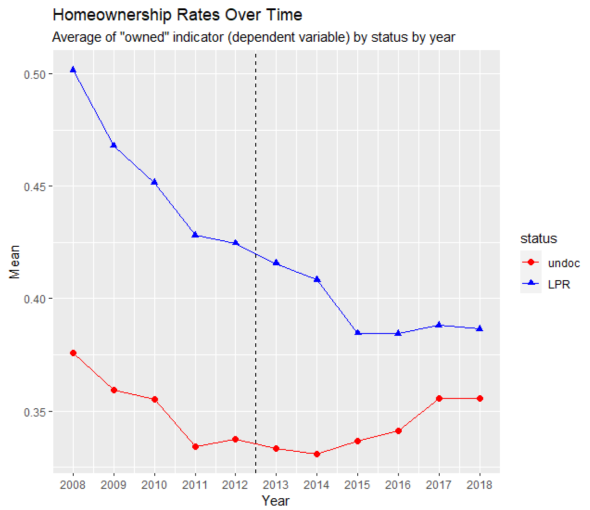

Figure 2.1: Homeownership rate by immigration status over time.

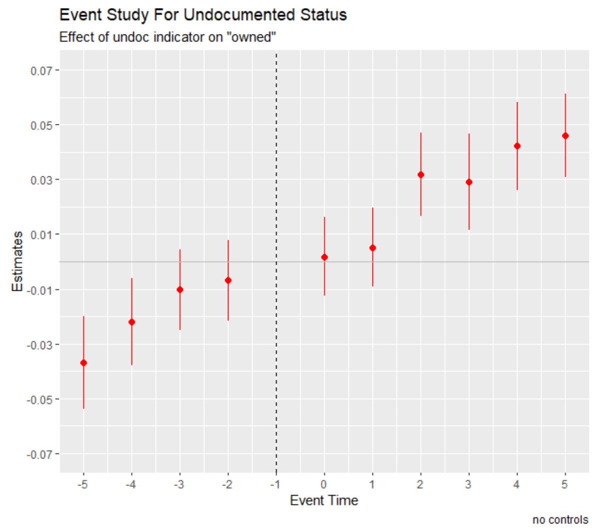

Figure 2.2: Event study corresponding to column 1 of Table 2.4.

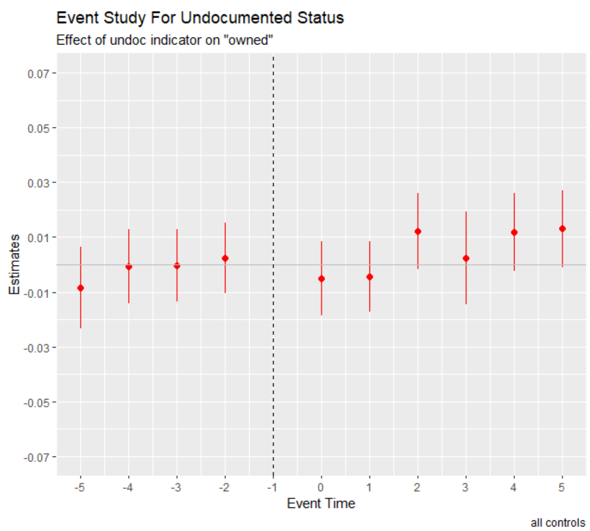

Figure 2.3: Event study corresponding to column 2 of Table 2.4.

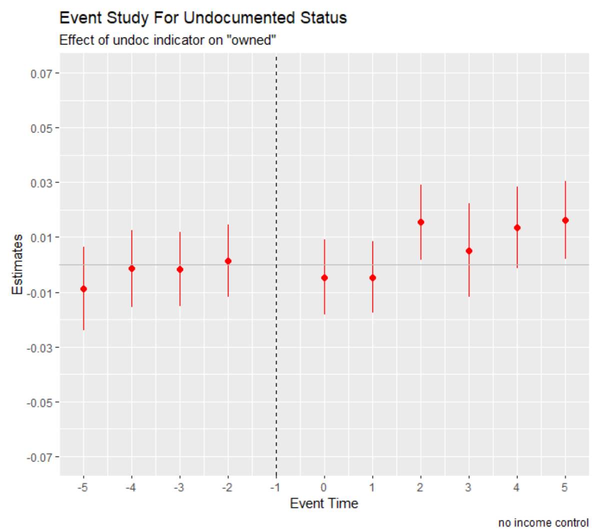

Figure 2.4: Event study corresponding to column 3 of Table 2.4.

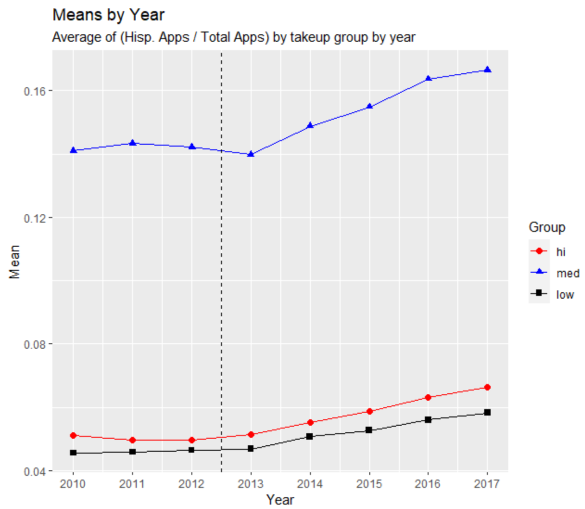

Figure 2.5: Average of county Hispanic home loan application rates by DACA takeup group over time.

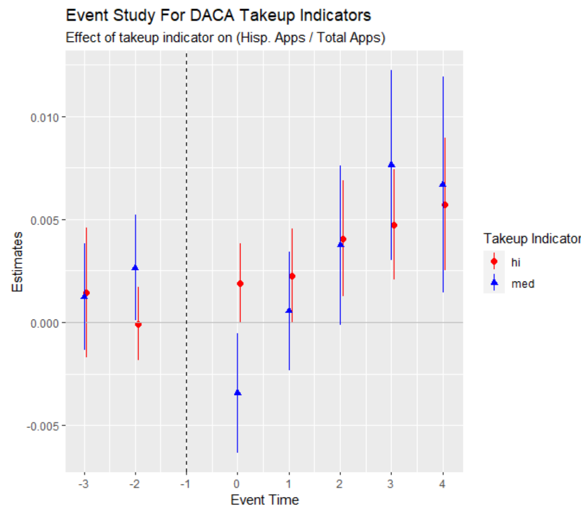

Figure 2.6: Event study corresponding to column 1 of Table 2.5.

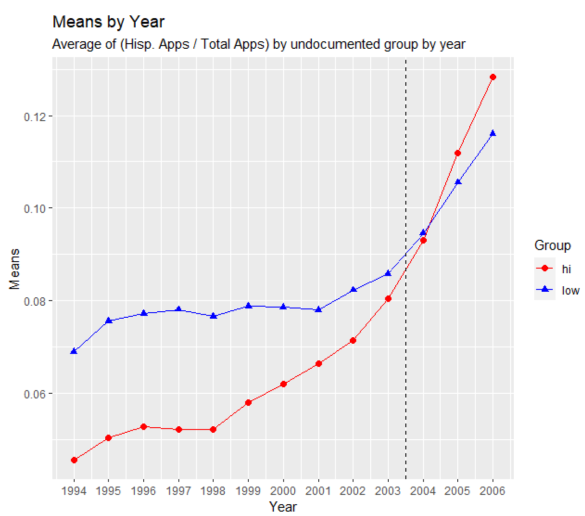

Figure 2.7: Average of county Hispanic home loan application rates by undocumented group over time.

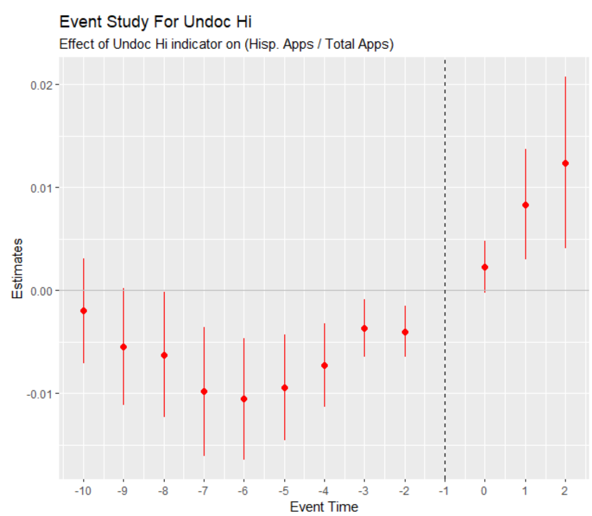

Figure 2.8: Event study corresponding to column 1 of Table 2.12.

Chapter 3

Publicly Provided and Private Health Insurance in Immigrant Populations

Publicly Provided and Private Health Insurance in Immigrant Populations

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I study the effect of Medicaid eligibility on health insurance and employment decisions in the context of immigrants in the United States. Using a fuzzy differences-in-discontinuities design that exploits variation in Medicaid eligibility rules across states, I find evidence of crowd out effects of Medicaid on private insurance. The crowd out appears to be driven by immigrants who take up Medicaid in place of insurance they otherwise would have purchased. In other words, Medicaid reduces rates of coverage by privately purchased health insurance, but I do not find a similar reduction in employer-provided health insurance. I also find some evidence that Medicaid reduces the uninsured rate among low-income immigrants. I do not find evidence that Medicaid reduces labor supply among immigrants. The study seeks to inform policymakers as greater consideration is given to the expansion of public health insurance and the use of public charge rules to determine an immigrant's eligibility to reside in the U.S.

Keywords: Immigration, public charge, health insurance, crowd out

JEL Classification: H4, H5, I1, I3, J6

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3.1. Introduction

The United States faces a humanitarian crisis at its southern border, and Europe has experienced a recent surge of migrants, refugees, and asylees. As a result, the treatment of prospective migrants has become an increasingly contentious political issue. A major concern, especially in the United States, is that public assistance programs will be unable to bear the burden of large-scale immigration when the immigrants are disproportionately under-educated and low-skilled workers. In line with this thinking, the Trump administration proposed new public charge rules that would restrict the pool of eligible migrants to those who were deemed to be unlikely to rely on public assistance. It is unclear, however, the extent to which low-skilled migrants would burden U.S. public assistance programs. Is there wisdom to implementing public charge rules? Is there merit to the arguments that extending more benefits to a broader range of immigrants in the U.S. would be highly costly?

Three overarching questions motivate this study. To what extent to immigrants actually use public assistance when eligible? What are the costs (direct and indirect) of such usage? And when it comes to the existing immigrant population already living in the United States, are there substantial benefits to increased usage that might help offset such costs?

In this paper, I consider immigrants' usage of and responsiveness to the country's largest public assistance program - Medicaid. I investigate the extent to which newly eligible immigrants take up the program. I consider potential costs of the program. In particular, I estimate the degree to which Medicaid crowds out enrollment in private insurance and if there is evidence that the program generates moral hazard in the labor market. Finally, I assess whether offering Medicaid to low-income immigrants has significant impacts on reducing the uninsured rate of this population.

Making use of a fuzzy differences-in-discontinuities design that exploits variation in

Medicaid eligibility rules across states and length of residence in the U.S., I find evidence that Medicaid provision reduces both the enrollment in private insurance and the uninsured rate. These findings add to the existing literature on costs and benefits of offering public health insurance in the U.S.[1] Importantly, I find evidence that, in contrast to prior studies, the crowd out effects of Medicaid are driven by reductions in purchased health insurance. I do not find evidence of a comparable effect on employer-sponsored health insurance (ESHI), and I detect no significant effects on other outcomes measuring labor supply.

3.2. Background and Existing Literature

The analysis in this study makes use of variation in immigrant eligibility for publicly provided health insurance. At the federal level, these rules were established in the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). PRWORA made several (restrictive) changes to Medicaid eligibility rules, and its effects on U.S. immigrant populations in the years immediately following its enactment have been studied[2] (though the studies come to somewhat different conclusions).

3.2.1. Benefits

A long literature has found empirical evidence of positive effects of provision of public health insurance on health-related outcomes.

[1]The setting of this study is of particular interest. Due to the recency of the passage of the Affordable Care Act, much of the existing literature on health insurance among immigrants studies a period prior to the act's implementation. Exceptions include studies of the impact of the ACA itself on immigrants such as Bustamante et al. (2019), Cohen and Schpero (2019), and Stimpson and Wilson (2018). Broadly consistent with their findings, I will provide evidence that aspects of the ACA had meaningful consequences for immigrant participation in health insurance markets.

[2]See, for instance, Borjas (2003), Royer (2005), Aizer (2007), and (Bronchetti, 2014).

Bronchetti (2014), who makes use of similar variation in immigrant eligibility for Medicaid over the period from 1998 to 2009, finds that such eligibility results in several improvements in health-related outcomes among immigrant children. She finds reductions in ER use and the probability a child in an immigrant family goes a year without visiting a doctor and increases in the use of preventative care, in the probability that a child in an immigrant family has a usual place where they receive health care, and the probability they are reported to be in excellent health. Focusing on take-up of public health insurance, Aizer (2007) finds that Medicaid reduces avoidable hospitalizations among enrolled children.

In contrast, Kaushal and Kaestner (2007) find that the 1996 welfare reform increased reports of delays in receipt (or no receipt) of care and reductions in health care visits for low-educated, foreign-born single mothers, but they do not find evidence that this translates to worsened health. Royer (2005) also finds that the reform (temporarily) reduced prenatal care but no concurrent effect on infant health.

Using data from the Behavioral Risk Factor Surveillance System (BRFSS), Bitler, Gelbach and Hoynes (2005) find that welfare reform in the 1990's led to reductions in health insurance and (some measures of) health care utilization among younger, single women and increased the likelihood that they needed care but found it unaffordable. Relevant to the present study, the authors find that effects are larger for Hispanic women and provide suggestive evidence that such effects may be, in part, driven by greater impacts on non-citizens.

Taken together, these studies suggest a host of potential positive effects from expanding public health insurance,[3] and several of these findings are accompanied by findings of reductions in the uninsured rate. To the extent that the extension of Medicaid to immigrants reduces the uninsured rate, the costs of such an extension may be offset by the resulting

[3] In addition to the effects on health, Jácome (2022) finds a negative relationship between Medicaid access and crime.

individual benefits and positive externalities.[4]

This does not imply that, in isolation, consideration of public charges posed by future immigrants not yet in the country should be dismissed. Clearly, immigrants not yet in the country (prospective immigrants) do not yet impose any costs[5] that may be mitigated by provision of public health insurance. However, it would imply that the average cost of each immigrant admitted currently is higher than it could be under a different welfare scheme. In that case, calculations of the public charge posed by an immigrant and consequently, the probability that an immigrant ought to be denied admission under more restrictive public charge rules would be overstated.[6]

3.2.2. Crowd Out

A number of studies have also provided estimates of crowd out caused by provision of public health insurance. Many of these studies too make use of variation from welfare reforms in the 1990's.

More broadly and with a focus on earlier reforms, Cutler and Gruber (1996) provide a crowd out estimate of 50% (50% of the increase in Medicaid coverage from expanding Medicaid - to pregnant women and children in their case - was associated with a reduction in private health insurance coverage). However, a subsequent study by Card and Shore-Sheppard (2004) found little or no crowd out evidenced by the Medicaid reforms in the 1990's and attribute Cutler and Gruber (1996)'s result to (restrictive assumptions implicit

[4]This may be especially relevant with respect to crime reductions (Jácome, 2022), reduced reliance on costly ER care (as in Bronchetti (2014)), and in settings where health care is important in mitigating the spread of communicable diseases.

[5]with the exception of any opportunity costs of restricting new migration (see, for instance, Battisti et al. (2018) and Peri (2012)).

[6]Relevant to this discussion, Cascio and Lewis (2019), finding that immigrants legalized by the Immigration Reform and Control Act of 1986 are more likely to file taxes but also receive assistance through the Earned Income Tax Credit, argue that increased participation may end up costly on net (i.e. new EITC payouts exceed new tax revenue) but that the social surplus generated from the benefits the EITC is demonstrated to have for children likely outweigh this cost.

in their) specification choice.

In the context of immigrants, Borjas (2003) estimates full (100%) crowd out of private insurance. Specifically, he concludes that losses in Medicaid enrollment (as a result of lost eligibility following PRWORA) are accompanied by equivalent increases in enrollment in employer-sponsored health insurance. However, in her analysis of the same policy environment, Royer (2005) finds no evidence of crowd out (and evidence of "crowd in"). She attributes some of the disparity in their estimates of crowd out to differences in sample restrictions and also notes that difference-in-differences estimates of the effect of PRWORA are sensitive to the choice of time periods used. Relatedly, Lofstrom and Bean (2002) show that changes in labor market conditions can explain at least one third of the relative decline in immigrant welfare receipt during the time period.[7]

More recently, Watson (2014), exploiting variation in immigration enforcement's effects on public insurance participation, estimates crowd out of private insurance of around 50%. Among children, Bronchetti (2014) estimates 25% crowd out (but no crowd out among first-generation immigrant children).

My estimates are most in line with Watson (2014). Because I can make use of data from the American Community Survey, I can estimate different crowd out effects by type of private insurance (purchased or employer-provided). Notably, in contrast to earlier studies that find evidence of crowd out, my results indicate that the crowd out of private insurance that does occur is driven by purchased insurance - not employer-provided.[8] Additionally, because my empirical design does not rely on a difference in policy timing,[9] one may be less concerned about Royer (2005)'s findings that difference-in-differences estimates are sensitive

[7]One implication is that improving labor market conditions may have led to more immigrants accessing employer-sponsored health insurance, which may help to explain Borjas (2003)'s results.

[8]This is perhaps a consequence of the difference in period of analysis. My findings are based on a sample of years after the implementation of the ACA. It is also possible that an increase in the share of private insurance that is purchased over time plays a role.

[9]E.g. broadly, a randomly sampled individual in 2014 should be just as likely to be "treated" as a randomly sampled individual in 2018.

to the period analyzed or Lofstrom and Bean (2002)'s findings that trends in labor market conditions may threaten the internal validity of estimated effects of crowd out.[10]

3.3. Framework

3.3.1. Policy Context

Under current federal policy (established by PRWORA), non-citizen immigrants are not eligible for Medicaid until they have resided in the U.S. for 5 years. At that time, a state may decide to offer Medicaid to qualifying immigrants. Most states do, offering Medicaid to immigrants who meet the other (poverty level) requirements imposed by the state on all residents. I will refer to these as "Standard" or "S" states. A handful of states do not provide Medicaid to most non-citizen immigrants even after they have met the minimum residency requirement. I will refer to these as "Never" states. Finally, a few states, including California and New York, circumvent the minimum residency requirement imposed by the federal government by using state funds to provide insurance comparable to Medicaid to legal resident immigrants, regardless of how long they have been in the country. I will refer to these as "Always" or "A" states.

Thus, when immigrants in "Standard" states reach 5 years of residency, the probability that they are eligible for publicly provided health insurance spikes discontinuously. Importantly, this is not the case for immigrants in "Always" states.

[10] Royer's findings are important for the interpretation and external validity of my results, however. As I will show, my findings differ when the sample period is restricted to pre versus post-ACA years.

3.3.2. Data

The data come from the American Community Survey annual microdata for 2015-2019 (Ruggles et al., 2022). These are the years after the ACA expansion but before the COVID-19 pandemic. In later analysis, I will test whether results hold in a pre-ACA expansion setting[11] and whether effects are persistent through the COVID-19 pandemic.[12] The sample is restricted to non-citizen immigrants aged 18-64 who are not likely refugees[13] and have incomes that place them below 200% of the federal poverty level (FPL).[14]

The running variable in regressions will be $years\ in\ U.S.$,[15] which is calculated (current year - year arrived in the U.S.).[16] However, calculating "years in the U.S." this way presents a problem. Since the precise date of the survey is not provided in public-use files and since the survey does not ask the precise date of arrival in the U.S. (just the year), some individuals' values of "years in the U.S." will be off by one year.[17] For this reason, choice specifications will exclude immigrants with a "years in U.S." value of 5 (who may be treated if the value is correct or untreated if the value is off because of how the variable is calculated). In robustness tests, I find that my results are largely unchanged when those immigrants are

[11] For example, Aizer (2007) suggests that increasing public health insurance take-up may be more important than expanding eligibility. Public health insurance take-up is impeded by information asymmetries and administrative or transaction costs. Aizer finds that outreach has been responsible for nontrivial increases in Medicaid enrollment. It might, then, be reasonable to expect a greater relationship between Medicaid eligibility and enrollment after aspects of the ACA, such as increased outreach and presumptive eligibility rules, are in place.

[12] For example, in addition to the potential effects of the increased probability of (life-threatening) illness brought on by the pandemic, Bertrand, Luttmer and Mullainathan (2000) find that immigrant networks, which were likely impacted by the associated lockdowns, play a role in the probability of welfare usage.

[13] This is determined by country of origin.

[14] For comparison, among the studies mentioned in section 3.2, Royer (2005) and Watson (2014) also choose to restrict their samples to individuals with incomes $<$ 200% FPL. Bronchetti (2014) restricts to income $<$ 400% FPL.

[15] In practice, the variable is re-coded to be $years\ in\ U.S. - 5$ so that the running variable takes value 0 at the threshold (it is centered at 0). Conceptually, the measures are equivalent.

[16] Though the direction of the relationship between years in the U.S. and enrollment in public health insurance is not important for the validity of the empirical design, prior literature (e.g. Leclere, Jensen and Biddlecom (1994) or Borjas and Trejo (1991)) might suggest an increasing relationship. Indeed, I find a slight positive correlation.

[17] For example, an immigrant interviewed in January of 2015 who arrived in the U.S. in July of 2010 will be assigned a $years\ in\ U.S.$ value of 5, despite only being in the U.S. for 4.5 years.

included or when the assignment of treatment is shifted to *years in U.S.* ≥ 6, instead of 5.

3.3.3. Empirical Design

The 5-year threshold faced by immigrants in most states creates a discontinuity in the trend of public insurance receipt over an immigrant's length of residence in the U.S. At the 5-year threshold, receipt of Medicaid or similar public insurance spikes by 5-10 percentage points, capturing the effect of Medicaid eligibility on Medicaid receipt. Then, the indicator for crossing the 5-year threshold for Medicaid eligibility can be used as an instrument for Medicaid receipt in a regression of y on Medicaid receipt, where y may be an outcome such as indicators for "receipt of private insurance," "receipt of any insurance," "employed," and so on.

As long as there are no other discontinuous changes in factors correlated with the outcome of interest at the 5-year threshold, these regressions yield unbiased estimates of the effect of Medicaid. A limitation of this study is that immigrants become eligible for various other kinds of public assistance when they reach 5 years in the U.S. I cannot rule out that these factors contribute to my estimates. For now, I will assume that Medicaid eligibility is the only meaningful change that occurs (in "Standard" states but not in "Always" states) at the threshold. This assumption will be relaxed later.

In Appendix C.1, I present plots of various characteristics of immigrants over time. With one exception, presented in Figure 3.1, the trends appear smooth across the 5-year threshold.[18] Immigrants are disproportionately likely to naturalize after 5 years of residence in the U.S., affecting the composition of the sample of non-citizen immigrants at the threshold. Thus, the observed change in Medicaid receipt (and its effect on other outcomes) will be biased by the changing sample, and it will not be clear how much of the discontinuity at

[18] Nonetheless, all regressions still include controls for age, ethnicity, and gender.

5 years in the U.S. is attributable to Medicaid eligibility and how much is attributable to the change in sample composition.[19]

If the national rules on eligibility for citizenship affect immigrants in all states equally and there is one group of states where eligibility for publicly provided health insurance does not change discontinuously at the 5-year threshold, then the difference between the effect of crossing that threshold in a state where eligibility changes and a state where eligibility does not change isolates the effect of the eligibility change. More specifically, immigrants in states in the "Always" category do not experience a sharp change in eligibility at the 5-year threshold, but they do experience the sharp change in probability of having naturalized. Immigrants in states in the "Standard" category experience both changes (eligibility for publicly provided health insurance and eligibility to naturalize).[20] Therefore, under the assumption that immigrants in "Always" states who choose to naturalize at the 5-year threshold are similar to immigrants in "Standard" states who choose to naturalize at the 5-year threshold, the difference in the effect of crossing the 5-year threshold in a "Standard" state and a "Always" state "nets out" the effect of changes in probability of naturalization, leaving only the effect of changes in eligibility for publicly provided health insurance.[21]

Before restricting my sample of immigrants to non-citizens, I plot the share of citizens by years in the U.S. separately for "Always" and "Standard" states in Figure 3.1. Two aspects of the figure stand out. First, there is, indeed, a sizable, discontinuous increase in the share of immigrants who are citizens at about 6 years in the U.S. - a threat to the validity of the fuzzy regression discontinuity design. Second, both the trends and the discontinuity in the share of immigrants who are citizens are nearly identical across "Always" and "Standard"

[19]This is comparable to the potential for bias from attrition in an RCT. If immigrants' decisions to naturalize (and thus, leave my sample) are orthogonal to their health insurance decisions, then attrition occurs as good as randomly, and RD estimates remain unbiased. However, it seems unlikely that there is *no* correlation between characteristics correlated with naturalization and those correlated with health insurance decisions, implying non-random attrition.

[20]The list of these states was compiled using information from the Urban Institute (Fortuny and Chaudry, 2011), National Immigration Law Center (NILC, 2022).

[21]For a mathematical demonstration, see Appendix C.2.

states, lending support to the argument (and necessary condition for the unbiasedness of the differences-in-discontinuities estimates) that immigrants who choose naturalize in "Always" states are comparable to immigrants who choose to naturalize in "Standard" states.[22]

3.3.4. Specifications

I will begin by presenting results from fuzzy RD specifications. The regressions are run for the subset of the sample of immigrants who live in "Standard" states (where variation in Medicaid eligibility occurs at the threshold) and take the following form:

$$y_{ist} = \beta_1 years\ in\ U.S._i + \beta_2 years\ in\ U.S._i^2 + \beta_3 D_i + X_i\theta + \alpha_s + \delta_t + \varepsilon_{ist} \tag{3.1}$$

years in U.S. is the running variable, years in the U.S. (centered at 0). D_i is an indicator that takes value 1 if an individual has a years in the U.S. value ≥ 5 (or, equivalently, when the running variable, once centered, is ≥ 0). X_i is a vector of controls including age, presence of at least one child in the household, number of (one's own) children in the household, marital status, gender, race, and ethnicity. Results are similar if log income or occupational fixed effects are also included as controls.[23] α_s and δ_t are state and year fixed effects, respectively. The specifications presented in the text use a global quadratic functional form for the running variable. Local linear specifications are identical if $years\ in\ U.S._i^2$ is replaced with $years\ in\ U.S._i \times D_i$.

In the first stage, $y_{ist} \equiv medicaid_{ist}$ is an indicator that takes value 1 if the individual

[22] In less formal terms, the necessary condition can be explained as follows: holding other characteristics besides state of residence constant, an immigrant's decision to naturalize would be no different if she lived in a state like California, Pennsylvania, or New York than it would be if she lived in a state like Arizona, Illinois, or New Jersey.

[23] There is reason to believe that these variables measure characteristics that could change in response to Medicaid eligibility or coverage, in which case, they would be bad controls. Therefore, choice specifications omit them altogether.

reports being covered by Medicaid or another public health insurance program.[24] In reduced-form equations, y_{ist} will represent, among other outcomes, indicators for coverage by private insurance, purchased insurance, employer-provided insurance, and any insurance.

As explained above, (potentially) non-random sample attrition due to naturalization may bias RD estimates. Therefore, the differences-in-discontinuities specifications will serve as the choice specifications throughout. The regressions are run on the sample of immigrants who live in either "Standard" or "Always" states and take the following form:

$$\begin{aligned} y_{ist} = {} & \beta_1 years\ in\ U.S._i + \beta_2 years\ in\ U.S._i^2 + \beta_3 D_i \\ & + \beta_4 (years\ in\ U.S. \times S)_{is} + \beta_5 (years\ in\ U.S.^2 \times S)_{is} + \beta_6 (D \times S)_{is} \\ & + X_i \theta + \alpha_s + \delta_t + \varepsilon_{ist} \end{aligned} \tag{3.2}$$

where S is an indicator that takes value 1 for "Standard" states and 0 otherwise.[25]

3.4. Results

3.4.1. Medicaid Take-Up (First Stage)

Table 3.1 presents estimates of the first-stage effect of eligibility on Medicaid enrollment (Figure 3.2 provides a graphical representation of the estimation strategy). Columns 1 and 2 correspond exactly to equations (3.1) and (3.2), respectively. The last 2 columns replace the global polynomial functional form for the running variable with a local linear one. Results are highly robust to choice of functional form here and throughout. For this reason, I will present results only for the global polynomial specifications going forward. Unless otherwise

[24]The ACS *medicaid* variable is coded such that it includes state-provided public health insurance.

[25]No indicator for being in a "Standard" state is explicitly included in the regressions because it is implicitly included in (or subsumed by) state fixed effects.

noted, the bandwidth chosen for regression estimates in tables is 5. Results for shorter bandwidths are presented in subsequent figures.

From Figure 3.2 and Table 3.1, we can see that immigrants have low baseline rates of Medicaid usage even when they are eligible,[26] but when offered publicly provided health insurance, many do take it up. I detect a highly significant increase in reported Medicaid coverage in response to eligibility changes at the 5-year threshold. The point estimate in the choice specification implies Medicaid enrollment rises 7.64 percentage points. Given the low rates of initial participation, this corresponds to as much as a 50% increase in Medicaid enrollment among immigrants in "Standard" states.

3.4.2. Crowd Out

All remaining tables in this section will follow the same format. There will be 6 columns. The first 3 columns will present estimates from the fuzzy RD specifications (i.e. equation (3.1)), and the last 3 will present estimates from the (preferred) fuzzy differences-in-discontinuities specifications (i.e. equation (3.2)). Columns 1 and 4 re-estimate the first-stage equations. Columns 2 and 5 estimate the reduced-form equations for the specified outcome (e.g. an indicator for being covered by private insurance). Columns 3 and 6 provide causal estimates of the effect of public health insurance (Medicaid) coverage on these outcomes, using the discontinuity in eligibility as an instrument for Medicaid coverage. The reduced-form estimates in the 5th columns are interesting, but the focus of the discussion will be on the estimated effects of Medicaid coverage presented in the 6th columns.[27]

In Table 3.2, the outcome of interest is an indicator for whether an individual is cov-

[26]Note that because I have restricted the sample to immigrants below 200% FPL and excluded those in the "Never" states, most individuals in the sample will be eligible for publicly provided health insurance (especially after 6 years in the U.S.).

[27]The reduced-form regressions estimate an intent-to-treat (ITT) effect (i.e. the effect of eligibility), whereas the IV estimates in the last column provide a measure of treatment on the treated (TOT).

ered by private insurance. The estimates in Table 3.2 imply just under 50% crowd out. Specifically, the point estimate indicates that 47% of the increase in Medicaid coverage can be explained by a reduction in private insurance coverage. In other words, on average, for every 2 immigrants who take up Medicaid upon eligibility, one of them would have otherwise been covered by private insurance (in the absence of such eligibility).

The ACS data allow me to decompose the measure of private insurance into its 2 components: health insurance that is purchased by the individuals themselves and health insurance that is provided by an employer. To my knowledge, prior studies of health insurance crowd out among immigrants have only ever attributed crowd out of private insurance to reductions in coverage by employer-sponsored health insurance, but recent work on the responsiveness of low-income individuals to costs associated with health insurance coverage suggests we might observe a greater effect on (the more expensive) purchased insurance.[28] This distinction is nontrivial as the social welfare implications may be very different if crowd out is driven by reductions in purchased versus employer-provided health insurance.

In Table 3.3, the outcome of interest is an indicator for whether an individual is covered by purchased insurance, and in Table 3.4, the outcome is an indicator for coverage by employer-provided health insurance. The estimates imply that the observed crowd out of private insurance is largely (if not entirely) driven by reductions in purchased health insurance. With a bandwidth of 5, both point estimates are negative, but the effect on purchased insurance (significant at the 5% level) is nearly 4 times the magnitude of the effect on employer-provided insurance (not significant at any conventional level). Notably, the difference in the estimates increases as the bandwidth shrinks. See Figures 3.7 and 3.9. The effect on employer-provided insurance remains a statistical zero with a point estimate that becomes more positive while the effect on purchased insurance becomes increasingly negative.

[28] See Finkelstein, Hendren and Shepard (2019) and Holmes (2021).

3.4.3. The Uninsured Rate

Table 3.5 presents results when the dependent variable is an indicator for whether an individual has any health insurance. Medicaid has a positive and significant effect on reducing the uninsured rate of low-income immigrants. The point estimate (0.47) indicates that roughly half of the increase in Medicaid coverage had the effect of reducing the uninsured rate. In other words, on average, for every 2 immigrants who take up Medicaid upon eligibility, one of them would have otherwise been uninsured (in the absence of such eligibility).

3.4.4. Other Labor Market Outcomes

I also investigate whether providing Medicaid has any discernible effects on labor market outcomes where we might expect to see evidence of moral hazard. For instance, is there any evidence that providing immigrants with public assistance (in this case, in the form of health insurance) induces them to leave their jobs or otherwise reduce their labor supply? I consider the following outcomes: an indicator for employment, weekly hours worked, and weekly hours worked among those who are employed. I do not find evidence of significant effects (positive or negative) on any of these outcomes, though estimates are noisy.

Estimates for a bandwidth of 5 are provided in Tables 3.6 - 3.8. Point estimates in choice specifications are negative but noisy and always statistically insignificant. Additionally, as the bandwidth is reduced, these estimates tend to approach zero or even become positive.

3.5. Interpretation and Discussion

Taken together, the results of the analysis provide a picture of how immigrants respond when made eligible for the largest public assistance program in the United States.

First, I document a high responsiveness to the extension of public health insurance among low-income immigrants. The findings of Aizer (2007) suggest that further expanding eligibility for public assistance (in isolation) may not achieve policy goals due to low take-up rates among immigrants. If take-up is persistently low, then costs of low-skilled immigration, in terms of the "public charge" these immigrants may pose, may be negligible, but as mentioned, the social benefits the policies are intended to generate will also go unrealized. My findings suggest that, though baseline participation is low, immigrants are responsive to changes in eligibility for public health insurance. In the period studied, enrollment in publicly provided health insurance spikes by 5-10 percentage points (an increase of as much as 50%) among low-income immigrants when they reach the residency requirement for Medicaid eligibility.

Second, in investigating the efficiency costs of extending public health insurance to low-income immigrants, I find evidence that Medicaid crowds out private insurance to an extent. In other words, half of the effect of Medicaid provision to this population is to shift individuals from private to public insurance. Importantly and in contrast to earlier studies, I find that crowd out of private insurance is driven mostly or entirely by reductions in purchased insurance. This finding may be interpreted in different ways. One may interpret my findings as evidence that many immigrants who can afford private health insurance (at least in the sense that, in the absence of a public option, they would be observed to have purchased health insurance) take advantage of public assistance, which has negative implications for efficiency. Under this interpretation, one could argue that half of the cost of extending public health insurance to low-income immigrants is "wasted," which is a relevant consideration for estimates of public charge. However, this interpretation assumes enrollment in Medicaid is only beneficial to the extent that it reduces the uninsured rate. An at least equally valid interpretation of the crowd out finding is that some of the benefit of public assistance is reducing the cost burden (from purchasing insurance) on the population of low-income immigrants. Thus, the redistributive implications of providing public health insurance and any resulting spillover effects of that redistribution are also relevant considerations.

Third, I find that increasing eligibility for Medicaid has resulted in meaningful reductions in the uninsured rate among low-income immigrants. Thus, Medicaid is at least partially successful in achieving increased insurance coverage in the U.S. Finally, my results do not indicate that Medicaid reduces labor supply among low-income immigrants. Providing public health insurance does not result in demonstrable labor market externalities - another speculated indirect cost of providing public assistance.

3.6. Conclusion

As migrants line up (sometimes quite literally) to enter the United States, whether they are admitted will be determined by U.S. policymakers who, among other considerations, must assess the economic costs and benefits of admitting an additional immigrant. As non-citizens of various statuses in the U.S. continue to influence and contribute to the country's economy, policymakers must assess the wisdom of extending (more or less) public assistance to immigrants already present. With a focus on the nation's largest public assistance program, this study contributes to our understanding of the economic ramifications of extending public assistance to low-income immigrants.

I find that while take-up rates of public health insurance remain low, immigrants do increase their participation when they are made eligible. Investigating the downstream impacts of this increased participation, I find that for every 2 low-income immigrants newly covered by Medicaid, 1 would have been uninsured in the absence of the program and 1 would have enrolled in private insurance. Importantly, my estimates suggest that this private insurance would have been purchased, not employer-provided. Consistent with this result, I do not detect an effect of Medicaid on the labor supply of low-income immigrants.

3.7. Tables and Figures

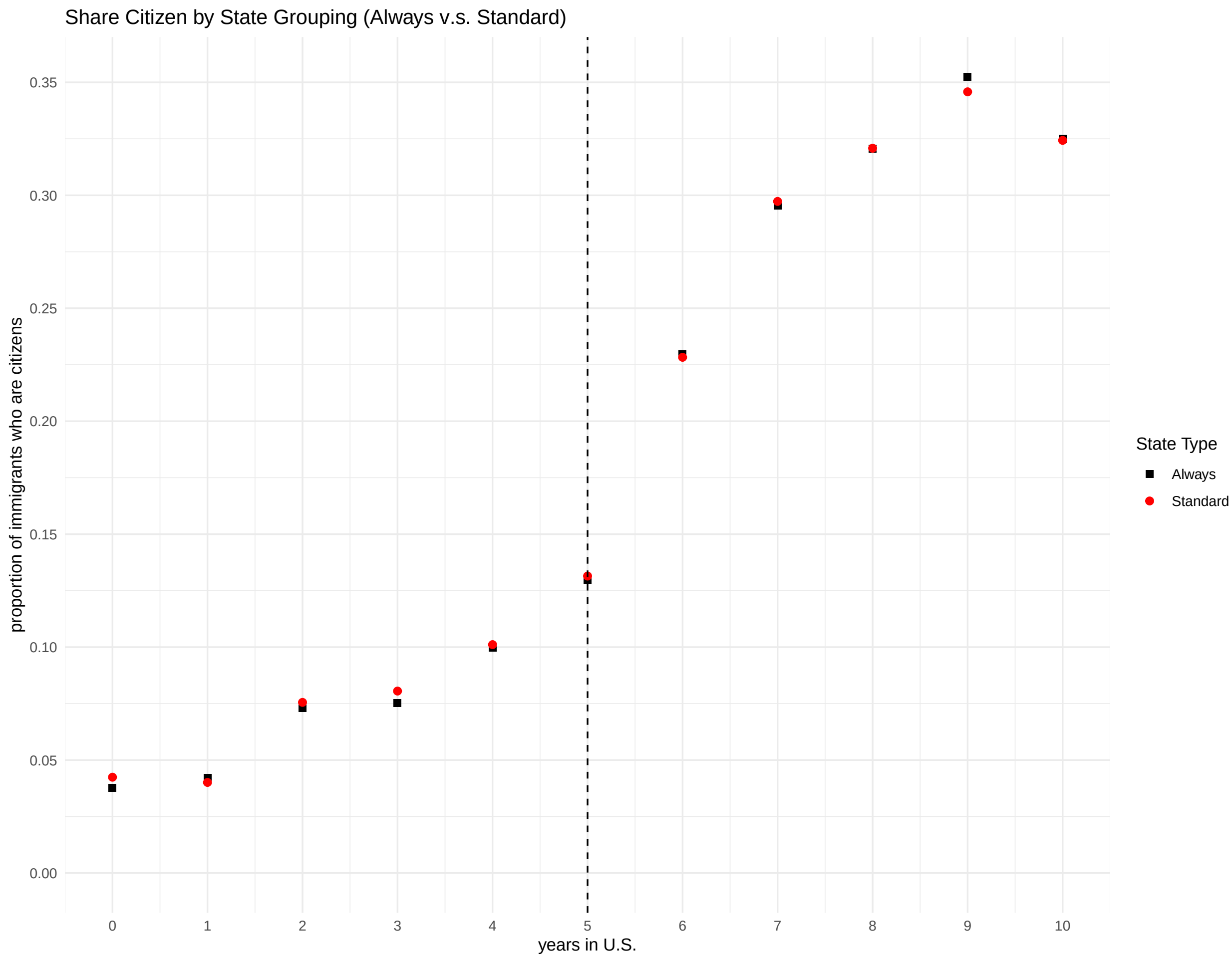

Figure 3.1: Share of sample that are citizens (before restricting to non-citizens).

	Global Polynomial		Local Linear	
	RD	Diff-in-Disc	RD	Diff-in-Disc
years in US	-0.0010	0.0173^{***}	0.0067^{***}	0.0489^{***}
	(0.0012)	(0.0017)	(0.0014)	(0.0022)
$years\ in\ US^2$	-0.0013^{***}	-0.0054^{***}		
	(0.0002)	(0.0003)		
$years\ in\ US \times S$		-0.0199^{***}		-0.0440^{***}
		(0.0021)		(0.0026)
$years\ in\ US^2 \times S$		0.0041^{***}		
		(0.0003)		
$years\ in\ US \times D$			-0.0152^{***}	-0.0634^{***}
			(0.0024)	(0.0035)
$years\ in\ US \times S \times D$				0.0487^{***}
				(0.0042)
D	0.0393^{***}	-0.0378^{***}	0.0385^{***}	-0.0386^{***}
	(0.0082)	(0.0119)	(0.0082)	(0.0119)
$D \times S$		0.0764^{***}		0.0764^{***}
		(0.0145)		(0.0145)
Adj. R^2	0.0515	0.1432	0.0513	0.1428
Num. obs.	47547	85542	47547	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: The effect of eligibility from crossing the 5-year threshold on Medicaid enrollment. Bandwidth = 5. The functional form for the running variable is a global polynomial (quadratic) in columns 1-2 and local linear in columns 3-4. Columns 1 and 3 present estimates from the fuzzy regression discontinuity design. Columns 2 and 4 present (the preferred) estimates from the fuzzy differences-in-discontinuities design. Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included. Results are practically identical across functional forms as a result of low degrees of freedom.

Table 3.1: First-stage estimates.

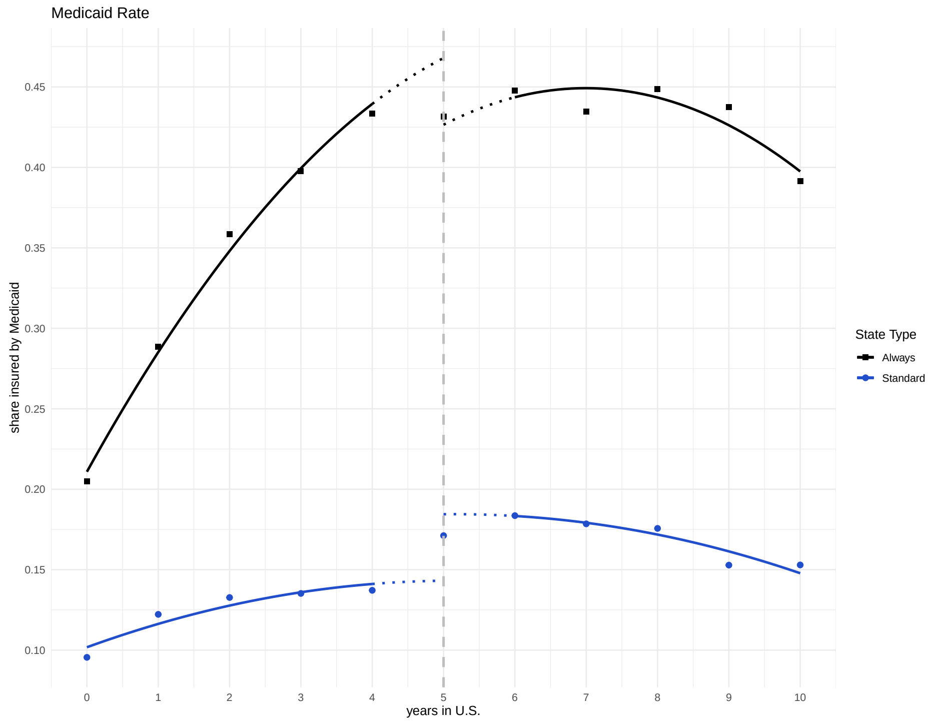

Figure 3.2: (Unadjusted) Means of Medicaid enrollment by state grouping with fitted global polynomial trend lines.

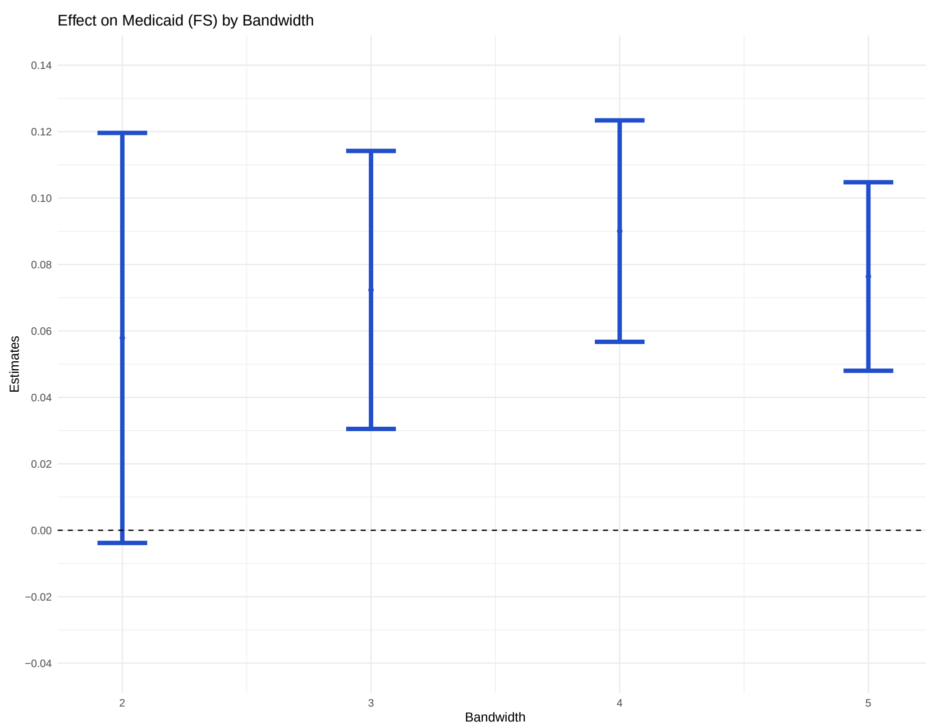

Figure 3.3: Estimated effects on Medicaid enrollment by bandwidth.

	Medicaid	Private	Private	Medicaid	Private	Private
	(FS)	(RF)	RD	(FS)	(RF)	Diff-in-Disc
years in US	-0.0010	-0.0039^{**}	-0.0046^{***}	0.0173^{***}	-0.0112^{***}	-0.0030
	(0.0012)	(0.0016)	(0.0013)	(0.0017)	(0.0017)	(0.0026)
years in US^2	-0.0013^{***}	-0.0016^{***}	-0.0027^{***}	-0.0054^{***}	0.0015^{***}	-0.0010
	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0010)
years in $US \times S$				-0.0199^{***}	0.0058^{**}	-0.0036^{*}
				(0.0021)	(0.0023)	(0.0020)
years in $US^2 \times S$				0.0041^{***}	-0.0032^{***}	-0.0013
				(0.0003)	(0.0004)	(0.0009)
D	0.0393^{***}	-0.0298^{***}		-0.0378^{***}	0.0106	-0.0071
	(0.0082)	(0.0107)		(0.0119)	(0.0114)	(0.0071)
$D \times S$				0.0764^{***}	-0.0359^{**}	
				(0.0145)	(0.0156)	
medicaid			-0.7581^{***}			-0.4694^{**}
			(0.2683)			(0.1851)
Adj. R^2	0.0515	0.1546	0.1667	0.1432	0.1494	0.2753
Num. obs.	47547	47547	47547	85542	85542	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.2: Crowd out of private insurance.

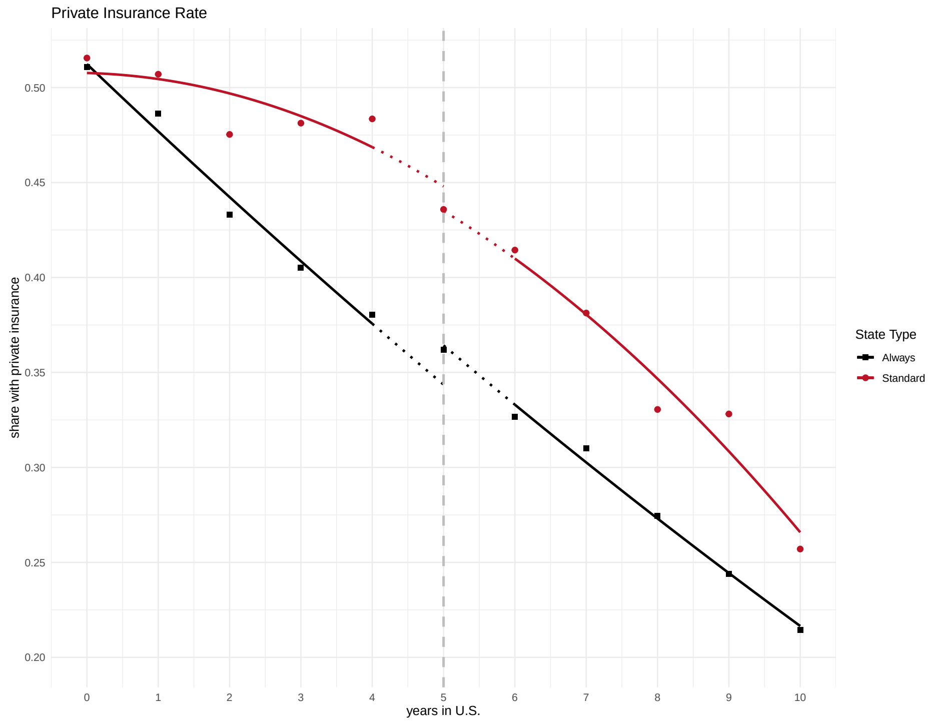

Figure 3.4: (Unadjusted) Means of enrollment in private insurance by state grouping with fitted global polynomial trend lines.

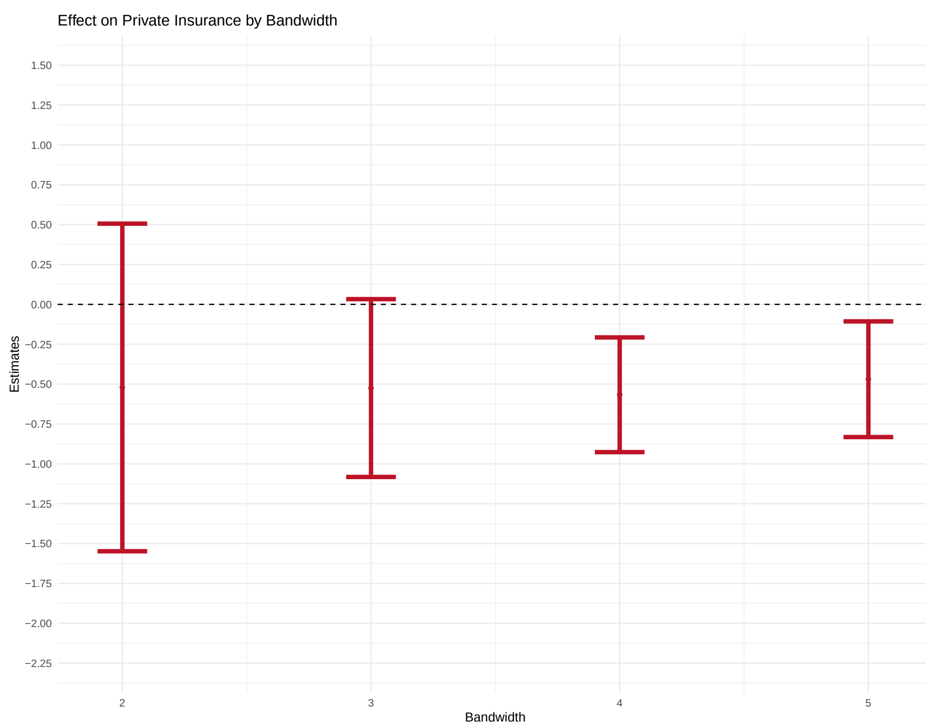

Figure 3.5: Estimated effects on private insurance coverage by bandwidth.

	Medicaid	Purchased	Purchased	Medicaid	Purchased	Purchased
	(FS)	(RF)	RD	(FS)	(RF)	Diff-in-Disc
years in US	-0.0010	-0.0051^{***}	-0.0059^{***}	0.0173^{***}	-0.0114^{***}	-0.0046^{**}
	(0.0012)	(0.0013)	(0.0012)	(0.0017)	(0.0013)	(0.0022)
years in US^2	-0.0013^{***}	-0.0005^{**}	-0.0015^{***}	-0.0054^{***}	0.0013^{***}	-0.0008
	(0.0002)	(0.0002)	(0.0004)	(0.0003)	(0.0002)	(0.0009)
years in US $\times S$				-0.0199^{***}	0.0063^{***}	-0.0015
				(0.0021)	(0.0018)	(0.0017)
years in $US^2 \times S$				0.0041^{***}	-0.0017^{***}	-0.0001
				(0.0003)	(0.0003)	(0.0007)
D	0.0393^{***}	-0.0304^{***}		-0.0378^{***}	0.0013	-0.0136^{**}
	(0.0082)	(0.0086)		(0.0119)	(0.0089)	(0.0062)
$D \times S$				0.0764^{***}	-0.0300^{**}	
				(0.0145)	(0.0124)	
medicaid			-0.7739^{***}			-0.3931^{**}
			(0.2522)			(0.1627)
Adj. R^2	0.0515	0.0987	-0.1345	0.1432	0.1103	0.1118
Num. obs.	47547	47547	47547	85542	85542	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.3: Crowd out of purchased insurance.

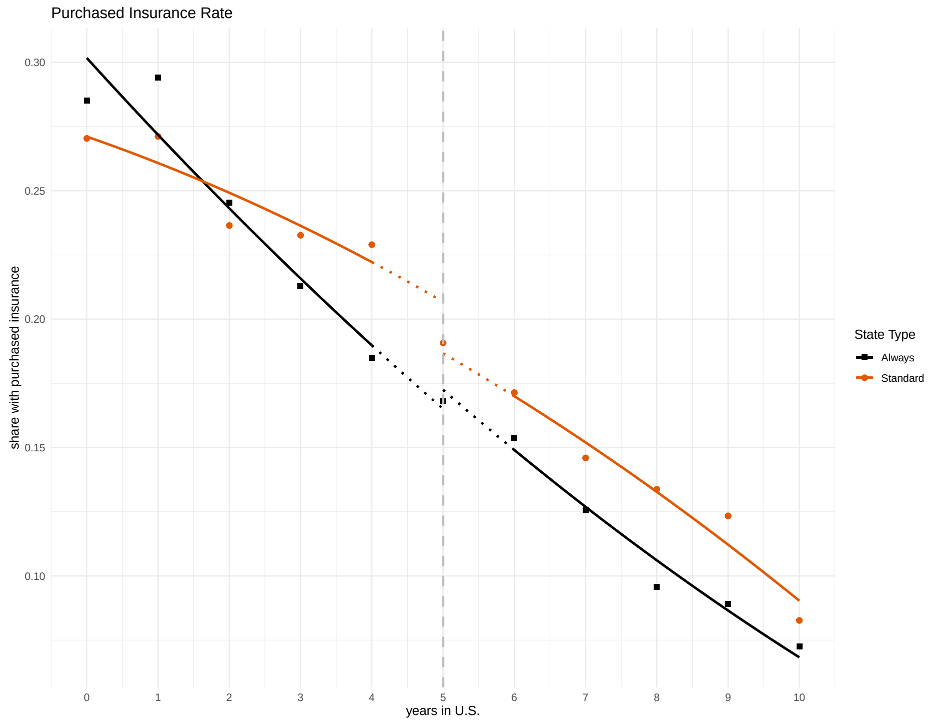

Figure 3.6: (Unadjusted) Means of enrollment in purchased insurance by state grouping with fitted global polynomial trend lines.

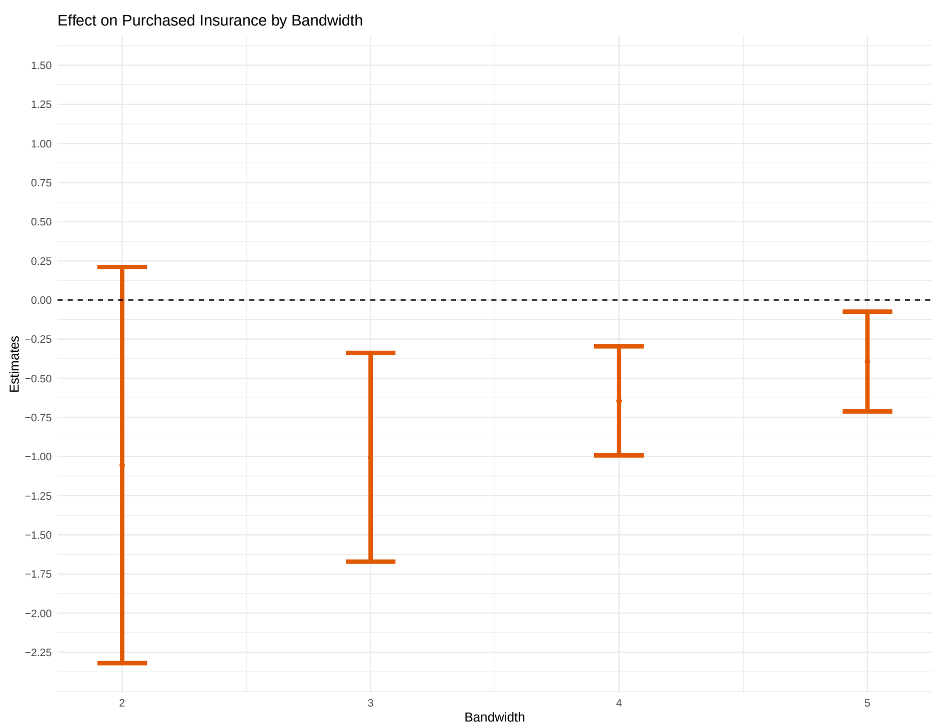

Figure 3.7: Estimated effects on purchased insurance coverage by bandwidth.

	Medicaid	Employer	Employer	Medicaid	Employer	Employer
	(FS)	(RF)	RD	(FS)	(RF)	Diff-in-Disc
years in US	-0.0010	0.0001	0.0002	0.0173^{***}	-0.0010	0.0007
	(0.0012)	(0.0014)	(0.0012)	(0.0017)	(0.0015)	(0.0024)
years in US^2	-0.0013^{***}	-0.0010^{***}	-0.0010^{**}	-0.0054^{***}	0.0004^{*}	-0.0001
	(0.0002)	(0.0002)	(0.0004)	(0.0003)	(0.0002)	(0.0010)
years in US $\times$ *S*				-0.0199^{***}	-0.0005	-0.0025
				(0.0021)	(0.0020)	(0.0019)
years in US$^2 \times$ *S*				0.0041^{***}	-0.0015^{***}	-0.0011
				(0.0003)	(0.0003)	(0.0008)
D	0.0393^{***}	0.0014		-0.0378^{***}	0.0120	0.0081
	(0.0082)	(0.0097)		(0.0119)	(0.0099)	(0.0068)
$D \times S$				0.0764^{***}	-0.0078	
				(0.0145)	(0.0139)	
medicaid			0.0353			-0.1018
			(0.2496)			(0.1772)
Adj. R^2	0.0515	0.0545	0.0436	0.1432	0.0440	0.0809
Num. obs.	47547	47547	47547	85542	85542	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.4: Crowd out of employer-provided insurance.

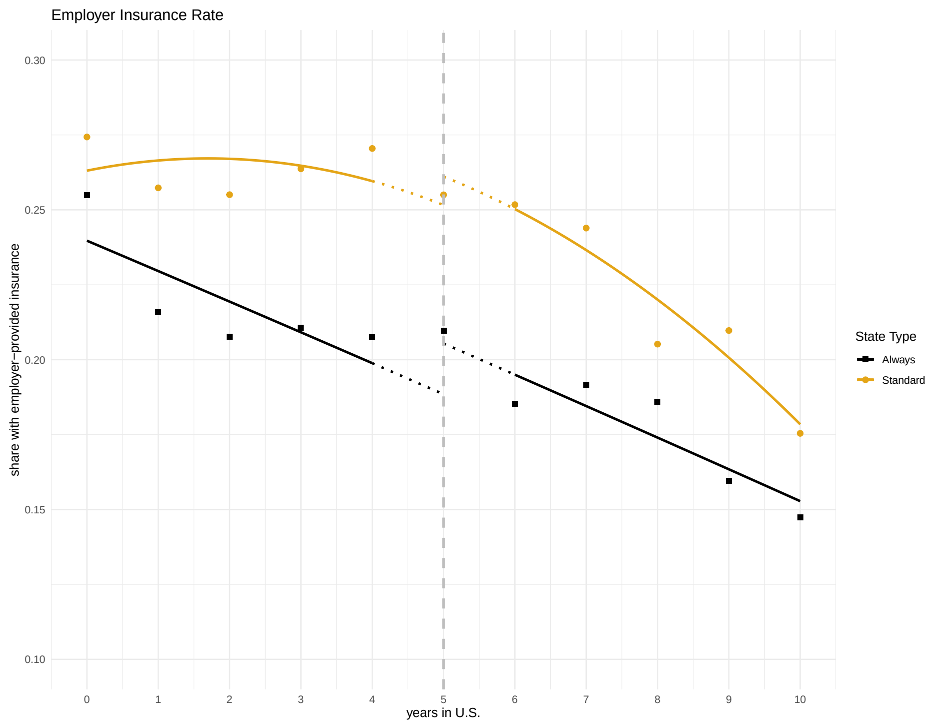

Figure 3.8: (Unadjusted) Means of enrollment in employer-provided insurance by state grouping with fitted global polynomial trend lines.

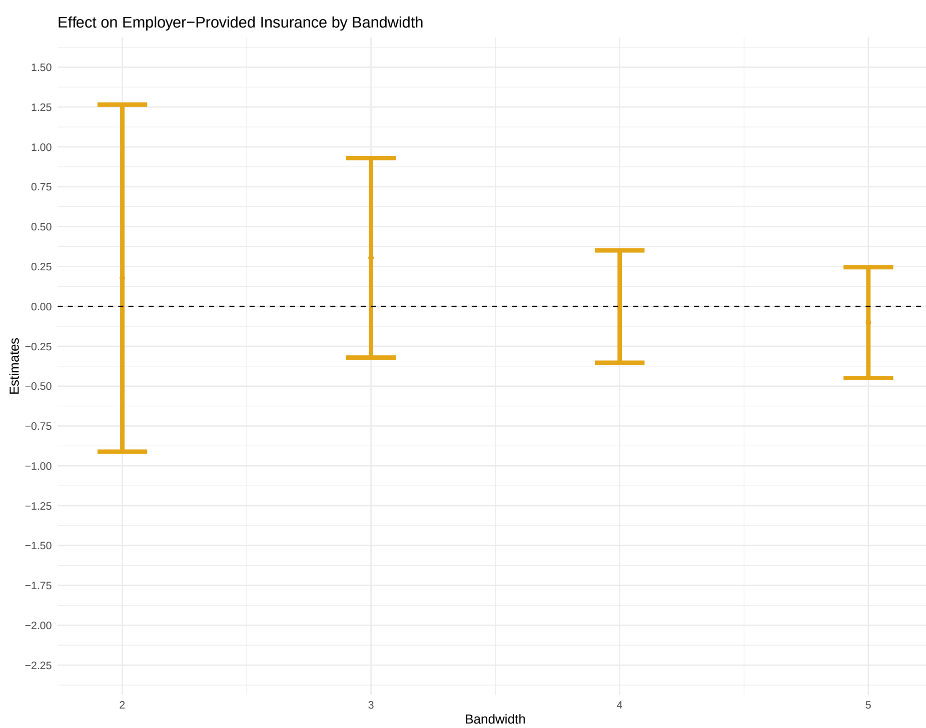

Figure 3.9: Estimated effects on employer-provided insurance coverage by bandwidth.

	Medicaid	Any	Any	Medicaid	Any	Any
	(FS)	(RF)	RD	(FS)	(RF)	Diff-in-Disc
years in US	-0.0010	-0.0050^{***}	-0.0048^{***}	0.0173^{***}	0.0053^{***}	-0.0029
	(0.0012)	(0.0016)	(0.0013)	(0.0017)	(0.0016)	(0.0025)
years in US^2	-0.0013^{***}	-0.0028^{***}	-0.0025^{***}	-0.0054^{***}	-0.0036^{***}	-0.0011
	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0003)	(0.0010)
years in $US \times S$				-0.0199^{***}	-0.0134^{***}	-0.0040^{**}
				(0.0021)	(0.0022)	(0.0019)
years in $US^2 \times S$				0.0041^{***}	0.0008^{**}	-0.0011
				(0.0003)	(0.0004)	(0.0008)
D	0.0393^{***}	0.0101		-0.0378^{***}	-0.0223^{**}	-0.0044
	(0.0082)	(0.0107)		(0.0119)	(0.0104)	(0.0067)
$D \times S$				0.0764^{***}	0.0361^{**}	
				(0.0145)	(0.0149)	
medicaid			0.2573			0.4732^{***}
			(0.2555)			(0.1750)
Adj. R^2	0.0515	0.1417	0.2325	0.1432	0.1460	0.2998
Num. obs.	47547	47547	47547	85542	85542	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.5: Effect on having any insurance coverage.

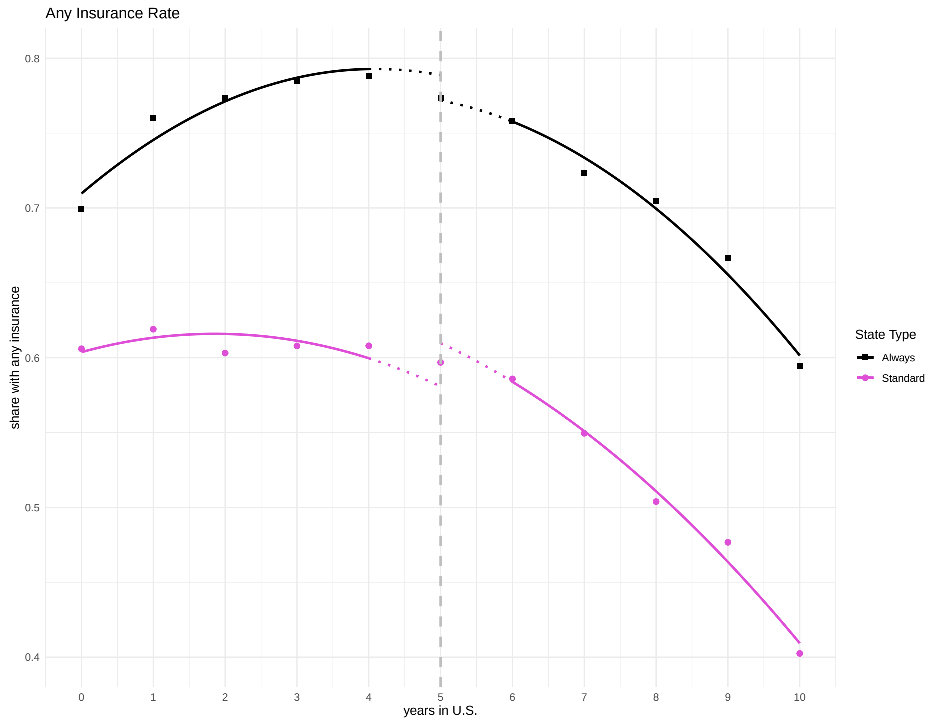

Figure 3.10: (Unadjusted) Means of of any health insurance coverage by state grouping with fitted global polynomial trend lines.

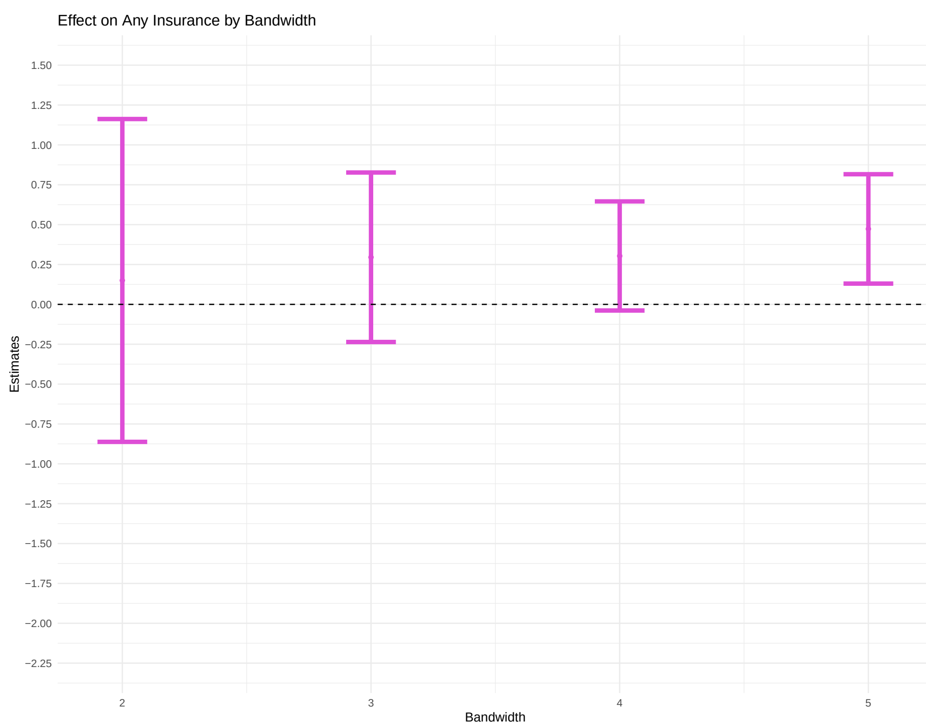

Figure 3.11: Estimated effects on any insurance coverage by bandwidth.

	Medicaid (FS)	Employed (RF)	Employed RD	Medicaid (FS)	Employed (RF)	Employed Diff-in-Disc
years in US	-0.0010	0.0203^{***}	0.0195^{***}	0.0173^{***}	0.0175^{***}	0.0225^{***}
	(0.0012)	(0.0016)	(0.0016)	(0.0017)	(0.0018)	(0.0029)
years in US^2	-0.0013^{***}	-0.0036^{***}	-0.0046^{***}	-0.0054^{***}	-0.0029^{***}	-0.0045^{***}
	(0.0002)	(0.0003)	(0.0005)	(0.0003)	(0.0003)	(0.0012)
years in US $\times S$				-0.0199^{***}	0.0026	-0.0032
				(0.0021)	(0.0024)	(0.0023)
years in US$^2 \times S$				0.0041^{***}	-0.0007^{*}	0.0005
				(0.0003)	(0.0004)	(0.0010)
D	0.0393^{***}	-0.0313^{***}		-0.0378^{***}	-0.0089	-0.0200^{**}
	(0.0082)	(0.0109)		(0.0119)	(0.0120)	(0.0083)
$D \times S$				0.0764^{***}	-0.0223	
				(0.0145)	(0.0162)	
medicaid			-0.7961^{**}			-0.2924
			(0.3157)			(0.2170)
Adj. R^2	0.0515	0.1053	-0.1390	0.1432	0.1028	0.0648
Num. obs.	47547	47547	47547	85542	85542	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.6: Effect on employment.

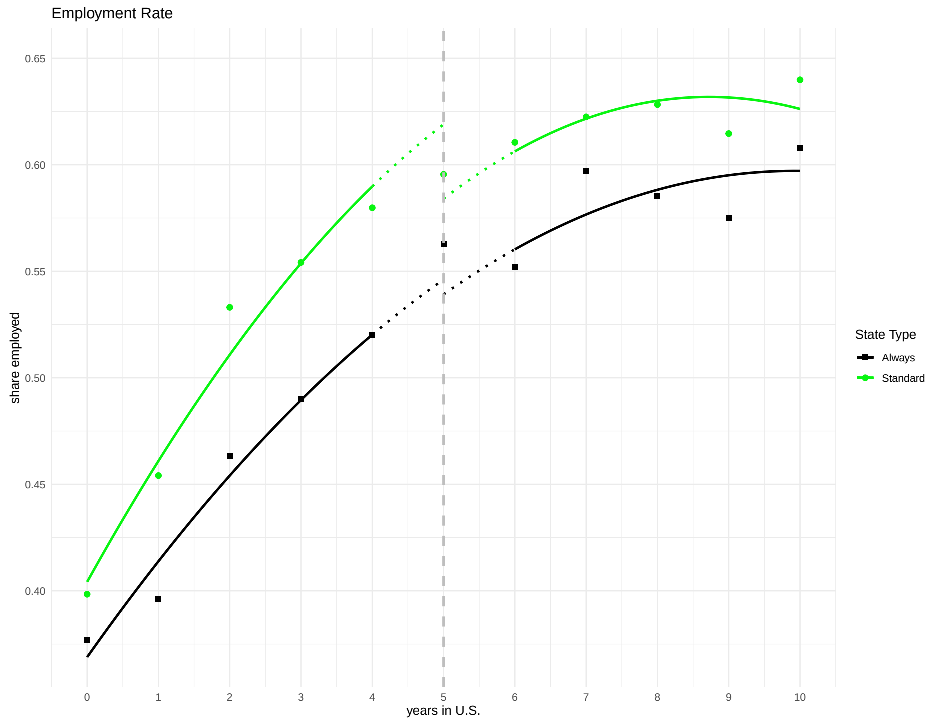

Figure 3.12: (Unadjusted) Share employed by state grouping with fitted global polynomial trend lines.

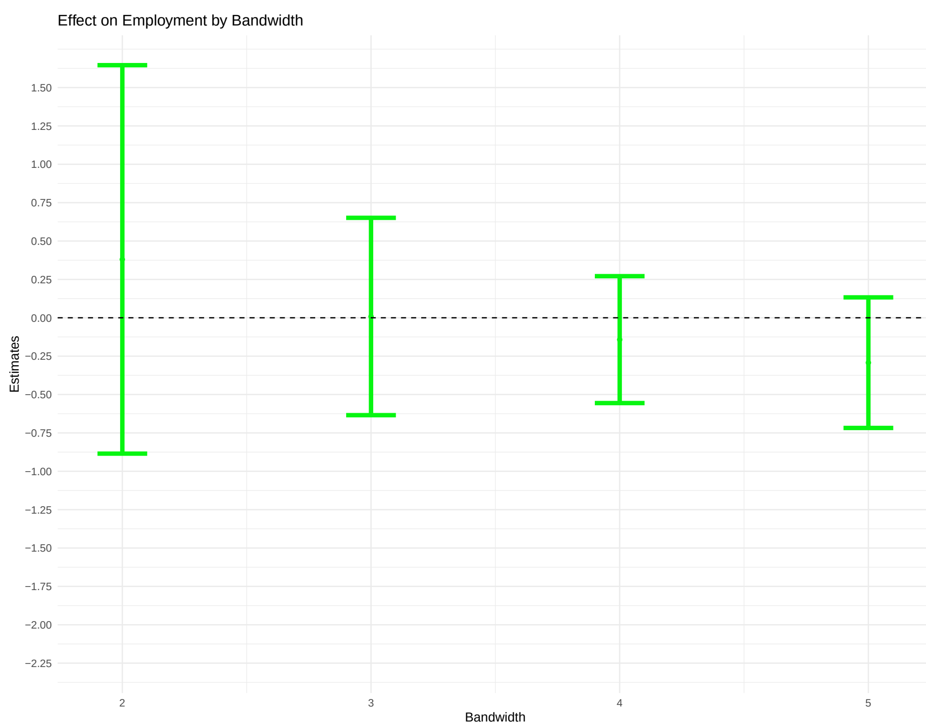

Figure 3.13: Estimated effects on employment by bandwidth.

	Medicaid	Hours	Hours	Medicaid	Hours	Hours
	(FS)	(RF)	RD	(FS)	(RF)	Diff-in-Disc
years in US	-0.0010	0.4554^{***}	0.4692^{***}	0.0173^{***}	0.2526^{***}	0.4763^{***}
	(0.0012)	(0.0632)	(0.0568)	(0.0017)	(0.0694)	(0.1114)
years in US^2	-0.0013^{***}	-0.0866^{***}	-0.0676^{***}	-0.0054^{***}	-0.0675^{***}	-0.1367^{***}
	(0.0002)	(0.0103)	(0.0170)	(0.0003)	(0.0113)	(0.0475)
years in US $\times S$				-0.0199^{***}	0.2014^{**}	-0.0549
				(0.0021)	(0.0931)	(0.0902)
years in US$^2 \times S$				0.0041^{***}	-0.0209	0.0317
				(0.0003)	(0.0153)	(0.0395)
D	0.0393^{***}	0.5567		-0.0378^{***}	1.5371^{***}	1.0489^{***}
	(0.0082)	(0.4223)		(0.0119)	(0.4641)	(0.3226)
$D \times S$				0.0764^{***}	-0.9853	
				(0.0145)	(0.6275)	
medicaid			14.1666			-12.8988
			(11.3140)			(8.4350)
Adj. R^2	0.0515	0.1327	0.0521	0.1432	0.1290	0.0839
Num. obs.	47547	47547	47547	85542	85542	85542

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.7: Effect on weekly hours worked.

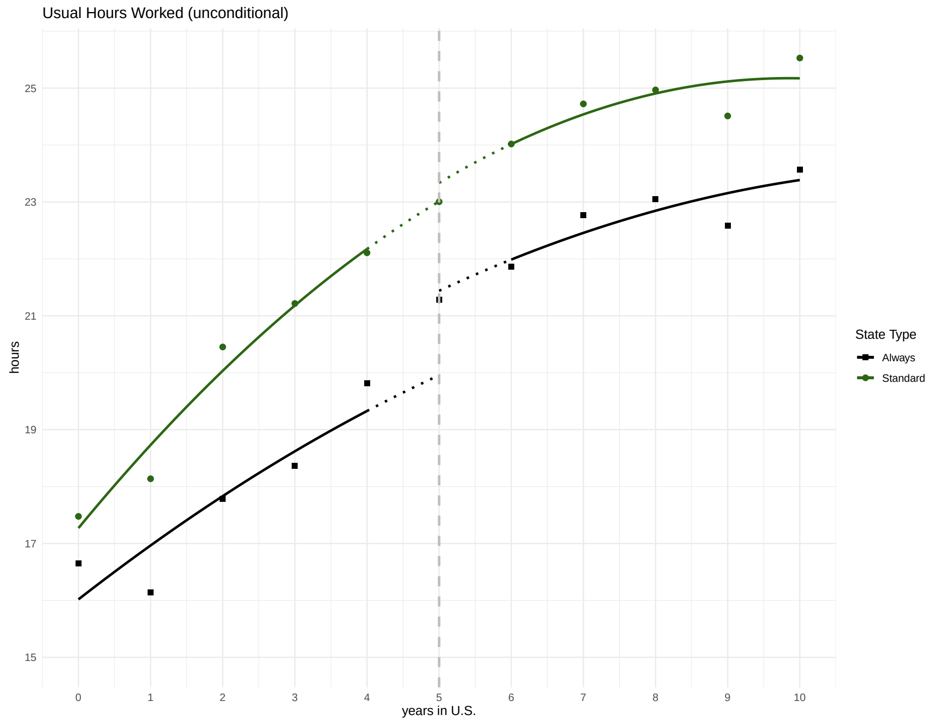

Figure 3.14: (Unadjusted) Average hours worked by state grouping with fitted global polynomial trend lines.

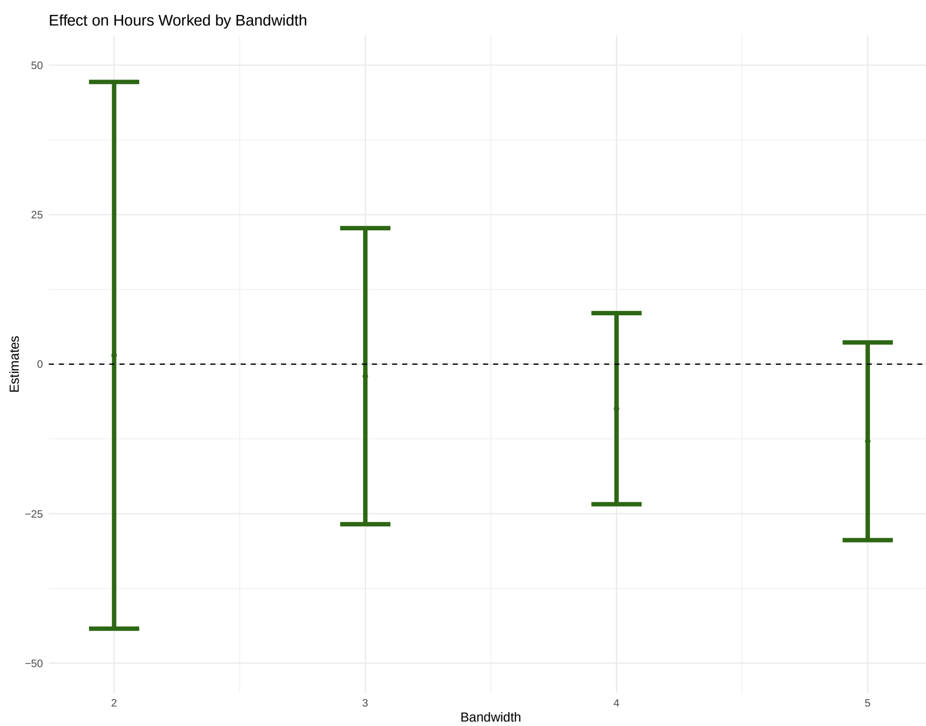

Figure 3.15: Estimated effects on hours worked by bandwidth.

	Medicaid	Hours	Hours	Medicaid	Hours	Hours
	(FS)	(RF)	RD	(FS)	(RF)	Diff-in-Disc
years in US	-0.0038^{**}	-0.2179^{***}	-0.0472	0.0151^{***}	-0.3320^{***}	-0.2556^{***}
	(0.0015)	(0.0533)	(0.0438)	(0.0024)	(0.0653)	(0.0664)
years in US^2	-0.0015^{***}	0.0357^{***}	0.1046^{***}	-0.0051^{***}	0.0453^{***}	0.0194
	(0.0002)	(0.0087)	(0.0257)	(0.0004)	(0.0107)	(0.0329)
years in US $\times$ *S*				-0.0199^{***}	0.1219	0.0215
				(0.0028)	(0.0837)	(0.0578)
years in US^2 $\times$ *S*				0.0038^{***}	-0.0096	0.0096
				(0.0005)	(0.0137)	(0.0270)
D	0.0385^{***}	1.7185^{***}		-0.0522^{***}	2.1509^{***}	1.8870^{***}
	(0.0101)	(0.3523)		(0.0161)	(0.4302)	(0.2733)
$D \times S$				0.0907^{***}	-0.4586	
				(0.0190)	(0.5560)	
medicaid			44.5832^{***}			-5.0541
			(15.2937)			(6.1383)
Adj. R^2	0.0517	0.0858	-1.4008	0.1512	0.0795	0.0736
Num. obs.	25818	25818	25818	44823	44823	44823

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Columns 1-3 present estimates from the fuzzy regression discontinuity design (first stage, reduced form, and IV, respectively). Columns 4-6 present estimates from the fuzzy differences in discontinuities design (first stage, reduced form, and IV, respectively). The functional form for the running variable is a global polynomial (quadratic). Controls for age, number of children in household, marital status, gender, race, and ethnicity are included. State and year fixed effects are also included.

Table 3.8: Effect on weekly hours worked where the sample is restricted to employed individuals only.

In the second figure presented below, the estimate at bandwidth = 2 carries a standard error of over 190 and is omitted for clarity.

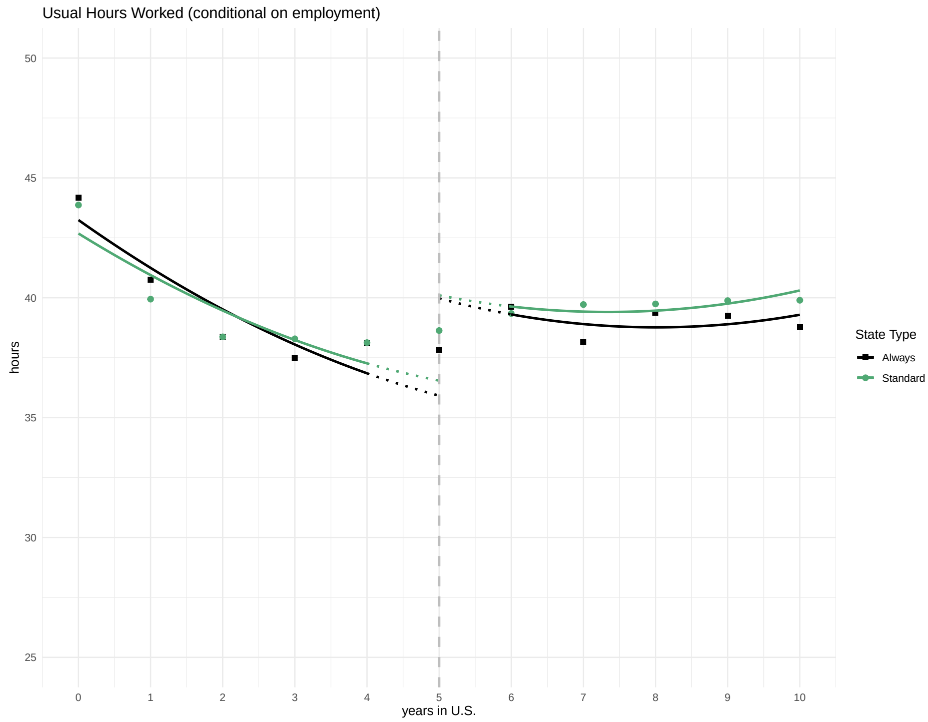

Figure 3.16: (Unadjusted) Average hours worked (conditional on being employed) by state grouping with fitted global polynomial trend lines.

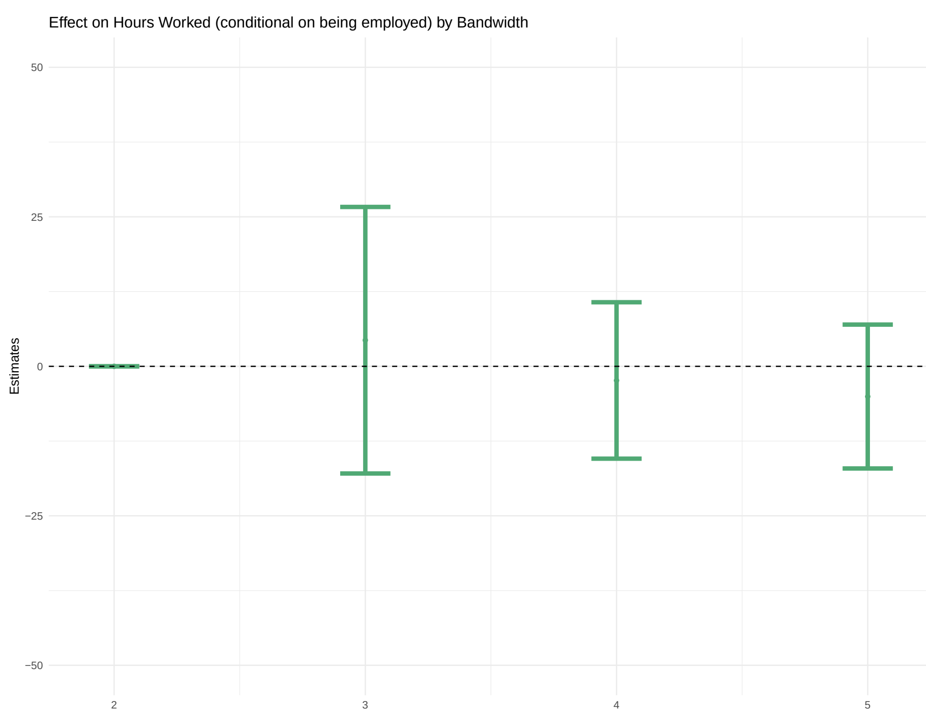

Figure 3.17: Estimated effects on hours worked (conditional on being employed) by bandwidth.

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Appendix A

Appendix to Seeking Sanctuary: Housing Undocumented Immigrants

A.1. Additional Descriptive Statistics

Means for indicators (0 or 1) for each of the logical edits applied by status

	LPR	undocumented
military	0.01041	0
arrived before 1980	0.04019	0
public health insurance	0.5616	0
medicaid	0.5136	0
medicare	0.1191	0
VA insurance	0.002705	0
welfare	0.07737	0
SSI	0.05211	0
SS	0.08909	0
licensed job	0.02368	0
Cuban	0.07931	0
student visa	0	0
foodstamps*	0.04017	0
H1B	0.04483	0
American Samoan*	0.002832	0
legal by marriage	0.5042	0

Table A.1: Foodstamps/SNAP receipt is only counted for households with one adult where the indicator for anyone in the household receiving foodstamps is true. This is to account for the possibility that an undocumented parent has collected foodstamps on the behalf of a legally present dependent. Individuals from American Samoa, while technically non-citizens (contrary to all other U.S. territories), are all legally eligible to live and work in the United States. Note that student visa is 0 because the sample excludes individuals currently enrolled in college. Also note that any individual can fulfill any number of these conditions at a time (e.g. an immigrant who has received medicaid and is married to a U.S. citizen or legal resident, would be coded as “1” for both conditions).

Additional Statistics on the Undocumented Population

	Birthplace	2012-2017 total	2012-2017 renters	2016 Pew	2017 CMS	2015 DHS
1	Mexico	43.8	49.4	50.9	49.6	55
2	El Salvador	5.1	5.9	6.8	6.3	6
3	Guatemala	4.2	4.9	5.4	5.1	5
4	Honduras	2.9	3.4	4.0	3.6	4
5	India	5.4	2.4	4.4	5.9	4
6	Dominican Republic	1.6	2.0	2.0	1.8	
7	Philippines	2.3	2.0	1.3	1.6	3
8	Korea	1.9	1.9	1.2	1.6	2
9	China	3.8	1.9	3.0	2.9	3
10	Ecuador	1.2	1.6	1.1	1.2	1
11	Colombia	1.4	1.5	1.1	1.4	
12	Haiti	1.2	1.3	0.9	1.2	

Table A.2: Birthplace of undocumented renters by country. Numbers represent the percent of undocumented immigrants by country of birth. Estimates from the full population are presented first. Column 2 estimates are derived from the sample of renters used in the analysis (in Sections 1.4 and 1.5.3). The remaining 3 columns provide estimates of the percent of the undocumented population by country of birth from other sources for comparison (Pew, the Center for Migration Studies, and the Department of Homeland Security).

	State	2012-2017 Choice	2017 Choice	2017 Pew	2017 CMS	2016 MPI	2015 DHS
1	California	2500	2100	2000	2400	3100	2900
2	Texas	1800	1800	1600	1800	1600	1900
3	Florida	888	900	825	766	656	810
4	New York	886	777	650	753	940	590
5	New Jersey	525	505	450	452	526	440
6	Illinois	502	465	425	460	487	450
7	Georgia	404	388	375	335	351	390
8	North Carolina	340	329	325	300	321	390
9	Virginia	313	295	275	243	269	310
10	Arizona	275	249	275	252	226	
11	Maryland	262	247	250	224	247	
12	Washington	257	260	250	251	229	
	Total	11.6	11.1	10.5	10.7	11.3	12.0

Table A.3: Estimates of the undocumented population by state of residence (in thousands). For comparison, estimates from Pew, CMS, DHS, and the Migration Policy Institute are provided as well.

A.2. Robustness to Inclusion of Citizens

To ensure that the story of the rent premium is not an artifact of the composition of the subsample of immigrants, I run regressions that include citizens and allow for different premiums for legal resident immigrants and undocumented immigrants. Equation (A.1) is the citizen-inclusive analog to equation (1.2).

$$\begin{aligned} Rent_{ipt} = {} & \beta_1 LPR_i + \beta_2 undocumented_i + \beta_3 years\ in\ U.S._i + \beta_4 multi\text{-}unit_i \\ & + \beta_5 (LPR_i \times years\ in\ U.S._i) + \beta_6 (undocumented_i \times years\ in\ U.S._i) \\ & + \beta_7 (LPR_i \times multi\text{-}unit_i) + \beta_8 (undocumented_i \times multi\text{-}unit_i) \\ & + X_i \theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \end{aligned} \tag{A.1}$$

LPR_i is 1 if the householder is a legal immigrant (but not a citizen) and 0 otherwise. Results from the full sample are presented in Table A.4 and reinforce the results from the restricted sample. Interestingly, estimates in Table A.4 suggest that all non-citizen immigrants pay a rent premium (that seems to disappear over time), but only undocumented immigrants pay a premium specifically for multi-unit housing, consistent with the theory of search frictions specific to undocumented renters of these types of units (there is no obvious reason that any premium legal resident immigrants face would vary based on the type of housing unit rented, but the search frictions theory I propose provides reason to expect the positive coefficient on the interaction of undocumented status and the multi-unit indicator as multi-unit housing like apartments may appear especially risky to prospective undocumented tenants).[1]

[1] Though not an immediately obvious explanation, one may have believed *ex ante* that discrimination is responsible for the premium legal resident immigrants pay and that this premium is different for multi-unit housing because discrimination is different for multi-unit housing. Hanson, Hawley and Taylor (2011) find that racial discrimination in housing is greater for these kinds of units. The results in Table A.4 suggest that, if the same kind of differential discrimination exists in the context of immigration status (i.e. if legal resident immigrants face additional discrimination in multi-unit housing like black applicants do), it does not manifest as a premium. Thus, either legal resident immigrants (compared to citizens) do not face differential discrimination by housing unit type in the same way prospective black tenants (compared to prospective white tenants) do, or they do but such discrimination (at least in the context of immigration status) does

Additionally, the fact that legal resident immigrants pay higher rents than citizens implies that the choice to use legal resident immigrants as the comparison group throughout this study results in more conservative estimates of how much more undocumented immigrants pay for rents and further suggests that the coefficient on *undocumented* is truly capturing just the effect of undocumented status on rents (and not other characteristics correlated both with undocumented or immigrant status and higher rents).

	Model 1	Model 2	Model 3	Model 4
LPR	-11.58^{***}	-49.61^{***}	32.90^{***}	106.30^{***}
	(2.45)	(4.82)	(3.94)	(8.12)
undocumented	4.77^{**}	-11.60^{*}	61.73^{***}	122.24^{***}
	(2.30)	(6.00)	(4.98)	(9.51)
years in U.S.			4.10^{***}	4.61^{***}
			(0.16)	(0.17)
LPR × years in U.S.				-4.16^{***}
				(0.21)
undocumented × years in U.S.				-5.41^{***}
				(0.35)
multi-unit			-127.74^{***}	-131.69^{***}
			(3.80)	(4.03)
LPR × multi-unit				3.51
				(5.44)
undocumented × multi-unit				31.15^{***}
				(5.77)
Year fixed effects	yes	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
Controls	no	no	yes	yes
Adj. R^2	0.00	0.27	0.52	0.52
Num. obs.	1046700	1046700	1046700	1046700

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.4: Effect on monthly gross rent in the sample including citizens.

not result in a rent premium.

A.3. Effect of Sanctuary Cities on Movement

Since sanctuary city policies appear to raise household incomes of undocumented renters, a plausible explanation for higher rents (despite evidence of alleviated search frictions from the reduced or eliminated premium specific to multi-unit housing) is that higher incomes induce undocumented immigrants to select into more expensive rental housing. In other words, it is possible that search frictions are alleviated (as previous results for renters of multi-unit housing suggest), allowing undocumented renters to better optimize their housing, resulting in the reduction or elimination of premiums. At the same time, though, if incomes have systematically risen for undocumented renters, their optimal housing consumption would change through this income channel as well, resulting in undocumented immigrants paying more for housing. To provide suggestive evidence that re-optimization is occurring, consistent with what we would expect if search frictions are reduced and consistent with higher rents resulting from selection into higher-price housing (due to increases in income), I run regressions corresponding to linear probability models where the outcome of interest is whether a renter (technically, household head) has moved within the last year. Results (for the sample from section 1.5.3) are presented in Table A.5.

	Model 1	Model 2	Model 3	Model 4
undocumented	-0.0118^{***}	-0.0336^{***}		
	(0.0044)	(0.0042)		
treat	-0.0132^{*}	-0.0123^{*}	-0.0048	-0.0006
	(0.0070)	(0.0069)	(0.0090)	(0.0087)
treat × undocumented	0.0177^{***}	0.0189^{***}	0.0064	0.0024
	(0.0068)	(0.0064)	(0.0115)	(0.0111)
years in U.S.		-0.0050^{***}		-0.0050^{***}
		(0.0002)		(0.0002)
multi-unit		0.0115^{***}		0.0116^{***}
		(0.0040)		(0.0041)
PUMA × undocumented fe	No	No	Yes	Yes
Year × undocumented fe	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Adj. R^2	0.0366	0.0885	0.0429	0.0934
Num. obs.	111713	111713	111713	111713

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.5: Linear probability model for movement within last year. Controls, where included, are age, age squared, household income, marital status, gender, number of people in household, number of workers in household, number of rooms, number of bedrooms, and build year (intervalled).

There are arguments to be made that the choice set of fixed effects is not appropriate or necessary or that it asks too much of the data when considering linear probability models for movement.[2] I remain agnostic in this case and characterize the results in Table A.5 as only suggestive evidence that the policies induce undocumented renters to move more (since specifications with the choice set of fixed effects produce still positive, but statistically insignificant, estimated effects). Appendix A.9 presents additional evidence to suggest that movement from outside the current PUMA of residence into the current PUMA (a sanctuary jurisdiction) occurs more frequently after a policy is enacted and drives the marginally positive coefficients in columns 3 and 4.

[2]One may be less concerned about drastic differences between undocumented immigrants and legal resident immigrants across PUMA's or time in their propensities to move than one might be about drastic differences in rent premiums (or income differences) across locations.

Since I am unable to determine with much precision when a household moved into their current residence and am restricted to evaluating whether the household moved within the last year, I am not surprised by the lack of power in these regressions. Still, despite the constraints on my ability to evaluate changes in mobility resulting from sanctuary city policies, I argue that Table A.5 suggests that movement (the primary mechanism through which individuals can re-optimize their housing consumption decisions) may occur more frequently in the undocumented renter population after sanctuary city policies are in place than it would absent the policies.[3]

[3] Also note that, an *increase* in probability of moving is not a necessary condition for re-optimization to be occurring. Any non-zero amount of movement (e.g. even the amount of movement absent the policy) allows households to re-optimize. It may be that households move with the same frequency following the policy but are able to make "better" moves.

A.4. Further Validation of Parallel Trends

Below, Figure A.1 adds β_2^k, the estimated effects of treatment (not interacted with undocumented status), to Figure 1.1. There are no obvious pre-trends in the effect of treatment for either class of immigrants.

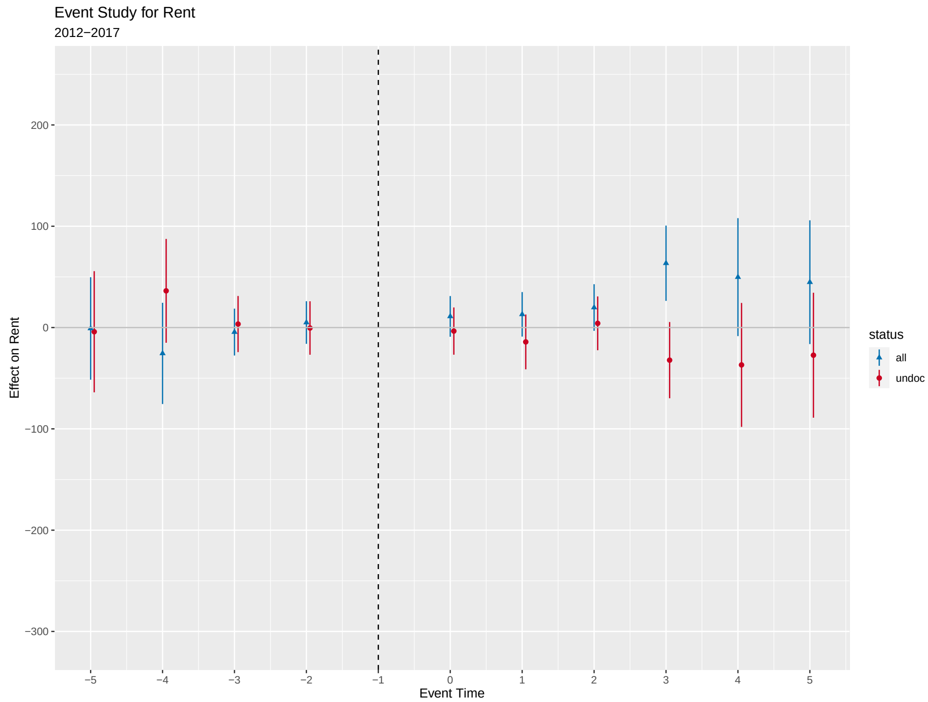

Figure A.1: Event study plot based on equation (1.6). Plots both β_2 (blue) and β_3 (red) estimates.

Instead of plotting β_3^k and β_7^k from equation (1.7) (which capture the effect of treatment for undocumented immigrants in general and the effect of treatment specific to undocumented immigrants renting multi-unit housing, respectively) as in Figure 1.2, Figure A.2 plots β_2^k and β_6^k (which capture the general effect of treatment on rents for the full sample immigrants and the effect of treatment specific to all immigrants in the sample renting multi-unit housing, respectively). Again, there are no apparent pre-trends. Also note the stability of the estimated effect of treatment for all immigrants. These figures illustrate that the only demonstrable effects of sanctuary city policies on rents occur only for undocumented immigrants (effects of treatment are only distinguishable from zero in the terms where treatment is interacted with undocumented status).

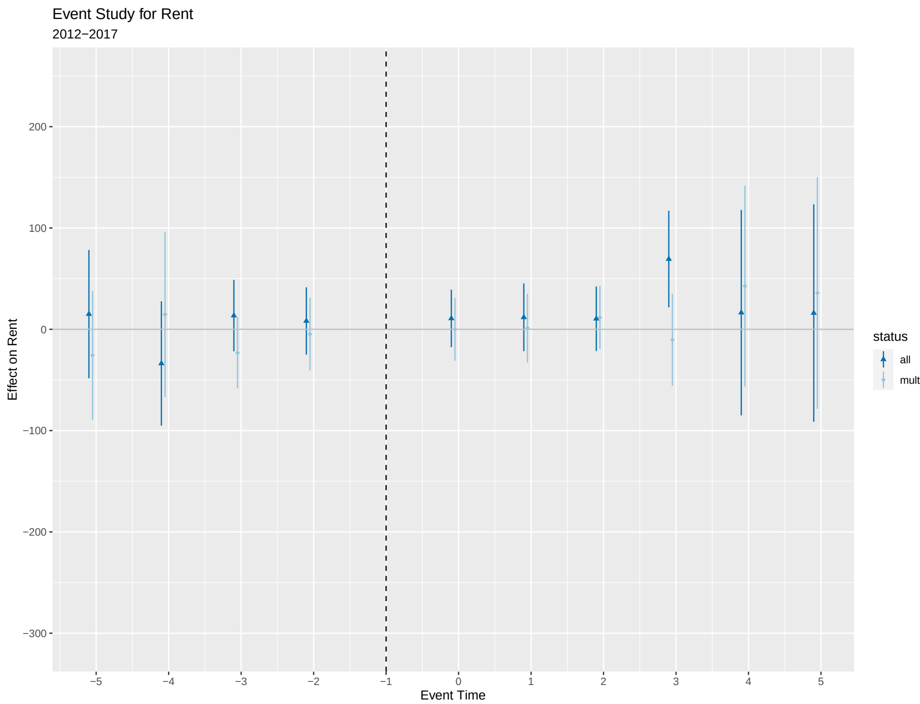

Figure A.2: Event study plot based on equation (1.7). Plots β_2 (blue) and β_6 (light blue) estimates. Compare to Figure 1.2.

To more clearly illustrate that treatment affects undocumented renters without having any clear effect on legal residents, Figure A.3 combines estimates from Figures 1.2 and A.2. This is a more cluttered graphic, but it more concisely shows the diverging effects of treatment for undocumented immigrants in contrast to the stable effects for legal residents.

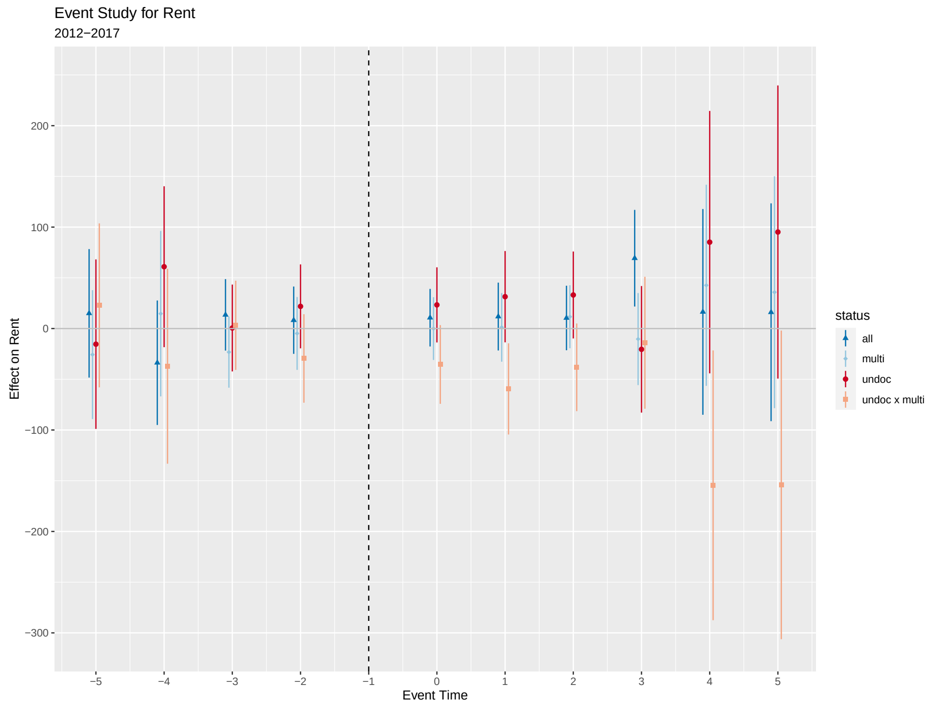

Figure A.3: Event study plot based on equation (1.7). Plots β_2 (blue), β_3 (red), β_6 (light blue), and β_7 (orange) estimates together.

Finally, just as Figure A.1 adds estimates of β_2^k from equation (6) to Figure 1.1, Figure A.4 adds estimates of ρ_2^k from equation (1.8) to Figure 1.3.

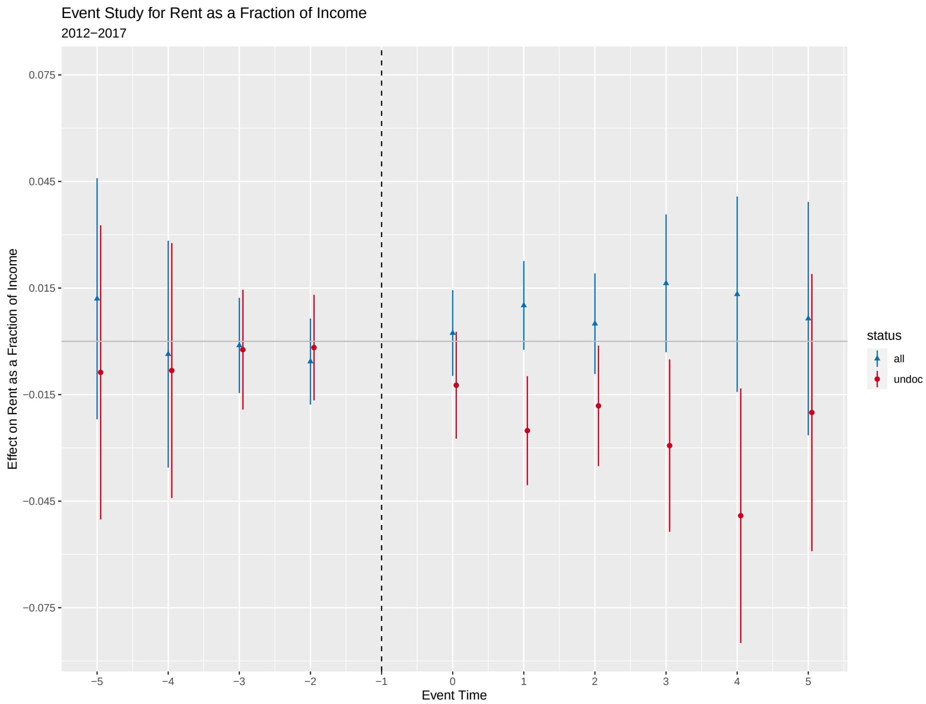

Figure A.4: Event study plot based on equation (1.8). Plots both ρ_2 (blue) and ρ_3 (red) estimates.

Often, the first-order effect of treatment in the post-period is positive. This is consistent with some regression results that suggest sanctuary city policies may lead to increases in the fraction of income legal resident immigrants devote to rent. While choice specifications throughout this paper nearly always fail to reject that the policies do not alter rent as a fraction of income for legal resident immigrants, several of the less stringent specifications would come to the conclusion that the policies lead to lower household incomes of legal resident immigrants, resulting in higher fractions of their incomes allocated to rent.[4] It is hard to say whether the effect of sanctuary city policies on incomes or the ratios of rents to incomes is truly non-zero for legal resident immigrants. Even harder to answer is, if their household incomes do fall in response to sanctuary city policies, why they fall and what the broader welfare implications are.[5]

[4]Note that the effect of these policies on rents of all non-citizen immigrants is nearly always a fairly precise zero.

[5]One possibility is increased labor market competition. Undocumented immigrants may face lessened

A.5. "All Adults Undocumented" Restriction

I rerun the regressions from sections 1.4 and 1.5 under the assumption that a household is an "undocumented household" only if all adults in the household are undocumented. I believe this runs the risks of more heavily weighting misclassified renters (e.g. households with only one adult member), including households with adult citizen or legal resident dependents (e.g. undocumented parents of 18 year-old citizens still living at home) in the legal resident category inappropriately, and ignoring the possibility that a search friction may force undocumented immigrants to select units with a legal resident or citizen roommate (even though that selection may be sub-optimal). Nonetheless, if results hold under this restriction, then it is reasonable to believe they would hold under less strict restrictions as well and results are not simply an artifact of how I have classified households as "undocumented." Results are remarkably similar.

labor market frictions as well as housing market frictions. Another possibility is "legal resident flight." Saiz and Wachter (2011) find evidence that natives, especially those with higher incomes, move in response to growing immigrant populations. It is possible that higher-income legal residents leave sanctuary cities as more housing becomes accessible to undocumented immigrants.

	Model 1	Model 2	Model 3	Model 4
undocumented	-51.90^{***}	-6.73	24.13^{***}	2.65
	(3.55)	(5.06)	(3.51)	(8.32)
years in U.S.			-1.28^{***}	-0.88^{***}
			(0.24)	(0.25)
multi-unit			-109.08^{***}	-133.07^{***}
			(5.19)	(6.20)
undocumented × years in U.S.				-1.22^{***}
				(0.34)
undocumented × multi-unit				54.14^{***}
				(6.84)
Year fixed effects	no	yes	yes	yes
PUMA fixed effects	no	yes	yes	yes
controls	no	no	yes	yes
Adj. R^2	0.01	0.33	0.55	0.55
Num. obs.	105558	105558	105558	105558

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.6: Section 1.4 equivalent. Effect on gross rent.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	16.87^{***}	-1.84	-5.46			
	(3.41)	(8.33)	(8.67)			
treat	-4.67	-4.20	9.58	3.57	4.22	15.96
	(6.25)	(6.26)	(9.23)	(6.82)	(6.84)	(9.87)
treat × undocumented	23.02^{***}	20.65^{***}	44.00^{***}	7.52	5.52	32.33^{**}
	(6.93)	(6.86)	(13.66)	(8.12)	(8.12)	(15.01)
years in U.S.	-1.29^{***}	-0.86^{***}	-0.86^{***}	-1.33^{***}	-0.97^{***}	-0.97^{***}
	(0.24)	(0.25)	(0.25)	(0.23)	(0.25)	(0.25)
multi-unit	-109.12^{***}	-132.40^{***}	-126.35^{***}	-108.79^{***}	-127.04^{***}	-121.81^{***}
	(5.20)	(6.16)	(6.36)	(5.12)	(5.81)	(6.15)
undocumented × years in U.S.		-1.28^{***}	-1.30^{***}		-1.05^{***}	-1.06^{***}
		(0.34)	(0.34)		(0.34)	(0.34)
undocumented × multi-unit		52.56^{***}	57.50^{***}		41.37^{***}	47.48^{***}
		(6.72)	(7.04)		(6.65)	(7.24)
treat × multi-unit			-18.55^{*}			-15.88
			(9.79)			(9.70)
treat × multi-unit × undocumented			-28.69^{*}			-33.03^{**}
			(15.25)			(15.36)
Adj. R^2	0.55	0.55	0.55	0.56	0.56	0.56
Num. obs.	105558	105558	105558	105558	105558	105558

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.7: Section 1.5.3 equivalent. Effect on gross rent

	Rent	*Rent*	*Income*	$\frac{Rent}{Income}$
treat	6.56	15.60*	-74.54^{*}	0.0043
	(6.47)	(9.28)	(45.03)	(0.0036)
treat × undocumented	-8.46	7.53	113.76**	-0.0179^{***}
	(7.19)	(13.27)	(57.72)	(0.0042)
undocumented × multi-unit	41.36***	44.58***		
	(6.07)	(6.63)		
treat × multi-unit		-12.45		
		(9.32)		
treat × multi-unit × undocumented		-19.24		
		(13.52)		
Adj. R^2	0.57	0.57	0.33	0.2221
Num. obs.	87496	87496	87496	87496

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.8: Section 1.5.4 results under new definition of "undocumented household." The sample restrictions applied are equivalent to those of the "Unr" (unrestricted) sample in the main text. Results are quite similar across the other subsamples. In columns 1 and 2, the dependent variable is gross monthly rent. In column 3, the dependent variable is monthly income. In column 4, the dependent variable is rent as a fraction of income.

A.6. Naturalized Citizens as Legal Residents

I believe it is more appropriate to use noncitizen authorized immigrants as the comparison group for this analysis, as we might expect naturalized citizens to have very different characteristics from other immigrants. However, an argument can be made to include naturalized citizens in the LPR category, as they are immigrants too. As with the previous section, I rerun all results from sections 1.4 and 1.5 on samples that include naturalized citizens. Throughout, results are similar to those in the text. I note that discrepancies that arise (generally, in the significance of an effect) tend to be consistent with the story that some undocumented immigrants lie about their citizenship status on the ACS forms. For example, Table A.10 suggests that sanctuary city policies are effective at reducing the amount paid for multi-unit housing for *all* immigrants and have smaller effects (with large standard errors) for undocumented immigrants, specifically. If undocumented immigrants report being citizens when they respond to the ACS, they will now be included in the "control" group of legal resident immigrants and the effect of policy on this subset of individuals will influence the estimates for legal resident immigrants, not undocumented immigrants, specifically. Note that, even with this possibility, the direction (if not always the significance) of estimates of interest is consistent throughout.

	Model 1	Model 2	Model 3	Model 4
undocumented	-17.91^{***}	51.82^{***}	16.41^{***}	1.21
	(2.91)	(5.81)	(3.36)	(7.47)
years in U.S.			0.56^{***}	1.14^{***}
			(0.17)	(0.19)
multi-unit			-128.26^{***}	-154.13^{***}
			(5.21)	(6.07)
undocumented × years in U.S.				-2.02^{***}
				(0.30)
undocumented × multi-unit				65.82^{***}
				(6.00)
Adj. R^2	0.01	0.26	0.52	0.52
Num. obs.	188191	188191	188191	188191

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.9: Section 1.4 equivalent. Effect on gross rent.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	4.15	-6.66	-7.02			
	(3.35)	(7.46)	(7.71)			
treat	-13.67^{***}	-13.39^{**}	9.67	-8.48	-8.42	15.72^{*}
	(5.27)	(5.25)	(8.43)	(5.71)	(5.71)	(9.11)
treat × undocumented	38.25^{***}	36.66^{***}	46.45^{***}	21.48^{***}	21.60^{***}	32.35^{**}
	(6.52)	(6.42)	(10.70)	(8.04)	(8.03)	(12.70)
years in U.S.	0.54^{***}	1.16^{***}	1.17^{***}	0.69^{***}	1.24^{***}	1.24^{***}
	(0.17)	(0.19)	(0.19)	(0.16)	(0.18)	(0.18)
multi-unit	-128.45^{***}	-153.30^{***}	-143.71^{***}	-128.64^{***}	-147.42^{***}	-137.29^{***}
	(5.21)	(6.04)	(5.99)	(5.16)	(5.72)	(5.67)
undocumented × years in U.S.		-2.14^{***}	-2.15^{***}		-1.95^{***}	-1.96^{***}
		(0.30)	(0.30)		(0.29)	(0.29)
undocumented × multi-unit		63.23^{***}	64.36^{***}		48.61^{***}	49.75^{***}
		(5.83)	(6.26)		(5.68)	(6.32)
treat × multi-unit			-29.53^{***}			-30.64^{***}
			(8.92)			(9.02)
treat × multi-unit × undocumented			-13.26			-15.33
			(11.95)			(11.87)
Adj. R^2	0.52	0.52	0.52	0.53	0.53	0.53
Num. obs.	188191	188191	188191	188191	188191	188191

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.10: Section 1.5.3 equivalent. Effect on gross rent.

	Rent	*Rent*	*Income*	$\frac{Rent}{Income}$
treat	−5.65	5.13	−20.49	−0.0001
	(5.37)	(8.25)	(36.35)	(0.0028)
treat × undocumented	12.46^{*}	29.59^{***}	45.56	-0.0078^{*}
	(7.13)	(11.44)	(57.87)	(0.0045)
undocumented × multi-unit	44.17^{***}	48.75^{***}		
	(5.23)	(5.60)		
treat × multi-unit		-13.74^{*}		
		(8.13)		
treat × multi-unit × undocumented		-23.24^{**}		
		(10.90)		
Adj. R^2	0.56	0.56	0.33	0.1917
Num. obs.	158648	158648	158648	158648

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.11: Section 1.5.4 results where naturalized citizens are categorized as legal residents. The sample restrictions applied are equivalent to those of the "Unr" (unrestricted) sample in the main text. Results are quite similar across the other subsamples. In columns 1 and 2, the dependent variable is gross monthly rent. In column 3, the dependent variable is monthly income. In column 4, the dependent variable is rent as a fraction of income.

A.7. Declined Detainers Image via ICE

Section III: Table of Jurisdictions that have Enacted Policies which Restrict Cooperation with ICE

All jurisdictions and their corresponding detainer ordinances listed in this document are based upon public announcements, news report statements, and publicly disclosed policies. As such, there may be other non-cooperative jurisdictions not contained in this table if publicly available information does not exist. The entries below are sorted by the date a policy was enacted in the stated jurisdiction with the most recent date first.

Jurisdiction (AOR)	Date Enacted	Policy	Criteria for Honoring Detainer
Baltimore City, Maryland (Baltimore)	March 2017	Baltimore Police Commissioner	• Public statement of noncooperation with Immigration and Customs Enforcement
Maricopa, Arizona (Phoenix)	February 2017	Sheriff's Statement	• Maricopa County will not honor requests to hold individual
Tulare, California (San Francisco)	February 2017	Sheriff's Statement	• Will notify ICE five days prior to the inmates release but will not hold
Ithaca, New York (Buffalo)	February 2017	Municipal Code Change	• Will only honor "warrantless detainer requests from the federal government under limited, specified circumstances" such as violent or serious crimes or terrorist activities
City of Seattle, Washington (Seattle)	February 2017	Resolution 31730	• City department directors are directed to comply with City's practice to defer to King County on all ICE detainer requests • City of Seattle employees are directed, unless provided with a criminal warrant issued by a federal judge or magistrate, to not detain or arrest any individual based upon an administrative or civil immigration warrant for a violation of federal civil immigration law, including administrative and civil immigration warrants entered in the National Crime Information Center database
Travis County, Texas (San Antonio)	January 2017	Travis County Sheriff's Office Policy on Cooperation with U.S. Immigration and Customs Enforcement	• Willing to accept requests accompanied by a court order • Willing to accept requests when the subject of the detainer request is charged with or has been convicted of Capital Murder, First Degree Murder, Aggravated Sexual Assault, or Continuous Smuggling of Persons
Iowa City, Johnson County, Iowa (Saint Paul)	January 2017	Resolution Reaffirming the Public Safety Function of Local Law Enforcement	• Willing to only accept some notifications on detainers

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The full report can be found under archived reports on ICE's website: https://www.ice.gov/declined-detainer-report.

A.8. Section 4 Results for Section 5.4 Subsamples

Regressions from section 1.4 are rerun for the subsamples used in section 1.5.4. Column 3 consistently illustrates that undocumented immigrants pay a premium for rental housing. Column 4 consistently finds that the premium is driven by multi-unit housing.

	Model 1	Model 2	Model 3	Model 4
undocumented	-17.64^{***}	27.40^{***}	29.45^{***}	-0.47
	(3.36)	(4.22)	(3.13)	(7.32)
years in U.S.			-0.43^{**}	-0.48^{*}
			(0.20)	(0.25)
multi-unit			-83.79^{***}	-109.10^{***}
			(4.52)	(5.95)
undocumented × years in U.S.				0.03
				(0.30)
undocumented × multi-unit				41.34^{***}
				(5.98)
Adj. R^2	0.01	0.33	0.57	0.57
Num. obs.	93776	93776	93776	93776

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.12: "Unr" or Unrestricted sample

	Model 1	Model 2	Model 3	Model 4
undocumented	-47.97^{***}	-4.57	13.10^{***}	-7.28
	(3.27)	(3.33)	(2.73)	(7.10)
years in U.S.			0.30	0.23
			(0.18)	(0.24)
multi-unit			-69.84^{***}	-86.49^{***}
			(4.21)	(5.44)
undocumented × years in U.S.				0.10
				(0.28)
undocumented × multi-unit				26.61^{***}
				(5.60)
Adj. R^2	0.01	0.32	0.54	0.54
Num. obs.	72167	72167	72167	72167

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.13: "Inc" restriction

	Model 1	Model 2	Model 3	Model 4
undocumented	-18.57^{***}	15.58^{***}	18.50^{***}	-17.06^{**}
	(3.51)	(4.01)	(3.01)	(7.56)
years in U.S.			0.98^{***}	0.52^{*}
			(0.17)	(0.27)
multi-unit			-61.68^{***}	-82.51^{***}
			(4.32)	(5.75)
undocumented × years in U.S.				0.78^{**}
				(0.32)
undocumented × multi-unit				33.37^{***}
				(6.01)
Adj. R^2	0.01	0.34	0.58	0.58
Num. obs.	61481	61481	61481	61481

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.14: "Hisp" restriction

	Model 1	Model 2	Model 3	Model 4
undocumented	-10.73^{***}	28.03^{***}	21.40^{***}	-27.39^{***}
	(3.55)	(4.27)	(3.23)	(7.91)
years in U.S.			1.07^{***}	0.34
			(0.19)	(0.28)
multi-unit			-63.22^{***}	-87.18^{***}
			(4.43)	(5.95)
undocumented × years in U.S.				1.31^{***}
				(0.33)
undocumented × multi-unit				38.79^{***}
				(6.35)
Adj. R^2	0.01	0.32	0.56	0.56
Num. obs.	60169	60169	60169	60169

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.15: "Educ" restriction

A.9. Other Outcomes of Interest

Linear Probability Model for Type of Unit Rented

Results from a regression where the outcome of interest is the indicator for whether a renter resides in multi-unit housing. Estimates provide suggestive evidence that undocumented immigrants are more likely to reside in multi-unit housing if they live in a sanctuary city. Note that increased demand for these units would, in isolation, *increase* the amount undocumented renters pay for multi-unit housing. However, results from section 1.5 indicate that the policies induce undocumented renters to pay significantly *less* for these kinds of units, further reinforcing the theory that search frictions drive the results for multi-unit housing.

	Model 1	Model 2	Model 3	Model 4
undocumented	-0.0054	-0.0124^{***}		
	(0.0041)	(0.0039)		
treat	-0.0055	-0.0063	-0.0039	-0.0025
	(0.0067)	(0.0062)	(0.0086)	(0.0078)
treat $\times$ undocumented	0.0062	0.0127^{**}	0.0018	0.0057
	(0.0064)	(0.0059)	(0.0111)	(0.0100)
PUMA $\times$ undocumented fe	No	No	Yes	Yes
Year $\times$ undocumented fe	No	No	Yes	Yes
Controls	No	Yes	No	Yes
Adj. R^2	0.2092	0.3624	0.2137	0.3657
Num. obs.	111713	111713	111713	111713

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.16: LPM for multi-unit indicator. Controls, where included, are age, age squared, household income, marital status, gender, years in the U.S., time in residence (intervalled), number of people in household, number of workers in household, number of rooms, number of bedrooms, and build year (intervalled).

Linear Probability Models for Movement

These results resemble those presented in Appendix A.3 but examine how movement within the PUMA or from outside the PUMA of current residence may drive results.

	Moved	Moved	W/in PUMA	W/in PUMA	Out	Out
undocumented	-0.0118^{***}	-0.0336^{***}	-0.0038	-0.0170^{***}	-0.0019	-0.0042^{***}
	(0.0044)	(0.0042)	(0.0035)	(0.0034)	(0.0013)	(0.0014)
treat	-0.0132^{*}	-0.0123^{*}	-0.0013	-0.0009	-0.0024	-0.0021
	(0.0070)	(0.0069)	(0.0054)	(0.0054)	(0.0019)	(0.0019)
treat × undocumented	0.0177^{***}	0.0189^{***}	0.0066	0.0071	0.0046^{**}	0.0042^{**}
	(0.0068)	(0.0064)	(0.0052)	(0.0051)	(0.0021)	(0.0020)
years in U.S.		-0.0050^{***}		-0.0011^{***}		-0.0002^{***}
		(0.0002)		(0.0002)		(0.0001)
multi-unit		0.0115^{***}		0.0049		0.0000
		(0.0040)		(0.0035)		(0.0012)
Adj. R^2	0.0366	0.0885	0.0247	0.0431	0.0177	0.0245
Num. obs.	111713	111713	111713	111713	111713	111713

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.17: Excludes fixed effects interacted with undocumented status

	Moved	Moved	W/in PUMA	W/in PUMA	Out	Out
treat	-0.0048	-0.0006	0.0021	0.0035	-0.0034	-0.0032
	(0.0090)	(0.0087)	(0.0067)	(0.0067)	(0.0026)	(0.0025)
treat × undocumented	0.0064	0.0023	0.0018	0.0007	0.0067^{*}	0.0062^{*}
	(0.0115)	(0.0111)	(0.0089)	(0.0088)	(0.0037)	(0.0036)
years in U.S.		-0.0050^{***}		-0.0011^{***}		-0.0002^{***}
		(0.0002)		(0.0002)		(0.0001)
multi-unit		0.0116^{***}		0.0051		0.0002
		(0.0041)		(0.0035)		(0.0012)
Adj. R^2	0.0429	0.0934	0.0295	0.0474	0.0267	0.0331
Num. obs.	111713	111713	111713	111713	111713	111713

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.18: Includes fixed effects interacted with undocumented status

A.10. Inclusion of Data from 2008-2011

There are several reasons to restrict my analysis to the period of 2012-2017. First, sanctuary city policies were typically enacted after Secure Communities completed its roll-out at the beginning of 2013. If sanctuary city policies are thought of as the "turning off" of Secure Communities policies, then it makes sense to begin the analysis only after most or all locations had Secure Communities in place (and therefore, had something to "turn off"). Secondly, the geographic boundaries, PUMA's, change between 2011 and 2012. Therefore, I can no longer include fixed effects for PUMA's in regressions. IPUMS provides a variable for consistent PUMA's (CPUMA's are broader geographic areas that remain consistent over time, but of course, lack the same degree of geographic precision that PUMA's have). Thus, in all specifications that include data from years prior to 2012 (i.e. all regressions in Appendix A.10), PUMA fixed effects are replaced with CPUMA fixed effects.[6] [7]

Results are presented in Tables A.19 through A.23. Results are of similar directions and magnitudes, and despite the lost precision most retain statistical significance at conventional levels.

[6]ACS samples prior to 2008 lack important information used in the imputation procedure to determine undocumented status.

[7]The regressions on the extended sample also (somewhat inadvertently) address the concerns one may have about the restriction I impose of limiting to counties with at least 25 undocumented households each year (Section 1.3). Because geographic boundaries change between 2011 and 2012, the set of counties identifiable in the data is slightly different pre- and post- 2012. Therefore, in A.10, this restriction to counties with 25 or more undocumented households per year is not applied. Despite the additional room for error the lifting of this restriction creates, results are qualitatively, quite robust.

	Model 1	Model 2	Model 3	Model 4
undocumented	8.50***	41.11***	35.68***	9.98
	(2.45)	(5.52)	(3.59)	(8.84)
years in U.S.			−1.45***	−1.19***
			(0.23)	(0.25)
multi-unit			−96.78***	−128.13***
			(6.86)	(7.36)
undocumented × years in U.S.				−0.60*
				(0.31)
undocumented × multi-unit				49.27***
				(6.06)
Adj. R^2	0.00	0.27	0.52	0.52
Num. obs.	220143	220143	220143	220143

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.19: Effect on rent. Compare to Table 1.2.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
undocumented	32.08***	7.81	3.19			
	(3.78)	(8.97)	(9.03)			
treat	14.65**	15.21**	6.96	19.06**	18.92**	9.99
	(6.98)	(7.13)	(11.56)	(7.70)	(7.72)	(12.48)
treat × undocumented	19.87***	18.52***	52.20***	13.62*	13.57*	48.76***
	(7.04)	(6.94)	(12.16)	(7.52)	(7.54)	(12.91)
years in U.S.	−1.47***	−1.18***	−1.18***	−1.51***	−1.24***	−1.24***
	(0.23)	(0.25)	(0.25)	(0.23)	(0.23)	(0.23)
multi-unit	−96.99***	−127.82***	−130.18***	−96.98***	−117.41***	−119.70***
	(6.89)	(7.37)	(8.26)	(6.93)	(7.09)	(7.90)
undocumented × years in U.S.		−0.64**	−0.65**		−0.56*	−0.57*
		(0.31)	(0.31)		(0.30)	(0.30)
undocumented × multi-unit		48.48***	55.34***		32.20***	39.01***
		(5.96)	(6.20)		(6.23)	(6.50)
treat × multi-unit			11.36			11.84
			(12.25)			(12.47)
treat × multi-unit × undocumented			−45.22***			−46.47***
			(13.45)			(13.46)
Adj. R^2	0.52	0.52	0.52	0.52	0.52	0.52
Num. obs.	220143	220143	220143	220143	220143	220143

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.20: Effect on rent. Compare to Table 1.3.

	Unr	Unr	Inc	Inc	Hisp	Hisp	Educ	Educ
treat	20.73***	7.05	20.29***	3.75	12.19*	7.68	15.38**	13.38
	(6.78)	(10.42)	(6.98)	(9.96)	(7.33)	(10.70)	(6.93)	(11.08)
treat × undocumented	4.64	37.86***	3.76	36.59***	4.54	30.64**	9.28	30.68**
	(6.62)	(12.10)	(7.05)	(12.62)	(7.15)	(12.25)	(7.30)	(12.00)
undocumented × multi-unit	25.99***	32.86***	17.16***	24.51***	28.32***	33.33***	27.41***	31.26***
	(5.35)	(5.61)	(5.62)	(6.01)	(5.75)	(6.36)	(5.05)	(5.49)
treat × multi-unit		18.21		22.28**		6.44		2.74
		(11.32)		(11.24)		(11.43)		(12.12)
treat × multi-unit × undocumented		−43.97***		−43.83***		−36.33***		−28.98**
		(12.31)		(12.90)		(13.25)		(12.05)
Adj. R^2	0.54	0.54	0.53	0.53	0.55	0.55	0.53	0.53
Num. obs.	189675	189675	171334	171334	129205	129205	132225	132225

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.21: Effect on rent. Compare to Table 1.4.

	Unr	Inc	Hisp	Educ	Unr	Inc	Hisp	Educ
undocumented	−310.80***	−349.59***	−78.61***	−44.61***				
	(32.06)	(33.05)	(19.98)	(16.83)				
treat	−234.49***	−223.12***	−161.97***	−141.80***	−133.75***	−143.98***	−93.64**	−55.44
	(40.86)	(43.84)	(34.52)	(30.20)	(40.48)	(43.33)	(40.04)	(36.10)
treat × undocumented	284.02***	258.83***	180.59***	168.88***	146.79***	153.49***	77.11*	37.99
	(45.97)	(48.16)	(30.95)	(29.04)	(45.38)	(48.49)	(40.94)	(38.77)
Adj. R^2	0.29	0.28	0.40	0.40	0.30	0.29	0.40	0.40
Num. obs.	189675	171334	129205	132225	189675	171334	129205	132225

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.22: Effect on income. Compare to Table 1.5.

	Unr	Inc	Hisp	Educ				
undocumented	0.0142***	0.0154***	0.0056***	0.0027				
	(0.0018)	(0.0017)	(0.0018)	(0.0018)				
treat	0.0194***	0.0188***	0.0177***	0.0171***	0.0139***	0.0137***	0.0105**	0.0111***
	(0.0029)	(0.0028)	(0.0036)	(0.0035)	(0.0031)	(0.0032)	(0.0042)	(0.0040)
treat × undocumented	−0.0206***	−0.0185***	−0.0176***	−0.0160***	−0.0126***	−0.0111***	−0.0070	−0.0066
	(0.0028)	(0.0027)	(0.0031)	(0.0032)	(0.0039)	(0.0037)	(0.0048)	(0.0048)
Adj. R^2	0.2044	0.2357	0.2366	0.2309	0.2081	0.2393	0.2393	0.2345
Num. obs.	189675	171334	129205	132225	189675	171334	129205	132225

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.23: Effect on rent as a fraction of income. Compare to Table 1.6.

A.11. Excluding Select States

One may be concerned that the findings presented in this paper are driven by a subset of states with large undocumented populations. If, for example, premiums for housing only exist in California or sanctuary city policies are only effective in California, then the estimates produced by the analysis so far, may simply be a result of the sheer number of observations in California. It could be the case that the findings do not hold in other states and merely arise because average effects are driven by the large number of observations in states where the results *do* hold. To address this possibility, I create 4 subsamples on which I rerun the regressions that characterize the findings of this study. The first subsample drops all observations from the state of California. The second drops all observations from California and Texas. The third drops all observations from California, Texas, and Florida. The fourth drops all observations from California, Texas, Florida, and New York.[8] The regression results are presented in the tables below and support the story that premiums arising from undocumented status and alleviated by sanctuary city policies are nationwide phenomena. Compared to the results from the nationwide samples (presented in the text), the coefficients of interest nearly always retain their significance (and approximate magnitudes) and only occasionally become statistical zeroes, despite a rapidly dwindling sample size.

[8] These are the states with the largest undocumented populations.

	Model 1	Model 2	Model 3	Model 4
undocumented	41.44^{***}	-0.02	52.68^{***}	21.65^{*}
	(5.20)	(9.80)	(6.22)	(12.13)
years in U.S.	-1.10^{***}	-1.12^{***}	-1.15^{***}	-1.00^{**}
	(0.30)	(0.39)	(0.37)	(0.46)
multi-unit	-91.58^{***}	-127.54^{***}	-108.93^{***}	-137.34^{***}
	(6.17)	(8.85)	(7.36)	(10.48)
undocumented × years in U.S.		-0.04		-0.33
		(0.40)		(0.49)
undocumented × multi-unit		54.81^{***}		45.17^{***}
		(8.55)		(10.44)
Adj. R^2	0.53	0.53	0.50	0.50
Num. obs.	68518	68518	52475	52475

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.24: Effect on rent (compare to last two columns of Table 1.2). The first 2 columns are from the sample that excludes California. The last 2 columns are from the sample that excludes both California and Texas.

	Model 1	Model 2	Model 3	Model 4
undocumented	53.11^{***}	11.99	28.21^{***}	19.25
	(7.24)	(14.88)	(6.16)	(15.59)
years in U.S.	-1.34^{***}	-1.53^{***}	-0.72^{**}	-0.48
	(0.44)	(0.55)	(0.35)	(0.49)
multi-unit	-119.40^{***}	-149.10^{***}	-116.13^{***}	-130.08^{***}
	(9.05)	(13.83)	(8.63)	(12.39)
undocumented × years in U.S.		0.34		-0.44
		(0.56)		(0.62)
undocumented × multi-unit		44.23^{***}		19.92
		(13.18)		(12.48)
Adj. R^2	0.50	0.50	0.53	0.53
Num. obs.	41617	41617	28002	28002

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.25: Effect on rent (compare to last two columns of Table 1.2). The first 2 columns are from the sample that excludes California, Texas, and Florida. The last 2 columns are from the sample that excludes California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4
treat	26.70^{**}	8.66	29.43^{**}	1.03
	(11.46)	(20.89)	(12.32)	(21.85)
treat × undocumented	−13.47	38.97	−12.62	31.39
	(13.30)	(27.31)	(14.31)	(28.63)
years in U.S.	-1.01^{***}	-1.01^{***}	-0.86^{*}	-0.86^{*}
	(0.38)	(0.38)	(0.45)	(0.45)
multi-unit	-107.50^{***}	-110.84^{***}	-116.69^{***}	-123.38^{***}
	(7.75)	(7.91)	(9.18)	(9.48)
undocumented × years in U.S.	−0.13	−0.12	−0.41	−0.40
	(0.41)	(0.41)	(0.51)	(0.51)
undocumented × multi-unit	23.59^{***}	32.14^{***}	11.68	21.67^{**}
	(8.05)	(8.30)	(10.07)	(10.72)
treat × multi-unit		20.27		31.85
		(20.00)		(20.65)
treat × multi-unit × undocumented		-59.94^{**}		-49.80^{*}
		(26.05)		(27.09)
Adj. R^2	0.54	0.54	0.51	0.51
Num. obs.	68518	68518	52475	52475

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.26: Effect on rent, including the treatment effect (compare to Table 1.3). First 2 columns exclude California. Last 2 columns exclude both California and Texas.

	Model 1	Model 2	Model 3	Model 4
treat	37.75^{***}	8.94	31.34	0.14
	(13.63)	(23.91)	(20.47)	(26.31)
treat × undocumented	−8.97	28.17	−23.22	24.58
	(15.36)	(30.73)	(20.78)	(33.31)
years in U.S.	-1.31^{**}	-1.32^{**}	−0.54	−0.54
	(0.55)	(0.55)	(0.53)	(0.53)
multi-unit	-119.05^{***}	-129.82^{***}	-121.57^{***}	-133.63^{***}
	(12.56)	(13.98)	(13.09)	(14.20)
undocumented × years in U.S.	0.23	0.24	−0.29	−0.28
	(0.60)	(0.60)	(0.65)	(0.65)
undocumented × multi-unit	−0.59	12.43	7.05	24.13
	(12.90)	(14.62)	(14.08)	(15.24)
treat × multi-unit		32.47		39.35
		(22.13)		(26.26)
treat × multi-unit × undocumented		−42.07		-60.85^{*}
		(28.57)		(32.10)
Adj. R^2	0.51	0.51	0.54	0.54
Num. obs.	41617	41617	28002	28002

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.27: Effect on rent, including the treatment effect (compare to Table 1.3). First 2 columns exclude California, Texas, and Florida. Last 2 columns exclude California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
treat	30.82^{***}	4.60	30.58^{**}	−4.38	39.60^{***}	11.66	33.22^{*}	19.24
	(11.72)	(19.89)	(12.44)	(20.28)	(13.86)	(22.22)	(18.58)	(24.96)
treat × undocumented	-23.52^{*}	34.45	−20.83	32.86	−16.72	23.16	−27.23	3.62
	(12.63)	(26.04)	(13.49)	(26.72)	(14.77)	(28.80)	(18.05)	(30.84)
undocumented × multi-unit	26.23^{***}	35.72^{***}	17.03^{*}	29.14^{***}	−1.94	12.02	3.97	14.51
	(7.74)	(8.08)	(9.39)	(10.01)	(11.89)	(13.75)	(12.35)	(13.11)
undocumented × multi-unit × treat		-66.26^{***}		-61.04^{**}		-45.50^{*}		−39.80
		(24.81)		(25.26)		(26.86)		(30.30)
Adj. R^2	0.56	0.56	0.52	0.52	0.52	0.52	0.56	0.56
Num. obs.	56416	56416	42607	42607	33501	33501	22946	22946

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.28: Effect on rent after applying the sample restrictions described in section 1.5.4 (compare to "Unr" columns in Table 1.4). Columns 1 and 2 exclude just California. Columns 3 and 4 exclude California and Texas. Columns 5 and 6 exclude California, Texas, and Florida. Columns 7 and 8 exclude California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4
treat	−81.92	−62.76	−41.49	−44.08
	(71.02)	(75.10)	(88.73)	(128.06)
treat × undocumented	10.21	51.36	46.51	−13.48
	(92.76)	(97.60)	(110.89)	(148.36)
Adj. R^2	0.33	0.33	0.33	0.34
Num. obs.	56416	42607	33501	22946

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.29: Effect on income after applying the sample restrictions described in section 5.4 and including fixed effect interacted with undocumented status (compare to the "Unr" column in the second half of Table 1.5). Columns 1 excludes just California. Column 2 excludes California and Texas. Column 3 excludes California, Texas, and Florida. Column 4 excludes California, Texas, Florida, and New York.

	Model 1	Model 2	Model 3	Model 4
treat	0.0113*	0.0110	0.0068	0.0102
	(0.0066)	(0.0070)	(0.0077)	(0.0100)
treat × undocumented	−0.0153**	−0.0128	−0.0075	−0.0083
	(0.0077)	(0.0083)	(0.0092)	(0.0118)
Adj. R^2	0.2223	0.2169	0.2098	0.2219
Num. obs.	56416	42607	33501	22946

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.30: Effect on rent as a fraction of income after applying the sample restrictions described in section 5.4 and including fixed effect interacted with undocumented status (compare to the "Unr" column in the second half of Table 1.6). Columns 1 excludes just California. Column 2 excludes California and Texas. Column 3 excludes California, Texas, and Florida. Column 4 excludes California, Texas, Florida, and New York.

A.12. Other 3 Sample Restrictions

"10 yrs" refers to the sample restricted to immigrants who arrived in the U.S. at least 10 years ago. "Jobs" refers to the sample restricted just to the jobs Pew Hispanic lists have an over-representation of undocumented workers. "Deports" refers to the sample restricted to just immigrants from the 10 countries that see the highest number of deported individuals from the U.S.

	10 yrs	10 yrs	jobs	jobs	deports	deports
treat	0.58	2.48	4.06	-0.59	-2.43	4.31
	(8.54)	(11.32)	(8.59)	(11.23)	(8.42)	(11.17)
treat × undocumented	2.76	21.16	-0.50	31.02^{**}	6.13	25.01^{*}
	(9.56)	(13.91)	(9.74)	(13.83)	(9.37)	(13.50)
multi-unit × undocumented	36.36^{***}	43.06^{***}	34.59^{***}	45.96^{***}	30.44^{***}	35.63^{***}
	(7.27)	(8.00)	(6.69)	(7.30)	(6.72)	(7.11)
treat × multi-unit × undocumented		-25.24^{*}		-42.78^{***}		-26.27^{*}
		(13.49)		(13.69)		(13.41)
Adj. R^2	0.56	0.56	0.57	0.57	0.59	0.59
Num. obs.	61613	61613	60465	60465	58230	58230

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.31: Effect on Gross Rent.

	10 yrs	jobs	deports	10 yrs	jobs	deports
undocumented	-97.50^{***}	-64.00^{***}	-104.56^{***}			
	(27.22)	(21.51)	(23.41)			
treat	-134.43^{***}	-91.26^{**}	-122.34^{***}	-81.54	-45.11	-61.12
	(43.65)	(36.87)	(38.55)	(51.12)	(46.63)	(51.85)
treat × undocumented	194.19^{***}	187.98^{***}	201.69^{***}	104.08	123.34^{**}	118.41^{*}
	(42.27)	(37.54)	(37.32)	(65.90)	(61.21)	(61.57)
Adj. R^2	0.35	0.39	0.41	0.36	0.40	0.42
Num. obs.	61613	60465	58230	61613	60465	58230

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.32: Effect on Household Income.

	10 yrs	jobs	deports
undocumented	0.0040^{*}	0.0048^{*}	0.0057^{**}
	(0.0025)	(0.0025)	(0.0026)
treat	0.0045	0.0067	0.0065
	(0.0041)	(0.0043)	(0.0045)
treat × undocumented	-0.0149^{***}	-0.0179^{***}	-0.0165^{***}
	(0.0036)	(0.0040)	(0.0039)
Adj. R^2	0.2239	0.2367	0.2497
Num. obs.	61613	60465	58230

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.33: Effect on Rent as a Fraction of Household Income.

	10yrs	jobs	deports
treat	0.0045	0.0063	0.0039
	(0.0053)	(0.0057)	(0.0063)
treat × undocumented	-0.0139^{**}	-0.0174^{**}	-0.0125^{*}
	(0.0066)	(0.0069)	(0.0069)
Adj. R^2	0.2280	0.2413	0.2536
Num. obs.	61613	60465	58230

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.34: Effect on Rent as a Fraction of Household Income (first-order effect of undocumented subsumed by fixed effects).

Appendix B

Appendix to Homeownership in the Undocumented Population and the Consequences of Credit Constraints

B.1. Parallel Trends and Synthetic Control Summary

Each difference-in-differences specification relies on the assumption of parallel trends. This section will assess each of the (in-text) county-level specifications in turn.[1] For each outcome, I present event study plots for transparency and to illustrate that, in most cases, there is little to no evidence of pre-trends that would bias the difference-in-differences estimates presented in the text. Then, because all of the analysis conducted at the county-level (sections 2.4 and 2.5) relies on a panel of the same counties observed over time (as opposed to the household-level analysis, which is cross-sectional where individuals are observed only once), it is possible to produce estimates based on a synthetic control design. In the cases where the parallel trends assumption is unlikely to hold, estimates from synthetic control may be interpreted as more credible. In most cases, where there is little evidence of pre-trends, synthetic control estimates should closely resemble the difference-in-differences estimates and are therefore, presented for completeness and as tests of robustness to an alternative empirical strategy.[2]

I present two different p-values for the estimates throughout this section. They are defined in Galiani and Quistorff (2016). In essence, the first will be the standard, basic p-value computed for synthetic control (the proportion of times a placebo effect size exceeds the treated effect size), and the second will be a scaled alternative (the proportion of times a placebo effect size scaled by its pre-period RMSPE exceeds the treated effect size scaled by its pre-period RMSPE) to account for potential differences in the ability of the synthetic control procedure to accurately match pre-period trends in treated and placebo units. In Appendix B.4, I extend the analysis to include two additional p-values that test the joint significance

[1]The trends assumptions for the analysis conducted at the household level have been assessed in other sections.

[2]Note that when there are two treatment categories (as in all of the county-level DACA analysis), synthetic control is run for the sample that excludes units in the "medium" category (i.e. synthetic control compares high DACA take-up units with the excluded category - low DACA take-up units).

of the post-period estimates and where appropriate, also provide p-values corresponding to one-sided hypothesis tests (as defined in Galiani and Quistorff (2016) and Abadie (2021)).

Readers who wish to read a (lengthier) more thorough breakdown of the synthetic control results, the methods used, and the myriad ways statistical significance has been tested should now skip to Appendix B.4 as the analysis in this section is repeated there. Note that the conclusions are consistent across sections.[3]

The remaining subsections are structured as follows. The first four subsections present results for DACA's effects on home loan applications, home loan approvals, size of home loans applied for, and size of home loans approved, respectively (sections 2.4.2, 2.4.4, and 2.4.4). The remaining four subsections present results for the Treasury rule change's effects on the same outcomes (sections 2.5.3, 2.5.4, and 2.5.5). For each subsection, I

1. Refer to the table corresponding to the in-text difference-in-differences results
2. Present 2 figures that plot
    i. point estimates for the treated group relative to its synthetic control
    ii. point estimates for the placebo group relative to its synthetic control[4]
3. Include a table that shows estimated effects and corresponding p-values by year

[3]The primary difference in Appendix B.4 is the addition of "joint p-values" and the complications they introduce. Occasionally, joint p-values are wildly inconsistent with the other p-values across the post-period. In every instance, the inconsistency is resolved by imposing restrictions on outliers in the placebo set (eliminating placebo units that could not be matched well in the pre-period) and/or switching to one-sided hypothesis testing (because joint p-values are computed using *squared* differences, direction is not taken into account, and large, negative deviations can make positive effects appear insignificant, especially when over-fitting is a problem).

[4]Recall that the placebo units (either "DACA Takeup LOW" counties or "Hisp. Undoc LOW" counties) are still counties that may experience some effect of treatment (i.e. "DACA Takeup LOW" still means *some* DACA Takeup may have occurred and "Hisp. Undoc LOW" still means that *some* Hispanic resident are likely undocumented), but the intensity should be lower. Therefore, while the figures for the treated units should illustrate a relatively large deviation from the synthetic trend (when the policy truly has an effect), the figures for the placebo units may exhibit similar but smaller deviations as the placebo counties aren't totally untreated.

Presented below are the event study figures (one corresponding to each of the four outcomes for each of the two policies). If the event study indicates that parallel trends does not hold, then the synthetic control estimates should be considered more credible. On each figure, I have added the point estimate from the corresponding difference-in-differences specification (all of which can be found in the tables in the text) for reference.[5] The parallel trends assumption appears to hold in all cases except when measuring the effect of the Treasury rule change on Hispanic applications and maybe when measuring the effect of DACA on Hispanic approvals. Therefore, in the former case, the estimated effect of 1.34 percentage points is overstated, and in the latter case, the estimated 1.33 percentage point effect may also be overstated. In all other cases, we should expect synthetic control to produce estimated effects that are comparable to the difference-in-differences estimates.

[5] In principle, the difference-in-differences estimate is a weighted average of the event study estimates across all post periods

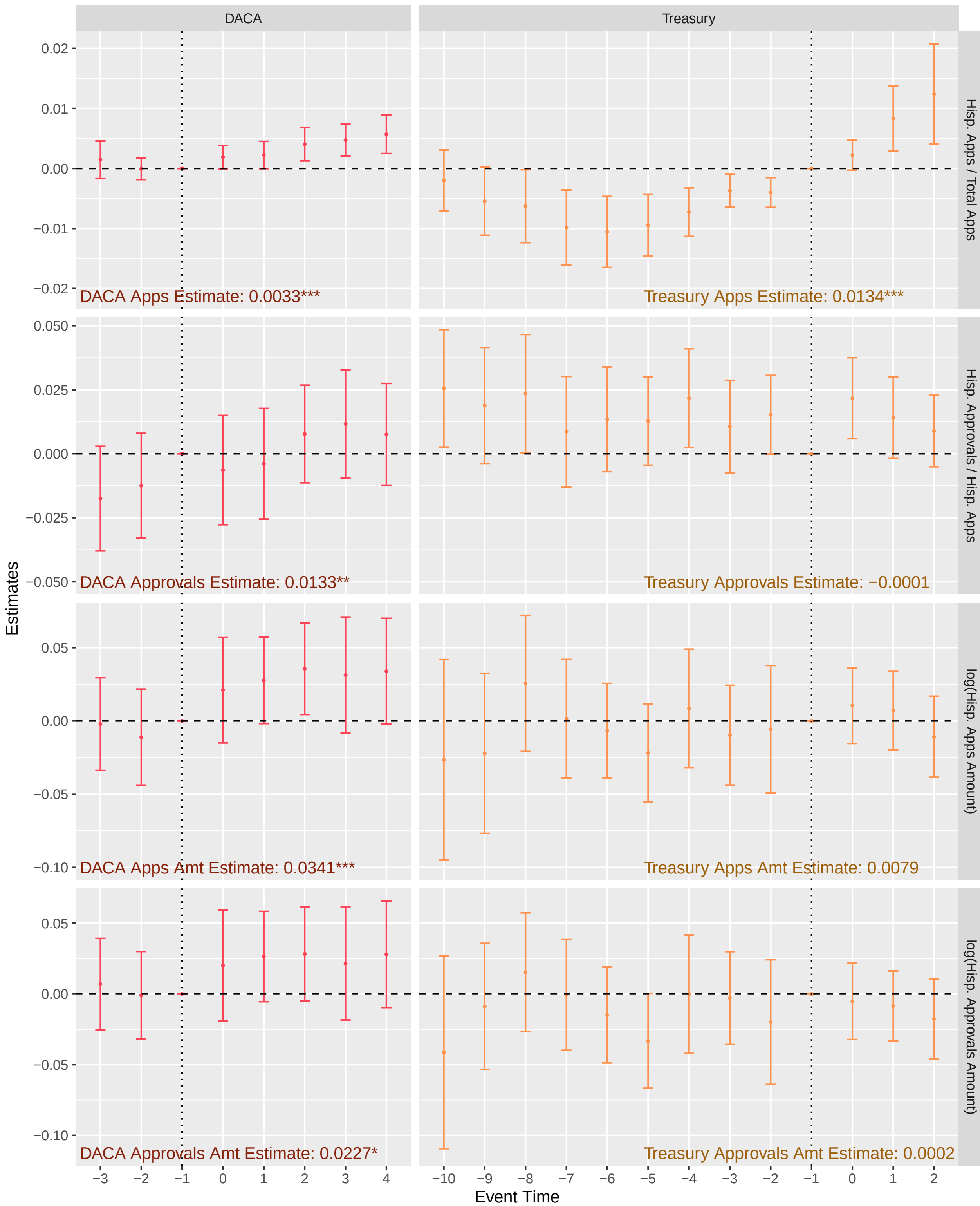

Figure B.1: Event studies for each policy's effect on each of the 4 main county-level outcomes of interest.

B.1.1. Applications Outcome (DACA)

Refer to the estimates in Table 2.5 and the event study in Figure 2.6.

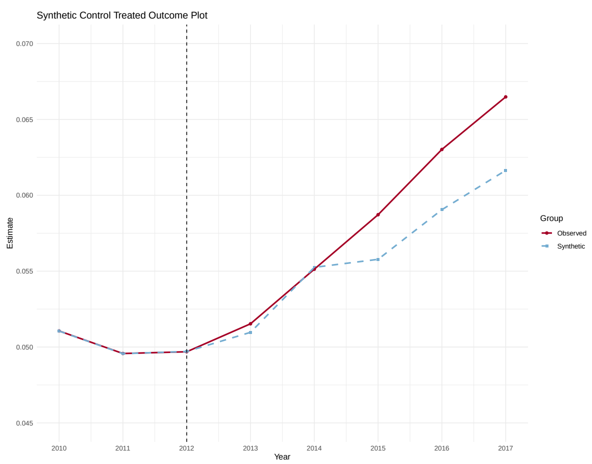

Figure B.2: Treated units

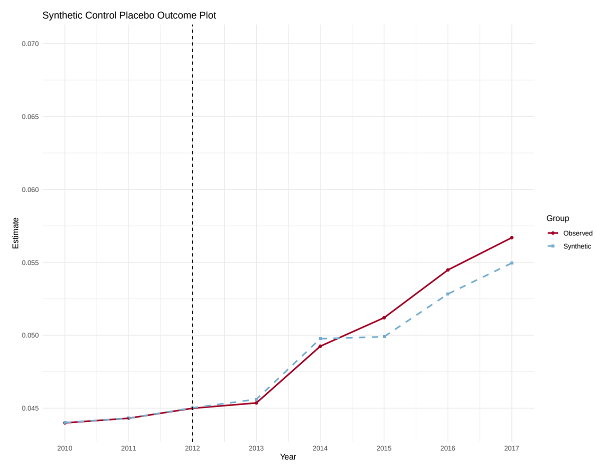

Figure B.3: Placebo units

Year	Effect	p-value	p-value scaled
2013	0.0006	0.3233	0.0829
2014	-0.0001	0.8909	0.4884
2015	0.0029**	0.0043	0.0325
2016	0.0040***	0.0003	0.0061
2017	0.0048**	0.0000	0.0339

Table B.1: Effect of DACA on the Hispanic home loan application rate ($\frac{Hisp.Apps}{Total Apps}$) in counties with high DACA take-up estimated by synthetic control. Stars denote significance as indicated by the larger (more conservative) of the two p-values.

Consistent with findings from event studies and difference-in-differences estimates, synthetic control detects a positive effect that is greater (in magnitude and significance) after two years have passed (i.e. the "adjustment period"). Weighting each post-period year equally, the joint post-period estimated effect is a 0.244 percentage point increase in the relative number of Hispanic home loan applications, which is close to the unweighted difference-in-differences estimate of 0.33 percentage points.

B.1.2. Approvals Outcome (DACA)

Refer to the estimates in Table 2.9.

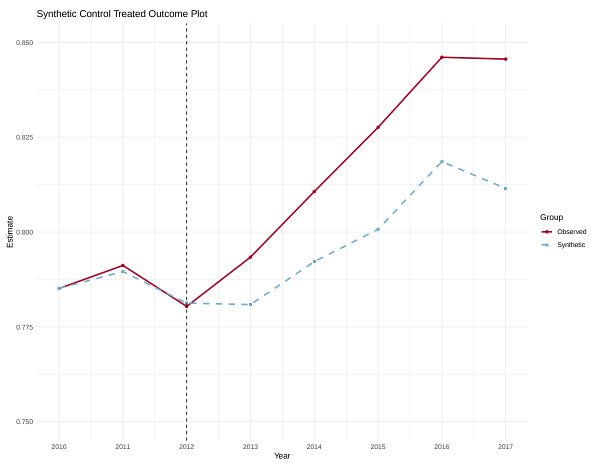

Figure B.4: Treated units

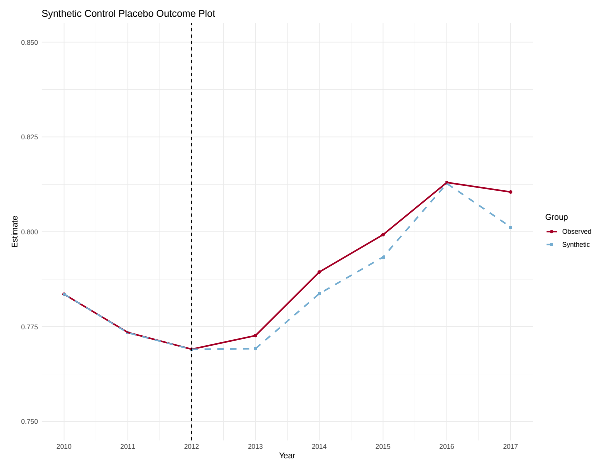

Figure B.5: Placebo units

Year	Effect	p-value	p-value scaled
2013	0.0125**	0.0206	0.0005
2014	0.0185	0.0006	0.1645
2015	0.0269***	0.0000	0.0000
2016	0.0275***	0.0000	0.0000
2017	0.0341***	0.0000	0.0000

Table B.2: Effect of DACA on the Hispanic home loan approval rate in counties with high DACA take-up estimated by synthetic control. Stars denote significance as indicated by the larger (more conservative) of the two p-values.

Estimates from the synthetic control empirical strategy support the in-text, difference-in-differences results. Weighting each post-period year equally, the joint post-period estimated effect is a 2.39 percentage point increase in the Hispanic home loan approval rate, which is even larger than the unweighted difference-in-differences estimate of 1.33 percentage points. The two types of p-values both indicate statistical significance at (at least) the 95% confidence level in all periods but one.

B.1.3. Loan Amount (Applications) Outcome (DACA)

Refer to the estimates in Table 2.10.

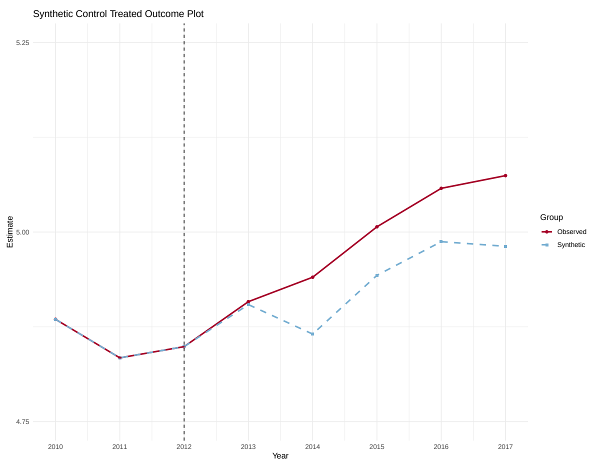

Figure B.6: Treated units

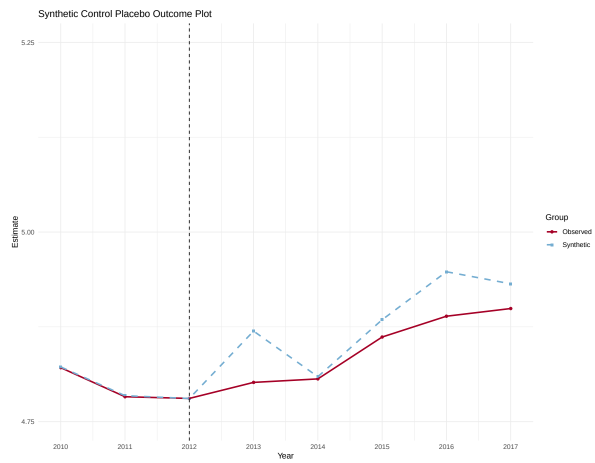

Figure B.7: Placebo units

Year	Effect	p-value	p-value scaled
2013	0.0039	1.0000	1.0000
2014	0.0749***	0.0000	0.0004
2015	0.0640**	0.0000	0.0117
2016	0.0703	0.1121	0.2585
2017	0.0932***	0.0000	0.0000

Table B.3: Effect of DACA on the size of Hispanic home loan applications in counties with high DACA take-up estimated by synthetic control. Stars denote significance as indicated by the larger (more conservative) of the two p-values.

Results are, again, in line with the results from the difference-in-differences specifications. Weighting each post-period year equally, the joint post-period estimated effect is a 6.13% increase in the size of Hispanic home loan applications, which is even larger than the unweighted difference-in-differences estimate of 3.41%. P-values indicate statistical significance at (at least) the 95% confidence level in three of five post-period years.

B.1.4. Loan Amount (Approvals) Outcome (DACA)

Refer to the estimates in Table 2.11.

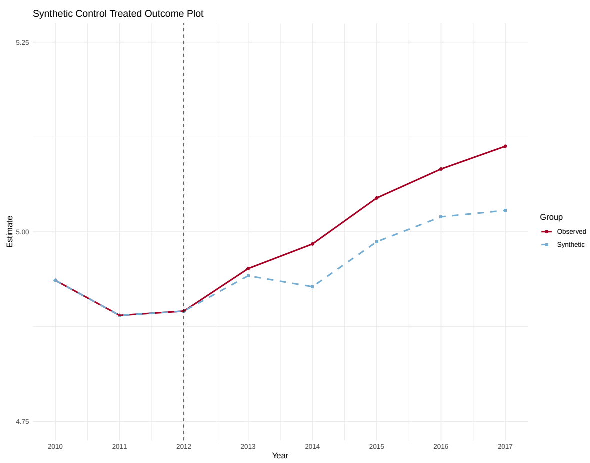

Figure B.8: Treated units

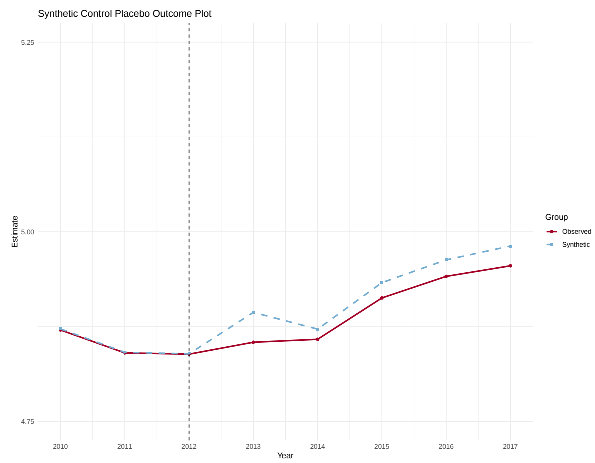

Figure B.9: Placebo units

Year	Effect	p-value	p-value scaled
2013	0.0095	0.9968	0.9989
2014	0.0563**	0.0000	0.0145
2015	0.0577***	0.0000	0.0011
2016	0.0630***	0.0000	0.0000
2017	0.0845***	0.0000	0.0000

Table B.4: Effect of DACA on the size of approved Hispanic home loan applications in counties with high DACA take-up estimated by synthetic control. Stars denote significance as indicated by the larger (more conservative) of the two p-values.

Results are, again, in line with the results from the difference-in-differences specifications. Weighting each post-period year equally, the joint post-period estimated effect is a 5.42% increase in the size of Hispanic home loan applications, which is even larger than the unweighted difference-in-differences estimate of 2.27%. P-values indicate statistical significance at (at least) the 95% confidence level in four of the five post-period years.

B.1.5. Applications Outcome (Treasury)

Refer to the estimates in Table 2.12. The event study corresponding to column 1 is presented in Figure 2.8 (or in Figure B.1 with the other event studies). The pre-trends suggest that difference-in-differences estimates are likely to be positively biased. Therefore, an effective synthetic control strategy that does not suffer such bias would be expected to yield smaller estimated effects. Plots, estimated effects, and p-values are presented below.

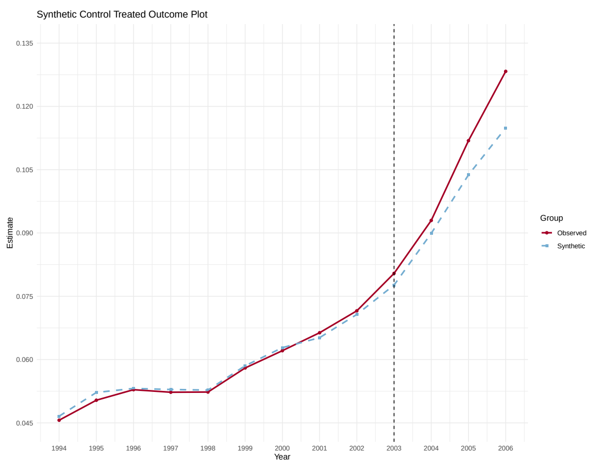

Figure B.10: Treated units

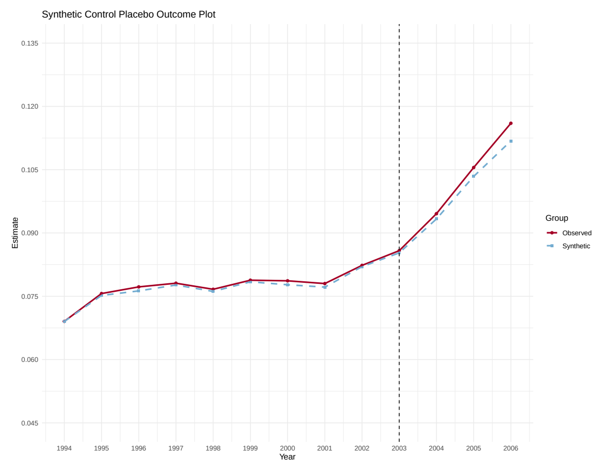

Figure B.11: Placebo units

Year	Effect	p-value	p-value scaled
2004	0.0031**	0.0204	0.0004
2005	0.0081***	0.0000	0.0000
2006	0.0134***	0.0000	0.0000

Table B.5: Effect of Treasury rule change on the Hispanic home loan application rate ($\frac{Hisp.Apps}{Total Apps}$) in counties with high undocumented populations estimated by synthetic control. Stars denote significance as indicated by the larger (more conservative) of the two p-values.

The effects are consistent with expectations. All estimates are positive and significant at (at least) the 95% confidence level, and consistent with the idea that difference-in-differences estimates are upwards biased due to trends, the synthetic control estimates are smaller in magnitude. Thus, the synthetic control estimated effect of a 0.95 percentage point effect on the Hispanic home loan application rate should be considered more accurate than the 1.34 percentage point change indicated by the (biased) difference-in-differences results.

B.1.6. Approvals Outcome (Treasury)

Refer to the estimates in Table 2.13.

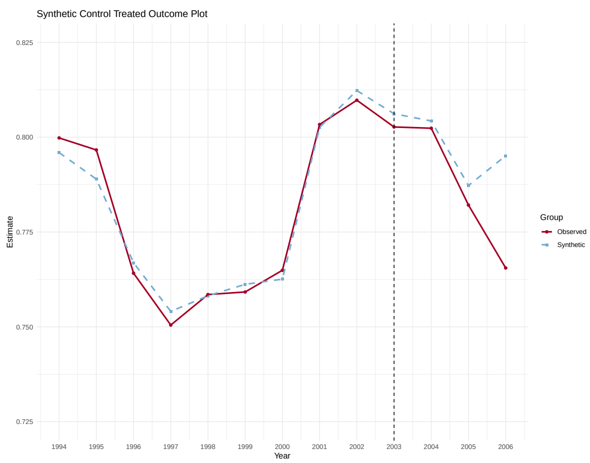

Figure B.12: Treated units

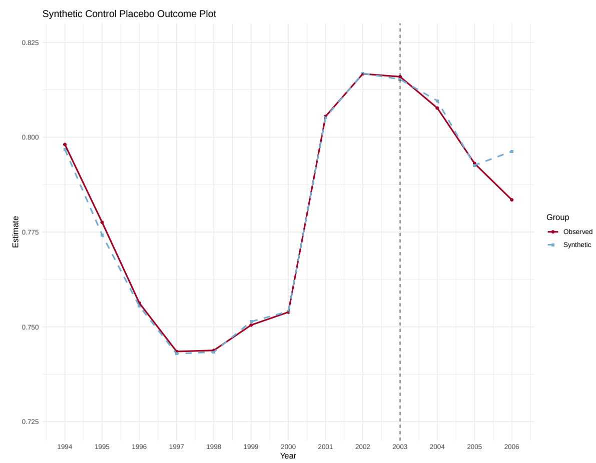

Figure B.13: Placebo units

Year	Effect	p-value	p-value scaled
2004	-0.0019	0.6329	0.6797
2005	-0.0052	0.2284	0.5428
2006	-0.0296	0.0001	0.1962

Table B.6: Effect of Treasury rule change on Hispanic home loan approval rate in counties with high undocumented populations estimated by synthetic control.

Estimates are negative in all years and mostly larger in magnitude than the difference-in-differences estimates (the largest diff-in-diff estimate is a -0.28 percentage point effect - a value between the 2004 and 2005 estimates from synthetic control). The first p-value (column 3) indicates that the effect in 2006 is statistically significant at conventional levels. However, once pre-period fit is accounted for (column 4), the significance is lost. The estimates are insignificant in all other periods. Thus, the results are consistent with the results from the difference-in-differences specifications where point estimates were negative but statistically insignificant.

B.1.7. Loan Amount (Applications) Outcome (Treasury)

Refer to the estimates in Table 2.14.

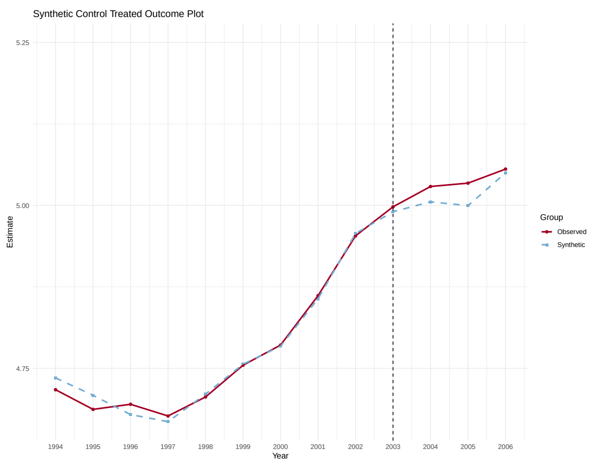

Figure B.14: Treated units

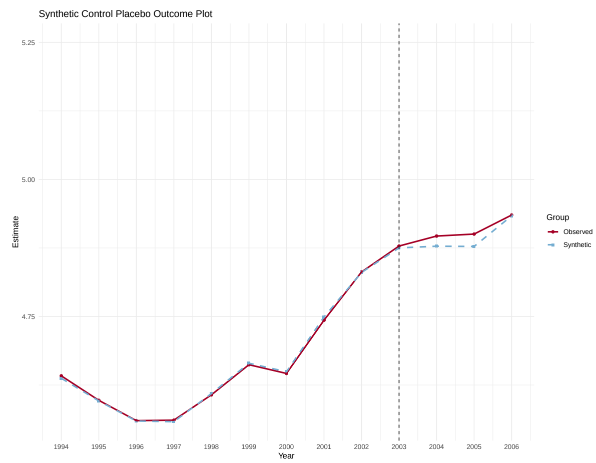

Figure B.15: Placebo units

Year	Effect	p-value	p-value scaled
2004	0.0235	0.3076	0.8325
2005	0.0345	0.1369	0.9380
2006	0.0058	0.6389	0.6634

Table B.7: Effect of Treasury rule change on size of Hispanic home loan applications in counties with high undocumented populations estimated by synthetic control.

The evidence from synthetic control is broadly consistent with the difference-in-differences results and the accompanying event study. Estimates are positive in direction (though larger on average), which is consistent with the comparable difference-in-differences specification (where California is included and population weights are not applied), and all p-values indicate that the estimated effects are statistically indistinguishable from zero.

B.1.8. Loan Amount (Approvals) Outcome (Treasury)

Refer to the estimates in Table 2.15.

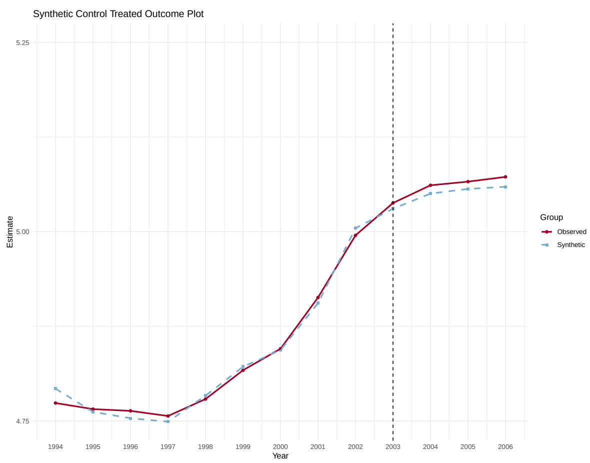

Figure B.16: Treated units

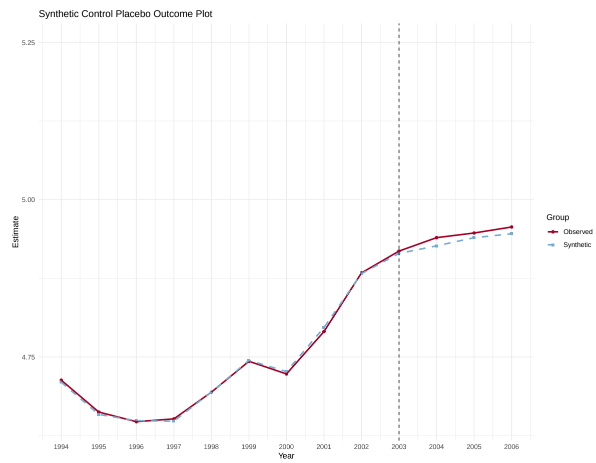

Figure B.17: Placebo units

Year	Effect	p-value	p-value scaled
2004	0.0110	0.5913	0.9816
2005	0.0096	0.4876	0.9256
2006	0.0131	0.4729	0.7001

Table B.8: Effect of Treasury rule change on the size of approved Hispanic home loan applications in counties with high undocumented populations estimated by synthetic control.

Estimates from synthetic control are again, larger in magnitude, but like the estimates from the difference-in-differences specifications, they are statistically indistinguishable from zero.

Thus, all synthetic control estimates are consistent with their corresponding difference-in-differences estimates when the parallel trends assumption appears to hold.

B.2. Further Assessment of Imputation

To assess the possibility that the procedure used to impute undocumented status introduces a bias towards lower homeownership among those classified as undocumented immigrants, I run a similar imputation procedure on the sample of U.S. citizens. If it is the procedure, itself, that drives the correlation between undocumented status and homeownership, then we should expect to see the same correlation arise among U.S. citizens who fulfill the imputation's criteria to be considered "undocumented" if it weren't for their citizenship status. I provide evidence that little, if any, of the observed relationship between undocumented status and homeownership arises mechanically from the imputation procedure employed.

I first return to the imputation procedure described in section 2.3.1, but instead apply each of the logical edits to citizens where applicable. The only difference in the imputation procedure applied to citizens is that any logical edit that relies on when a person arrived in the U.S. is not excluded.[6]

After citizens have been assigned their "pseudo-status" (the status they would be assigned by the imputation if they hadn't already been observed to be citizens), I restrict the sample in the same way the choice sample of immigrants was restricted in section 2.3.1[7] and generate summary statistics akin to those in Table 2.2. As can be seen in Table B.9, the raw ownership gap between undocumented immigrants and legal residents is much larger than the equivalent gap between citizens who are categorized as undocumented and citizens categorized as legal residents by a similar procedure. In other words, if a homeownership gap of 3

[6]This means that the edits to account for likely student visa holders, individuals who likely achieved legal status through IRCA 1982, and those who are likely in the U.S. on H-1B visas are not applied. Additionally, if a citizen's spouse is a citizen, they are not assigned legal resident status. However, if an individual's spouse has been assigned legal resident status by another logical edit, that individual *is* considered to be a legal resident by the last edit of the imputation procedure.

[7]The exception is that the sample is not restricted to those with a years in the U.S. term of 0 or greater than 37 because years in the U.S. is not meaningful for the majority of the sample of citizens.

percentage points is attributable to the imputation procedure (because that is approximately the observed difference between “pseudo-undocumented and pseudo-legal residents”), then an unexplained gap of roughly 5 percentage points (as opposed to 8) between immigrants of different statuses still remains. Alternatively, if the imputation procedure mechanically drives those who are legal residents to be 3.8 (the percent change from 0.7066 to 0.7333) percent more likely to be homeowners, then legal residents are still nearly 18 percent more likely to be homeowners than undocumented immigrants (as opposed to roughly 21.5% more likely). In short, Table B.9 illustrates that very little of the raw homeownership gap between undocumented immigrants and legal residents can be attributed to any mechanical correlation that could arise from the imputation procedure used to assign immigrant status.

To further buttress the argument that the imputation procedure only negligibly influences the association between undocumented status and lower homeownership rates (if at all), I rerun descriptive regressions like those in section 2.3.2. Table B.10 presents results from the various descriptive regression specifications run on the sample of citizens who have been assigned their “pseudo-status” (i.e. the sample includes only citizens, and “undocumented” is now 1 if the citizen was categorized as “undocumented” by the modified imputation procedure and 0 otherwise). Columns 1-3 are identical to columns 4-6 in Table 2.3 and are provided for reference. Note that once controls are included, citizens classified as undocumented by the procedure are actually more likely to be homeowners, suggesting the imputation procedure applied to immigrants in the text may even yield estimates that are *lower* in magnitude than the true effect (i.e. the effect absent any mechanical bias from the imputation procedure). Even the negative coefficient estimates observed in the specifications that lack controls (columns 4 and 5) are of much smaller magnitudes than those observed for the sample of immigrants in columns 1 and 2. Altogether, there appears to be little evidence to suggest that the magnitude of the homeownership gap between undocumented immigrants and legal residents is inflated mechanically by the imputation procedure employed to assign immigrant status.

	Legal Resident	Undocumented	Citizen	Pseudo-Legal Resident	Pseudo-Undocumented
owned	0.4217	0.3469	0.7219	0.7333	0.7066
age	45.92	40.8	54.17	62.88	45.02
male	0.5564	0.6106	0.5155	0.4999	0.5372
married	0.7089	0.5318	0.5281	0.5071	0.5343
years in us	17.61	14.7	NA	NA	NA
monthly income (2010 dollars)	4489	4206	6075	4537	6571
people in household	3.631	3.594	2.408	2.178	2.677
workers in household	1.49	1.66	1.135	0.7446	1.581
children in household	1.249	1.267	0.5196	0.3525	0.7066

Table B.9: Summary statistics for the household-level microdata sample by immigrant status. Columns 1-3 are equivalent to Table 2.2. Columns 4 and 5 are derived from the sample of citizen households after undergoing the imputation procedure used to assign undocumented status as described in this section.

	owned	owned	owned	owned	owned	owned
(Intercept)	0.3735***			0.6759***		
	(0.0099)			(0.0036)		
undoc	−0.0710***	−0.0928***	−0.0206***	−0.0292***	−0.0263***	0.0765***
	(0.0054)	(0.0043)	(0.0029)	(0.0036)	(0.0030)	(0.0012)
years in U.S.			0.0141***			
			(0.0007)			
years in U.S.^{2}			−0.0002***			
			(0.0000)			
age			0.0163***			0.0261***
			(0.0005)			(0.0002)
age^2			−0.0001***			−0.0002***
			(0.0000)			(0.0000)
log(income)			0.0413***			0.0576***
			(0.0013)			(0.0007)
never married			−0.1407***			−0.2411***
			(0.0034)			(0.0021)
female			0.0196***			−0.0157***
			(0.0021)			(0.0008)
number workers			−0.0040**			0.0127***
			(0.0016)			(0.0007)
number people			0.0203***			0.0002
			(0.0022)			(0.0009)
number kids			−0.0192***			0.0039***
			(0.0019)			(0.0009)
Fixed Effects	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Num. obs.	468960	468960	468960	10511358	10511358	10511358
Adj. R^2	0.0054	0.0945	0.2152	0.0009	0.0564	0.2690
N Clusters	1077	1077	1077	1078	1078	1078

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table B.10: Linear probability models for housing tenure (owned = 1) where columns 1-3 are run on the sample of immigrant households (equivalent to columns 4-6 of Table 2.3) and columns 4-6 are results from similar regressions run on citizen households that have been classified as undocumented or legal resident by the modified imputation procedure. Column 1 (2) is specified identically to column 4 (5). Column 3 includes controls for years in the U.S. and its square, whereas column 6 does not as years in the U.S. is not meaningful for most citizens (and would be almost perfectly collinear with age). Robust standard errors clustered at the CPUMA level (the most precise geographic variable available). All regressions use household weights provided by the ACS.

B.3. Alternative Household-Level Difference-in-differences

The specification chosen in section 2.4.1 may be altered to focus on households where DACA is most likely to have an effect. In this section, I assign each household head an indicator that takes value 1 if anyone in the household meets the eligibility criteria for DACA. Specifically, any household in which any individual is born after 1980, has been in the U.S. since at least 2007, and arrived in the U.S. when they were no older than 16 is assigned a value *daca in hh* = 1. If the sample is restricted to undocumented households only, then the following specification could verify that the change in share of households residing in owner-occupied housing is driven by households in which at least one member was plausibly eligible for the program.

$$owned_{ipt} = \beta_1 daca\ in\ hh_i + \beta_2(daca\ in\ hh_i \times post_t) + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{B.1}$$

This specification (or a triple differences specification) is not the choice specification for this paper for two reasons. First, this formulation does not account for any cases where a DACA recipient purchases a home in their name but does not live in that home. DACA recipients, who are primarily young adults with family members (of various statuses) living in the U.S., may use their DACA status as an avenue to procure a home loan for family members (e.g. parents) who would otherwise be restricted to mortgages offered to individuals without social security numbers, which are more limited in their prevalence and may be prohibitively costly in their terms. As an example, a DACA recipient may leave her parents' rental housing at 18 to move into her own apartment. Her parents have incomes (and willingness to pay) sufficient to afford the terms of a home loan for which she is eligible. She takes out the mortgage but remains in her apartment. Her parents (and perhaps siblings) move into the home and reimburse her for the mortgage payments. If the home the young DACA recipient can afford is small, it may be especially likely that she ends up living elsewhere to avoid crowding.

Second, as shown in Figures B.18 - B.20, it is less clear that the parallel trends assumption holds in these specifications, making it difficult to claim that the effect size is not biased due to pre-trends. If the trends are not believed to be parallel, then the estimated effects should be treated as upper bounds, and it is impossible to determine whether their statistical significance would remain absent the trends.

Nonetheless, if the trends are assumed to be parallel, the interpretation of the estimated effects is similar to the interpretation of the effects found in section 2.4.1. The primary difference is that these estimates, while still "intent-to-treat" effects, are closer to the effect of "treatment on the treated."[8] The results are included in the table below. The first 3 columns replicate the results from section 2.4.1 for comparison.

[8]Only a small fraction of the undocumented population (the treated group in section 2.4.1) received DACA, but roughly half of the DACA-eligible population (the treated group here) did.

	owned	owned	owned	owned	owned	owned
undoc	-0.1154^{***}	-0.0242^{***}	-0.0293^{***}			
	(0.0050)	(0.0033)	(0.0034)			
undoc × post	0.0413^{***}	0.0065^{*}	0.0090^{***}			
	(0.0039)	(0.0035)	(0.0035)			
daca in hh				-0.1177^{***}	-0.0770^{***}	-0.0791^{***}
				(0.0075)	(0.0069)	(0.0068)
daca in hh × post				0.0326^{***}	0.0455^{***}	0.0490^{***}
				(0.0091)	(0.0087)	(0.0086)
years in U.S.		0.0141^{***}	0.0144^{***}		0.0130^{***}	0.0136^{***}
		(0.0007)	(0.0007)		(0.0007)	(0.0007)
years in U.S.^{2}		-0.0002^{***}	-0.0002^{***}		-0.0001^{***}	-0.0001^{***}
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
age		0.0163^{***}	0.0173^{***}		0.0084^{***}	0.0110^{***}
		(0.0005)	(0.0006)		(0.0008)	(0.0009)
age^2		-0.0001^{***}	-0.0001^{***}		-0.0000^{*}	-0.0001^{***}
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
log(income)		0.0413^{***}			0.0365^{***}	
		(0.0013)			(0.0013)	
never married		-0.1407^{***}	-0.1572^{***}		-0.1212^{***}	-0.1336^{***}
		(0.0034)	(0.0036)		(0.0034)	(0.0036)
female		0.0196^{***}	0.0101^{***}		0.0225^{***}	0.0126^{***}
		(0.0021)	(0.0020)		(0.0029)	(0.0028)
number workers		-0.0040^{**}	0.0240^{***}		-0.0207^{***}	0.0049^{***}
		(0.0016)	(0.0016)		(0.0019)	(0.0018)
number people		0.0203^{***}	0.0167^{***}		0.0291^{***}	0.0250^{***}
		(0.0022)	(0.0022)		(0.0024)	(0.0025)
number kids		-0.0192^{***}	-0.0174^{***}		-0.0207^{***}	-0.0181^{***}
		(0.0019)	(0.0019)		(0.0021)	(0.0021)
Controls	No	Yes	Yes*	No	Yes	Yes*
Adj. R^2	0.0949	0.2152	0.2037	0.0783	0.1895	0.1800
Num. obs.	468960	468960	468960	273768	273768	273768
N Clusters	1077	1077	1077	1077	1077	1077
Outcome Mean	0.3780	0.3780	0.3780	0.3469	0.3469	0.3469

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.11: Difference-in-differences regression results for the *owned* indicator. Columns 1-3 are identical to Table 2.4 and are provided for reference. Columns 4-6 are based on equation (B.1). In these regressions, the sample is restricted to undocumented households. As with the first 3 columns, columns 4-6 differ from each other only in their sets of controls. Column 4 includes no controls beyond CPUMA and year fixed effects. Column 5 includes the full set of controls as listed in section 2.3. Column 6 includes the same controls except that log(income) is omitted as income is likely a bad control. Robust standard errors clustered at the CPUMA level.

Regardless of choice of controls, all three specifications find significant positive effects of DACA for households in which at least one member is eligible. The estimated effects of a four percentage point increase in homeownership propensities is notably larger than the effects in choice specifications. One explanation for this is that the proportion of the sample affected by treatment is several times larger here, meaning the intent-to-treat to effects more closely approximate what the treatment-on-treated effects would be (if it were possible to determine which individuals in the sample actually took up DACA). In other words, the treated group in these specifications is less contaminated by untreated households, which would bias estimates towards zero. However, given the event studies presented in Figures B.18 - B.20, it may be that effects are (artificially) larger due to a positive bias that could arise as a result of the failure of the parallel trends assumption. So, while the unbiasedness of the estimates in the final three columns of Table B.11 is subject to one's interpretation of the event studies below, the fact that estimates are, at least, in line with expectations is somewhat reassuring (the bias would have to be exceptionally large to yield significant and negative effects that would contradict the findings from choice specifications).

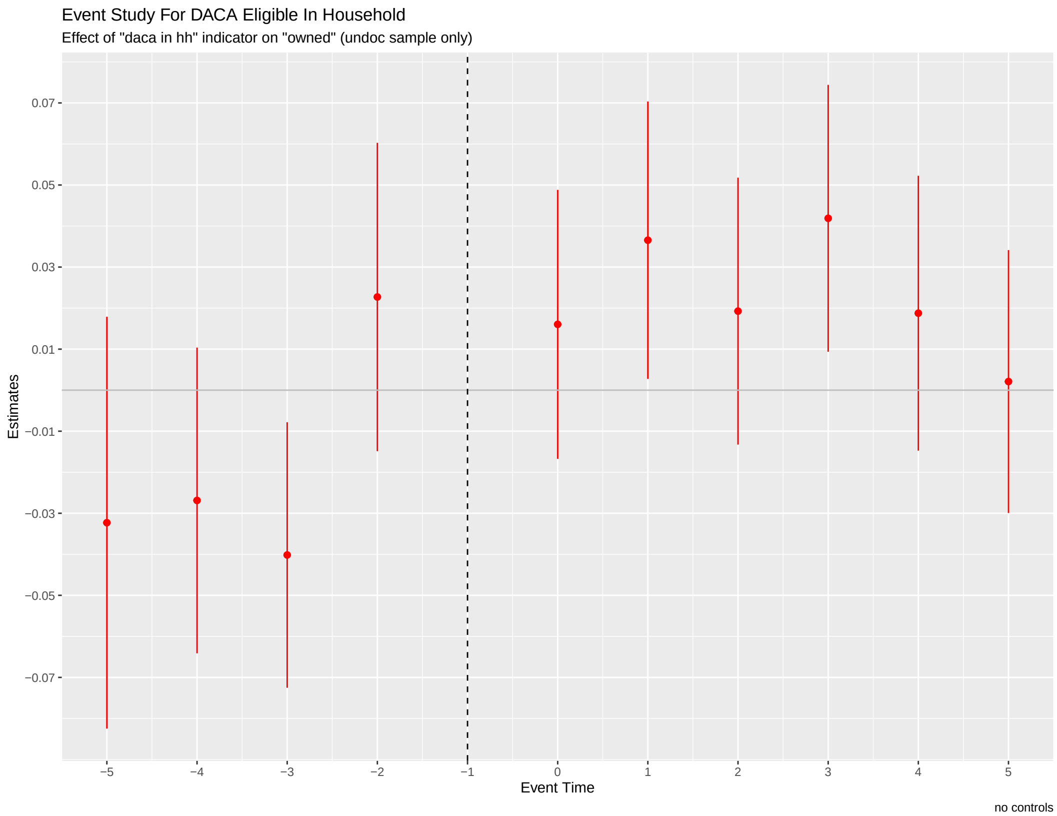

Figure B.18: Event study for the effect of having a DACA-eligible person living in the household (corresponds to the difference-in-differences results presented in column 4 of Table B.11)

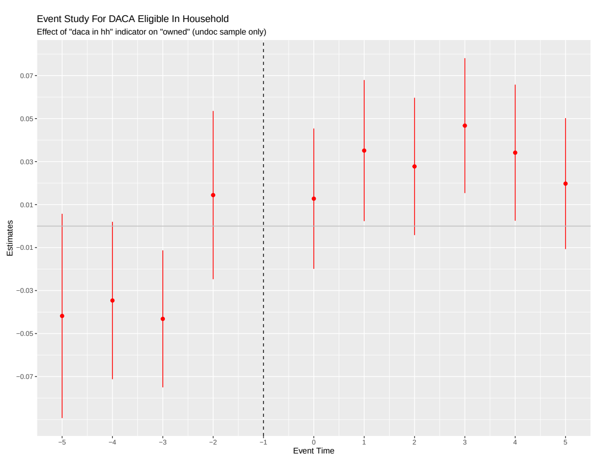

Figure B.19: Event study for the effect of having a DACA-eligible person living in the household (corresponds to the difference-in-differences results presented in column 5 of Table B.11)

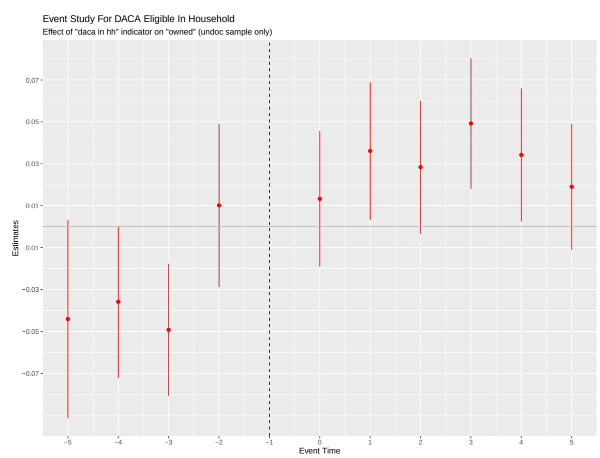

Figure B.20: Event study for the effect of having a DACA-eligible person living in the household (corresponds to the difference-in-differences results presented in column 6 of Table B.11)

B.4. Parallel Trends and Synthetic Control

Each difference-in-differences specification relies on the assumption of parallel trends. This section will assess each of the (in-text) county-level specifications in turn.[9] For each outcome, I present event study plots for transparency and to illustrate that, in most cases, there is little to no evidence of pre-trends that would bias the difference-in-differences estimates presented in the text. Then, because all of the analysis conducted at the county-level (sections 2.4 and 2.5) relies on a panel of the same counties observed over time (as opposed to the household-level analysis, which is cross-sectional where individuals are observed only once), it is possible to produce estimates based on a synthetic control design. In the cases where the parallel trends assumption is unlikely to hold, estimates from synthetic control may be interpreted as more credible. In most cases, where there is little evidence of pre-trends, synthetic control estimates should closely resemble the difference-in-differences estimates and are therefore, presented for completeness and as tests of robustness to an alternative empirical strategy.[10]

I present four different p-values for the estimates throughout this section. They are defined in Galiani and Quistorff (2016). Where presented, one-sided p-values are computed as defined in Galiani and Quistorff (2016) and Abadie (2021). For further details on the procedures used, see Appendix B.5.

[9]The trends assumptions for the analysis conducted at the household level have been assessed in other sections.

[10]Note that when there are two treatment categories (as in all of the county-level DACA analysis), synthetic control is run for the sample that excludes units in the "medium" category (i.e. synthetic control compares high DACA take-up units with the excluded category - low DACA take-up units).

B.4.1. Applications Outcome (DACA)

Refer to the estimates in Table 2.5. Figure 2.6 shows no evidence of pre-trends that may bias results, so synthetic control should produce estimates comparable to the difference-in-differences strategy. The synthetic control plots are presented below.[11]

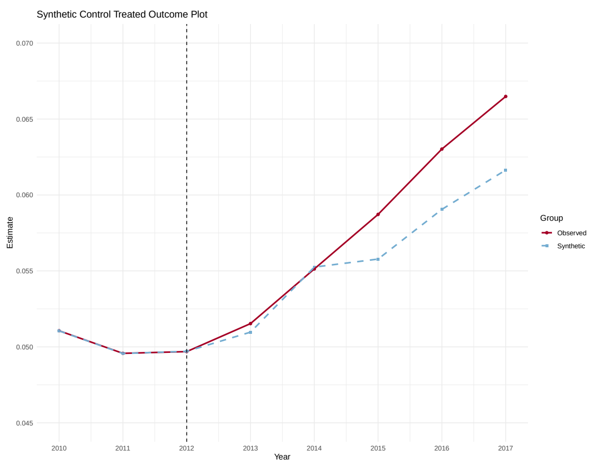

Figure B.21: Treated units

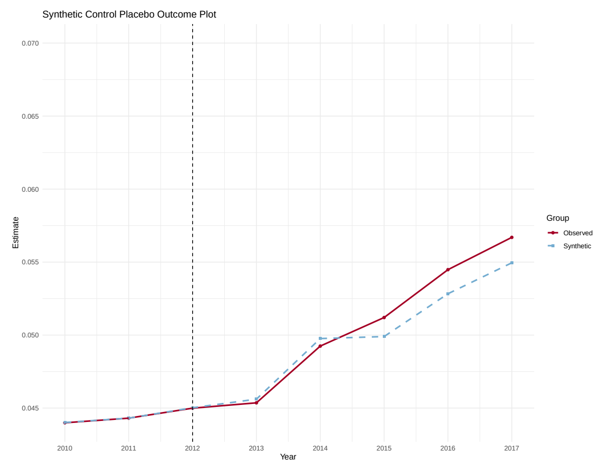

Figure B.22: Placebo units

Effect sizes and p-values are presented in the tables below.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0006	0.3233	0.0829		
2014	-0.0001	0.8909	0.4884		
2015	0.0029	0.0043	0.0325	0.8271	0.0002
2016	0.0040	0.0003	0.0061		
2017	0.0048	0.0000	0.0339		

Table B.12: unrestricted (pre-proportion = 1)

Note the surprisingly large p-value calculated using the post-period RMSPE. As noted

[11]Note that one unit in the placebo group that is exceedingly difficult to match (due to its large baseline values of the outcome) is dropped from the placebo set of counties before the following plots and tables are generated. The 8 periods in which the unit is observed hold the top 8 spots in terms of magnitude of error. Therefore, it is matched poorly in both the pre-period and post-period and adds little meaningful information. If this unit is included, the synthetic placebo trend does not match the observed placebo trend as well in either period. However, even when included, p-values (and, of course, effect sizes) are practically identical.

by Galiani and Quistorff (2016), this might occur when some placebo units cannot be matched well (i.e. their pre-period RMSPE and post-period RMSPE are both large). Thus, when only considering the post-period RMSPE, these units would appear to be highly affected (even though, in reality, their deviations from their synthetic counterpart in the post-period are not much different from their deviations from the synthetic counterpart in the pre-period). Galiani and Quistorff (2016) recommend scaling p-values by the pre-period RMSPE (e.g. columns 4 and 6) as a solution.[12] An indicator of poor fit is a statistic that is, effectively, a p-value for the pre-period (i.e. it is computed identically to how "p-value joint post" is computed except that, instead of comparing observed values to synthetic values in the post-period, observed values are compared to synthetic values in the pre-period over which the data is trained). I will refer to this as the "pre-proportion" (as it is the proportion of random placebo samples that generate a pre-period RMSPE larger than the treated average pre-period RMSPE). An extreme value (i.e. close to 0 or close to 1) is an indicator that the synthetic control procedure performed much better for one group (treated when close to 1, placebo when close to 0) than the other. Therefore, another remedy to this problem of poor fit in the placebo group, as suggested by Galiani and Quistorff (2016) and Abadie, Diamond and Hainmueller (2010), is to restrict the placebo set of units to those which have a pre-period RMSPE no more than m times the average treated pre-period RMSPE. If the large "p-value joint post" is merely an artifact of including placebo units that are generally matched poorly by the synthetic control procedure, then imposing such a restriction will reduce the p-value.[13] Therefore, in addition to tables where p-values are constructed absent any sample restrictions on the quality of pre-period fit, I will include a few tables where

[12]In other words, columns 4 and 6 are measurements of the size of deviations in the post-period(s) relative to the size of deviations in the pre-period. Columns 3 and 5 simply measure the size of deviations in the post-period(s), which is an adequate measure when the synthetic control procedure is able to produce trends that fit similarly well for both treated units and placebo units.

[13]This is based on the assumption that the units driving the large p-value vary largely in the post-period for the same reason they vary largely in the pre-period (poor fit). If the units driving the large p-value only match poorly in the post-period, this may be indicative of an actual "effect" or unaccounted for trend. Because the restriction applies only to units with poor pre-period fit, such units would (appropriately) remain in the sample even under this restriction.

p-values are re-computed under different restrictions (different values of m).[14]

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0006	0.2260	0.0958		
2014	-0.0001	0.9449	0.5054		
2015	0.0029	0.0003	0.0484	0.1110	0.0012
2016	0.0040	0.0000	0.0115		
2017	0.0048	0.0000	0.0523		

Table B.13: $m = 100$ restriction (pre-proportion = 0.96)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0006	0.2456	0.0996		
2014	-0.0001	0.9599	0.5094		
2015	0.0029	0.0002	0.0531	0.0740	0.0016
2016	0.0040	0.0000	0.0136		
2017	0.0048	0.0000	0.0582		

Table B.14: $m = 75$ restriction (pre-proportion = 0.35)

Consistent with findings from event studies and difference-in-differences estimates, synthetic control detects a positive effect that is greater (in magnitude and significance) after two years have passed (i.e. the "adjustment period"). Weighting each post-period year equally, the joint post-period estimated effect is a 0.244 percentage point increase in the relative number of Hispanic home loan applications, which is close to the unweighted difference-in-differences estimate of 0.33 percentage points.

[14] Arguably, the comparison is most "fair" when the pre-proportion is close to 0.5.

B.4.2. Approvals Outcome (DACA)

Refer to the estimates in Table 2.9. Presented below is the event study corresponding to column 4.

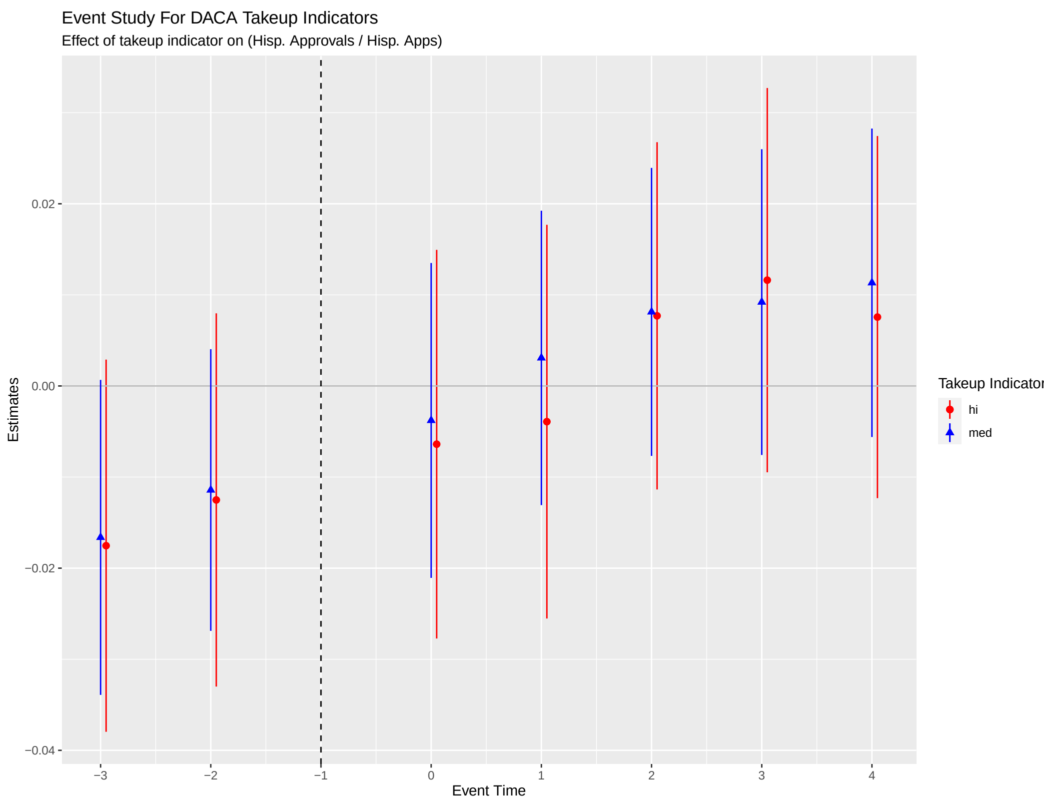

Figure B.23: Event study for Hispanic approval rate

Though pre-period estimates are not significantly below zero and the first two post-period estimates remain below zero, the points do appear to exhibit an upward trend, which would bias difference-in-differences estimates away from zero. If there is a meaningful pre-trend, one might find estimates from synthetic control to be more credible. While the resulting changes are not substantial, for the purpose of match accuracy, I impose that all counties must have at least 10 Hispanic home loan applications (the denominator of the outcome) in every year to be included in the sample. Prior to running synthetic control, any county with fewer than 10 Hispanic home loan applications in any year is dropped. The synthetic and observed trends are presented below.

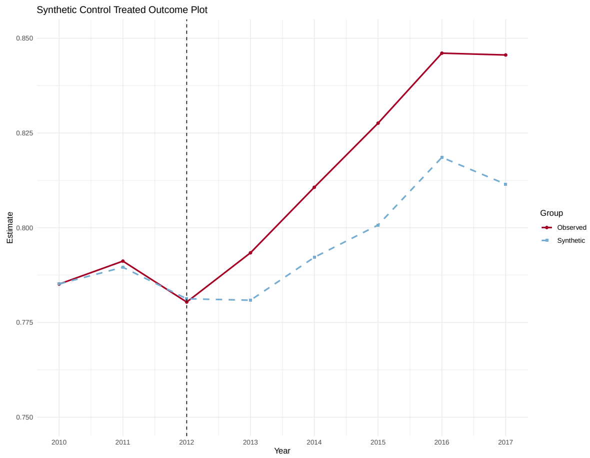

Figure B.24: Treated units

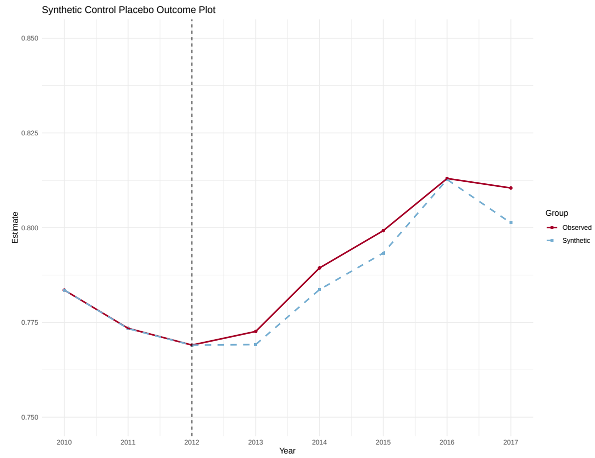

Figure B.25: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0125	0.0206	0.0005		
2014	0.0185	0.0006	0.1645		
2015	0.0269	0.0000	0.0000	1.0000	0.8336
2016	0.0275	0.0000	0.0000		
2017	0.0341	0.0000	0.0000		

Table B.15: unrestricted (pre-proportion = 0.88)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0125	0.0219	0.0006		
2014	0.0185	0.0003	0.1698		
2015	0.0269	0.0000	0.0000	1.0000	0.8509
2016	0.0275	0.0000	0.0000		
2017	0.0341	0.0000	0.0000		

Table B.16: $m = 50$ restriction (pre-proportion = 0.54)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0125	0.0212	0.0007		
2014	0.0185	0.0005	0.1841		
2015	0.0269	0.0000	0.0000	1.0000	0.8977
2016	0.0275	0.0000	0.0000		
2017	0.0341	0.0000	0.0000		

Table B.17: $m = 25$ restriction (pre-proportion $= 0.005$)

Even when restrictions are imposed to reduce the pre-proportion, the p-values based on post-period RMSPE are exceedingly large even though p-values for individual periods are more reasonable and even indicate significance in most cases. This is likely the result of over-fitting. The first two sets of p-values (columns 3 and 4) are derived from comparing average "effects" (observed value - synthetic value) in the treated group with average, randomly sampled placebo effects. Over-fitting would result in synthetic values very close to average observed values in the placebo group. However, the deviations of any single unit from the synthetic prediction may be wild (e.g. placebo unit A's estimate is far below the synthetic, but placebo unit B's estimate is far above the synthetic to compensate). Then, the calculated average in a given period will likely be close to the synthetic prediction, but because RMSPE is calculated using a sum of *squared* deviations, it may still be large in the case of over-fitting. In this case, one-sided inference may prove more informative. The two-sided testing so far has tested against the null that (placebo) values (mean differences between observed and synthetic values or post-period RMSPE) are at least as extreme as the average of the values in the treated group. In other words, in two-sided inference, a comparison is made between the absolute value of mean differences (or between post-period RMSPE values, which, by construction, are non-negative). Galiani and Quistorff (2016) provide a method for one-sided inference for the p-values presented in columns 3 and 4. Abadie (2021) provide a method for one-sided inference for the p-values in columns 5 and 6.[15] The following tables present the p-values from one-sided inference.

[15] Adapting the two-sided testing to one-sided is not difficult. For the p-values in columns 3 and 4, rather than comparing absolute values of mean differences, the direction of the difference is taken into account. For the others, when computing the sum of squared deviations for calculating the post-period RMSPE, accept only positive (or only negative) deviations, treating all other deviations as zero. In other words, disallowing the possibility of a negative effect, any observed difference that takes a value less than zero must be evidence of a zero effect.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0125	0.0211	0.0005			
2014	0.0185	0.0006	0.1643			
2015	0.0269	0.0000	0.0000	0.1101	0.0159	positive
2016	0.0275	0.0000	0.0000			
2017	0.0341	0.0000	0.0000			

Table B.18: unrestricted (pre-proportion = 0.88)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0125	0.0218	0.0005			
2014	0.0185	0.0003	0.1697			
2015	0.0269	0.0000	0.0000	0.0756	0.0183	positive
2016	0.0275	0.0000	0.0000			
2017	0.0341	0.0000	0.0000			

Table B.19: $m = 50$ restriction (pre-proportion = 0.54)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0125	0.0209	0.0007			
2014	0.0185	0.0005	0.1844			
2015	0.0269	0.0000	0.0000	0.0533	0.0283	positive
2016	0.0275	0.0000	0.0000			
2017	0.0341	0.0000	0.0000			

Table B.20: $m = 25$ restriction (pre-proportion = 0.004)

Estimates from the synthetic control empirical strategy support the in-text, difference-in-differences results. If anything, estimated effects are larger under synthetic control. The first two types of p-values both indicate statistical significance at (at least) the 95% confidence level in all periods but one. The p-values based on post-period RMSPE calculations are extremely high (indicating insignificance) under two-sided inference, but one-sided inference yields values that indicate statistical significance at the 95% confidence level when accounting for pre-period RMSPE (column 6) and at the 90% or 85% confidence level (depending on the restriction imposed) when pre-period RMSPE is not taken into account (column 5).

B.4.3. Loan Amount (Applications) Outcome (DACA)

Refer to the estimates in Table 2.10. Presented below is the event study corresponding to column 4.

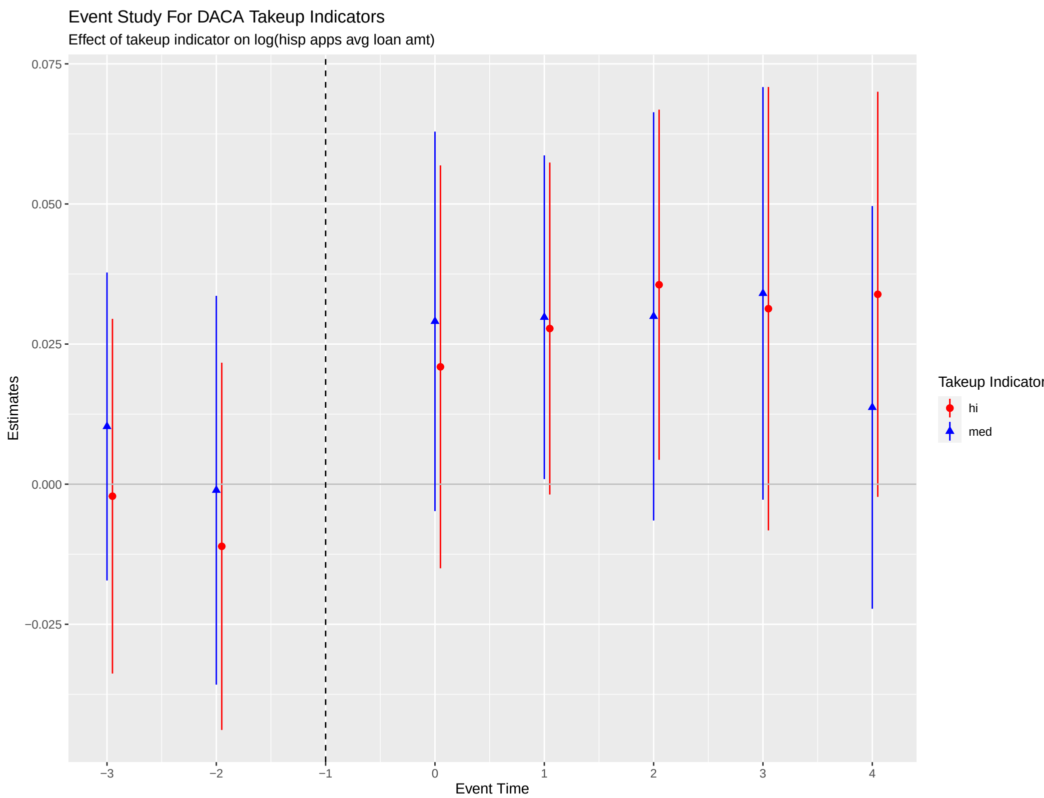

Figure B.26: Event study for log loan amount of Hispanic home loan applications

There is no noticeable pre-trend, so synthetic control estimates should be close to the difference-in-differences estimates in-text. Plots, estimated effects, and p-values are presented below.

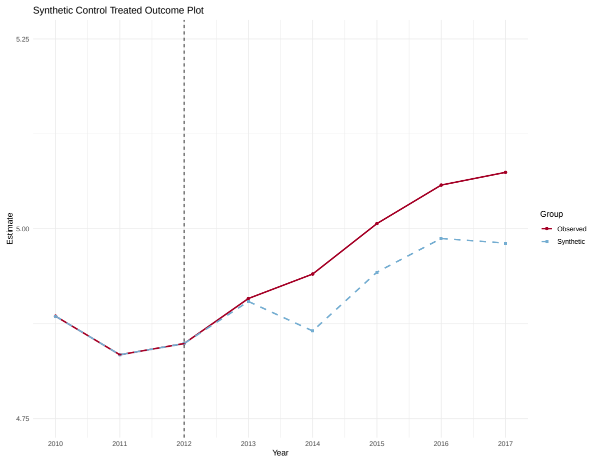

Figure B.27: Treated units

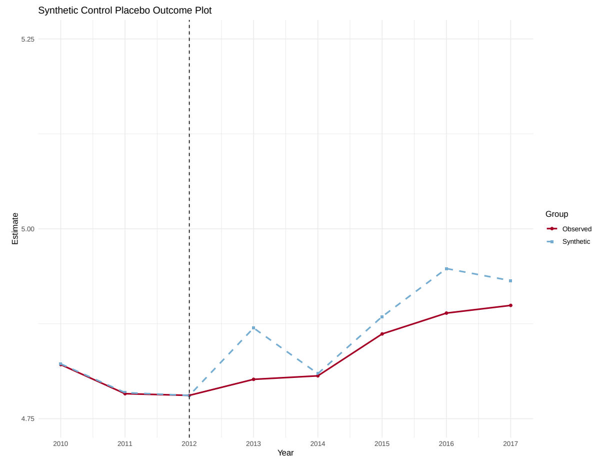

Figure B.28: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0039	1.0000	1.0000		
2014	0.0749	0.0000	0.0004		
2015	0.0640	0.0000	0.0117	1.0000	0.9999
2016	0.0703	0.1121	0.2585		
2017	0.0932	0.0000	0.0000		

Table B.21: unrestricted (pre-proportion = 1)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0039	1.0000	1.0000		
2014	0.0749	0.0000	0.0006		
2015	0.0640	0.0000	0.0172	1.0000	1.0000
2016	0.0703	0.0889	0.3314		
2017	0.0932	0.0000	0.0000		

Table B.22: $m = 50$ restriction (pre-proportion = 0.49)

As in the previous section, the synthetic control appears to suffer from an issue of over-fitting. As before, tables with p-values for one-sided inference are produced.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0039	0.0000	0.0000			
2014	0.0749	0.0000	0.0004			
2015	0.0640	0.0000	0.0000	0.0000	0.0056	positive
2016	0.0703	0.0000	0.0000			
2017	0.0932	0.0000	0.0000			

Table B.23: unrestricted (pre-proportion = 1)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0039	0.0000	0.0000			
2014	0.0749	0.0000	0.0006			
2015	0.0640	0.0000	0.0000	0.0000	0.0162	positive
2016	0.0703	0.0000	0.0000			
2017	0.0932	0.0000	0.0000			

Table B.24: $m = 50$ restriction (pre-proportion = 0.5)

Results are, again, in line with the results from the difference-in-differences specifications. If anything, estimated effects are larger under synthetic control. Under two-sided inference, the first two versions of p-values indicate statistical significance at the 99% confidence level in 2014 and 2017 and at the 95% confidence level in 2015. Under one-sided inference, all p-values under all restrictions except one indicate significance at the 99% confidence level (the exception indicates significance at the 95% level).

B.4.4. Loan Amount (Approvals) Outcome (DACA)

Refer to the estimates in Table 2.11. Presented below is the event study corresponding to column 4.

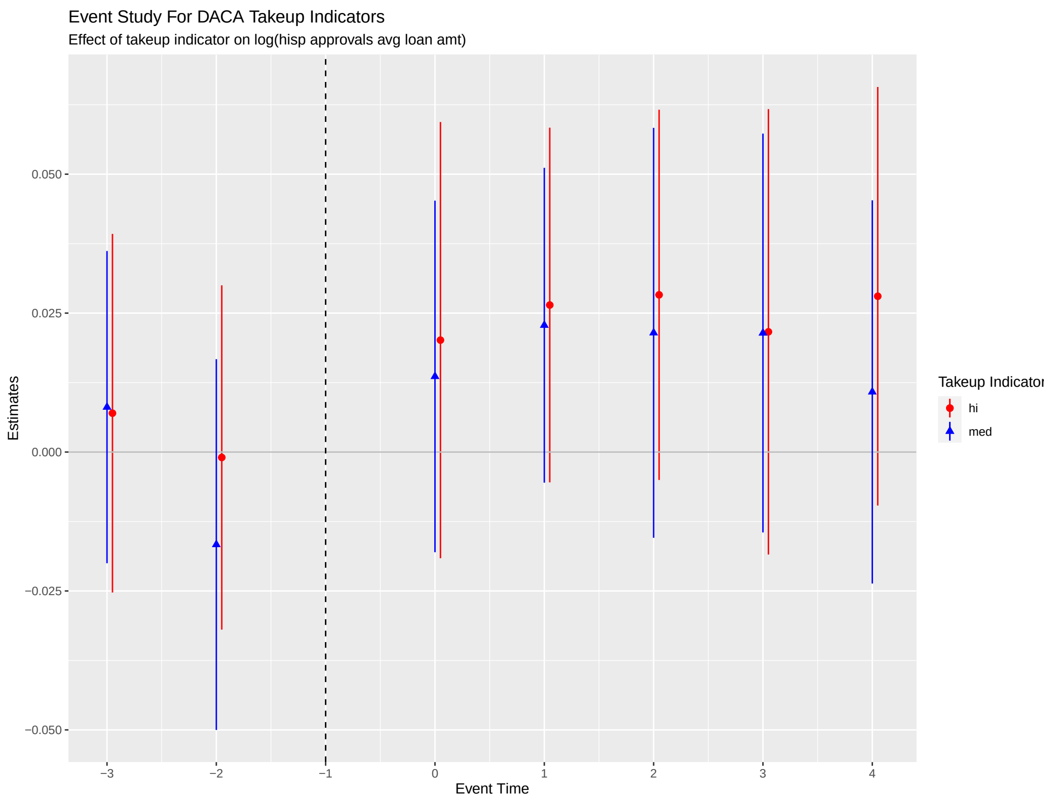

Figure B.29: Event study for log loan amount of Hispanic home loan applications that were approved

There is no noticeable pre-trend, so synthetic control estimates should be close to the difference-in-differences estimates in-text. Plots, estimated effects, and p-values are presented below.

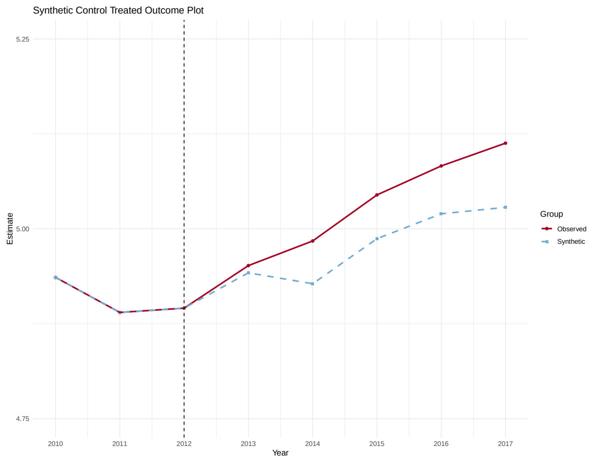

Figure B.30: Treated units

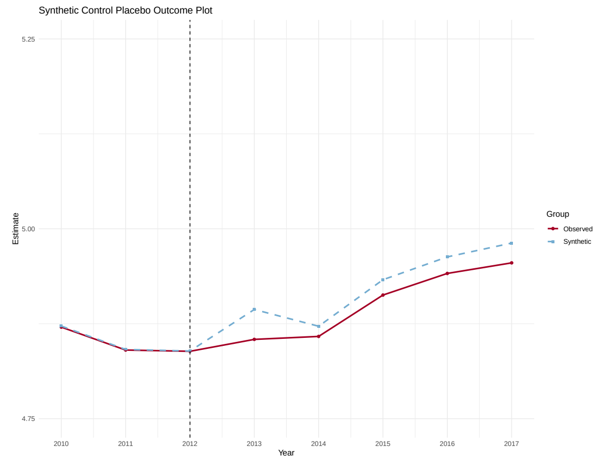

Figure B.31: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0095	0.9968	0.9989		
2014	0.0563	0.0000	0.0145		
2015	0.0577	0.0000	0.0011	1.0000	0.9999
2016	0.0630	0.0000	0.0000		
2017	0.0845	0.0000	0.0000		

Table B.25: unrestricted (pre-proportion = 1)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2013	0.0095	0.9981	0.9990		
2014	0.0563	0.0000	0.0212		
2015	0.0577	0.0000	0.0022	1.0000	1.0000
2016	0.0630	0.0000	0.0001		
2017	0.0845	0.0000	0.0000		

Table B.26: $m = 50$ restriction (pre-proportion = 0.06)

As in the previous two sections, the synthetic control appears to suffer from an issue of over-fitting. As before, tables with p-values for one-sided inference are produced.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0095	0.0000	0.0010			
2014	0.0563	0.0000	0.0000			
2015	0.0577	0.0000	0.0000	0.0001	0.0001	positive
2016	0.0630	0.0000	0.0000			
2017	0.0845	0.0000	0.0000			

Table B.27: unrestricted (pre-proportion = 1)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2013	0.0095	0.0000	0.0010			
2014	0.0563	0.0000	0.0000			
2015	0.0577	0.0000	0.0000	0.0000	0.0011	positive
2016	0.0630	0.0000	0.0000			
2017	0.0845	0.0000	0.0000			

Table B.28: $m = 50$ restriction (pre-proportion = 0.06)

Results are, again, in line with the results from the difference-in-differences specifications. If anything, estimated effects are larger under synthetic control. Under two-sided inference, the first two versions of p-values indicate statistical significance at the 99% confidence level in 2015, 2016, and 2017 and at the 95% confidence level in 2014. Under one-sided inference, all p-values under all restrictions indicate significance at the 99% confidence level.

B.4.5. Applications Outcome (Treasury)

Refer to the estimates in Table 2.12. The event study corresponding to column 1 is presented in Figure 2.8. The pre-trends suggest that difference-in-differences estimates are likely to be positively biased. Therefore, an effective synthetic control strategy that does not suffer such bias would be expected to yield smaller estimated effects. Plots, estimated effects, and p-values are presented below.

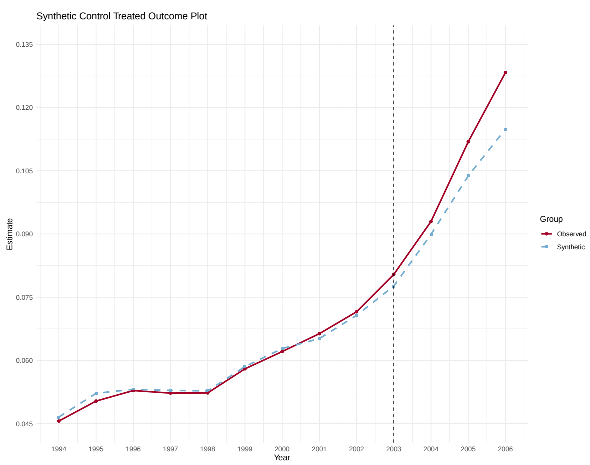

Figure B.32: Treated units

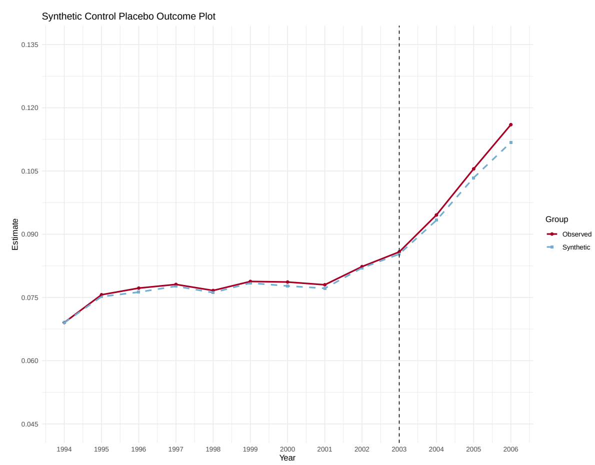

Figure B.33: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0031	0.0204	0.0004		
2005	0.0081	0.0000	0.0000	0.0000	0.0017
2006	0.0134	0.0000	0.0000		

Table B.29: unrestricted (pre-proportion = 0.58)

The effects are consistent with expectations. All estimates are positive and significant at (at least) the 95% confidence level, and consistent with the idea that difference-in-differences estimates are upwards biased due to trends, the synthetic control estimates are smaller in magnitude. Thus, the synthetic control estimated effect of a 0.95 percentage point effect on the Hispanic home loan application rate should be considered more accurate than the 1.34 percentage point change indicated by the (biased) difference-in-differences results.

B.4.6. Approvals Outcome (Treasury)

Refer to the estimates in Table 2.13. The event study corresponding to column 4 is presented below.

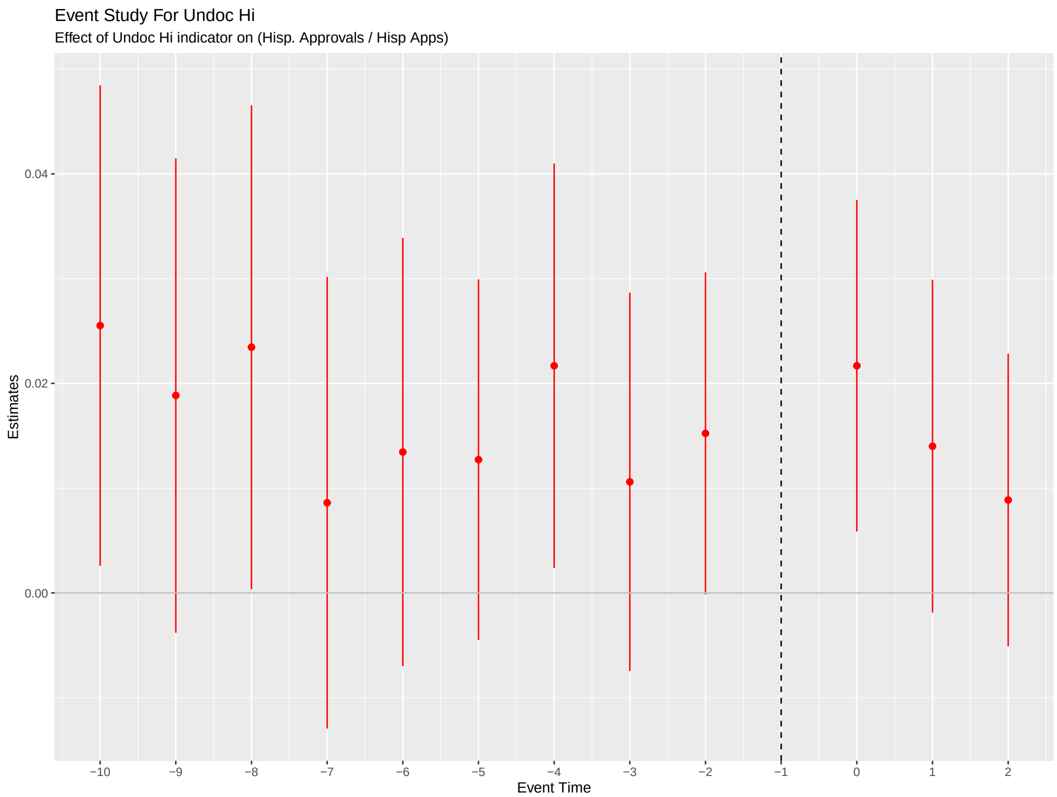

Figure B.34: Event study for log loan amount of Hispanic home loan applications

There are no apparent pre-trends, so we should expect synthetic control to yield estimates similar to those produced by the in-text difference-in-differences strategy. Plots, estimated effects, and p-values are presented below.

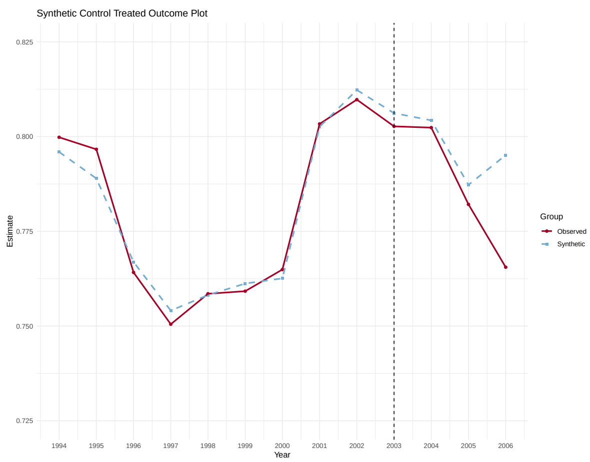

Figure B.35: Treated units

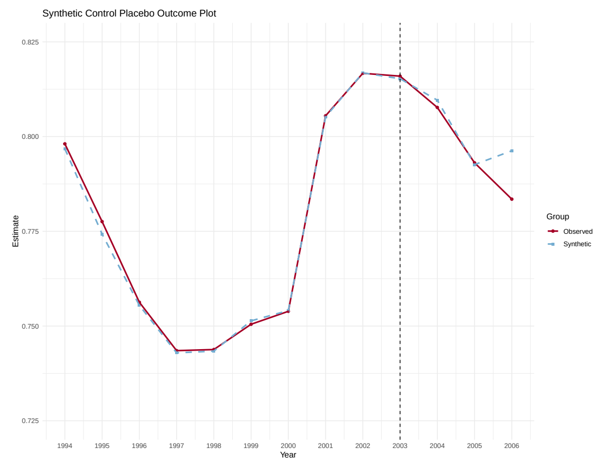

Figure B.36: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	-0.0019	0.6329	0.6797		
2005	-0.0052	0.2284	0.5428	0.5124	0.2997
2006	-0.0296	0.0001	0.1962		

Table B.30: unrestricted (pre-proportion = 0.99)

Because the pre-proportion value is near 1, I test to see if imposing a restriction on the placebo set meaningfully changes the p-values.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	-0.0019	0.6207	0.7193		
2005	-0.0052	0.3096	0.6070	0.4858	0.4012
2006	-0.0296	0.0005	0.2755		

Table B.31: $m = 4$ restriction (pre-proportion = 0.47)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	-0.0019	0.7202	0.8065		
2005	-0.0052	0.2595	0.7515	0.3044	0.6773
2006	-0.0296	0.0118	0.5260		

Table B.32: $m = 2$ restriction (pre-proportion = 0)

Results are similar regardless of pre-proportion value. Joint p-values are not extreme either,[16] suggesting over-fitting is not an issue in the way it was with the synthetic control for DACA's effect on some outcomes. Estimates are negative in all years and somewhat larger in magnitude than the difference-in-differences estimates. The first p-value (column 3) indicates that the effect in 2006 is statistically significant at conventional levels. However, once pre-period fit is accounted for, the significance is lost. The estimates are insignificant in all other periods, and p-values for the joint effect across all post-period years (columns 5 and 6) indicate statistical insignificance, as well. Thus, the results are consistent with the results from the difference-in-differences specifications where point estimates were negative but statistically insignificant.

[16] Additionally, p-values in columns 5 and 6 are consistent with those in columns 3 and 4.

B.4.7. Loan Amount (Applications) Outcome (Treasury)

Refer to the estimates in Table 2.14. The event study corresponding to column 4 is presented below.

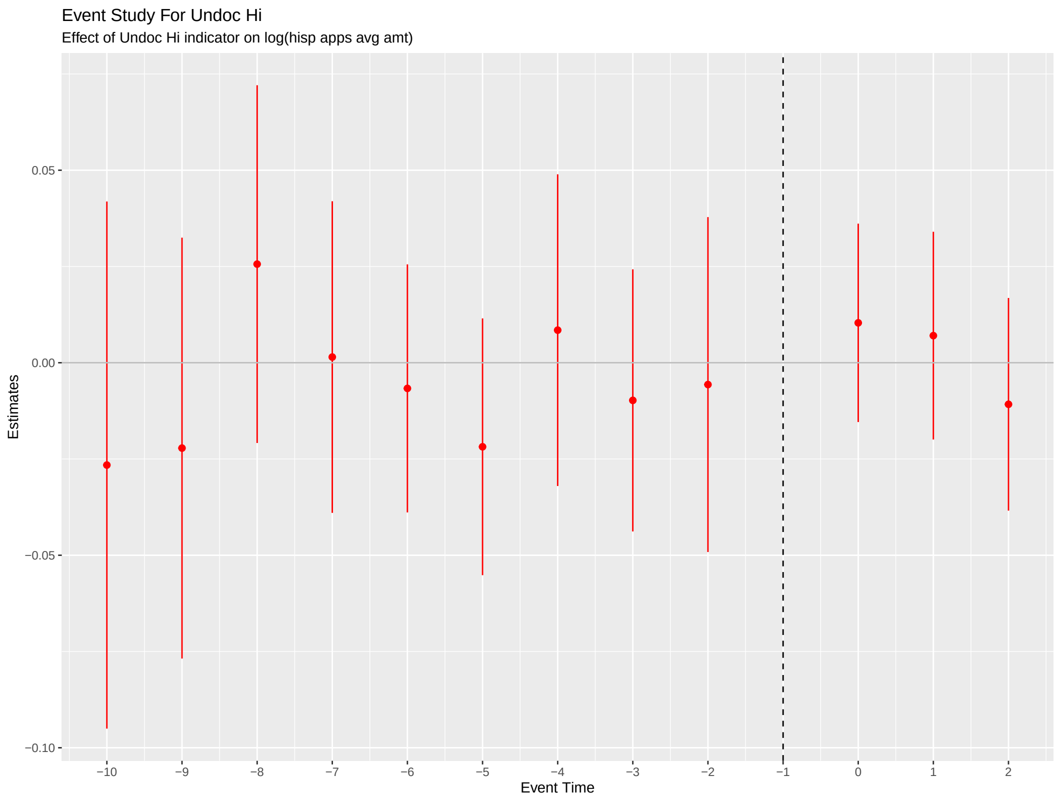

Figure B.37: Event study for log loan amount of Hispanic home loan applications

There are no apparent pre-trends, so we should expect synthetic control to yield estimates similar to those produced by the in-text difference-in-differences strategy. Plots, estimated effects, and p-values are presented below.

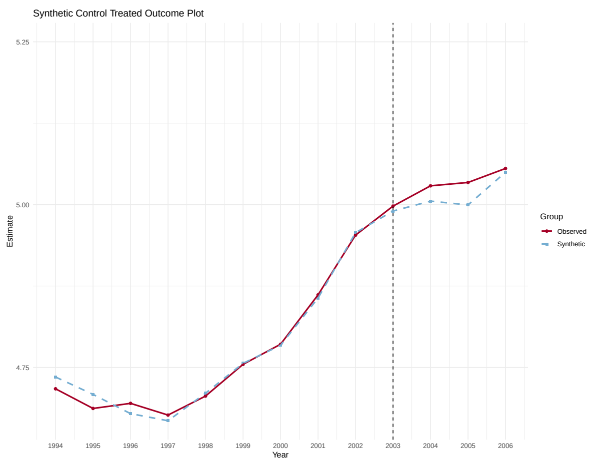

Figure B.38: Treated units

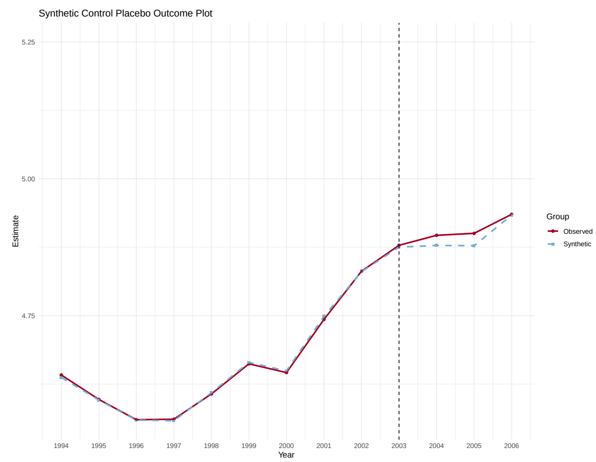

Figure B.39: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0235	0.3076	0.8325		
2005	0.0345	0.1369	0.9380	0.4043	0.8638
2006	0.0058	0.6389	0.6634		

Table B.33: unrestricted (pre-proportion = 0.998)

Because the pre-proportion value is near 1, I test to see if imposing a restriction on the placebo set meaningfully changes the p-values.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0235	0.3163	0.8376		
2005	0.0345	0.2096	0.9403	0.1526	0.8982
2006	0.0058	0.6437	0.6746		

Table B.34: $m = 10$ restriction (pre-proportion = 0.82)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0235	0.4494	0.8471		
2005	0.0345	0.5798	0.9433	0.0623	0.9426
2006	0.0058	0.7224	0.6946		

Table B.35: $m = 5$ restriction (pre-proportion = 0.02)

Since the "p-value joint post" indicates significance under certain restrictions but the other p-values do not similarly indicate statistical significance, it is worth checking whether this significance remains under one-sided inference (which, if there are real positive effects, should produce p-values that are indicative of significance at even greater levels of confidence).

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2004	0.0235	0.3077	0.8329			
2005	0.0345	0.1368	0.9051	0.2913	0.9880	positive
2006	0.0058	0.3631	0.9877			

Table B.36: unrestricted (pre-proportion = 0.998)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2004	0.0235	0.3172	0.8311			
2005	0.0345	0.2095	0.9074	0.1814	0.9920	positive
2006	0.0058	0.4068	0.9864			

Table B.37: $m = 10$ restriction (pre-proportion = 0.82)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2004	0.0235	0.4497	0.8264			
2005	0.0345	0.5805	0.9092	0.2967	0.9960	positive
2006	0.0058	0.6118	0.9847			

Table B.38: $m = 5$ restriction (pre-proportion = 0.02)

The absence of statistical significance when p-values for one-sided inference are considered is evidence that the marginal statistical significance detected under two-sided inference is the result of poor fit, not a true effect.[17]

The evidence from synthetic control is broadly consistent with the difference-in-differences results and the accompanying event study. Estimates are positive in direction, which is

[17] Contrast this with the one-sided testing for effects of DACA where switching to one-sided inference drastically increased statistical significance.

consistent with the comparable difference-in-differences specification (where California is included and weights are not applied), and nearly all p-values, including all p-values for one-sided inference, indicate that the estimated effects are statistically indistinguishable from zero.

B.4.8. Loan Amount (Approvals) Outcome (Treasury)

Refer to the estimates in Table 2.15. The event study corresponding to column 4 is presented below.

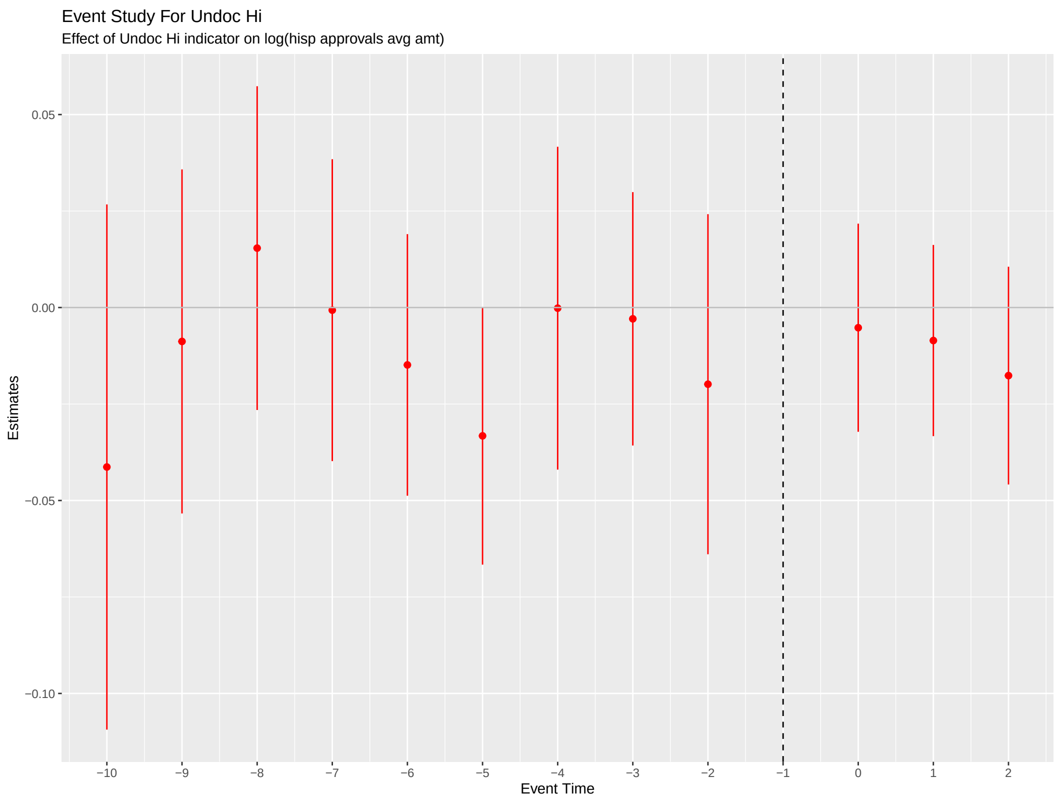

Figure B.40: Event study for log loan amount of Hispanic home loan approvals

There are no apparent pre-trends, so we should expect synthetic control to yield estimates similar to those produced by the in-text difference-in-differences strategy. Plots, estimated effects, and p-values are presented below.

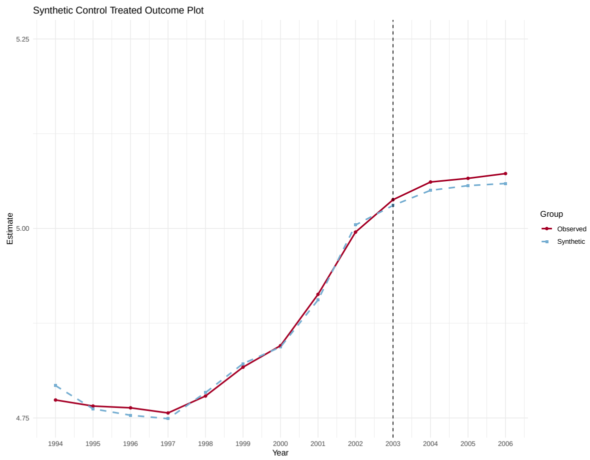

Figure B.41: Treated units

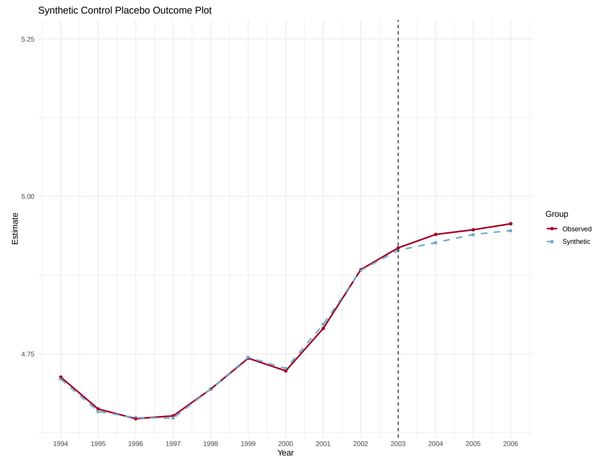

Figure B.42: Placebo units

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0110	0.5913	0.9816		
2005	0.0096	0.4876	0.9256	0.5230	0.9546
2006	0.0131	0.4729	0.7001		

Table B.39: unrestricted (pre-proportion = 0.99)

Because the pre-proportion value is near 1, I test to see if imposing a restriction on the placebo set meaningfully changes the p-values.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0110	0.5721	0.9816		
2005	0.0096	0.5306	0.9279	0.3424	0.9686
2006	0.0131	0.4894	0.7094		

Table B.40: $m = 10$ restriction (pre-proportion = 0.71)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled
2004	0.0110	0.5984	0.9831		
2005	0.0096	0.6148	0.9316	0.3047	0.9853
2006	0.0131	0.4784	0.7266		

Table B.41: $m = 5$ restriction (pre-proportion = 0.01)

Unlike the results for changes in average loan amounts among loan applications, changes in the size of approved loans are statistically insignificant even in the most restrictive case under two-sided inference. Thus, computing p-values under one-sided inference isn't as informative, but for completeness, I present them below, anyway.

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2004	0.0110	0.5758	0.9782			
2005	0.0096	0.4253	0.8994	0.5618	0.9750	positive
2006	0.0131	0.4334	0.6900			

Table B.42: unrestricted (pre-proportion = 0.99)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2004	0.0110	0.5588	0.9781			
2005	0.0096	0.4808	0.9010	0.4835	0.9813	positive
2006	0.0131	0.4548	0.6991			

Table B.43: $m = 10$ restriction (pre-proportion = 0.71)

Year	Effect	p-value	p-value scaled	p-value joint post	p-value joint post scaled	one-sided
2004	0.0110	0.5864	0.9777			
2005	0.0096	0.5855	0.9039	0.4855	0.9897	positive
2006	0.0131	0.4366	0.7147			

Table B.44: $m = 5$ restriction (pre-proportion = 0.01)

Estimates from synthetic control are again, somewhat larger in magnitude, but like the estimates from the difference-in-differences specifications, they are statistically indistinguishable from zero.

Thus, all synthetic control estimates are consistent with their corresponding difference-in-differences estimates when the parallel trends assumption appears to hold.

B.5. Details of Synthetic Control Procedures

Synthetic control is carried out using the synth package[18] in R (a subset of results were validated using the Stata version of the synth package). In all cases, predictors are the pre-period observations of the dependent variable. Choice of predictor weights is data-driven. Weights are chosen by an optimization algorithm that minimizes mean squared prediction error (MSPE) over all pre-treatment periods (the optimization algorithm used is the Broyden-Fletcher-Goldfarb-Shanno (BFGS) algorithm, which, in general, produced better synthetic trends than alternatives such as the Nelder-Mead, albeit at the cost of computation speed). Additional details about the optimization procedure are available upon request.

P-values are generated as suggested by Galiani and Quistorff (2016) in the case of multiple treated units. In all cases, the size of the full set of placebo averages exceeds 10 to the hundredth power. For this reason, as suggested by Galiani and Quistorff (2016), random samples of 1,000,000 are selected in the computations of all p-values.

[18] Abadie, Diamond and Hainmueller (2011)

B.6. Details on the Treasury Legal Clarification

A handful of media articles[19] have published that the rules implemented by the Treasury Department in 2003 allowed customers to set up bank accounts using ITIN's in place of Social Security numbers. Other sources[20] claim that (in or around) 2003 was when banks and credit unions first began offering mortgages to undocumented immigrants. These claims are *close* to the truth. In this section, I will elaborate on some of the relevant details that led to the massive spike in ITIN loans circa 2003.

In 2003, rules proposed by the PATRIOT Act's section on "Customer Identification Programs for banks, savings associations, credit unions, and certain non-federally regulated banks" were implemented. These new rules mark the first instance that Treasury Department policy formally listed the ITIN as an acceptable form of identification for the purpose of establishing bank accounts. Prior to the new rules, identifying information to be collected was regulated by the Bank Secrecy Act (BSA), which had not been updated since prior to 1996, when ITIN's were created. The BSA listed, more broadly, that institutions needed to secure a tax identification number as defined by IRS code 6109 of 1954. This IRS code states that it "shall determine what constitutes a taxpayer ID number..." However, the code is vague and only explicitly mentions Social Security numbers, employer identification numbers, or "an alternative identification number for purposes of identifying themselves." The BSA also stated that, for non-resident aliens, institutions also needed to retain a passport number or "a description of some other government document used to verify his identity."

This seems to leave room for institutions to justify offering ITIN loans if they are confident in their interpretation of existing Treasury rules. However, in 2002, the Treasury Department issued a statement that said, in part, "... because ITINs are issued without

[19]See, for instance, Khimm (2014) and Roosevelt (2017).
[20]See, for example, Jordan (2008).

rigorous verification, financial institutions must avoid relying on the ITIN to verify the identity of a foreign national." Thus, at best, the rules on establishing bank accounts using an ITIN were ambiguous. At worst, they barred the use of ITIN's as acceptable identification for the establishment of bank accounts.

The ambiguity of the rules made the issuance of ITIN loans rare prior to 2003, though there is record of some smaller institutions reportedly offering such loans as early as the late 90's. In 2003, the Treasury Rules in the PATRIOT Act rendered parts of the Bank Secrecy Act obsolete and explicitly listed ITIN's as acceptable forms of ID for establishing bank accounts. Beginning in 2004, there are reports of organizations and financial entities beginning to engage with ITIN loans on a large scale.[21] In 2004, Suspicious Activity Reports for borrowers with ITIN's spiked, which may be a result of institutions reacting to the new stringency of the PATRIOT Act rules, but an alternative explanation would be that there simply were not many borrowers using ITIN's prior to 2004, following the Treasury's legal clarification.

A publication by the Chicago Fed's Consumer and Community Affairs Division in 2005 (Gallagher, 2005) reported that, as of September of 2004, there were 18 banks and 1 credit union accepting ITIN's for mortgage underwriting, including TCF Bank and Fifth Third Bank. It is also reported that "[t]he regulatory community cites language in Section 326 of the PATRIOT Act in explaining" that an ITIN is an acceptable form of ID. Finally, and perhaps most importantly, in 2004, Citibank (one of "the big 4") started issuing ITIN mortgages.[22]

[21]For example, banks associated with the New Alliance Task Force, which is argued to have "pioneered" the creation of ITIN mortgage products for individuals lacking Social Security numbers in 2003, reportedly used alternative forms of ID to open more than 50,000 new accounts for Latin American Immigrants in 2004. In January of 2004, Mortgage Guaranty Insurance Corporation became the first company to insure ITIN loans. In April of 2004, the Wisconsin Housing and Economic Development Authority created the first governmental agency to promote the use of secondary markets for ITIN loans, but they would be shut down by the state government the following year.

[22]In late 2005, Wells Fargo also experimented with offering ITIN mortgages in LA and Orange counties in California.

In summary, there was some ITIN mortgage activity prior to 2003, but it appears to have been rare and legally ambiguous, at best. In 2003, through changes brought on by the PATRIOT Act, the Treasury Department amended the Bank Secrecy Act's rules to explicitly allow for the use of ITIN's as acceptable identification for the opening of bank accounts. An "explosion" of ITIN usage in banking followed, including Citigroup's decision to offer ITIN mortgages the next year.

B.7. Robustness to Recent Movers

To further validate the results from the household-level tenure analysis, I rerun the difference-in-differences regressions corresponding to equation (2.3), replacing the dependent variable with an indicator for whether the household head moved into their current residence within the last 4 years *and* that current residence is owner-occupied housing. This outcome variable is similar to the indicator for residing in owner-occupied housing but ensures that the estimated effect of DACA is being driven by recent movers (those who are likely to have moved since DACA). Results are presented in B.45 below. The first three columns are identical to Table 2.4 and are provided for comparison. Once controls are included, results from these new specifications are extremely similar and statistically indistinguishable from results from choice specifications.

	owned	owned	owned	moved in	moved in	moved in
undoc	-0.1154^{***}	-0.0242^{***}	-0.0293^{***}	-0.0193^{***}	-0.0135^{***}	-0.0160^{***}
	(0.0050)	(0.0033)	(0.0034)	(0.0026)	(0.0024)	(0.0025)
undoc × post	0.0413^{***}	0.0065^{*}	0.0090^{***}	0.0054^{**}	0.0082^{***}	0.0095^{***}
	(0.0039)	(0.0035)	(0.0035)	(0.0024)	(0.0023)	(0.0023)
years in U.S.		0.0141^{***}	0.0144^{***}		-0.0000	0.0001
		(0.0007)	(0.0007)		(0.0004)	(0.0004)
years in U.S.^{2}		-0.0002^{***}	-0.0002^{***}		-0.0001^{***}	-0.0001^{***}
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
age		0.0163^{***}	0.0173^{***}		0.0040^{***}	0.0045^{***}
		(0.0005)	(0.0006)		(0.0003)	(0.0004)
age^2		-0.0001^{***}	-0.0001^{***}		-0.0000^{***}	-0.0000^{***}
		(0.0000)	(0.0000)		(0.0000)	(0.0000)
log(income)		0.0413^{***}			0.0201^{***}	
		(0.0013)			(0.0008)	
never married		-0.1407^{***}	-0.1572^{***}		-0.0608^{***}	-0.0689^{***}
		(0.0034)	(0.0036)		(0.0022)	(0.0024)
female		0.0196^{***}	0.0101^{***}		-0.0002	-0.0048^{***}
		(0.0021)	(0.0020)		(0.0014)	(0.0013)
number workers		-0.0040^{**}	0.0240^{***}		-0.0053^{***}	0.0083^{***}
		(0.0016)	(0.0016)		(0.0009)	(0.0009)
number people		0.0203^{***}	0.0167^{***}		-0.0052^{***}	-0.0070^{***}
		(0.0022)	(0.0022)		(0.0008)	(0.0008)
number kids		-0.0192^{***}	-0.0174^{***}		0.0078^{***}	0.0087^{***}
		(0.0019)	(0.0019)		(0.0011)	(0.0011)
Adj. R^2	0.0949	0.2152	0.2037	0.0375	0.0555	0.0501
Num. obs.	468960	468960	468960	468960	468960	468960
N Clusters	1077	1077	1077	1077	1077	1077

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.45: Difference-in-differences regression results. Columns 1-3 are identical to Table 2.4 and are provided for reference. In columns 4-6, the outcome is replaced with an indicator for whether the household moved into their current owner-occupied housing sometime within the last 4 years. As with the first 3 columns, columns 4-6 differ from each other only in their sets of controls. Robust standard errors clustered at the CPUMA level.

B.8. The Indirect Effect of DACA through Income

The effect of DACA on homeownership is clearly positive, but the question of how much of this effect is driven by the policy's effect on incomes remains. The results from the analysis of the Treasury Department's rule change make it reasonable to believe that DACA's effect on homeownership propensities is partially driven by increased credit access among its recipients. However, because income is a "bad control" in the household-level difference-in-differences regressions, it is impossible to say with much certainty how much of the effect of DACA on homeownership is attributable to income changes. Under some assumptions, though, the effect attributable to non-income factors (e.g. credit access) can be bounded.

Running a regression for the effect of DACA on household income indicates that DACA increased household income by 6.26%.[23] See Table B.46. Using these results, the effect of DACA through non-income channels can be expressed as $0.0090 - 0.0626 \times c$ where c is the effect of log income on the probability of homeownership. Therefore, for increased income to be the *only* mechanism responsible for DACA's positive effect on homeownership, the effect of a 1% increase in income must be more than a 0.14 percentage point[24] increase in the probability of homeownership.[25] Estimates of the relationship between income and housing tenure vary tremendously depending on the sample and setting. The implied relationship based on the estimates in Coulson (1999), Gabriel and Rosenthal (2005), Hall and Greenman (2013), Haurin and Morrow-Jones (2006), and Linneman and Wachter (1989) are all fairly close to this threshold value, with some falling below and some above. Without an estimate derived from a sample and setting comparable to this study's, it is unclear what the precise,

[23]The implied effect is around \$3000, which is larger than Pope (2016)'s estimate (for individual income) of around \$1300.

[24]Based on the sample mean, this translates to a little under 0.4%.

[25]Stated another way, if household income increases 5%, the probability homeownership would have to rise by about 2%.

relevant elasticity (or value of c) is. However, it is insightful to know the conditions that must hold for DACA's effect on homeownership to operate solely through its impact on income, and with the estimates in Table B.46, the share of the impact attributable to non-income effects of the program can be obtained under an assumption about the relationship between income and homeownership.

	owned	owned	owned	log(income)
undoc	-0.1154^{***}	-0.0242^{***}	-0.0293^{***}	-0.1238^{***}
	(0.0050)	(0.0033)	(0.0034)	(0.0106)
undoc × post	0.0413^{***}	0.0065^{*}	0.0090^{***}	0.0626^{***}
	(0.0039)	(0.0035)	(0.0035)	(0.0098)
years in U.S.		0.0141^{***}	0.0144^{***}	0.0079^{***}
		(0.0007)	(0.0007)	(0.0014)
years in U.S.^{2}		-0.0002^{***}	-0.0002^{***}	-0.0000
		(0.0000)	(0.0000)	(0.0000)
age		0.0163^{***}	0.0173^{***}	0.0264^{***}
		(0.0005)	(0.0006)	(0.0022)
age^2		-0.0001^{***}	-0.0001^{***}	-0.0004^{***}
		(0.0000)	(0.0000)	(0.0000)
log(income)		0.0413^{***}		
		(0.0013)		
never married		-0.1407^{***}	-0.1572^{***}	-0.3982^{***}
		(0.0034)	(0.0036)	(0.0126)
female		0.0196^{***}	0.0101^{***}	-0.2292^{***}
		(0.0021)	(0.0020)	(0.0069)
number workers		-0.0040^{**}	0.0240^{***}	0.6771^{***}
		(0.0016)	(0.0016)	(0.0131)
number people		0.0203^{***}	0.0167^{***}	-0.0877^{***}
		(0.0022)	(0.0022)	(0.0077)
number kids		-0.0192^{***}	-0.0174^{***}	0.0436^{***}
		(0.0019)	(0.0019)	(0.0075)
Adj. R^2	0.0949	0.2152	0.2037	0.2805
Num. obs.	468960	468960	468960	468960
N Clusters	1077	1077	1077	1077

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table B.46: Difference-in-differences regression results. Columns 1-3 are identical to Table 2.4 and are provided for reference. The specification corresponding to column 4 is identical to the that of column 3 with the exception that the dependent variable is log income instead of the *owned* indicator.

Appendix C

Appendix to Publicly Provided and Private Health Insurance in Immigrant Populations

C.1. Trends in Observables

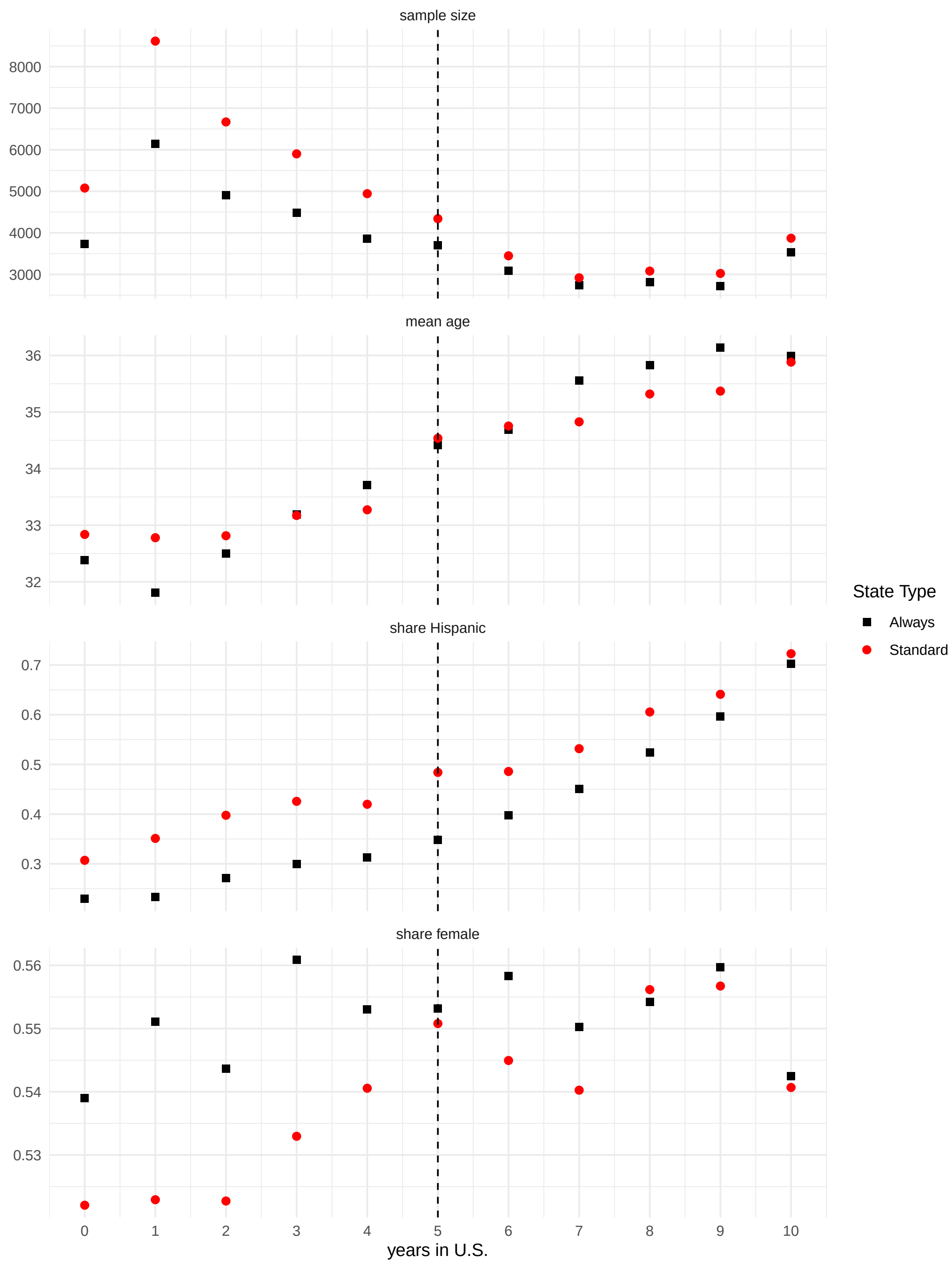

Figure C.1: Trends in sample size and means of various characteristics.

C.2. Validating the Fuzzy Diff-in-Disc Design

Suppose we want to estimate the causal effect of Medicaid receipt on take-up of private insurance.

Consider the sample of immigrants in "Standard" states. Begin with the following equation which surely suffers from omitted variable bias:[1]

$$private_i = \beta_0 + f(years\ in\ U.S._i) + \beta_1 medicaid_i + u_i \tag{C.1}$$

where

$$f(years\ in\ U.S._i) = \rho_0 + \alpha D_i + \rho_1 years\ in\ U.S._i$$

and

$$D_i = 1[years\ in\ U.S._i \geq 5]$$

In other words, f is a function capturing the relationship between $private_i$ and years in the U.S., and it is permitted to include a discontinuity at $years\ in\ U.S._i = 5$ where such a discontinuity is orthogonal to (or not already captured by) $medicaid_i$. Substitution yields:

$$private_i = \beta_0 + \rho_0 + \alpha D_i + \rho_1 years\ in\ U.S._i + \beta_1 medicaid_i + u_i \tag{C.2}$$

Now, let receipt of Medicaid be expressed as a function of years in the U.S.:

$$medicaid_i = \gamma_0 + \gamma_1 D_i + \gamma_2 years\ in\ U.S._i + e_i \tag{C.3}$$

[1] Note that throughout, functional forms are simplified to be linear and control variables, where omitted, are omitted without loss of generality. Changing functional forms to other well-behaved functions (such as higher order polynomials) or adding controls will not change the conclusions of this section.

Then, under more convenient circumstances, we could substitute equation (C.3) into equation (C.2), and if the necessary assumptions held, we would have the reduced form (and first stage) equation for the fuzzy regression discontinuity. However, we must first assess the assumptions for the validity of the design.

Assumptions:

(a) $\gamma_1 \neq 0$ (i.e. there is a first stage effect / the relevance assumption for instrumental variables)

(b) $cov(D_i, \alpha D_i + u_i) = 0$ (other assumption necessary for IV)

(c) $\alpha = 0$ (the sharp regression discontinuity assumption)

In this study, assumptions (b) and (c) fail to hold if there is an effect of eligibility to naturalize on receipt of private insurance (even if that effect only manifests through changes in sample composition from non-random attrition). Let this be "Violation 2."

Another assumption necessary for a causal interpretation of regression estimates is also violated in this case. In equation (C.3), γ_1 cannot be interpreted as the effect of Medicaid eligibility from crossing the 5-year residency threshold if $cov(D_i, e_i) \neq 0$. If eligibility to naturalize has an effect on Medicaid receipt and there is a discontinuity in eligibility to naturalize at the cutoff (i.e. the probability of being eligible to naturalize does not trend smoothly across the threshold), then $cov(D_i, e_i) \neq 0$ if γ_1 is to be interpreted as the effect of crossing the 5-year threshold attributable to the change in Medicaid eligibility. Let this be "Violation 1." The two violations are addressed in turn.

Violation 1:

Express

$$e_i = \delta D_i + \varepsilon_i \tag{C.4}$$

where $cov(D_i, \varepsilon_i) = 0$. Then, equation (C.3) becomes

$$medicaid_i = \gamma_0 + (\gamma_1 + \delta) D_i + \gamma_2 years\ in\ U.S._i + \varepsilon_i \tag{C.5}$$

Now, δ captures the effect of the discontinuity in eligibility to naturalize at the 5-year residency threshold. If all other unobservables trend smoothly across the threshold, γ_1 can be interpreted as the effect of the discontinuity in Medicaid eligibility at the 5-year residency threshold.

Now, let us introduce the sample of immigrants in "Always" states where eligibility for publicly provided health insurance *does not* change at the 5-year threshold. I will refer to this set of states as being "state A" and the set of "Standard" states as "state S." Thus, we have 2 equations for Medicaid (one for each state).

$$\begin{aligned} &medicaid_{i,s=S} = \gamma_0 + (\gamma_1 + \delta) D_i + \gamma_2 years\ in\ U.S._i + \varepsilon_i \\ &\text{and} \\ &medicaid_{i,s=A} = \gamma_0^{'} + (\gamma_1^{'} + \delta^{'}) D_i + \gamma_2^{'} years\ in\ U.S._i + \varepsilon_i^{'} \end{aligned} \tag{C.6}$$

If state A does not experience a change in insurance eligibility at the threshold (and the standard RD assumption that all other unobservables trend smoothly across the threshold holds), $\gamma_1^{'} = 0$.

Now, introduce Assumption (d).

(d) $\delta = \delta'$

In words, the effect of the discontinuity in eligibility to naturalize on Medicaid receipt must be the same in "Always" states as it is in "Standard" states. In light of Figure 3.1 and given that eligibility to naturalize is determined at the national level, not the state level, it seems reasonable to believe this assumption holds.

From (C.6), we have:

$$\begin{aligned} medicaid_{i,s=S} - medicaid_{i,s=A} &= (\gamma_0 - \gamma_0') + (\gamma_1 + \delta - \delta')D_i \\ &\quad + (\gamma_2 - \gamma_2')years\ in\ U.S._{\cdot i} + \varepsilon_i - \varepsilon_i' \end{aligned}$$

which, under (d), becomes

$$\begin{aligned} medicaid_{i,s=S} - medicaid_{i,s=A} &= (\gamma_0 - \gamma_0') + \gamma_1 D_i \\ &\quad + (\gamma_2 - \gamma_2')years\ in\ U.S._{\cdot i} + \varepsilon_i - \varepsilon_i' \end{aligned} \tag{C.7}$$

The first-stage regression specification is derived from the above.

$$\begin{aligned} medicaid_{is} = \phi_0 &+ \phi_1 D_i + \phi_2 years\ in\ U.S._{\cdot i} \\ &+ \phi_3(D_i \times Standard_s) + \phi_4(years\ in\ U.S._{\cdot i} \times Standard_s) \\ &+ \lambda_s + \eta_{is} \end{aligned} \tag{FS}$$

where $\hat{\phi_3} = \gamma_1$ and $\hat{\phi_1} = \delta = \delta'$

Violation 2:

Now that separate states have been introduced, we should update the functional form for the relationship between years in the U.S. and take-up of private insurance to allow for differences by state.[2] Suppose now that

$$
\begin{aligned}
f(years\ in\ U.S._i) = \rho_0 + \alpha D_i + \rho_1 years\ in\ U.S._i \\
+ \alpha_2 (D_i \times Standard_s) + \rho_2 (years\ in\ U.S._i \times Standard_s)
\end{aligned}
$$

Plugging the first-stage equation and the new equation for $f(years\ in\ U.S._{is})$ into (C.2) yields

$$
\begin{aligned}
private_{is} = \beta_0^{'} + \rho_1^{'} years\ in\ U.S._i + (\beta_1\phi_1 + \alpha) D_i \\
+ \rho_2^{'} (years\ in\ U.S._i \times Standard_s) \\
+ (\beta_1\phi_3 + \alpha_2)(D_i \times Standard_s) + \lambda_s^{'} + \nu_{is}
\end{aligned}
\tag{C.8}
$$

where $\beta_0^{'} = \beta_0 + \rho_0 + \beta_1\phi_0$,
$\rho_1^{'} = \rho_1 + \beta_1\phi_2$,
$\rho_2^{'} = \beta_1\phi_1 + \alpha$,
$\lambda_s^{'} = \beta_1\lambda_s$, and $\nu_{is} = \beta_1\eta_{is} + u_{is}$.

The assumptions for the validity of this differences-in-discontinuities design are now:

(a)′ $\gamma_1 \neq 0$ (approximated by $\hat{\phi}_3$)

(b)′ $cov((D_i \times Standard_s), \alpha_2(D_i \times Standard_s) + \nu_{is}) = 0$

(c)′ $\alpha_2 = 0$

(d)′ $\delta = \delta^{'}$ (approximated by $\hat{\phi}_1$)

[2] It is a stronger assumption to leave the equation unaltered.

In words:

(a)′ There exists a first-stage effect of the change in Medicaid eligibility from crossing the 5-year residency threshold. This is validated by the significant first-stage estimates in the tables in the text.

(b)′ Conditional on controls, the interaction term (which can be thought of as the "instrument" here) - crossing the 5-year threshold specifically in a "Standard" state - must only be correlated with take-up of private insurance through its correlation with Medicaid receipt. This holds when $\alpha_2 = 0$ (addressed next).

(c)′ The effect of years in the U.S. on take-up of private health insurance may be a discontinuous function at $years\ in\ U.S. = 5$ (i.e. $\alpha \neq 0$), but any discontinuity at that point not attributable to the change in Medicaid eligibility must apply to both states equally (namely, the discontinuity caused by the change in eligibility to naturalize must not be unique to the "Always" states or "Standard" states, alone).

(d)′ The effect of the discontinuous change in eligibility to naturalize on Medicaid must be the same across states (i.e. the effect of this discontinuous change is the same in "Always" states as "Standard" states).

If the above assumptions hold, then estimating a reduced-form equation based on (C.8) yields a reduced-form estimate of $\beta_1\phi_3$, and estimating the first-stage equation yields a first-stage estimate of ϕ_3. The ratio of these estimates yields an unbiased estimate of the effect of Medicaid take-up on private insurance take-up (β_1).