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.

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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 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,

[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.

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

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 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 provides background information about homeownership in the context of immigrant populations and the role of credit constraints in the

[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 (2021) examines changes in rents of likely undocumented households in response to sanctuary city policies.

[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 Online Appendix V.

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

market for owner-occupied housing. Section 3 presents descriptive estimates of the relationship between undocumented status and homeownership. In section 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 5 addresses the effects of the 2003 Treasury Department rule change using similar data and a comparable empirical strategy. Section 6 concludes.

2. Background

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 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

[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.

[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).

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]

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

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

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

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

[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.

even if it achieves the reduction only through its effects on individual attributes correlated 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.

[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.

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 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

[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

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.

3. Household-Level Data and Analysis

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]
- Is a veteran or currently serving in the U.S. military
- Received public health insurance, Medicaid, Medicare, or VA insurance

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.

[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 (2021).

[24]This assumption results in an overestimate of the undocumented population, leaving some legal residents to be assigned undocumented status. 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.

- 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 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 "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.

[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.

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 presents descriptive statistics by immigration status.

3.2. Descriptive Regressions

The primary outcome of interest is the indicator variable for whether the housing unit is owner-occupied. Specification 1 yields regression-adjusted estimates of the relationship between undocumented status and homeownership. Results from specification 1 are presented in Table 3.

$$owned_{ipt} = \beta_1 undoc_i + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{1}$$

$Owned_{ipt}$ 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

[31] In Online Appendix I, 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.

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 3, the results are qualitatively identical despite the difference in geographic fixed effects.

Estimates in columns 3 and 6 of Table 3 account for differences in observable characteristics of undocumented immigrants (e.g. the fact that undocumented immigrants tend to be younger and younger people are less likely to be homeowners) by including them as controls. The results in Table 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 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.

4. Evidence from DACA

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, 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).

$$owned_{ipt} = \beta_1 undoc_i + \beta_2(undoc_i \times post_t) + X_i\theta + \alpha_p + \gamma_t + \varepsilon_{ipt} \tag{2}$$

In specification 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

[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.

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 are presented in Table 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 to determine the specific channel through which DACA affected homeownership, the results in Table 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.

4.1.1. 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 1 plots mean (unadjusted) homeownership rates by immigration status over the sample period.

[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 4. If DACA raised household income, then household income is a bad control and contaminates the estimate of interest in column 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.

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 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 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 through 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 4.1 are likely to be upward biased. Figures 2 through 4 plot point estimates from event studies. The controls included in the regressions used to generate the event study plots in figures 2 through 4 are the same sets of controls indicated by columns 1 through 3, respectively, of Table 4.

The event study plots suggest that the parallel trends assumption is satisfied once controls are added (columns 2 and 3 of Table 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 4.2 as well.

4.2. County-Level Difference-in-differences

4.2.1. 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 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, 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)

[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.

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 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]

4.2.2. County-Level Specifications

The choice specification for this section is given by equation 3.

$$\begin{aligned} \frac{Hisp\ Apps}{Total\ Apps}_{ct} = \alpha_c + \gamma_t + \beta_1 \frac{Hisp\ Pop}{Total\ Pop}_{ct} + \beta_2(DACA\ Takeup\ MED_c \times post_t) \\ + \beta_3(DACA\ Takeup\ HI_c \times post_t) + \varepsilon_{ct} \end{aligned} \tag{3}$$

[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.

[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.

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 3 but used to define the independent variables 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 3 are presented in Table 5. The second column applies weights based on county population. The final column also drops California from the data.[43]

[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.

[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

Depending on the weighting and sample choice, the results imply a .33 to .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.

4.3. Robustness

4.3.1. 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 5 and 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 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 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 6. The first 3 columns repeat the regressions in Table 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

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 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.

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 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.

4.3.2. 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] 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 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 num-

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

[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.

ber 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 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 7. The coefficients on DACA Eligible HI × Post are positive but smaller in magnitude (and statistically insignificant in two of three columns) than the corresponding coefficients on DACA Takeup HI × Post, consistent with the story that it is the *take-up* of DACA that is responsible for the measured effect.

4.3.3. 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 3, using all counties in CBSA's[47] and

[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.

[47] A number of small counties in the U.S. do not belong to any CBSA. These are excluded. Note that

present results in Table 8. The first 3 columns are analogous to Table 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 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.

4.4. Related Outcomes

4.4.1. 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 4.2) will lead to an increase in homeownership rates (observed in section 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 3.

$$\begin{aligned} \frac{Hisp\ Approvals}{Hisp\ Apps}_{ct} &= \alpha_c + \gamma_t + \beta_1 \frac{Nonhisp\ Approvals}{Nonhisp\ Apps}_{ct} \\ &+ \beta_2 (DACA\ Takeup\ MED_c \times post_t) \\ &+ \beta_3 (DACA\ Takeup\ HI_c \times post_t) + \varepsilon_{ct} \end{aligned} \tag{4}$$

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 9. The first 3 columns are identical to Table 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

Alaska and the other counties mentioned in footnote 39 are still excluded.

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 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 9 offers more support for the hypothesis that DACA increased homeownership rates and at least some of its effect is attributable to its impact on the incomes of undocumented immigrants.

4.4.2. 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

[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.

[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.

loan amount for each county-year. These averages are broken out by ethnicity and loan decision (e.g. approved v.s. denied). I then run regressions using the following specification:

$$\begin{aligned} log(Hisp\ Loan\ Amt)_{ct} = \alpha_c + \gamma_t &+ \beta_1 log(Nonhisp\ Loan\ Amt)_{ct} \\ &+ \beta_2(DACA\ Takeup\ MED_c \times post_t) \\ &+ \beta_3(DACA\ Takeup\ HI_c \times post_t) + \varepsilon_{ct} \end{aligned} \tag{5}$$

Results are presented in Tables 10 and 11. In Table 10, $log(Hisp\ Loan\ Amt)$ refers to the average loan amount among Hispanic home loan *applications*. In Table 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.

5. Treasury Department's New Rule (Effects of 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.

5.1. 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 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

[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 Online Appendix V.

Research[52] in combination with estimates from the imputation procedure described in section 3.1. Specifically, if it is assumed that county undocumented populations within a state grow at the same rate as the state undocumented population, then the following equality holds:

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

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.

5.2. Specifications and Results

5.2.1. Difference-in-differences

Specifications resemble those of section 4.2, but here, $post_t$ refers to post-2003 (i.e. $post_t = 1$ if year ≥ 2004). The choice specification is equation 6.

$$\frac{Hisp\ Apps}{Total\ Apps}_{ct} = \alpha_c + \gamma_t + \beta_1 \frac{Hisp\ Pop}{Total\ Pop}_{ct} + \beta_2 (Undoc\ HI_c \times post_t) + \varepsilon_{ct} \tag{6}$$

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

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

[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.

equation 5, but Table 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 7 and 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 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 12 should be considered upper bounds on the true effect of the ruling change.

5.2.2. 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 A,[56] specifically Appendix A.5. For completeness and as tests of the identifying assumptions of the difference-in-differences designs, Appendix A 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

[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 A, 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 Online Appendix III.

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.

5.3. Results for Loan Approval Rates

Section 4.4.1 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 4.4.1, I run regressions like those in equation 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 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 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 13), more applications (as indicated by columns 1 through 3 of Table 13) will still result in higher homeownership rates.

5.4. 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 4.4.2 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.

[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.

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 5.

$$\begin{aligned} log(Hisp\ Loan\ Amt)_{ct} = {} & \alpha_c + \gamma_t + \beta_1 log(Nonhisp\ Loan\ Amt)_{ct} \\ & + \beta_2(Undoc\ HI_c \times post_t) + \varepsilon_{ct} \end{aligned} \tag{7}$$

Results are presented in Tables 14 and 15. As before, in Table 14, $log(Hisp\ Loan\ Amt)$ refers to the average loan amount among Hispanic home loan *applications*. In Table 15, $log(Hisp\ Loan\ Amt)$ 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

[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."

[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).

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 5.2 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.

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.

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

Table 1: 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.

	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: 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

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

Table 3: Linear probability models for housing tenure (owned = 1). 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.

	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)
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

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

Table 4: Linear probability models for housing tenure (owned = 1). Robust standard errors clustered at the smallest possible geographic unit for the sample. All regressions use household weights provided by the ACS.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$
$\frac{Hisp.\ Pop}{Total\ Pop}$	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

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

Table 5: Choice specifications for county-level DACA analysis, corresponding to equation 3. Outcome is $\frac{Hisp.\ Apps}{TotalApps}$. 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.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$
$\frac{Hisp.\ Pop}{Total\ Pop}$	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

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

Table 6: First set of robustness tests for county-level DACA analysis. First 3 columns replicate Table 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.

	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

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

Table 7: Second set of robustness tests for county-level DACA analysis. Each pair of columns corresponds to a column in Table 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.

	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

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

Table 8: Third set of robustness tests for county-level DACA analysis. The first 3 columns are identical to Table 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.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$
$\frac{HispPop}{TotalPop}$	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

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

Table 9: Effect on relative number of Hispanic applications and on Hispanic approval rate. Columns 1 through 3 replicate Table 5 and are provided for reference.

	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	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

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

Table 10: Effect on relative number of Hispanic applications and on log average Hispanic applications loan amount. Columns 1 through 3 replicate Table 5 and are provided for reference.

	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	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

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

Table 11: Effect on Hispanic approval rate and on log average Hispanic approved loan amount. Columns 1 through 3 replicate the second half of Table 9 and are provided for reference.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$
$\frac{HispPop}{TotalPop}$	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

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

Table 12: Results from the Treasury ruling analysis. The first 3 columns correspond to equation 4. 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.

	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Apps}{Total\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$	$\frac{Hisp.\ Approvals}{Hisp.\ Apps}$
$\frac{HispPop}{TotalPop}$	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

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

Table 13: Effect on relative number of Hispanic applications and on Hispanic approval rate. Columns 1 through 3 replicate the first half of Table 12 and are provided for reference.

	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	$\frac{HispApps}{TotalApps}$	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

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

Table 14: Effect on relative number of Hispanic applications and on log average Hispanic applications loan amount. Columns 1 through 3 replicate the first half of Table 12 and are provided for reference.

	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	$\frac{HispApprovals}{HispApps}$	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

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

Table 15: Effect on Hispanic approval rate and on log average Hispanic approved loan amount. Columns 1 through 3 replicate the second half of Table 13 and are provided for reference.

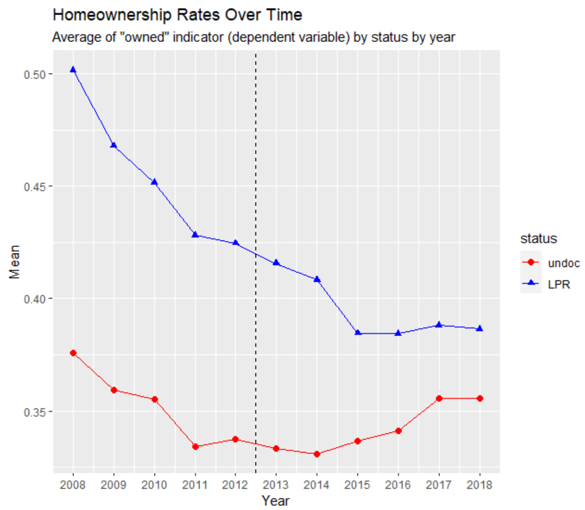

Figure 1: Homeownership rate by immigration status over time.

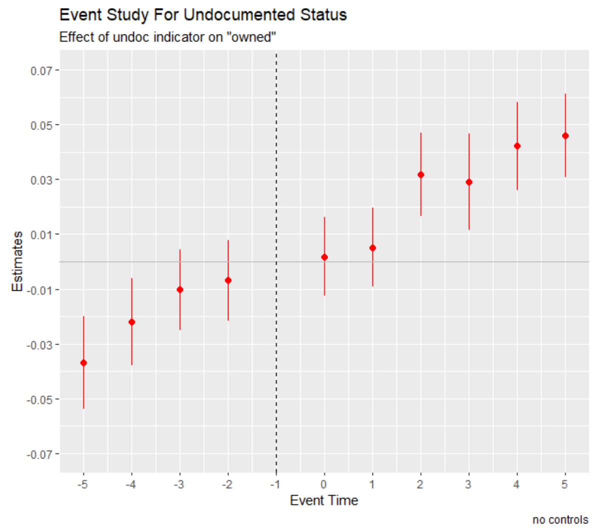

Figure 2: Event study corresponding to column 1 of Table 4.

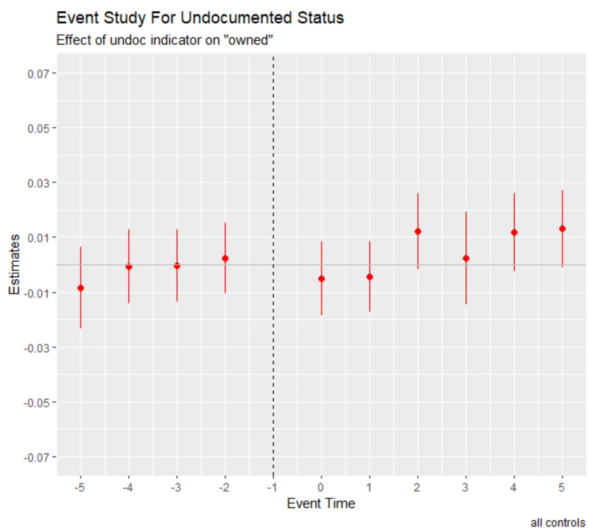

Figure 3: Event study corresponding to column 2 of Table 4.

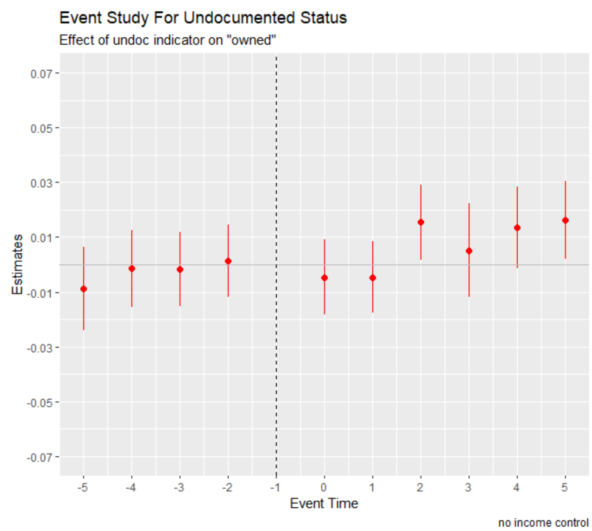

Figure 4: Event study corresponding to column 3 of Table 4.

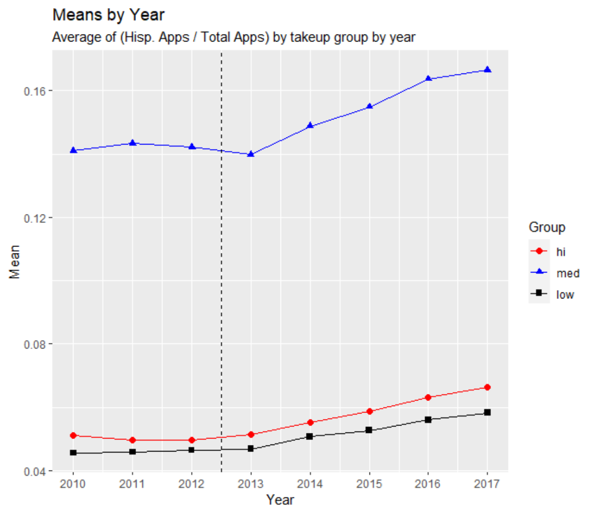

Figure 5: Average of county Hispanic home loan application rates by DACA takeup group over time.

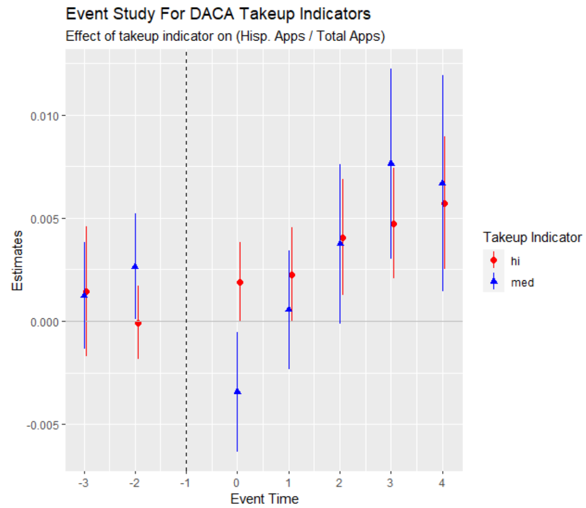

Figure 6: Event study corresponding to column 1 of Table 5.

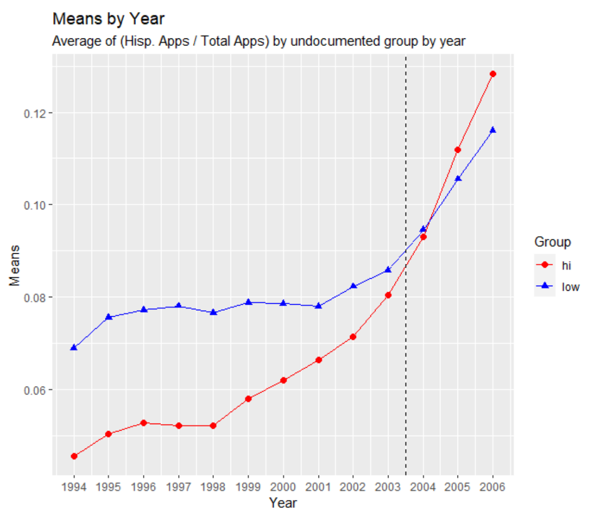

Figure 7: Average of county Hispanic home loan application rates by undocumented group over time.

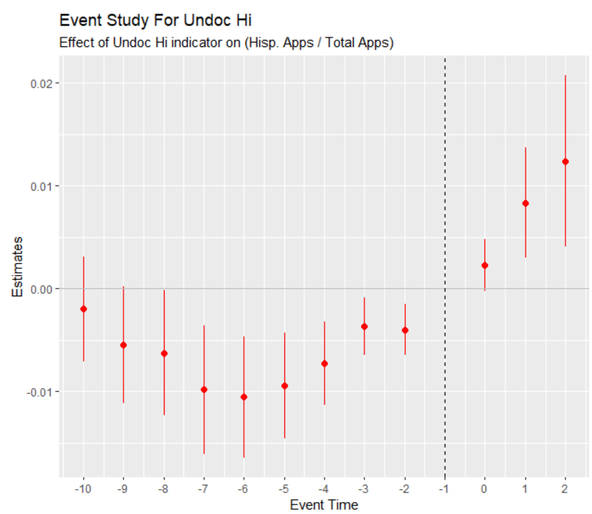

Figure 8: Event study corresponding to column 1 of Table 12.

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Appendix

Appendix A. Parallel Trends and Synthetic Control Summary of Results

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.[60] 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 4 and 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.[61]

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 Online Appendix III, I extend the analysis to include two additional p-values that test the joint significance 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 Online Appendix III as the analysis in this section is repeated there. Note that the conclusions are consistent across sections.[62]

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

[61]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).

[62]The primary difference in Online Appendix III 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-

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 4.2.2, 4.4.1, and 4.4.2). The remaining four subsections present results for the Treasury rule change's effects on the same outcomes (sections 5.2.1, 5.3, and 5.4). 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[63]
3. Include a table that shows estimated effects and corresponding p-values by year

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.[64] 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.

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).

[63] 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.

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

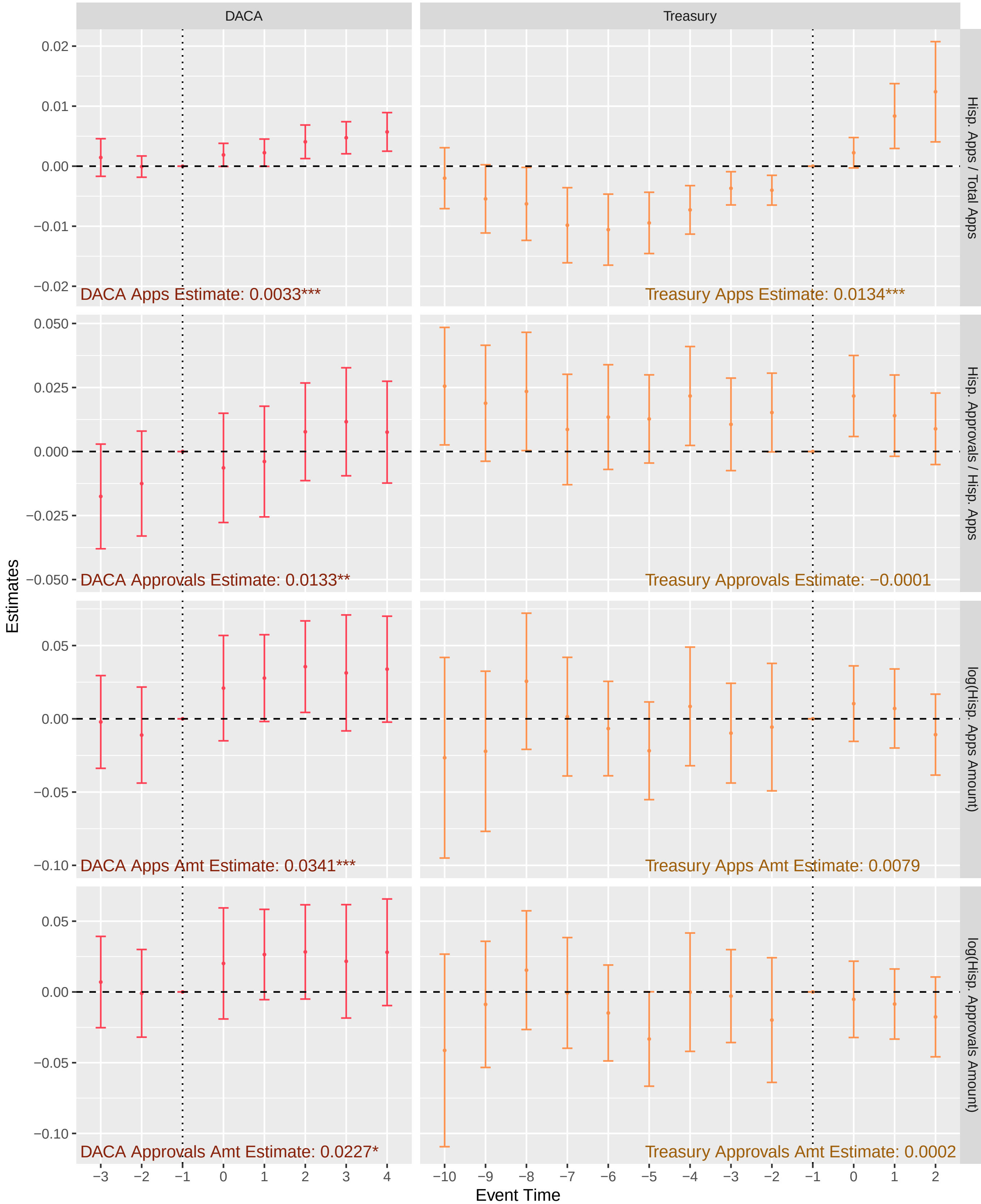

Figure A.0.1: Event studies for each policy's effect on each of the 4 main county-level outcomes of interest.

Appendix A.1. Applications Outcome (DACA)

Refer to the estimates in Table 5 and the event study in Figure 6.

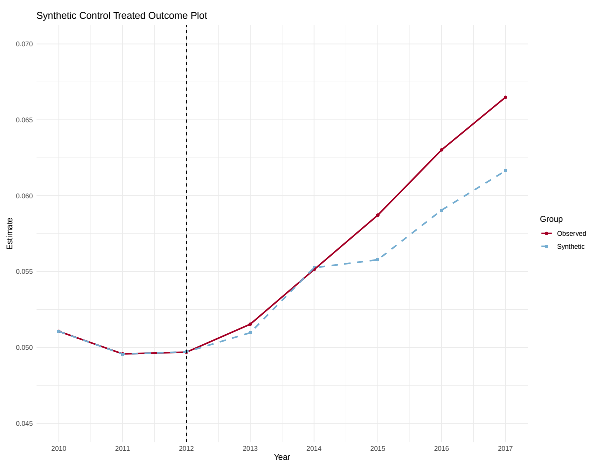

Figure A.1.1: Treated units

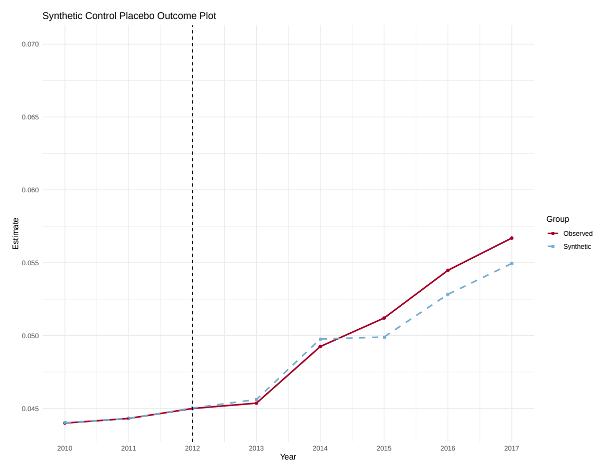

Figure A.1.2: Placebo units

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 A.1.1: 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.

Appendix A.2. Approvals Outcome (DACA)

Refer to the estimates in Table 9.

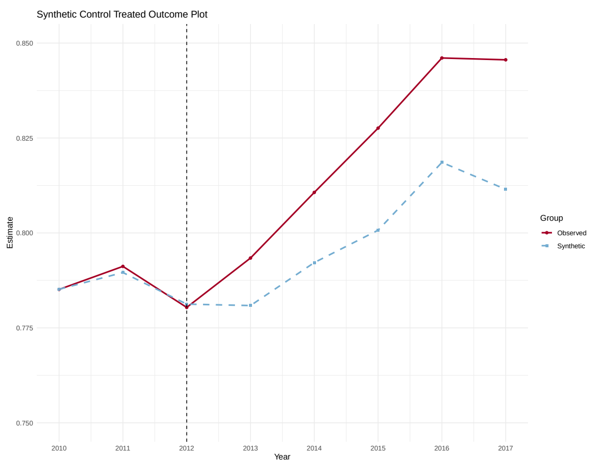

Figure A.2.1: Treated units

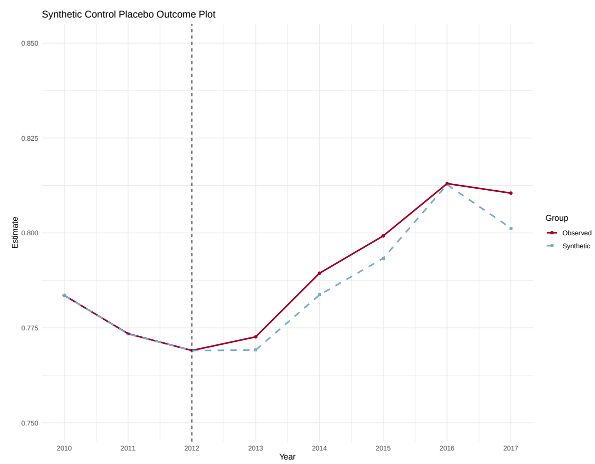

Figure A.2.2: Placebo units

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 A.2.1: 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.

Appendix A.3. Loan Amount (Applications) Outcome (DACA)

Refer to the estimates in Table 10.

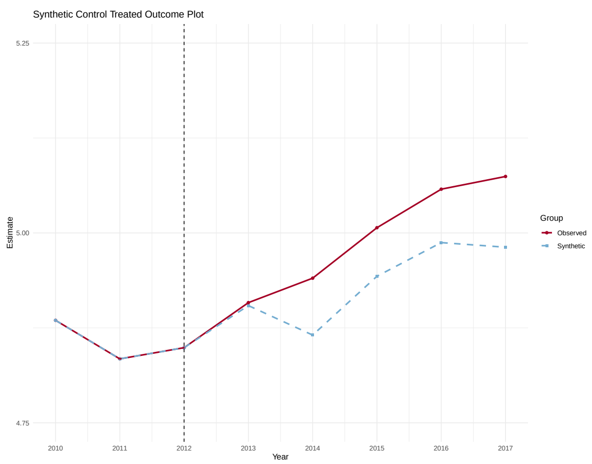

Figure A.3.1: Treated units

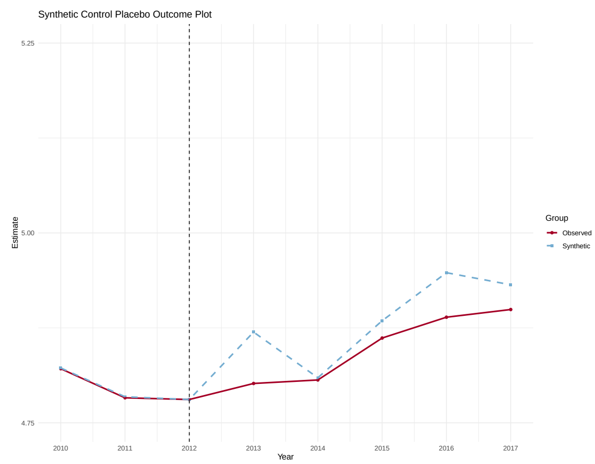

Figure A.3.2: Placebo units

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 A.3.1: 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.

Appendix A.4. Loan Amount (Approvals) Outcome (DACA)

Refer to the estimates in Table 11.

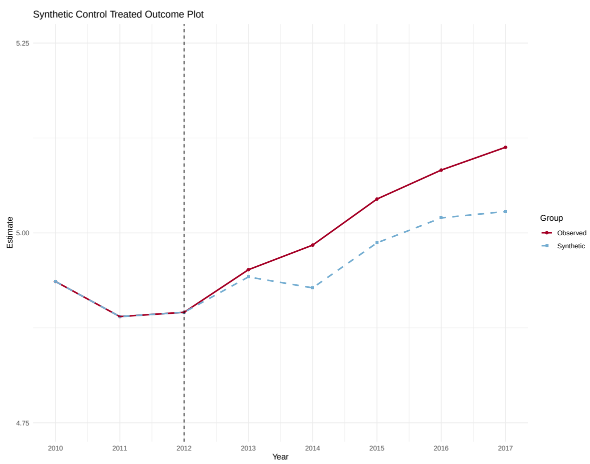

Figure A.4.1: Treated units

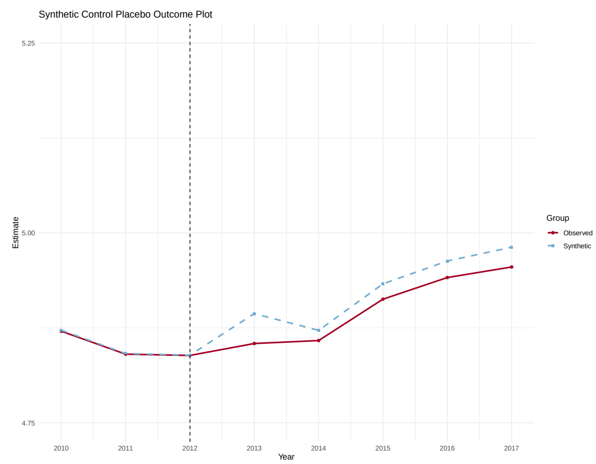

Figure A.4.2: Placebo units

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 A.4.1: 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.

Appendix A.5. Applications Outcome (Treasury)

Refer to the estimates in Table 12. The event study corresponding to column 1 is presented in Figure 8 (or in Figure A.0.1 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.

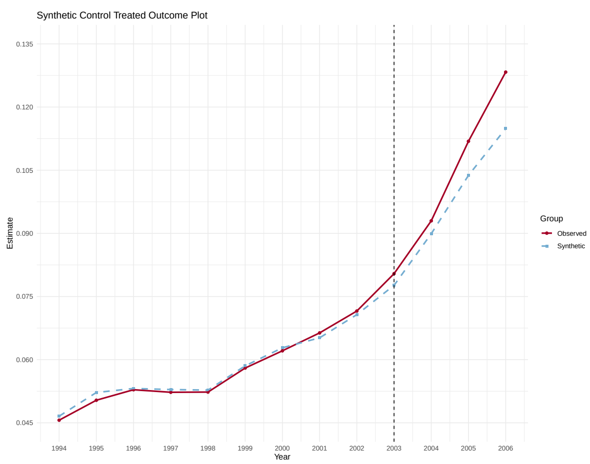

Figure A.5.1: Treated units

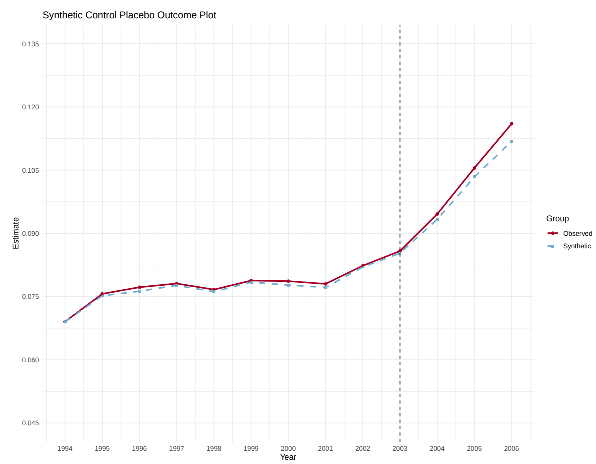

Figure A.5.2: Placebo units

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 A.5.1: 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.

Appendix A.6. Approvals Outcome (Treasury)

Refer to the estimates in Table 13.

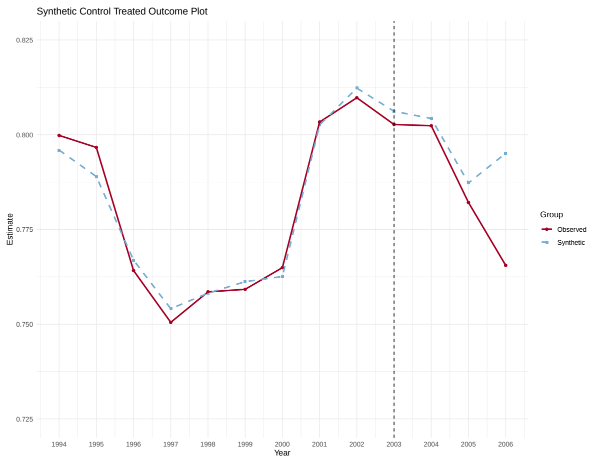

Figure A.6.1: Treated units

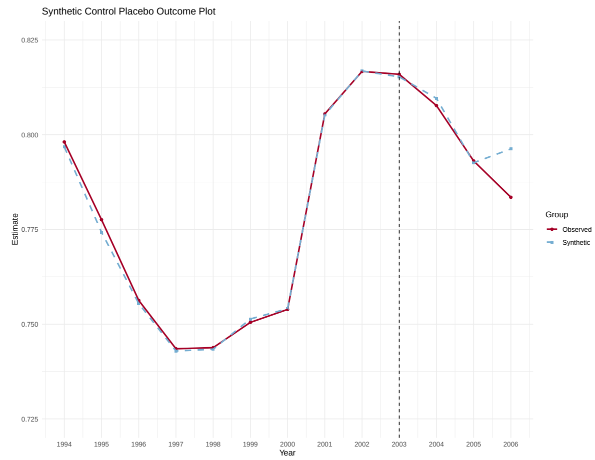

Figure A.6.2: Placebo units

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 A.6.1: 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.

Appendix A.7. Loan Amount (Applications) Outcome (Treasury)

Refer to the estimates in Table 14.

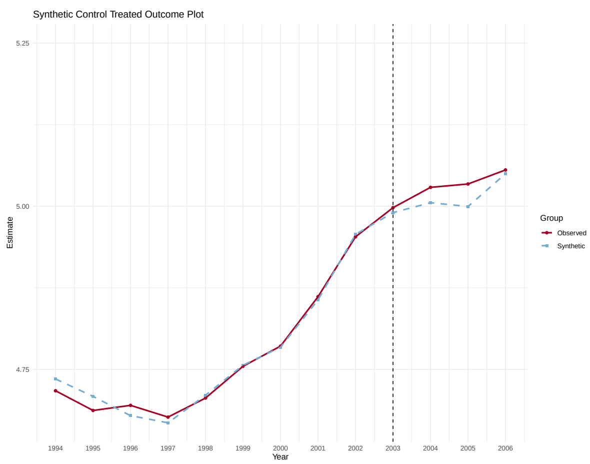

Figure A.7.1: Treated units

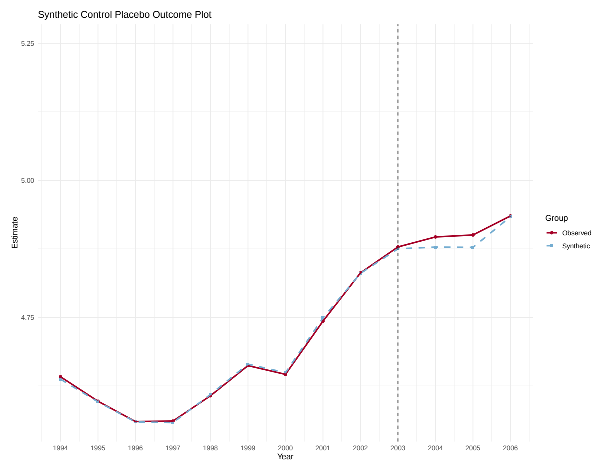

Figure A.7.2: Placebo units

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 A.7.1: 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.

Appendix A.8. Loan Amount (Approvals) Outcome (Treasury)

Refer to the estimates in Table 15.

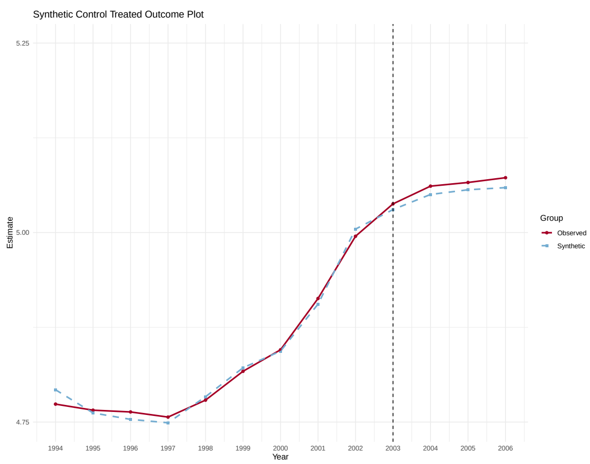

Figure A.8.1: Treated units

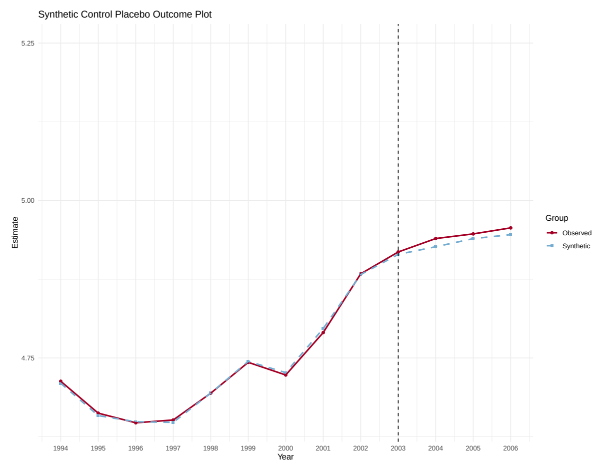

Figure A.8.2: Placebo units

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 A.8.1: 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.

Online Appendix

Online Appendix I. 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 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.[65]

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 3.1[66] and generate summary statistics akin to those in Table 2. As can be seen in Table I.1, 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 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 I.1 illustrates that very little of the raw homeownership gap be-

[65] 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.

[66] 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.

tween 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 3.2. Table I.2 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 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 I.1: Summary statistics for the household-level microdata sample by immigrant status. Columns 1-3 are equivalent to Table 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 I.2: 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 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.

Online Appendix II. Alternative Household-Level Difference-in-differences

The specification chosen in Section 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.

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

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 II.1 - II.3, 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 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."[67] The results are included in the table below. The first 3 columns replicate the results from Section 4.1 for comparison.

	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 II.1: Difference-in-differences regression results for the *owned* indicator. Columns 1-3 are identical to Table 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 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.

[67] Only a small fraction of the undocumented population (the treated group in Section 4.1) received DACA, but roughly half of the DACA-eligible population (the treated group here) did.

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 II.1 - II.3, 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 II.1 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).

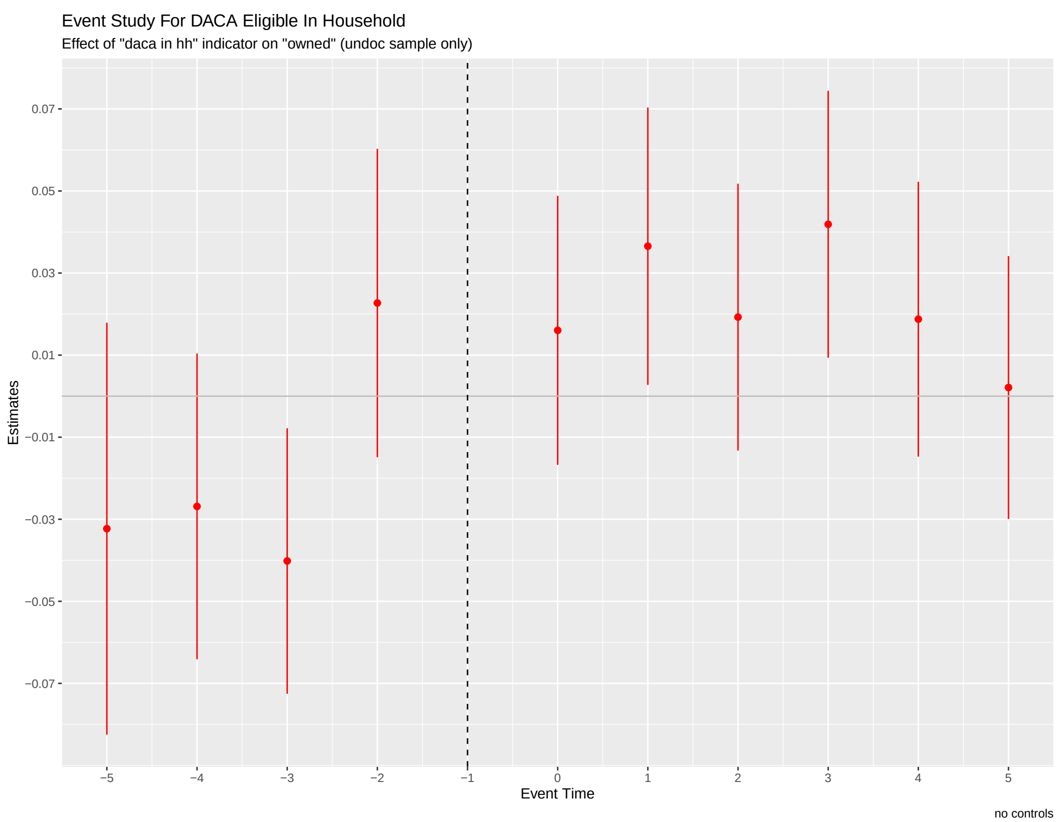

Figure II.1: Event study for the effect of having a DACA-eligible person living in the household (corresponds to the difference-in-differences results presented in column 4 of Table II.1)

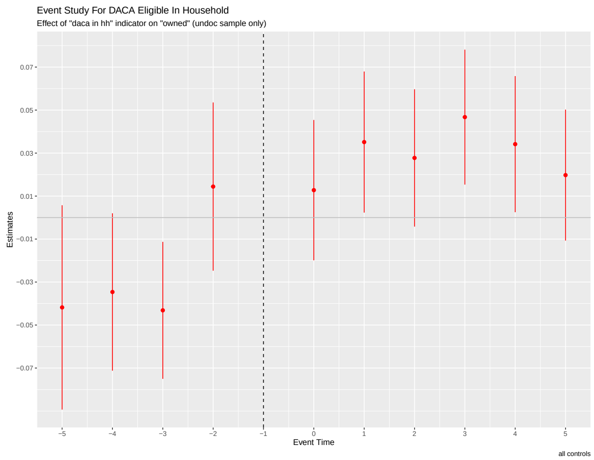

Figure II.2: Event study for the effect of having a DACA-eligible person living in the household (corresponds to the difference-in-differences results presented in column 5 of Table II.1)

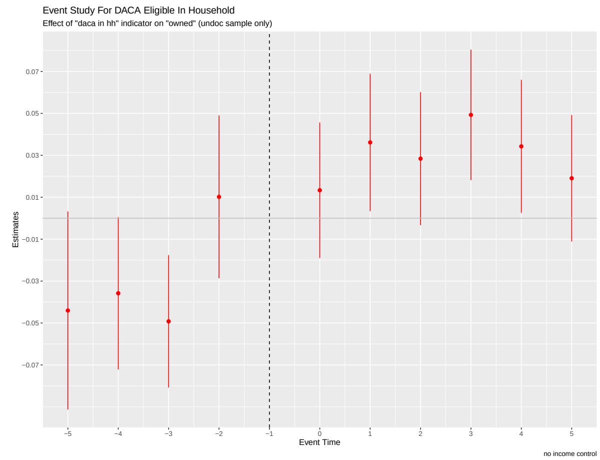

Figure II.3: Event study for the effect of having a DACA-eligible person living in the household (corresponds to the difference-in-differences results presented in column 6 of Table II.1)

Online Appendix III. 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.[68] 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 4 and 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.[69]

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 Online Appendix IV.

Online Appendix III.1. Applications Outcome (DACA)

Refer to the estimates in Table 5. Figure 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.[70]

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

[69]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).

[70]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.

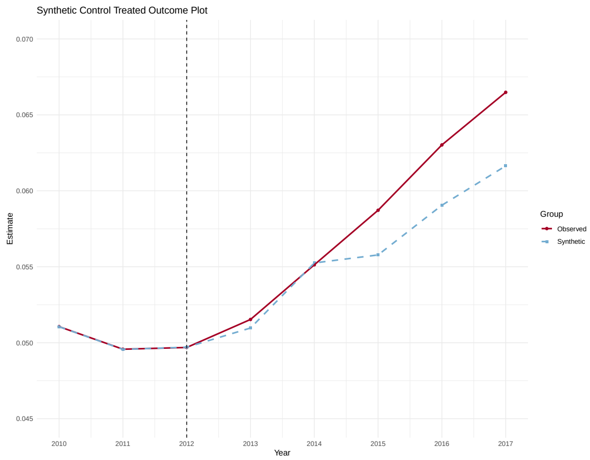

Figure III.1.1: Treated units

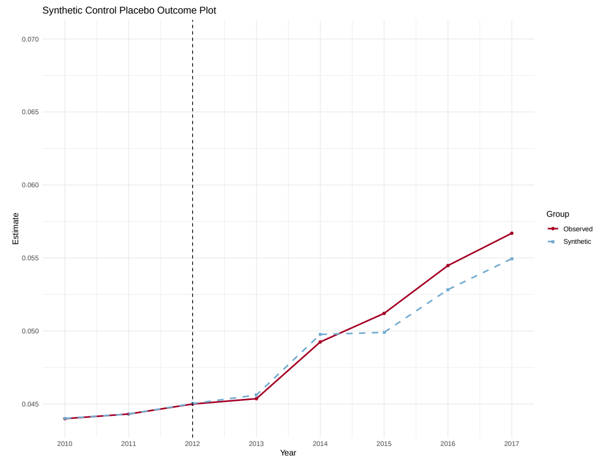

Figure III.1.2: Placebo units

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 III.1.1: unrestricted (pre-proportion = 1)

Note the surprisingly large p-value calculated using the post-period RMSPE. As noted 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.[71] 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

[71]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.

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.[72] 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 p-values are re-computed under different restrictions (different values of m).[73]

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 III.1.2: $m = 100$ 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 III.1.3: $m = 75$ 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

[72]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.

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

number of Hispanic home loan applications, which is close to the unweighted difference-in-differences estimate of 0.33 percentage points.

Online Appendix III.2. Approvals Outcome (DACA)

Refer to the estimates in Table 9. Presented below is the event study corresponding to column 4.

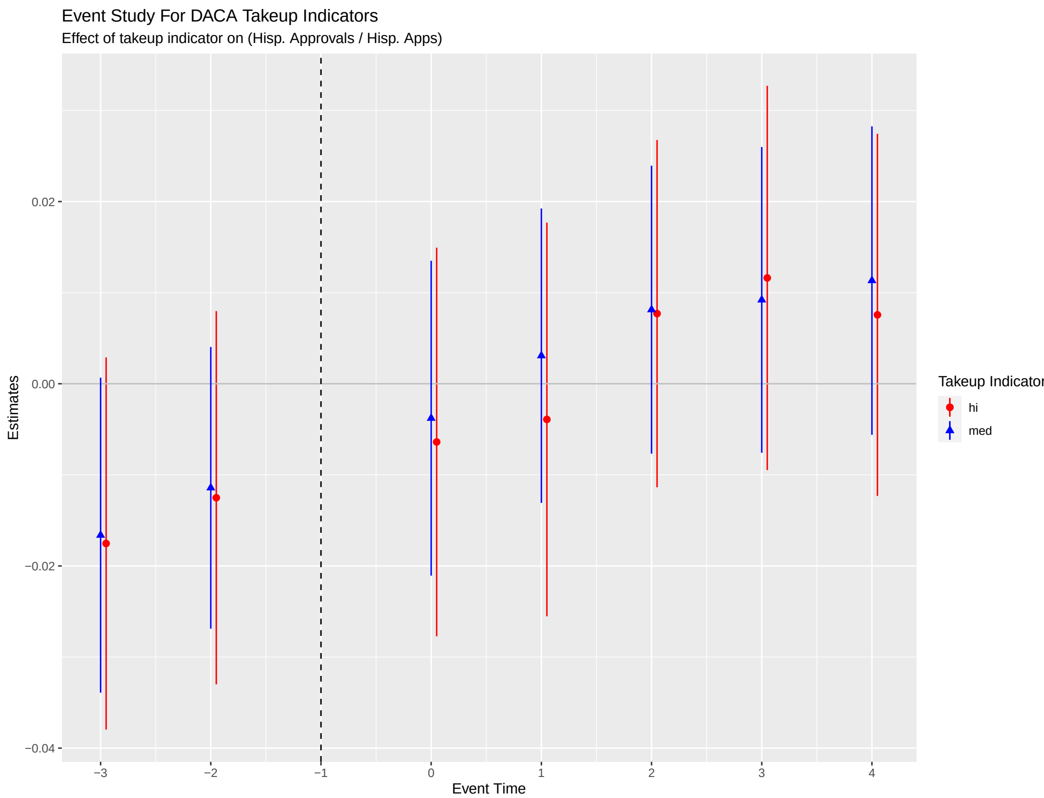

Figure III.2.1: Event study for Hispanic approval rate

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.

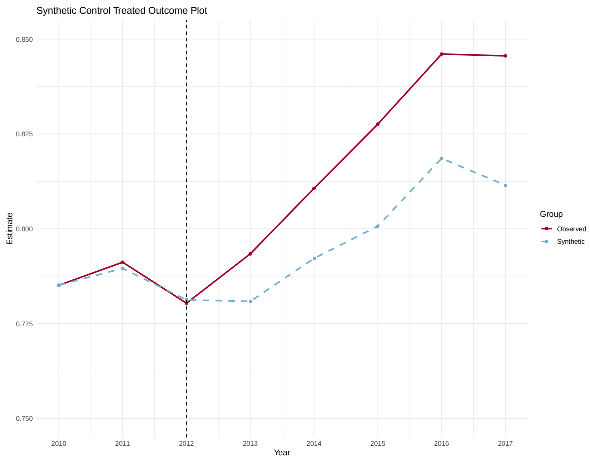

Figure III.2.2: Treated units

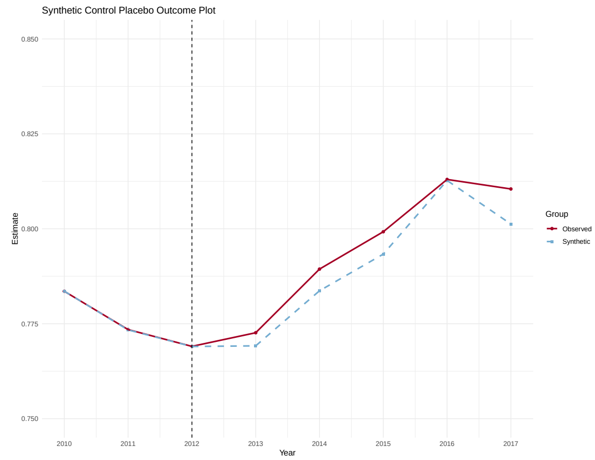

Figure III.2.3: Placebo units

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 III.2.1: 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 III.2.2: $m = 50$ 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 III.2.3: $m = 25$ 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.[74] The following tables present the p-values from one-sided inference.

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 III.2.4: unrestricted (pre-proportion = 0.88)

[74] 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	0.0218	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 III.2.5: $m = 50$ 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 III.2.6: $m = 25$ 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% significance level (depending on the restriction imposed) when pre-period RMSPE is not taken into account (column 5).

Online Appendix III.3. Loan Amount (Applications) Outcome (DACA)

Refer to the estimates in Table 10. Presented below is the event study corresponding to column 4.

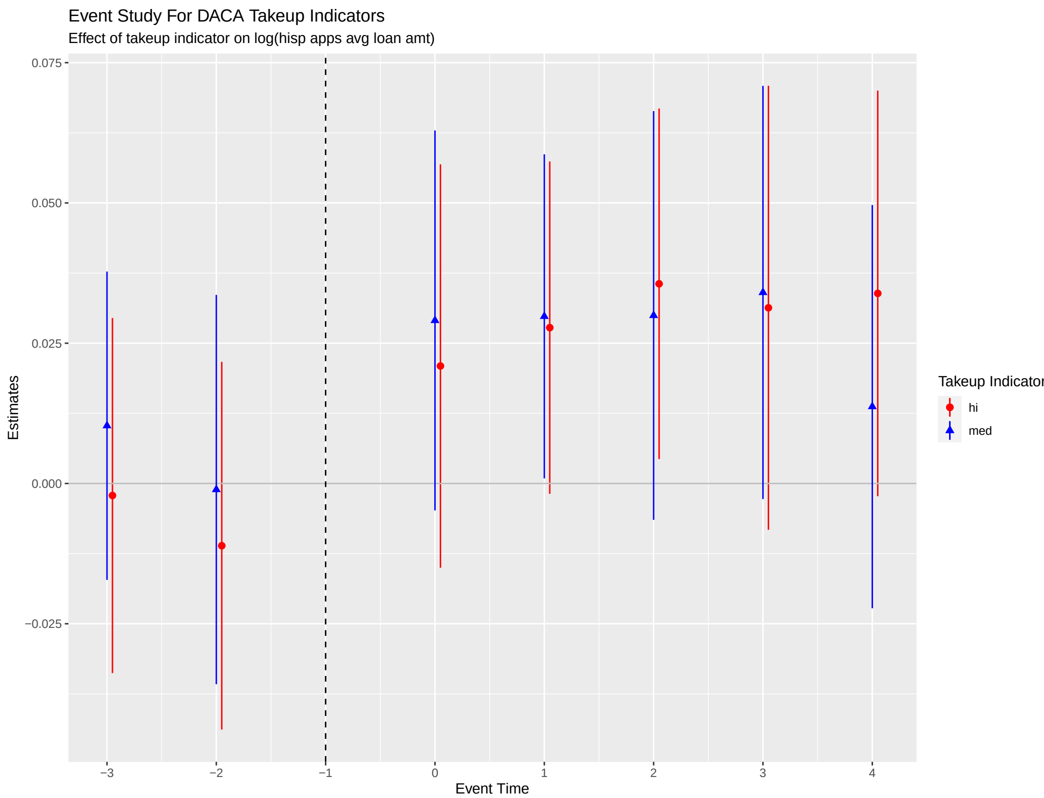

Figure III.3.1: Event study for log loan amount of Hispanic home loan applications

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.

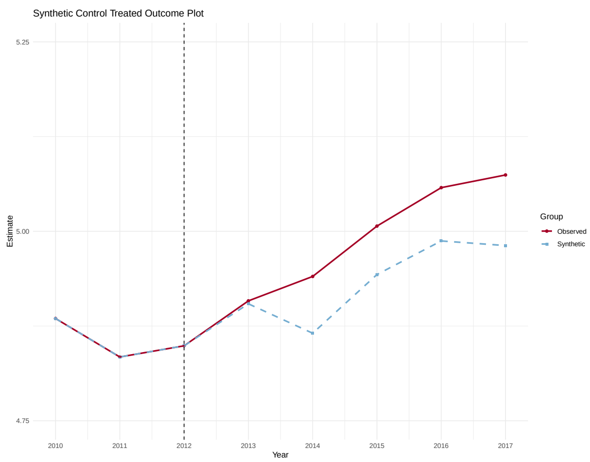

Figure III.3.2: Treated units

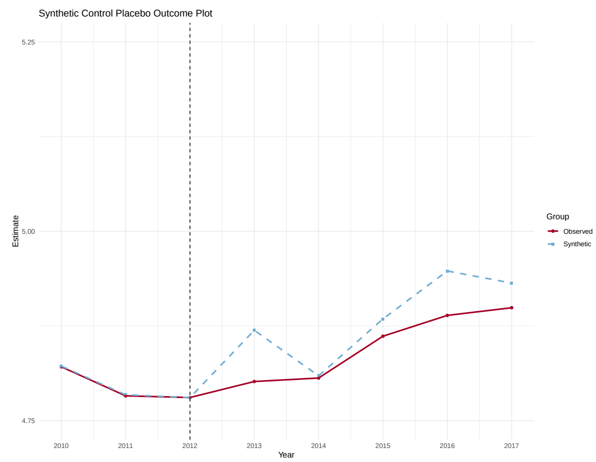

Figure III.3.3: Placebo units

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 III.3.1: 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 III.3.2: $m = 50$ 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 III.3.3: 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 III.3.4: $m = 50$ 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).

Online Appendix III.4. Loan Amount (Approvals) Outcome (DACA)

Refer to the estimates in Table 11. Presented below is the event study corresponding to column 4.

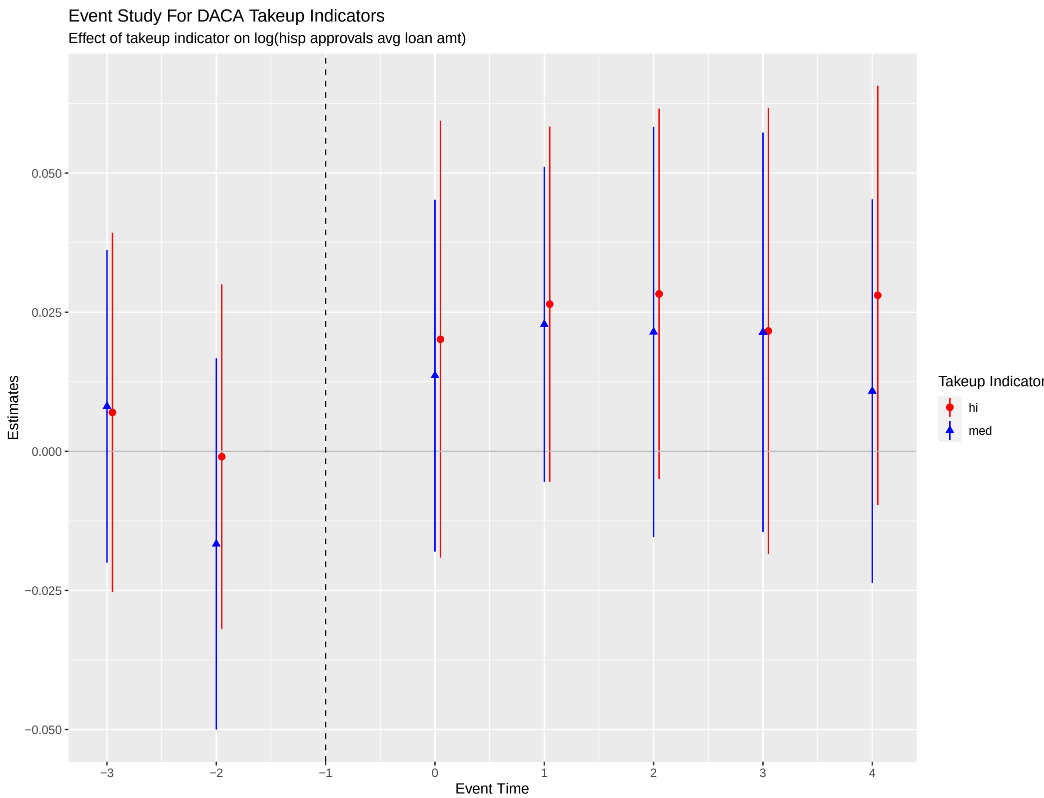

Figure III.4.1: Event study for log loan amount of Hispanic home loan applications that were approved

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.

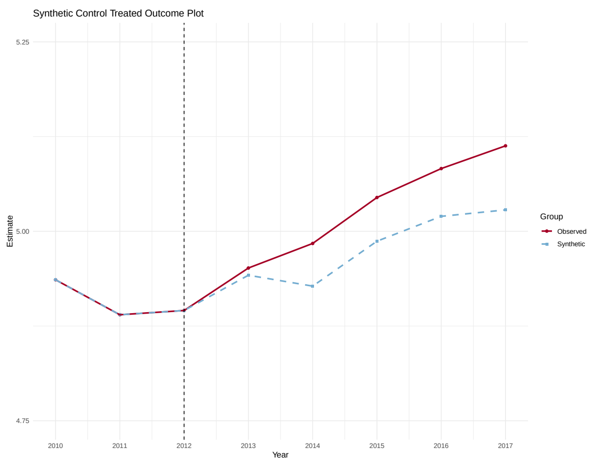

Figure III.4.2: Treated units

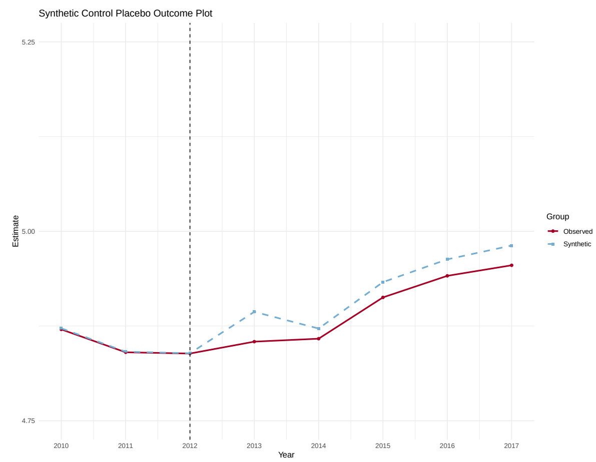

Figure III.4.3: Placebo units

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 III.4.1: 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 III.4.2: $m = 50$ 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 III.4.3: 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 III.4.4: $m = 50$ 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.

Online Appendix III.5. Applications Outcome (Treasury)

Refer to the estimates in Table 12. The event study corresponding to column 1 is presented in Figure 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.

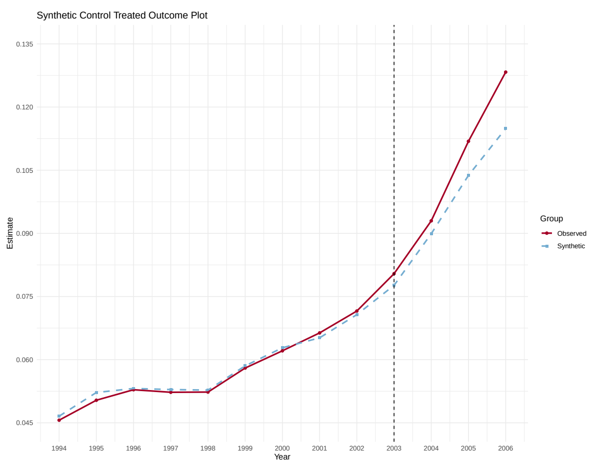

Figure III.5.1: Treated units

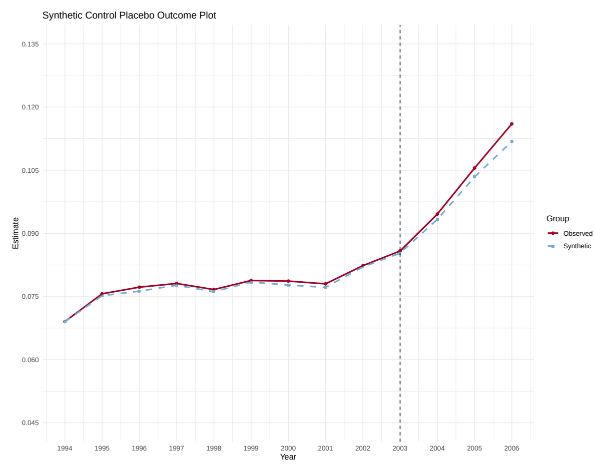

Figure III.5.2: Placebo units

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 III.5.1: 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.

Online Appendix III.6. Approvals Outcome (Treasury)

Refer to the estimates in Table 13. The event study corresponding to column 4 is presented below.

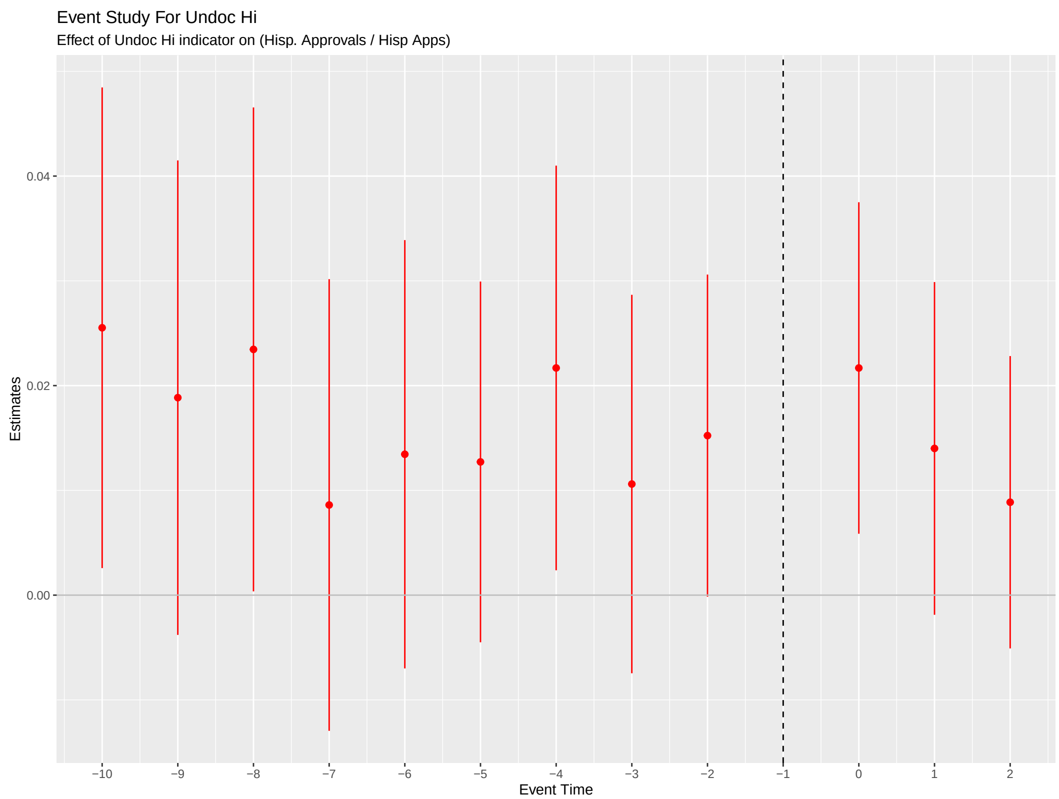

Figure III.6.1: Event study for log loan amount of Hispanic home loan applications

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.

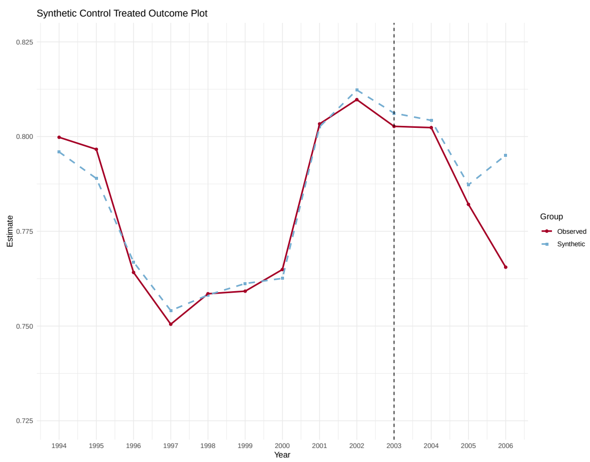

Figure III.6.2: Treated units

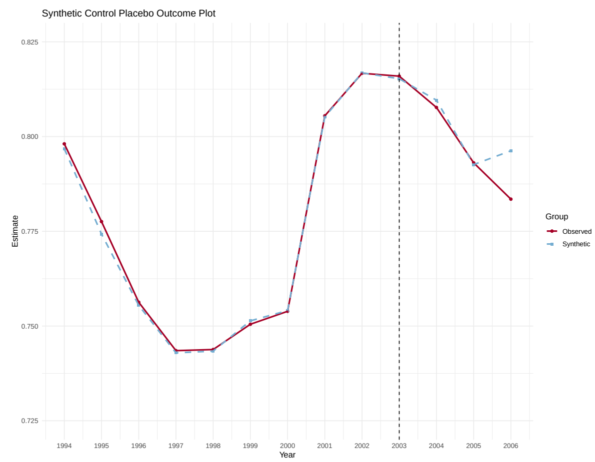

Figure III.6.3: Placebo units

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 III.6.1: 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 III.6.2: $m = 4$ 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 III.6.3: $m = 2$ restriction (pre-proportion = 0)

Results are similar regardless of pre-proportion value. Joint p-values are not extreme either,[75] 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

[75] Additionally, p-values in columns 5 and 6 are consistent with those in columns 3 and 4.

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.

Online Appendix III.7. Loan Amount (Applications) Outcome (Treasury)

Refer to the estimates in Table 14. The event study corresponding to column 4 is presented below.

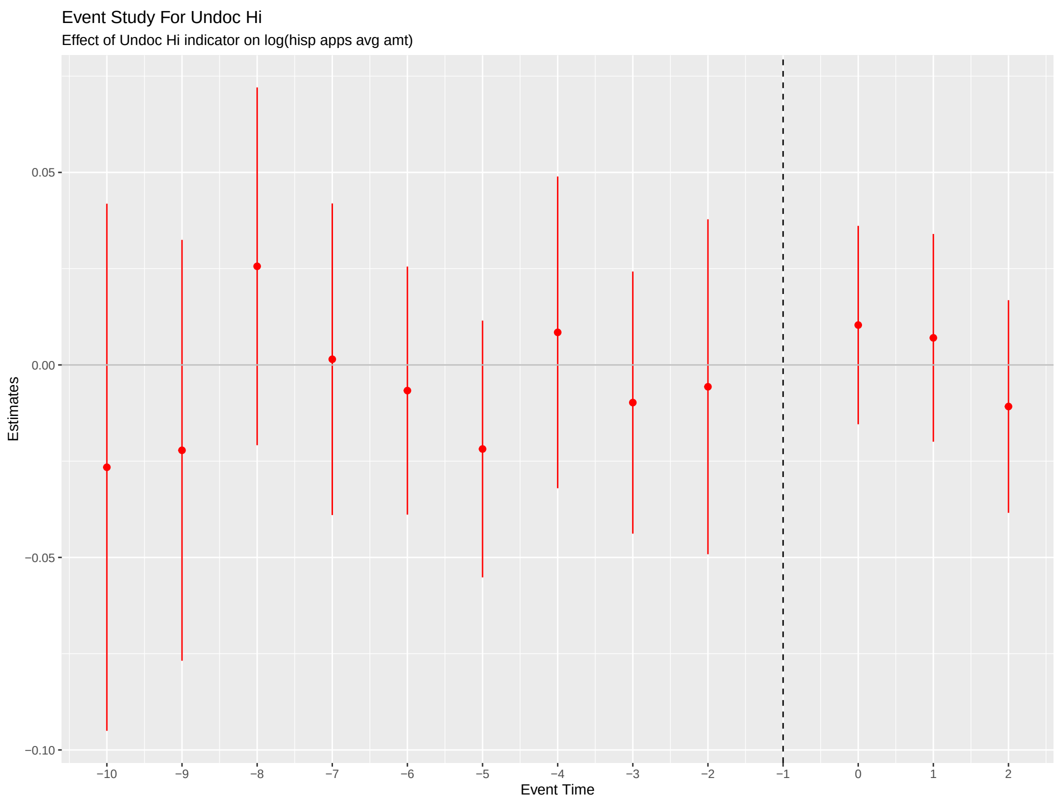

Figure III.7.1: Event study for log loan amount of Hispanic home loan applications

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.

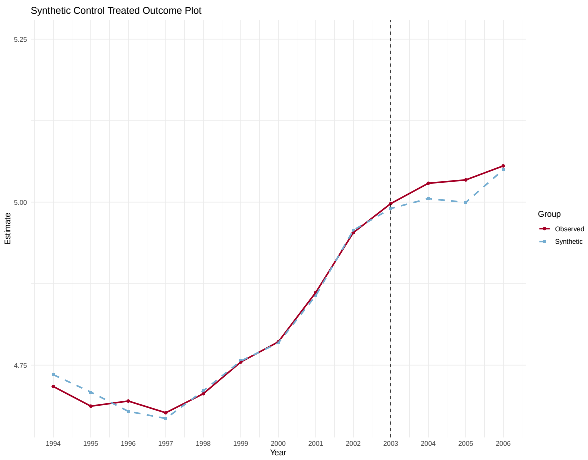

Figure III.7.2: Treated units

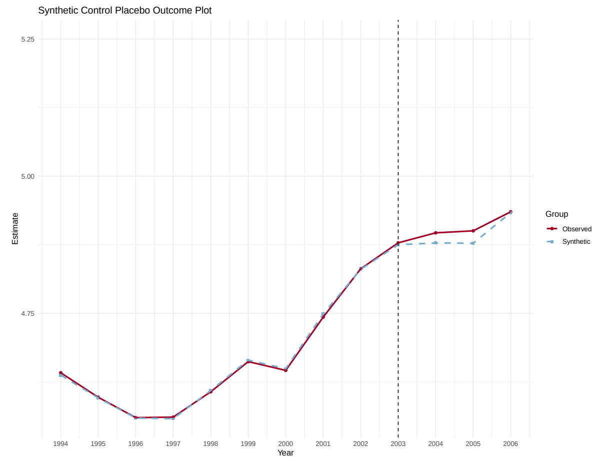

Figure III.7.3: Placebo units

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 III.7.1: 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 III.7.2: $m = 10$ 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 III.7.3: $m = 5$ 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 III.7.4: 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 III.7.5: $m = 10$ 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 III.7.6: $m = 5$ 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.[76]

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 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.

[76]Contrast this with the one-sided testing for effects of DACA where switching to one-sided inference drastically increased statistical significance.

Online Appendix III.8. Loan Amount (Approvals) Outcome (Treasury)

Refer to the estimates in Table 15. The event study corresponding to column 4 is presented below.

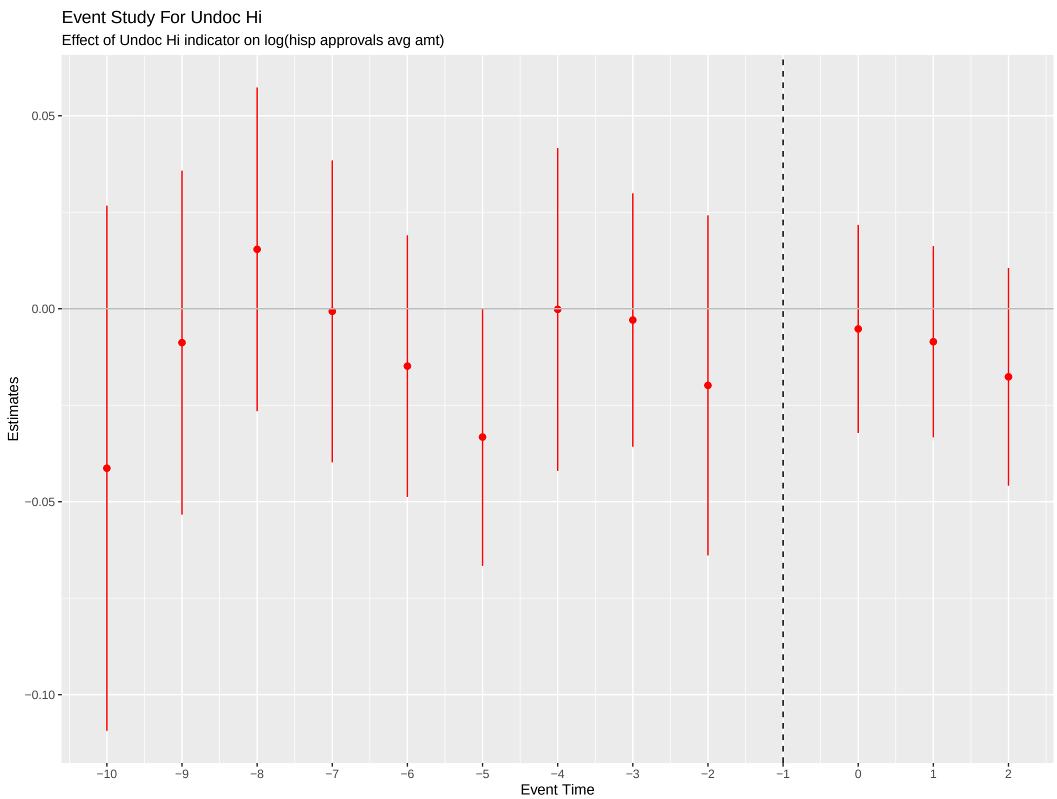

Figure III.8.1: Event study for log loan amount of Hispanic home loan approvals

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.

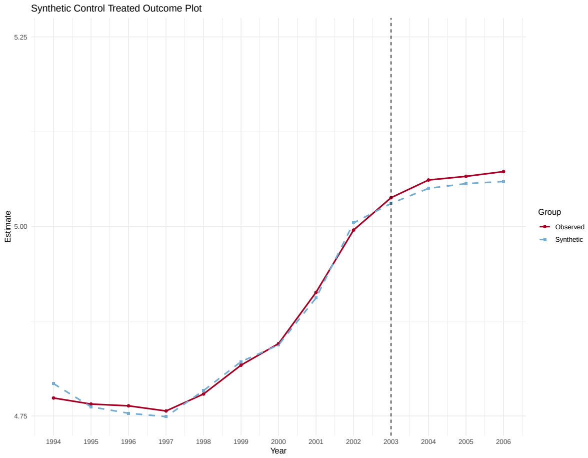

Figure III.8.2: Treated units

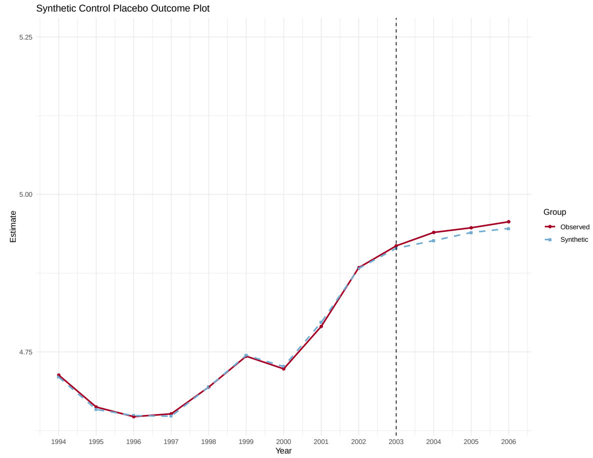

Figure III.8.3: Placebo units

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 III.8.1: 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 III.8.2: $m = 10$ 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 III.8.3: $m = 5$ 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 III.8.4: 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 III.8.5: $m = 10$ 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 III.8.6: $m = 5$ 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.

Online Appendix IV. Details of Synthetic Control Procedures

Synthetic control is carried out using the synth package[77] 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.

[77] Abadie, Diamond and Hainmueller (2011)

Online Appendix V. Details on the Treasury Legal Clarification

A handful of media articles[78] 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[79] 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 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.[80] In 2004, Suspicious Activity Reports

[78]See, for instance, Khimm (2014) and Roosevelt (2017).

[79]See, for example, Jordan (2008).

[80]For example, banks associated with the New Alliance Task Force, which is argued to have "pioneered"

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.[81]

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.

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.

[81] In late 2005, Wells Fargo also experimented with offering ITIN mortgages in LA and Orange counties in California.