Can legal status help unauthorized immigrants achieve the American Dream? Evidence from the Deferred Action for Childhood Arrivals program

Jia Wang[a,*], John V. Winters[b], Weici Yuan[c]

[a] Department of Economics & Finance, University of Dayton, 300 College Park, Dayton, OH 45469, United States
[b] Department of Economics, Iowa State University, 518 Farm House Lane, Ames, IA 50011, United States
[c] Department of Economics, Finance, Insurance & Risk Management, University of Central Arkansas, 201 Donaghey Ave, Conway, AR 72035, United States

Abstract: This paper examines the housing tenure choices of unauthorized immigrants following the largest immigration policy change in recent years. Our identification strategy exploits the discontinuity in eligibility criteria of the 2012 Deferred Action for Childhood Arrivals (DACA) program, which provides a renewable two-year reprieve from deportation and work authorization to eligible immigrants. We estimate a difference-in-differences model that compares eligible with ineligible individuals before and after the program's implementation. Our results indicate that DACA eligible household heads become more likely to be homeowners. Thus, DACA increases access to not only the US labor market but also the benefits of homeownership.

JEL Codes: J1, R2

Keywords: immigration policy, homeownership, unauthorized immigrants, Deferred Action for Childhood Arrivals, DACA

Declaration of interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

* Corresponding author.

E-mail addresses: jwang8@udayton.edu (Jia Wang), winters1@iastate.edu (John V. Winters), wyuan@uca.edu (Weici Yuan)

1. Introduction

Homeownership has long been considered a major component of the American Dream (e.g., Gabriel and Painter, 2008; Goodman and Mayer, 2018). A vast literature suggests various direct and indirect benefits of homeownership. Homeowners accumulate more wealth (Di et al., 2007; Goodman and Mayer, 2018; Wainer and Zabel, 2019), and home equity gains are linked to increased entrepreneurship and education (Harding and Rosenthal, 2017; Lovenheim, 2011; Lovenheim and Reynolds, 2013). Prior research also suggests numerous social benefits and neighborhood spillovers: homeowners are more active in local politics due to stronger financial motivation and lower mobility (Coulson and Li, 2013; DiPasquale and Glaeser, 1999; Engelhardt et al. 2010; Jiang, 2018). Homeownership is further associated with improved child outcomes through possible channels including higher housing quality, increased self-esteem, and a more supportive home environment from increased parental satisfaction and decreased stress (Aaronson, 2000; Blau et al., 2019; Green and White, 1997; Haurin et al., 2002).

Despite the potential benefits of homeownership, immigrant and minority households in the US have consistently lower rates of homeownership than native non-Hispanic white households (Borjas, 2002; Chakrabarty et al., 2019; Constant et al., 2009; Coulson, 1999; Gabriel and Rosenthal, 2005; Painter et al., 2001). Unauthorized immigrants are particularly less likely to be homeowners. Passel and Cohn (2009) found that only 35% of unauthorized immigrant households were homeowners, half the rate of native-born households.

In this paper, we examine the effect of legalization on housing tenure decisions following the 2012 Deferred Action for Childhood Arrivals (DACA) program, the largest immigration overhaul in recent years. Though temporary, DACA provides deportation relief and work authorization to eligible immigrants. With the risk of deportation and lack of work authorization, unauthorized immigrants face additional housing market frictions. Eligible immigrants could potentially reduce these barriers and improve their housing market outcomes through receiving authorization via the DACA program. Given the potential benefits of homeownership, understanding the role of immigration status in influencing the housing demand of immigrants has broad implications.

We make important contributions to the literature on the relationship between housing tenure choices and immigration status as well as impacts of immigration reform in general. We are the first to examine the effect of the DACA program, a temporary immigration reform, on housing tenure decisions in the US. Eligibility for DACA largely hinges on age in 2012. We leverage the age in 2012 criteria to causally identify the role of legal status in housing tenure decisions. We additionally consider and provide evidence on possible underlying mechanisms. Our study also contributes to the broader policy discussion about immigration in general and DACA in particular.

DACA might influence the housing tenure decisions of unauthorized immigrants through multiple channels. First, work authorization lowers employment barriers faced by these migrants, so they are more likely to work and find jobs that better match their skills and pay higher wages. Prior research has provided ample empirical support for the positive labor market effects of DACA (Amuedo-Dorantes and Antman, 2017; Giuntella and Lonsky, 2020; Pope, 2016). The enhanced job opportunities and increased income could help them accumulate savings toward purchasing a home. Second, authorization might also reduce barriers these immigrants face in the credit market when applying for a mortgage loan. Third, DACA reduces the risk of deportation, which may give eligible individuals more confidence of staying in the US in the long term. Bradley et al. (2007) show that expected length of stay is an important determinant of

tenure status. When one expects to spend a long time in one place, the transaction costs of purchasing a home will be spread out over a longer time and make homeownership more desirable. All three possible channels point to a higher probability of being a homeowner for eligible individuals. However, uncertainty about the future of the DACA program might suppress its effects. These theoretical ambiguities underscore the importance of our empirical investigation here.

We use the American Community Survey (ACS) microdata (accessed via IPUMS) to investigate the homeownership effect of DACA. Identifying the causal effect of DACA, however, presents a unique challenge: we do not directly observe the legal status of immigrants. Following previous literature, we proxy for unauthorized status by relying on the lack of citizenship combined with the country of origin. Although we cannot precisely identify unauthorized immigrants, we focus on Mexican non-citizens, a group likely to be unauthorized (Amuedo-Dorantes and Antman, 2016). We also apply the residual method following Borjas and Cassidy (2019) as a robustness check, which excludes some non-citizens who are likely to be legal residents and leaves a residual group who are especially likely to be unauthorized.

Our main identification strategy uses a difference-in-differences (DID) approach that compares eligible immigrants with similar ineligible individuals before and after the program's implementation. We exploit the discontinuity in one eligibility criterion: being younger than 31 years old in 2012. We therefore restrict our attention to individuals who meet all other observable eligibility criteria and compare individuals who were above and below the age cutoff in 2012. Therefore, our treatment and control groups only differ in that the latter was older in 2012. We also include a variety of controls to ensure that the estimated effect is not confounded by differences in other demographic characteristics. Our empirical design closely follows the recent DACA literature (Amuedo-Dorantes and Antman, 2016; Pope, 2016).

We find that DACA eligibility greatly increased the likelihood of being a homeowner. Specifically, DACA-eligible household heads became 7 percentage points more likely to be a homeowner after DACA, relative to their ineligible counterparts. 35.4% of household heads in our analytical sample are homeowners, so DACA increased the likelihood of owning a home by 19% relative to the sample mean. Since our non-citizen sample also contains authorized immigrants, the intent-to-treat effect of DACA will be larger than the DID estimates. Our main result is robust to several alternative specifications and sample definitions, and our research design passes falsification tests. The large effect on homeownership reflects that DACA receipt is a life-changing event that profoundly alters income earning opportunities, expected duration in the US, human capital investments, and financial market access among other things.

As an extension to our main analysis, we further examine related outcomes and possible contributing factors to the increase in homeownership. We confirm findings in previous literature that DACA has positive effects on income, and this explains some but not all of the DACA effect on homeownership. DACA eligibility does not significantly affect marital status, household formation (being a household head or spouse), family size, or migration. We further find suggestive evidence for improved mortgage access and longer expected US stay following DACA implementation.

Our results suggest that DACA improves the well-being of unauthorized immigrants, their families, and communities. Our paper complements existing literature on DACA, which finds that DACA led to better labor market outcomes (Amuedo-Dorantes and Antman, 2016, 2017; Pope, 2016), improved health insurance access and outcomes (Giuntella and Lonsky, 2020), lower property crime rates (Gunadi, 2020), higher gross domestic product (Ortega et al., 2019), and mixed evidence on schooling (Amuedo-Dorantes and Antman, 2017; Hsin and Ortega, 2018; Kuka et al., 2020; Pope, 2016). Our work is also closely related

to Sharpe (2020), who investigates the effect of the 1986 Immigration Reform and Control Act (IRCA) on homeownership and finds a significant positive effect. We later discuss possible reasons why our coefficient estimate for DACA is of larger magnitude than the estimated effect in Sharpe (2020) for the 1986 IRCA**.**

The rest of the paper proceeds as follows. Section 2 provides further background information on the DACA program including benefits and eligibility criteria. Section 3 describes our identification strategy, while Section 4 discusses data sources, sample definition, and graphical evidence. Sections 5, 6, and 7 report the main results, robustness checks, and falsification tests, respectively. Section 8 concludes.

2. Background

President Obama issued an executive order that aimed at protecting unauthorized immigrants who came to the United States as children on June 15, 2012, known as the Deferred Action for Childhood Arrivals (DACA). It is the largest immigration reform since the 1986 Immigration Reform and Control Act. DACA benefits its recipients in two major ways. First, the recipients receive a two-year reprieve from deportation, which allows them to legally reside in the US. Second, applicants are granted work authorization, which allows application for a social security number (SSN). This provides access to other benefits such as obtaining a driver's license and opening a bank account. DACA status was initially granted for two years, but recipients can apply for a renewal, which is issued in 2-year increments. Although DACA does not provide a permanent legal status, recipients might be optimistic about the future of the program considering its regular renewal opportunities and potential expansion.

DACA eligibility rules closely mirror those in the DREAM Act legislation. An individual must meet a series of age, education, and criminal background requirements. Specifically, applicants need to meet all of the following requirements[1]: (1) be younger than 31 years old as of 15 June 2012; (2) came to the US before age 16; (3) have continuously resided in the US since 15 June, 2007; (4) were physically present in the US on 15 June, 2012; (5) no legal status as of June 15, 2012; (6) are currently in school, have obtained a high school degree or equivalent, or have been honorably discharged from the coast guard or armed forces of the US; (7) have not been convicted of a felony or significant misdemeanor, or three or more other misdemeanors, and do not otherwise pose a threat to national security or public safety.

Applicants are also required to pay a processing fee and provide evidence that they meet all requirements. Passel and Lopez (2012) estimated that 1.7 million of the 11.2 million unauthorized immigrants were potentially eligible for DACA, of which 950,000 were immediately eligible. Although the US Citizenship and Immigration Services (USCIS) began accepting applications in August 2012, applications and approvals did not pick up until later in the year. By March 2014, 86% of applications had been approved. As of September 2017, around 800,000 individuals had received DACA since its establishment. Among those, more than three quarters (79.4%) are from Mexico.[2]

In September 2017, President Trump sought to terminate the DACA program, but this was challenged and ultimately overturned by the US Supreme Court.[3] USCIS continued accepting renewal applications and

[1] https://www.uscis.gov/archive/consideration-of-deferred-action-for-childhood-arrivals-daca#guidelines
[2] https://www.uscis.gov/sites/default/files/document/data/daca_population_data.pdf
[3] https://www.npr.org/2020/06/18/829858289/supreme-court-upholds-daca-in-blow-to-trump-administration

DACA recipients remain protected. However, actions during the Trump Administration created substantial uncertainty about the program and the future of unauthorized immigrants.

3. Methodology

Following Amuedo-Dorantes and Antman (2016) and Pope (2016), we estimate a difference-in-differences (DID) regression model that compares the outcomes of immigrant household heads that are potentially eligible for DACA to the outcomes of those who are not before and after DACA to identify the causal effect of DACA on homeownership and other outcomes.[4] Specifically, we exploit the discontinuity in one eligibility criterion: being younger than 31 years old in 2012. We therefore restrict our attention to household heads who meet all other observable eligibility criteria and compare household heads who were above and below the age cutoff in 2012. Therefore, our treatment and control groups only differ in that the latter was older in 2012. Specifically, we estimate the following Eq. (1) via ordinary least squares:

$$(1)\ Y_{ist} = \beta_0 + \beta_1 (DACA_t \cdot Eligible_{ist}) + \beta_2 Eligible_{ist} + X_{ist}\,\xi + \gamma_{st} + \varepsilon_{ist}$$

where Y_{ist} indicates whether an individual (i) in state (s) in year (t) is a homeowner or not. We also consider alternative outcomes related to mortgages, income, and education. The variable $Eligible_{ist}$ is a dummy variable that equals 1 if an individual (i) meets observable DACA eligibility requirements. $DACA_t$ is a binary variable that equals 1 if the survey was administered after 2012. To ensure that our results are not driven by age differences, we fully control for age via a detailed set of age fixed effects (i.e., dummies for each age) in X_{ist}. We additionally control for age-at-arrival fixed effects. We also include sex and an indicator for race being white as control variables because they potentially affect an individual's housing tenure choice. We excluded controls for years of education, marital status, and family size because these are potentially influenced by DACA (Amuedo-Dorantes and Antman, 2017; Kuka et al., 2020; Pope, 2016). Finally, Eq. (1) incorporates state-by-year fixed effects (γ_{st}), which capture shocks that are specific to a state in a year.[5] Inclusion of state-by-year fixed effects means that eligible individuals are compared to ineligible individuals in the same state and year. All regressions use survey weights and cluster standard errors at the state level.

The parameter of interest (β_1) measures the change in the probability of homeownership for eligible household heads relative to their DACA-ineligible counterparts after the program went into effect. This is the intent-to-treat effect of DACA, which is expected to be smaller than the actual treatment-on-the-treated effect. An important assumption underlying the DID strategy is that the treatment and control groups would have maintained parallel trends in the outcome variables in the absence of DACA. We provide support for this assumption via an event study analysis.

4 Data and descriptive statistics

4.1 Data and sample consideration

[4] There was no other major immigration reform or housing policy change in the US around DACA's announcement likely to affect home purchasing behavior among unauthorized immigrants.

[5] This helps account for situations where some states were hit more than others during certain years related to the Great Recession. We also present estimates from specifications using state-specific linear time trends.

We use IPUMS ACS microdata (Ruggles et al., 2020) to examine the effect of DACA. Each year of the ACS includes a one-percent sample of US residents.[6] Following Lovenheim and Reynolds (2013), we obtain the seasonally adjusted MSA-level housing price index from the Freddie Mac. We additionally get seasonally adjusted MSA-level unemployment data from the Bureau of Labor Statistics (BLS). Our main sample spans from 2005 to 2019. In robustness checks, we restrict our attention to focus on a narrow window around DACA implementation.

According to Amuedo-Dorantes and Antman (2017), DACA was proposed in the months leading up to the 2012 presidential election to court Latino votes. The announcement was unexpected, and the program went into effect quickly. For the purpose of evaluating the impact of DACA, it is reasonable to assume that there were minimal anticipation effects before the program's announcement. As mentioned in the background section, relatively few cases were approved until late 2012 although the USCIS began to accept applications in August. Therefore, our definition of the post-DACA period starts in 2013.

We analyze multiple housing related outcomes. The first and main outcome is the probability of being a homeowner. We generate a dummy variable to indicate whether an individual is a homeowner or not. We also examine whether a home is owned free and clear or through a mortgage. Additionally, we explore the effect of DACA on mortgage payments and household formation.

The ACS data provide a rich set of individual characteristics and migration history that allow us to gauge whether individuals are DACA eligible or not. Using the information on the length of residence in the US and their age, we compute the age at which the individual entered the US. We limit the main analytical sample to household heads who satisfy DACA's education criteria, i.e., a high school degree or equivalent (including GED), or military service requirement.[7] Our main sample is further restricted to household heads ages 23–40 (at the time of survey) who arrived in the US under 16 years old and before the year 2007. Our treatment group consists of individuals ages 16-30 in 2012, while our control group consists of those ages 32-47 in 2012; we exclude persons aged 31 in 2012 because we do not know their exact birth-date and cannot confidently assign them to either the treatment or control group. Despite the detailed individual level data, we do not generally observe the criminal background of respondents.[8]

An important limitation of the survey, however, is that we do not observe an individual's legal status. Following previous DACA literature (Amuedo-Dorantes and Antman, 2016; Pope, 2016), we use citizenship and country of origin to proxy for unauthorized status.[9] Considering nearly 80% of DACA beneficiaries are from Mexico, we limit the analytical sample to Mexican non-citizens as they are likely unauthorized.

[6] Because the ACS collects information on a 1% representative sample of the US population regardless of their citizenship and immigration status, unauthorized immigrants are no more or less likely to be selected into the sample. Pope (2016) discusses the detailed sampling procedure and concludes that the ACS sample is likely representative of the unauthorized immigrant population both before and after the DACA program.

[7] Our results are not sensitive to excluding veterans with less than a high school diploma (available upon request).

[8] DACA eligibility required that the individual "not been convicted of a felony, significant misdemeanor, or three or more other misdemeanors, and do not otherwise pose a threat to national security or public safety." Young people in institutions are primarily in correctional institutions and would not be eligible. Institutionalized persons cannot be household heads or their spouses in ACS data, so they are already excluded from the main analysis. However, DACA could affect behavior and institutionalization, and we explore that possibility in an extension to the main analysis and find that institutionalization is unaffected.

[9] We also perform a robustness check using the residual method (Borjas and Cassidy, 2019) to impute unauthorized status.

However, not all Mexican non-citizens are unauthorized. In the case where Mexican non-citizens are authorized immigrants, their housing consumption behavior should not be affected by the DACA initiative. The inclusion of authorized immigrants in our sample, however, will bias the DID estimates toward zero and will underestimate the intent-to-treat effect. Thus, the DID estimation produces a lower bound for the intent-to-treat effect.

4.2 Graphical evidence

In Figure 1, we plot sample homeownership rates for the DACA-eligible cohorts and DACA-ineligible cohorts among Mexican-born non-citizen household heads between 2005 and 2019. We also present the results from a simple DID estimation (without control variables). The vertical line indicates the year 2012, when DACA was announced and implemented. It shows that the homeownership rates are consistently higher among the ineligible: there is a difference of roughly 18 percentage points from 2005 to 2012 on average.[10] In the post-DACA period, the gap narrows as the homeownership rate increased dramatically among the eligible. By 2019, the gap decreased by half. The evolution in homeownership patterns provides suggestive evidence of a positive treatment effect on homeownership, but Figure 1 does not account for differences in age or other characteristics.

4.3 Summary statistics

Table 1 presents summary statistics for our main sample by eligibility status before and after DACA. In addition to showing means for the eligible and ineligible groups, we also show the differences in group means and t-statistics of the mean differences. The most prominent difference between the two groups is age: DACA eligible householders were much younger (25.45 versus 32.57 years old) in the pre-DACA period. It follows that they were less likely to be married, had a smaller family size, and lower family income. Eligible householders were also less likely to own a home or have a mortgage. Echoing Figure 1, Table 1 also indicates that homeownership rates not only increased for the DACA eligible group after DACA (by four percentage points) but also decreased (by five percentage points) for the ineligible group after DACA. This warrants some consideration as to why homeownership decreased for the ineligible group and how that might affect our difference-in-difference regressions.

Note that overall homeownership rates fell during our sample period, a trend that holds nationally across regions, age groups, races and ethnicities with few exceptions.[11] It turns out that 2004-2005 saw the highest homeownership rate in recent US history. Since then, the homeownership rate has declined and only recovered modestly in recent years. Chakrabarty et al. (2019) and Goodman and Mayer (2018) also note these patterns. Both papers mention that credit expansion contributed to the housing boom of early to mid-2000s, and the subsequent credit tightening following the 2007-2008 financial crisis and the Great Recession along with changes in attitudes toward homeownership led to the fall in homeownership afterwards.

There are two opposing forces affecting homeownership for both the eligible and ineligible groups during our sample period: higher homeownership rates as people age due to aging/lifecycle effects, and lower homeownership rates due to credit tightening and changes in preferences after the 2007-2008 financial

[10] This is potentially due to the older age of the control group, which we control for in our regression analysis.
[11] https://www.census.gov/housing/hvs/data/charts/fig06.pdf; https://www.census.gov/housing/hvs/data/charts/fig05.pdf; https://www.census.gov/housing/hvs/data/charts/fig07.pdf; https://www.census.gov/housing/hvs/data/charts/fig08.pdf.

crisis. Table 1 summary statistics indicate that average age increased by about 3 and 5 years after DACA for the eligible and ineligible groups, respectively. We would expect homeownership rates to go up for both groups over time due to aging/lifecycle effects, *ceteris paribus*.[12] However, national trends in homeownership mentioned above should negatively affect both our treatment and control groups. Credit supply became much more restricted after the Great Recession (e.g., Goodman et al., 2015; Goodman and Mayer, 2018). Acolin et al. (2016) note that foreclosure rates rose to record levels due to crashing housing prices and a weakened economy along with the risky loans originated during the boom period. Consequently, lenders and secondary market institutions tightened the "credit box". They find that tightened borrowing constraints substantially lowered the probability of becoming a homeowner following the Great Recession. Goodman et al. (2015) estimate that more than 4 million additional loans would have been made between 2009 and 2013 had the lending standards of 2001 been in place. African American and Hispanic families were particularly affected by this tight credit environment. With this in mind, we still see a net increase in the homeownership rate among the eligible group but a net decrease in homeownership for the ineligible group. This suggests that DACA may have countered a drop in homeownership among the eligible group that would have occurred without DACA. However, the simple summary statistics cannot fully account for aging/lifecycle effects because the two groups start and end at different ages in our analytical sample. Thus, our DID regression is the preferred specification because it does account for age effects along with other explanatory variables. Our DID regression results below confirm that DACA eligibility is associated with significant positive effects on homeownership.

5. Results

5.1 Main results

We use Eq. (1) to estimate the impact of DACA on homeownership using a sample of Mexican-born non-citizen household heads between the ages of 23 and 40 who met DACA education requirements or are a veteran, who arrived in the US under the age of 16 before 2007. We perform a DID estimation on household heads via the age cutoff requirement in 2012.

Table 2 presents our main estimation results. The first row reports coefficient estimates on our variable of interest (DACA_Eligible), the interaction term between $Eligible_{ist}$ and $DACA_t$. We start with no controls in Column [1] and show how results change by progressively including more controls in Columns [2]-[4]. Specifically, Columns [2]-[4] successively add state and year fixed effects, demographic controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white), and state-specific linear time trends.

Column [5] includes demographic controls with state-by-year fixed effects, which is our preferred specification. It shows that DACA has increased the likelihood of being a homeowner. Our coefficient estimate suggests that DACA increases the probability of homeownership by 7 percentage points for eligible Mexican non-citizens relative to their ineligible counterparts after the program's implementation. With a sample homeownership rate of 35.4%, the estimate indicates that DACA increases the probability of being a homeowner by 19% relative to the sample mean. We note that the 95 percent confidence interval allows for an effect as small as 2 percentage points, a 5.6 percent effect, which is still an

[12] Homeownership generally increases with age during the ages of our analytical sample. For example, the homeownership rate goes up from 0.22 to 0.46 from age 23 to 40.

economically meaningful increase. Our estimates are lower bounds on the intent-to-treat effect. Although not reported in Table 2, the coefficients' estimates on the age fixed effects indicate that older household heads are more likely to purchase a home. Overall, our estimates are robust across specifications.

An important assumption underlying the validity of the DID estimator is that the treatment and control groups would have maintained common trends without treatment. Following Pope (2016) and Kuka et al. (2020), we estimate a full event study specification where we add interaction terms between the DACA eligibility indicator and a dummy variable for each survey year to our preferred specification in Column [5] of Table 2. If there were pre-existing trends, we would expect the pre-DACA interaction terms to produce statistically significant coefficient estimates. We graph the coefficient estimates and 95% confidence intervals of all the interaction terms in Figure 2. We see that the confidence intervals on the coefficient estimates for all the years preceding the DACA program include zero, i.e., being eligible for DACA has no statistically significant impact on homeownership before its announcement. The increase in homeownership only occurs afterwards, more specifically, after 2014. The delayed DACA effect likely reflects that it takes time for people to save up and build a credit history before purchasing a home, which we discuss in more detail below in Section 5.2. We can therefore be reasonably confident that the homeownership effect is attributable to the DACA program and unlikely biased by pre-existing trends.

Overall, the estimated effect of DACA on homeownership (7 percentage points) is relatively large, including when compared with the 4 percentage point effect in Sharpe (2020). Sharpe (2020) estimates the effect of the Immigration Reform and Control Act (IRCA) of 1986 amnesty on the homeownership of immigrants ages 18-60 years old. The estimated effects of IRCA and DACA are not directly comparable because of the policy differences and samples examined. For example, the policies happened in different time periods. Interest rates are much lower recently than in the 1980s (and 1990s).[13] However, both studies confirm that legal status strongly affects homeownership decisions. The DACA effect on homeownership is large but smaller than some other major life event factors like being married.[14] Thus, DACA had a large effect on homeownership, consistent with DACA receipt being a major life event.

To further alleviate concerns about the comparability between the eligible and ineligible groups, we supplement the main analysis with an alternative identification strategy using a different control group with similar ages as our treatment group. To implement this test, we first limit our sample to noncitizen household heads ages 23-40 who meet all observable DACA criteria including age at arrival, age in 2012, year of immigration, and education requirements; we then define treatment based on the country of origin with Mexicans being eligible and assume that immigrants from other countries are ineligible. We exclude noncitizens from Central and South America from this analysis because those are the top countries of birth among DACA recipients (after Mexico). Including them would especially contaminate the control group. That is, we compare potentially eligible Mexican immigrants to potentially eligible non-Mexican immigrants. This assumes that non-Mexican immigrants were ineligible for DACA, e.g., due to permanent residence status achieved pre-DACA. However, this is a strong assumption; realistically, some non-Mexican immigrants were eligible for and received DACA. This leads to measurement error in the DACA treatment variable for this alternative identification strategy which we expect to attenuate the DACA coefficient estimate toward zero. We report results for this exercise in Column [6] of Table 2. This

[13] https://www.valuepenguin.com/mortgages/historical-mortgage-rates

[14] The subsample difference in mean homeownership for those who are married vs. not married is around 17 percentage points.

identification ensures that treatment and control groups are of the same age range and no statistically significant differences in average ages exist between the two groups before and after DACA. We continue to find a positive and significant homeownership effect similar to our main identification, which reassures us that our finding is not driven by age differences and aging effects.

5.2 Discussion of delayed homeownership effect

Why might we expect DACA's impact on homeownership to be delayed by two years? Unfortunately, previous work on the role of legal status on housing tenure choices does not consider possible delayed effects. They typically account for differences in immigration or citizenship status by incorporating dummy variables in regressions that explain homeownership status. Their conclusions are largely based on whether the coefficient estimates on these dummy variables are statistically significant or not (e.g., Chakrabarty et al., 2019; Coulson, 1999; McConnell, 2015).

Sharpe's (2020) study on the homeownership effect of IRCA does not inform us about whether there was a delayed homeownership effect following the IRCA reform either. He uses homeownership information from the 2000 Census to study the impact of the 1986 immigration reform. The long time gap between IRCA and outcome of interest prevents an analysis of the dynamic effects of the reform.

Even though we are unaware of any previous research explicitly considering a delayed homeownership response from a change in legal status, the typical timeline for a homebuying process coupled with the additional time needed to overcome distinct barriers faced by unauthorized immigrants appear consistent with a two-year lagged effect of DACA on homeownership.

Broadly speaking, a transition into homeownership consists of three stages: deciding to buy a house, planning or preparation, and doing (Lee et al., 2004). The planning or preparation stage involves gathering information about the homebuying process and local housing market, saving down payment, finding an agent, and getting pre-approval. Doing involves active house searching, making offers, negotiating, securing financing, and closing. An average buyer can expect to finish the doing process in about 6 months, although this timeline varies greatly depending on individual circumstances.[15]

With the passing of DACA, eligible immigrants likely develop a stronger preference for homeownership due to longer expected stay and feelings of more permanence in the US. This shift in mentality would motivate many to pursue homeownership. Homeownership, however, is unlikely to happen immediately because a house purchase often requires months or even years of planning. Minority immigrants, especially unauthorized ones, face various barriers to homeownership. This often means that it takes them longer to navigate the complicated homebuying process, which further delays homeownership attainment.

5.2.1 Down payment

The large financial cost of housing means that mortgage financing is often needed to buy a home in addition to a sizable down payment. Previous research notes that most Hispanic immigrants enter the country through gateway cities in California, New York, Texas, and Florida, where housing costs are higher than the national average (Clark and Blue, 2004; Coulson, 1999; James III and Atiles, 2008). These are also

[15] https://www.rocketmortgage.com/learn/how-long-does-it-take-to-buy-a-house; https://www.zillow.com/home-buying-guide/how-long-does-it-take-to-buy-a-house/

the top states where DACA recipients reside.[16] Thus, the saving period might be prolonged for DACA recipients due to them being concentrated in areas with expensive housing.

5.2.2 Mortgage financing

Obtaining mortgage financing is a critical but particularly challenging step for unauthorized immigrants (Bradley et al., 2007). Mortgage approval generally requires valid identification (such as a Social Security Number), proof of income and employment, and a good credit rating. Unauthorized immigrants struggle to meet these requirements because they are less likely to be integrated into the financial system (Amuedo-Dorantes et al., 2005; McConnell, 2015).

Immigrants are less likely to have bank accounts compared with natives, and unauthorized Latino immigrants are less likely to do so than authorized ones (Amuedo-Dorantes and Bansak, 2006; Hall and Greenman, 2013; McConnell, 2012; McConnell, 2013; Osili and Paulson, 2009). About 35 percent of Latino households and 40 to 50 percent of Latino immigrant households do not have a bank account (Hogarth et al., 2004; Pew Hispanic Center/Kaiser Family Foundation, 2002). This is not surprising considering the historically low usage of financial services in Latin American countries (Amuedo-Dorantes and Bansak, 2006).

Many Mexican immigrants, especially unauthorized ones, do not have formal relationships with US financial institutions. They use cash for payment or rely on alternative financial practices, i.e., they are unbanked. Without bank accounts or credit cards, they are unable to establish a credit history (Cortes et al., 2006; Lee et al.,2004; Ralph, 2010). Many factors contribute to this phenomenon, such as plans to return to Mexico, lack of interaction with banking services before migration, inability to speak English, concerns about proper documentation, minimum balance requirements, and vulnerability to banking fees.

Although banks increasingly accept alternative documents such as the Matrícula Consular identification card issued by the Mexican government and the Individual Tax Identification Number issued by the US Internal Revenue Service, there is variation among banks about the forms of identification they allow (Amuedo-Dorantes and Bansak, 2006). Many immigrants are uninformed or confused about these developments (Lee et al., 2004).

Generally, it takes a minimum of 6 months to establish credit from scratch, though lenders would like to see a longer credit history.[17] It might take longer to reach a score of 620, which is required for conventional loans.[18]

To sum up, credit access has been consistently identified as a primary barrier to Hispanic homeownership (Cortes et al., 2006). This is directly attributable to their lack of interaction with the mainstream finance system and lack of information about how to build and maintain a good credit profile. Some Hispanics, especially unauthorized ones, exist in a cash-only living outside the formal economy, which leaves them with a very thin or no credit history at all. Building sufficient credit to quality for a mortgage takes time.

5.2.3 Evidence of employment and income

[16] https://www.uscis.gov/sites/default/files/document/data/Active_DACA_Recipients%E2%80%93December31%2C2020.pdf
[17] https://www.discover.com/credit-cards/resources/starting-credit-score/;
https://www.fool.com/the-ascent/credit-cards/articles/how-much-credit-history-do-i-really-need/
[18] https://www.experian.com/blogs/ask-experian/what-does-my-credit-score-need-to-be-to-get-approved-for-a-mortgage/

Mortgage lenders typically use documentation such as pay stubs and tax returns to confirm employment and income. This helps them assess the borrower's risk and capacity. Hispanic immigrants, especially unauthorized ones, face additional challenges in documenting sustained income and employment histories. Without valid identification like a Social Security Number, many unauthorized immigrants are employed in the informal economy, where they are paid in cash and sometimes work in seasonal positions and change work frequently (McConnell and Marcelli, 2007).

The borrower is generally expected to provide evidence for steady employment and income from the last two years.[19] Even though exceptions can be made, it does not seem that most DACA recipients fit the typical workaround situations.[20]

To conclude, the homebuying process can be long and complicated. Unauthorized immigrants face additional barriers due to their considerable lack of information about the process and lack of interactions with the formal economy, which further disadvantages them during the mortgage process. Considering the time to save up down payments, build a credit history, accumulate records of sustained employment and income, and time to complete a house search, it is reasonable to expect a delayed homeownership effect of DACA by two years.[21] As mentioned previously, there did not appear to be any immigration or housing policy reforms around this time that could potentially affect DACA recipients differentially, nor was there apparent pre-event trend from the full event study analysis. Therefore, we are reasonably confident that the homeownership effect is attributable to the DACA program and unlikely biased by some other policy changes or pre-existing trends.

5.3 Heterogeneous effects

In addition to our main analysis with the full sample, we examine subsamples to investigate possible heterogeneity in the DACA effect in Table 3. We first split the main sample by gender to explore potentially different effects on homeownership between male and female immigrant householders. Columns [1] and [2] of Table 3 present the results for men and women, respectively. We find that DACA significantly increases homeownership for both genders with similar coefficient magnitudes for men (0.08) and women (0.073). Columns [3]-[4] split the sample based on marital status. DACA increased homeownership among both married and non-married household heads with similar coefficient estimates. We next break down the sample by education level. Results for high school educated household heads are presented in Column [5], while those for householders with at least some college education are reported in Column [6]. DACA significantly increases homeownership for both householders with and without some college education. We also divide the sample by income level. The results are presented in Columns [7] and [8], where low and high income levels are distinguished by the real median family income. We find positive homeownership effects of DACA for both income groups. Finally, we estimate subsamples of householders by metro status. Estimates from Columns [9]-[10] indicate that DACA has a significant effect for householders living in a metro area, while the coefficient estimate for non-metro residents is large but imprecisely estimated.

[19] http://www.homebuyinginstitute.com/mortgage/years-of-income-needed-for-mortgage/

[20] https://themortgagereports.com/19085/first-time-home-buyer-guide-buying-with-a-new-job-gina-pogol

[21] Note that USCIS administration data suggest that DACA approvals from initial applications were largest in 2013, and our event study shows a significant effect starting in 2015.

We also estimated triple difference models to examine if the DACA effect is statistically significantly different among various groups. Our estimation indicates that there are no significant differences by sex, marital status, education, income, or metro status. Thus, while there is some weakly suggestive evidence of possible heterogeneity, we interpret the sub-sample analysis to indicate that the effects are largely similar across groups.

5.4 Other outcomes

To address concerns that DACA could affect criminal behavior and institutionalization and alter the composition of household heads, we explore institutionalization as an additional outcome in Column [1] of Table A.1. We find that institutionalization is unaffected, consistent with the recent findings of Gunadi (2020).

Previous literature finds that DACA led to improved economic outcomes for eligible individuals. This positive impact could make them more likely to get married, start a family, and/or move into their own residence, which could affect their housing tenure choices. If those pushed into headship or household formation are otherwise more likely to rent, then our estimated effect of DACA on homeownership could be downwardly biased due to changing sample composition of household heads. Alternatively, if those pushed into headship are more likely to become homeowners, then household formation could be a potential factor driving DACA's homeownership effect. Therefore, we investigate the effect of DACA on household formation and related outcomes.

Specifically, we examine three outcomes: an indicator for being a household head or spouse, being married, and family size in Columns [2]-[4] of Table A.1. None of the coefficient estimates is statistically significant at the conventional levels. Therefore, we are reassured that our estimated homeownership effect does not occur due to changes in family structures. Similarly, we investigate if DACA affects mobility (between different houses) and migration decisions (between MSAs) in Columns [5]-[6] and fail to find any migration response to DACA in the short run (1 year).

5.5 Potential channels

Overall, we find that DACA raised homeownership among eligible household heads. What might account for the positive effect? First, the increase in homeownership may be attributable to the positive labor market effects documented in previous studies (Amuedo-Dorantes and Antman, 2016; Giuntella and Lonsky, 2020; Pope, 2016). We explore the labor market outcomes in Pope (2016) with a sample of householders and individuals in Tables A.2 and A.3, respectively. We confirm that DACA significantly raised the probability of currently having a job or had a job in the previous year. In addition, DACA increased labor force participation. DACA also boosted the number of hours worked and personal income of eligible individuals.[22] Together, the positive labor market effects could help eligible immigrants to save up down

[22] Even though we also use ACS data like Pope (2016), different sample restrictions led to quantitative differences in findings. Pope (2016) estimates four different samples under four different identification strategies. The closest one to ours is the second sample (Panel B of Table 2 in his paper), which utilizes the requirement that eligible people were under 31 in 2012. Comparing results in Table A.2 (household heads) with those in Pope (2016), we find that DACA led to a similar increase in the probability of working (0.042 vs. 0.044), and larger effects on the probability of being in the labor force (0.034 vs. 0.028), hours per week (1.577 vs. 1.184), and the probability of having worked in the past year (0.053 vs. 0.027). Further, we find DACA led to positive and significant increases in personal income

payment sooner, thus expediting the home purchasing decision. We add these labor market variables to our main specification to see how our estimated DACA effect might change in Table A.4. We note that the inclusion of the personal income variable in Column [4] led to a noticeable drop in the estimated DACA effect.

To further investigate whether income is an important driver for higher homeownership, we include real family income measures as control variables in Table 4. To accommodate the potential nonlinear effects of income, we also present estimates from a quadratic specification in Column [2], semi-log specifications in Columns [3]-[4], and consider income dummies in Columns [5]-[6]. We find that income positively affects homeownership, and the estimated effect of DACA on homeownership decreases by as large as 18% compared with the main specification. This suggests that higher family income is a contributing factor to the overall effect of DACA on homeownership.

Second, the relief from deportation and work authorization provided by DACA may have also increased eligible individuals' confidence of staying in the US in the long term, which motivates them to buy a home as opposed to renting. While we do not have direct measures of individuals' expected stay in the US, we explore outcomes related to expected duration in Table 5. Educational attainment and English language skills are likely affected by expected duration. Those expecting to stay longer and more permanently in the US are likely to invest more in higher education and English skills to improve labor market outcomes and access to consumption, leisure, and social opportunities. Kuka et al. (2020) find that DACA has positive impacts on high school attendance, high school graduation rates, and suggestive positive impacts on college attendance. We also find that DACA raises education attainment beyond high school. Additionally, we find that DACA raises the probability of speaking English.

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and the probability of being in school, as opposed to insignificant results in Pope (2016). Pope (2016) finds that DACA significantly decreases the probability of being unemployed, while our estimate is negative and insignificant.

The DACA effect by gender in Pope (2016) utilizes a different identification strategy: being younger than 16 years old when entering the US. Our female subsample estimation generated qualitatively similar, but quantitatively larger estimates for all significant outcomes. We additionally find positive and significant increases in the probability of getting a GED following DACA. For male subsample analysis, we have smaller estimates for the probability of working, being in the labor force, and having worked last year. We fail to find significant effects on being unemployed, hours per week, and being in school.

Similar patterns exist when we compare our results in Table A.3 (individuals) with those in Pope (2016), larger estimates from our female subsample and smaller estimates from our male subsample. When looking at the full sample results, our estimates are larger in magnitudes compared with Panel B of Table 2 in Pope (2016), except we do not consistently find a negative effect on being unemployed and consistently find a positive effect on being in school.

Amuedo-Dorantes and Antman (2017) also investigated the labor market and schooling outcomes of DACA. Due to the many differences in sample criteria and empirical design, our results differ more from theirs. We find DACA raised the probability of school enrollment while Amuedo-Dorantes and Antman (2017) find the opposite. We also consistently find that DACA eligibility led to positive labor market outcomes, while Amuedo-Dorantes and Antman (2017) find some evidence of a higher probability of being employed for men. Our estimated magnitudes differ greatly as well: we find the probability of working went up by about 4-5 percentage points associated with DACA eligibility and about 2-3 percentage points for men, while Amuedo-Dorantes and Antman (2017) find the increase in the likelihood of being employed is around 10 percentage points.

Third, DACA likely reduces barriers unauthorized immigrants face in the credit market. Before DACA, unauthorized immigrants could borrow from family/friends and lenders that require less documentation regarding legal status and formal income. DACA increases access to credit for qualified immigrants in several ways. First, DACA provides renewable legal status and protection from deportation, which directly lowers the risk to lenders from lending to the DACA population since deportation would likely reduce an individual's ability to repay a loan. Second, DACA grants recipients work authorization and allows for the application of an SSN. Having an SSN facilitates access to credit cards and makes it easier for DACA recipients to build their credit history. Third, formal employment and credit history help DACA beneficiaries qualify for conventional loans.[23] DACA recipients were also able to get FHA loans before the Trump Administration excluded them from FHA.[24] More recently, DACA recipients officially qualify for mortgages backed by the FHA.[25]

In general, there are two ways to finance a home purchase: buying a home with cash (free and clear ownership) and buying a home through borrowing (ownership with a mortgage or loan). We explore the effects of DACA on homeownership by type of financing with linear probability models in Table 6. The dependent variables in Columns [1]-[2] are indicator variables representing two different housing situations: homeownership through a mortgage, and free and clear ownership, respectively. Our model predicts how DACA affects the likelihood of being in each category. The sample is the same as that in Table 2, so we are decomposing the overall effect of DACA on homeownership into the effects on homeownership with and without a mortgage.

We see that DACA significantly increases the likelihood of buying a home through a mortgage. Specifically, being DACA eligible is associated with an 8-percentage-point increase in the likelihood of ownership through a mortgage, 31% relative to the sample mean. Column [2] has a -0.012 coefficient estimate that is not statistically significant. DACA does not appear to have significantly affected free and clear homeownership among eligible household heads. Many households lack sufficient wealth to purchase a home without a loan, so mortgage access is critical. Thus, homeownership with a mortgage more than fully accounts for the overall increase in homeownership. Further, we find that DACA raised average real monthly first mortgage payments by 56 dollars in Column [3] of Table 6. This suggests that DACA allowed recipients to obtain larger mortgage loans.

We next utilize the Federal Home Loan (FHL) Bank public use data to further explore mortgage access.[26] The FHL dataset is available from 2009 to 2019 and reports information on approved mortgages. Panel A

[23] https://www.stilt.com/blog/2019/09/daca-home-loans/

[24] https://nahrep.org/nahrep-in-the-news/2020/06/12/an-administrative-trainwreck-documents-show-chaos-over-whether-dreamers-qualify-for-fha-loans/
https://www.housingwire.com/articles/49326-hud-declares-fha-is-no-longer-backing-daca-mortgages/
https://www.buzzfeednews.com/article/nidhiprakash/daca-trump-denied-federal-housing-loans

[25] https://www.hud.gov/sites/dfiles/SFH/documents/SFH_FHA_INFO_21-04.pdf

[26] Identifying unauthorized immigrants is even more challenging in the FHL data than ACS data because the FHL data do not report the citizenship status of the borrowers. This means our treatment group probably includes citizens who should not be affected by DACA. This biases our FHL results toward zero. We also explored the publicly available Home Mortgage Disclosure Act (HMDA) data from 2007-2017, which is the more comprehensive mortgage database because it includes all mortgage applications, not just approved ones. We focus our analysis on California where a large number of DACA recipients live and define being Hispanic as being DACA eligible. We find that DACA eligibility is positively associated with conventional loans being originated (i.e., the consumer obtained a mortgage) for owner-occupied primary residence purchases, controlling for race, gender, MSA-by-year fixed effects (or tract-by-year fixed

of Table 7 restricts the sample to Hispanic applicants ages 18-60 whose loan is for their primary residence and defines DACA eligibility as being younger than 31 in 2012; our DID approach indicates a significant positive effect on the loan being conventional. There is also a significant positive effect on having a credit score above 660. Panel B reports results from an alternative identification strategy where we restrict the sample to people younger than 31 in 2012 and define DACA eligibility as being Hispanic. We again find significant positive effects on having a conventional loan and credit score above 660.

Thus, we find evidence that there were more conventional loans used and better credit scores following DACA. It is difficult to untangle underlying mechanisms on the credit supply side (better access and more credit availability) from the credit demand side (more loan applications from more people wanting to buy homes due to longer expected duration in the US and higher incomes). However, the available evidence does suggest the credit access channel to explain at least some of the increase in homeownership due to DACA.

6. Robustness checks

We perform several robustness checks to examine the sensitivity of our main results, which are reported in Table 8.[27] In Column [1], we add unemployment rate and housing price index controls at the MSA level to our main specification. The sample size decreases somewhat due to excluding observations in non-metro areas and unidentifiable metro areas. The point estimate for the effect of DACA on homeownership increases slightly.

Following Goodman-Bacon (2016) and Kuka et al. (2020), we also implement a two-step approach to account for potential differential pre-trends. This procedure involves first estimating a pre-trend for all the variables from the pre-DACA period, then removing the trend from the data for the full sample period and estimating Eq. (1) on the de-trended data. Based on the nonlinear trends in homeownership before DACA shown in Figure 1, we estimate a quadratic trend instead of a linear trend. Results are reported in Column [2] of Table 8. The DACA point estimate is again slightly larger compared with our main specification.

To account for the eligibility status of spouses, we refine our definition of being DACA eligible for married couples to include the scenario where the householder is ineligible, but the spouse is eligible. The difference between this estimation and our main analysis is that we consider spouse eligibility, but the analysis is still restricted to only household heads. In this robustness check, we define a household head being eligible in two circumstances: either the head is eligible, or the head's spouse is. Since home purchasing is generally a family decision, a spouse being eligible for DACA potentially leads to the same outcome as the head being eligible. In the main analysis, the head is eligible only if he/she meets all DACA requirements regardless of the eligibility status of their spouses. In this case, an ineligible head who became a homeowner due to the spouse being eligible is placed into the control group, which biases the

effects). Additionally, DACA lowers the probabilities of loans being denied for debt income ratio, employment history, credit history, insufficient cash, and unverifiable information reasons. However, age information is suppressed in HMDA (Even when age information was released in the most recent surveys, it is in bins such as 25-34 years old), hindering its usefulness for us. Therefore, we only report results with the FHL data here.

[27] Although not reported, our results are also robust to the following changes to the main specification: (1) dropping the year 2012; (2) dropping veterans; (3) restricting the sample period to 2009-2019.

estimated DACA effect downwards. We report the results in Column [3]. Our estimated homeownership effect remains statistically significant and is of similar magnitude compared with the main analysis.

Following Borjas and Cassidy (2019), we consider using the residual approach to identify unauthorized immigrants in Column [4]. The residual method starts with foreign-born individuals, identifies those that are probably legal, and then specifies the remaining as unauthorized. In Column [5], we focus on a narrow window around DACA implementation (2009-2015) with a sample of householders of a smaller age range (27-34) around the eligibility threshold. Column [6] limits the sample to just California residents, the state with the most DACA recipients. Our estimated DACA effect is qualitatively robust to these specifications.

As our sample period spans the financial crisis, there might be some concern that the estimated DACA effect may result from the crisis scarring the older cohorts (who had higher homeownership rates before DACA) more significantly than the younger cohorts. To circumvent this issue, we employ another identification strategy: we use naturalized citizens of similar ages (28-30) and characteristics (defined as meeting all DACA criteria) observed in the same year as the control group. In this setup, the treatment and control groups have different citizenship status but are otherwise similar. Our results in Column [7] are qualitatively similar to our main result, which mitigates concerns that age or cohort effects might confound our main findings.

Overall, our main results are highly robust to different identification methods, definitions of treatment, and sample selections. Together, they strongly support the notion that the increased homeownership rate of eligible unauthorized immigrants resulted from the implementation of DACA as opposed to potential confounders.

7. Falsification tests

We perform two falsification tests in the last two columns of Table 8. To alleviate concerns regarding an age catch-up effect where the younger cohorts start out at a lower level and might be expected to catch up with older cohorts in homeownership rates as they age, we conduct falsification tests using a sample of Mexican-born naturalized citizens who are not supposed to be impacted by DACA in Column [8]. Here, we compare DACA "eligible" naturalized citizens to ineligible naturalized citizens with a similar identification strategy as our main analysis, except that these householders would not actually be eligible based on their citizenship status. As expected, we fail to find any statistically significant effect from the DACA placebo in Column [8]. In Column [9], we restrict the sample to noncitizens from Canada, the UK, Germany, Australia, and New Zealand. Noncitizens from these countries were very likely to be legal residents and thus very unlikely to be eligible for and affected by DACA. We otherwise apply the same sample inclusion criteria, identification strategy, and specification as our main analysis. As expected, we fail to find a significant policy effect from the DACA placebo in Column [9]. These results reinforce that the homeownership effect for DACA eligible householders comes from the policy rather than other factors.

8. Conclusion

Immigrants are an important part of the US economy, and immigration policy continues to be a topic of heated debate as it not only affects the lives of millions of immigrants but also those of Americans. The 2012 Deferred Action for Childhood Arrivals (DACA) program has received considerable attention in academia and media as the largest immigration reform in recent years. In this paper, we examine the impact of DACA on the homeownership rates of unauthorized immigrants.

Our identification strategy uses a difference-in-differences design comparing eligible with similar ineligible immigrants. We find that even a temporary legalization program like DACA has significant positive effects on homeownership. Our results suggest that DACA-eligible householders are 7 percentage points more likely to be a homeowner. We further conduct event analysis that rules out pre-DACA confounding trends. We also consider possible mechanisms and find suggestive evidence that income effects, improved credit access, and increased expected permanence in the US are all contributing factors driving the effect of DACA on homeownership.

As immigration continues to change America's demographic landscape and its demand for housing, understanding the housing market outcomes of immigration reforms carries important implications for policymakers. Our findings suggest that legalization not only helps unauthorized immigrants achieve their American dreams but also benefits their families and communities.

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Table 1
Summary statistics.

Variables	Pre-DACA period				Post-DACA period			
	Mean		Difference	t-statistic	Mean		Difference	t-statistic
	Eligible	Ineligible			Eligible	Ineligible		
Homeownership	0.27	0.47	-0.20	-18.88	0.31	0.42	-0.11	-8.98
Mortgage	0.21	0.40	-0.19	-19.72	0.20	0.28	-0.08	-7.35
Age	25.45	32.57	-7.12	-115.30	28.67	37.38	-8.71	-149.81
Age at arrival	10.71	13.89	-3.18	-33.01	9.68	13.10	-3.42	-36.31
Male	0.51	0.61	-0.10	-9.27	0.48	0.55	-0.07	-6.18
Years of education	12.61	12.58	0.03	1.20	12.74	12.61	0.13	4.31
Veteran	0.008	0.009	-0.001	-0.31	0.004	0.008	-0.004	-1.88
Married	0.48	0.61	-0.13	-12.23	0.48	0.59	-0.11	-8.28
White	0.58	0.55	0.03	2.42	0.60	0.63	-0.03	-2.69
Family size	3.51	4.10	-0.59	-15.78	3.65	4.24	-0.59	-13.57
Family income	26,496	32,908	-6,412	-10.42	30,694	33,653	-2,959	-4.07
Observations	3,493	4,929			9,754	1,896		
MSA unemployment rate	8.87	7.69	1.18	15.41	5.31	6.23	-0.92	-15.06
MSA Housing price index	136.95	154.69	-17.74	-21.22	179.93	173.02	6.91	6.48
Observations	3,036	4,500			8,355	1,692		

Notes: Summary statistics are based on authors' tabulations from the IPUMS ACS 2005-2019. Our sample consists of Mexican-born non-citizen household heads ages 23-40 who entered the US under the age of 16 before 2007 and have at least a high school degree (or equivalent) or are a veteran. Homeownership and mortgage are dummy variables that indicate whether the person is a homeowner and has a mortgage, respectively. Similarly, male, veteran, employed, and married are indicators for whether the person is male, a veteran, employed, or married.

Table 2

The effect of DACA on homeownership.

	[1]	[2]	[3]	[4]	[5] Main	[6] Alternative
DACA_Eligible	0.075**	0.044*	0.066***	0.060**	0.070***	0.052**
	(0.028)	(0.025)	(0.024)	(0.024)	(0.024)	(0.023)
Eligible	-0.181***	-0.182***	-0.040	-0.036	-0.041	-0.043
	(0.014)	(0.014)	(0.024)	(0.023)	(0.025)	(0.027)
Male			0.029***	0.029***	0.030***	0.029***
			(0.007)	(0.007)	(0.007)	(0.009)
Race_white			-0.097**	-0.101**	-0.096***	-0.030
			(0.037)	(0.038)	(0.034)	(0.019)
Mean of dep. var.	0.354	0.354	0.354	0.354	0.354	0.303
Observations	20,072	20,072	20,072	20,072	20,072	18,635
R-squared	0.022	0.071	0.091	0.094	0.129	0.100

Notes: Table 2 reports estimates from Eq. (1) with homeownership being the outcome variable. The first row reports coefficient estimates for our variable of interest (DACA_Eligible). Column [1] reports results with no control variables, while Columns [2]-[4] successively add state and year fixed effects, demographic controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white), and state-specific linear time trends. Column [5] presents results from our main specification including demographic controls with state-by-year fixed effects. In Column [6], we limit our sample to noncitizen household heads ages 23-40 who meet all observable DACA criteria including age at arrival, age in 2012, year of immigration, and education requirements, then define treatment based on the country of origin with Mexicans being eligible and assume that immigrants from other countries are ineligible. We exclude noncitizens from Central and South America from this analysis because those are the top countries of birth among DACA recipients (after Mexico). Including them would especially contaminate the control group. That is, we compare potentially eligible Mexican immigrants to potentially eligible non-Mexican immigrants. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 3
Effects of DACA on homeownership by sub-sample.

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
	Male	Female	Unmarried	Married	High school	College	Low income	High income	Non-metro	Metro
DACA_Eligible	0.080***	0.073**	0.061***	0.059*	0.072**	0.088*	0.051**	0.058**	0.098	0.073***
	(0.028)	(0.033)	(0.020)	(0.031)	(0.034)	(0.048)	(0.021)	(0.024)	(0.127)	(0.020)
Mean of dep. var.	0.378	0.329	0.263	0.437	0.345	0.377	0.243	0.466	0.490	0.336
Observations	10,511	9,561	9,531	10,541	13,959	6,113	10,046	10,026	2,370	17,702
R-squared	0.160	0.152	0.132	0.170	0.151	0.162	0.153	0.159	0.300	0.133

Notes: Table 3 reports estimates from Eq. (1) with homeownership being the outcome variable for subsamples indicated in column headings. Specifically, we split the sample along the following household head characteristic pairs: gender (Columns [1] and [2]), marital status (Columns [3] and [4]), education (Columns [5] and [6]), real family income (Columns [7] and [8]), and whether the household head lives in a metro area or not (Columns [9] and [10]). Regressions include state-by-year fixed effects and individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) except when collinear due to the subsample. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$. None of the pairs of coefficients are statistically significantly different from each other.

Table 4

Income as a potential channel for the DACA effect on homeownership.

	[1] Linear	[2] Quadratic	[3] Semilog 1	[4] Semilog 2	[5] Quintile dummies	[6] Percentile dummies
DACA_Eligible	0.061***	0.058**	0.062***	0.065***	0.055**	0.053**
	(0.022)	(0.022)	(0.022)	(0.024)	(0.021)	(0.021)
Income	3.78E-06***	4.69E-06***				
	(3.73E-07)	(2.95E-07)				
Income^2		-4.92E-12***				
		(1.55E-12)				
Ln (income)			0.126***			
			(0.006)			
Ln (income+1)				0.032***		
				(0.002)		
Income dummy 2					0.029***	
					(0.011)	
Income dummy 3					0.133***	
					(0.017)	
Income dummy 4					0.210***	
					(0.013)	
Income dummy 5					0.324***	
					(0.011)	
Mean of dep. var.	0.354	0.354	0.358	0.354	0.354	0.354
Observations	20,072	20,072	19,500	20,072	20,072	20,072
R-squared	0.171	0.178	0.175	0.144	0.187	0.194

Notes: Table 4 reports the homeownership effect of DACA based on Eq. (1) controlling for real family income via alternative specifications. Column [1] presents coefficient estimates where income is modelled linearly, while Column [2] applies a quadratic specification. Columns [3]-[4] both consider the semi-log specification, where we take the natural log of real family income directly in Column [3] and take the natural log of real family income plus one in Column [4]. In Column [5], we include 5 income dummy variables where we divide real family income into quintiles. In Column [6], we use the entire population (including non-immigrants) to construct income percentile cutoffs so that the changing composition of the sample does not influence the cutoffs, and include 99 dummies for income percentile group. The first row reports coefficient estimates for our variable of interest (DACA_Eligible). All regressions include additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) and state-by-year fixed effects. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 5

Human capital investment outcomes related to expected duration.

	[1] Beyond HS	[2] Speak English
DACA_Eligible	0.042**	0.015***
	(0.019)	(0.005)
Mean of dep. var.	0.384	0.971
Observations	20,072	20,072
R-squared	0.091	0.066

Notes: Table 5 reports effects of DACA on educational attainment and English-speaking ability. Column [1] reports the effect of DACA on whether the person achieved education beyond high school. Column [2] examines the effect of DACA on English language ability. All regressions include additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) and state-by-year fixed effects. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 6

Homeownership financing in the ACS data.

	[1] Own with mortgage	[2] Own free and clear	[3] First mortgage payment
DACA_Eligible	0.082***	-0.012	56.598**
	(0.016)	(0.014)	(20.830)
Mean of dep. var.	0.258	0.096	849.267
Observations	20,072	20,072	5,180
R-squared	0.111	0.089	0.446

Notes: Table 6 reports the effects of DACA on homeownership financing with the following indicator variables as outcomes: homeownership with a mortgage (Column [1]) and free and clear homeownership (Column [2]). Column [3] further examines the effect of DACA on monthly first mortgage payment in real dollars. The sample in Column [3] is restricted to household heads with monthly first mortgage payments, which results in a much smaller sample. All regressions include additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) and state-by-year fixed effects. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 7

Federal Home Loan (FHL) Bank public use data.

	[1] Conventional	[2] Credit score	[3] Conventional	[4] Credit score
Panel A: Hispanic Sample				
DACA_Eligible	0.128***	0.090***	0.133***	0.093***
	(0.028)	(0.019)	(0.029)	(0.019)
Mean of dep. var.	0.822	0.900	0.822	0.900
Observations	13,552	13,552	13,552	13,552
R-squared	0.322	0.119	0.313	0.114
Panel B: Young Person Sample				
DACA_Eligible	0.153***	0.135***	0.153***	0.135***
	(0.049)	(0.046)	(0.050)	(0.046)
Mean of dep. var.	0.847	0.947	0.847	0.947
Observations	121,414	121,414	121,414	121,414
R-squared	0.300	0.056	0.293	0.054

Notes: Table 7 reports the effect of DACA on credit related outcomes with the FHL data, which records mortgages purchased by each Federal Home Loan Bank and associated loan-level characteristics. Columns [1] and [3] examine whether the borrower used conventional loans, while Columns [2] and [4] investigate if the borrower's credit score is above 660. In Panel A, we restrict the sample to Hispanic applicants ages 18-60 whose loans are for primary residence purchases and define DACA eligibility as being younger than 31 in 2012. In Panel B, we restrict the sample to people younger than 31 in 2012 and define DACA eligibility as being Hispanic. All regressions include additional individual controls (age fixed effects, sex, and an indicator for race being white) and state-by-year fixed effects; Columns [1]-[2] also control for real personal income. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 8
Robustness and Falsification Checks.

	[1] MSA controls	[2] Two-step detrend	[3] Either eligible	[4] Borjas sample	[5] Ages 27-34 2009-2015	[6] California residents	[7] Similar naturalized	[8] Placebo 1	[9] Placebo 2
DACA_Eligible	0.072***	0.080***	0.072***	0.059***	0.080**	0.062**	0.064***	0.013	0.029
	(0.019)	(0.026)	(0.023)	(0.021)	(0.036)	(0.026)	(0.023)	(0.027)	(0.038)
Mean of dep. var.	0.337	0.207	0.354	0.310	0.326	0.260	0.420	0.577	0.442
Observations	17,583	20,072	20,072	14,546	4,625	7,316	16,248	15,225	1,930
R-squared	0.139	0.253	0.129	0.133	0.122	0.063	0.078	0.142	0.168

Notes: Table 8 presents numerous robustness and falsification checks following Eq. (1) on the homeownership effect of DACA. Column [1] reports results adding unemployment rate and housing price index controls at the MSA level for household heads residing in MSAs. Column [2] presents results from a two-step approach, where we first detrend the data based on trends from the pre-DACA period and then estimate Eq.(1). Column [3] reports estimates where we redefine DACA eligibility considering the eligibility of spouses, i.e., a married household head is eligible if he or his spouse is. In Column [4], we adopt the residual method of imputing unauthorized status proposed in Borjas and Cassidy (2019). Column [5] focuses on a narrow window around DACA implementation on a sample of householders of smaller age range (27-34) from 2009 to 2015, whereas Column [6] restricts analysis to California residents. Column [7] uses naturalized citizens of similar ages (28-30) as the control group instead of older non-citizens. Column [8] performs a falsification test with a sample of Mexican-born naturalized citizens, while Column [9] conducts a placebo test on a sample of non-citizens born in Canada, UK, Germany, Australia, and New Zealand. All regressions include additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) and state-by-year fixed effects. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table A.1
Other outcomes.

	[1] Institutionalized	[2] Head or Spouse	[3] Marital status	[4] Family size	[5] Mobility	[6] Migration
DACA_Eligible	0.003	0.014	0.015	-0.056	0.009	0.001
	(0.004)	(0.009)	(0.016)	(0.050)	(0.015)	(0.008)
Mean of dep. var.	0.025	0.547	0.470	4.228	0.792	0.032
Observations	58,371	58,371	58,371	58,371	20,072	20,072
R-squared	0.041	0.360	0.084	0.044	0.084	0.081

Notes: Table A.1 reports the effect of DACA on other outcomes. Columns [1]-[4] report the effect of DACA on being institutionalized, being a household head or spouse, being married, and family size. Columns [5] and [6] examine the effect of DACA on mobility (whether the respondent stayed in the same house) and MSA migration (whether the respondent moved to a different MSA) during the previous year. Columns [1]-[4] use samples of individuals, while Columns [5]-[6] use samples of household heads. All regressions include additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) and state-by-year fixed effects. Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table A.2
DACA and labor market outcomes (household heads).

	[1] Working	[2] Labor force	[3] Unemployed	[4] Income	[5] Hours per week	[6] Worked last year	[7] Self-employed	[8] School	[9] GED
Panel A: householders									
DACA_Eligible	0.045***	0.037**	-0.013	2,268.027***	1.716***	0.056***	-0.006	0.031***	0.014
	(0.012)	(0.015)	(0.016)	(526.480)	(0.439)	(0.010)	(0.009)	(0.010)	(0.013)
Mean of dep. var.	0.817	0.859	0.049	18843.66	34.896	0.872	0.086	0.103	0.060
Observations	20,072	20,072	17,233	20,072	20,072	20,072	18,464	20,072	20,072
R-squared	0.156	0.160	0.065	0.174	0.234	0.157	0.055	0.058	0.068
Panel B: men									
DACA_Eligible	0.028**	0.016*	-0.013	2,372.445**	0.682	0.021***	-0.009	0.026*	0.001
	(0.013)	(0.009)	(0.011)	(1,023.708)	(0.794)	(0.006)	(0.011)	(0.014)	(0.015)
Mean of dep. var.	0.934	0.967	0.034	24532.4	41.583	0.976	0.094	0.080	0.055
Observations	10,511	10,511	10,164	10,511	10,511	10,511	10,392	10,511	10,511
R-squared	0.080	0.071	0.075	0.106	0.093	0.073	0.077	0.084	0.104
Panel C: women									
DACA_Eligible	0.062***	0.056*	-0.019	1,216.124*	2.696***	0.099***	0.002	0.032*	0.028*
	(0.021)	(0.030)	(0.026)	(643.104)	(0.581)	(0.020)	(0.020)	(0.017)	(0.014)
Mean of dep. var.	0.687	0.739	0.071	12589.67	27.544	0.758	0.075	0.128	0.066
Observations	9,561	9,561	7,069	9,561	9,561	9,561	8,072	9,561	9,561
R-squared	0.092	0.093	0.098	0.099	0.093	0.090	0.089	0.074	0.098

Notes: Table A.2 replicates labor market effects of DACA in Pope (2016) with householder samples. Panel A uses all householders, while Panel B and C present results for men and women separately. Columns [1]-[3] examine whether the person is working, in the labor force, or unemployed, respectively. Columns [4]-[5] investigate real total personal income and hours of work per week, respectively. Columns [6]-[9] analyze if the person worked last year, is self-employed, attending school, or obtained a GED. All regressions include state-by-year fixed effects and additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white). Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table A.3
DACA and labor market outcomes (individuals).

	[1] Working	[2] Labor force	[3] Unemployed	[4] Income	[5] Hours per week	[6] Worked last year	[7] Self-employed	[8] School	[9] GED
Panel A: individuals									
DACA_Eligible	0.053***	0.042***	-0.017	1,375.216***	1.949***	0.053***	-0.006	0.034***	0.016
	(0.008)	(0.009)	(0.011)	(447.072)	(0.413)	(0.005)	(0.010)	(0.007)	(0.009)
Mean of dep. var.	0.755	0.811	0.069	15612.08	31.924	0.819	0.072	0.118	0.051
Observations	56,891	56,891	46,149	56,891	56,891	56,891	50,071	56,891	56,891
R-squared	0.114	0.123	0.036	0.144	0.171	0.118	0.033	0.056	0.033
Panel B: men									
DACA_Eligible	0.021**	0.008	-0.015*	1,464.981*	1.062**	0.020***	-0.011	0.030***	0.012
	(0.009)	(0.005)	(0.008)	(738.119)	(0.477)	(0.007)	(0.014)	(0.009)	(0.010)
Mean of dep. var.	0.882	0.934	0.055	20190.45	38.486	0.936	0.080	0.098	0.050
Observations	29,521	29,521	27,565	29,521	29,521	29,521	28,388	29,521	29,521
R-squared	0.048	0.039	0.045	0.092	0.061	0.041	0.046	0.060	0.049
Panel C: women									
DACA_Eligible	0.082***	0.076***	-0.021	765.559**	2.629***	0.081***	-0.0004	0.037***	0.018*
	(0.012)	(0.018)	(0.021)	(368.588)	(0.578)	(0.012)	(0.011)	(0.007)	(0.009)
Mean of dep. var.	0.619	0.679	0.088	10673.9	24.847	0.693	0.062	0.139	0.052
Observations	27,370	27,370	18,584	27,370	27,370	27,370	21,683	27,370	27,370
R-squared	0.048	0.050	0.059	0.065	0.049	0.047	0.053	0.063	0.048

Notes: Table A.3 replicates labor market effects of DACA in Pope (2016) with samples of all individuals. Panel A presents results for the full sample, while Panel B and C present results for men and women separately. Columns [1]-[3] examine whether the person is working, in the labor force, or unemployed, respectively. Columns [4]-[5] investigate real total personal income and hours of work per week, respectively. Columns [6]-[9] analyze if the person worked last year, is self-employed, attending school, or obtained a GED. All regressions include state-by-year fixed effects and additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white). Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

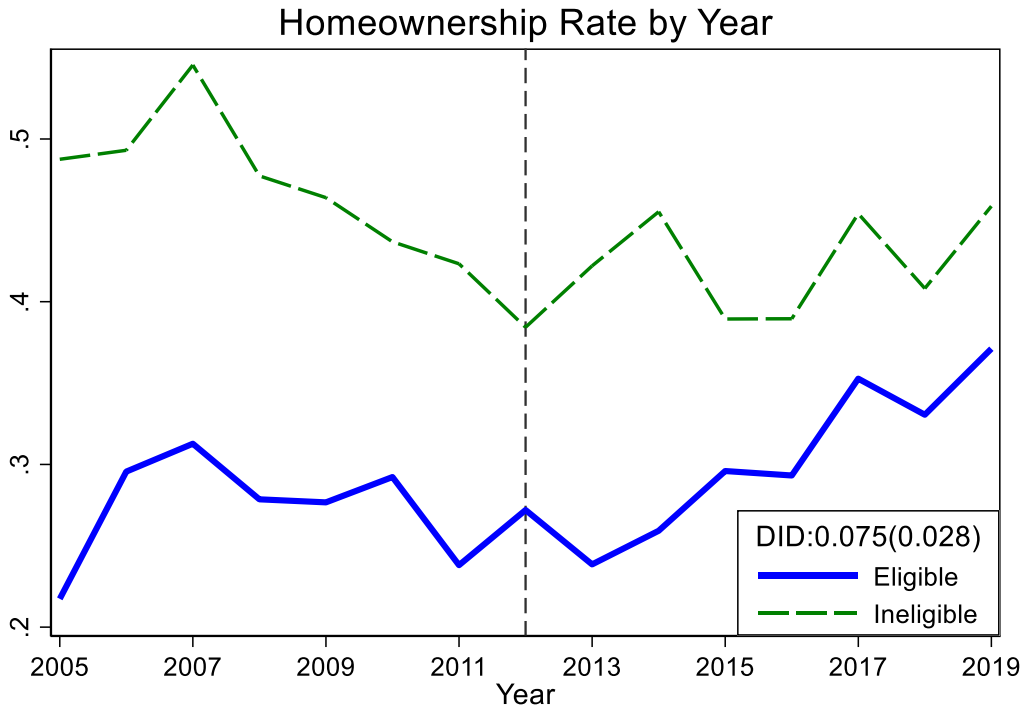
Table A.4

Effect of DACA on homeownership controlling for additional labor market channels.

	[1] Working	[2] Labor force	[3] Unemployed	[4] Income	[5] Hours per week	[6] Worked last year	[7] Self-employed	[8] School	[9] GED	[10] All
DACA_Eligible	0.069***	0.069***	0.071**	0.062**	0.068***	0.069***	0.074***	0.068***	0.070***	0.063**
	(0.024)	(0.024)	(0.026)	(0.024)	(0.025)	(0.024)	(0.024)	(0.024)	(0.024)	(0.026)
Mean of dep. var.	0.354	0.354	0.360	0.354	0.354	0.354	0.359	0.354	0.354	0.359
Observations	20,072	20,072	17,233	20,072	20,072	20,072	18,464	20,072	20,072	18,464
R-squared	0.130	0.129	0.133	0.145	0.130	0.129	0.130	0.131	0.129	0.153

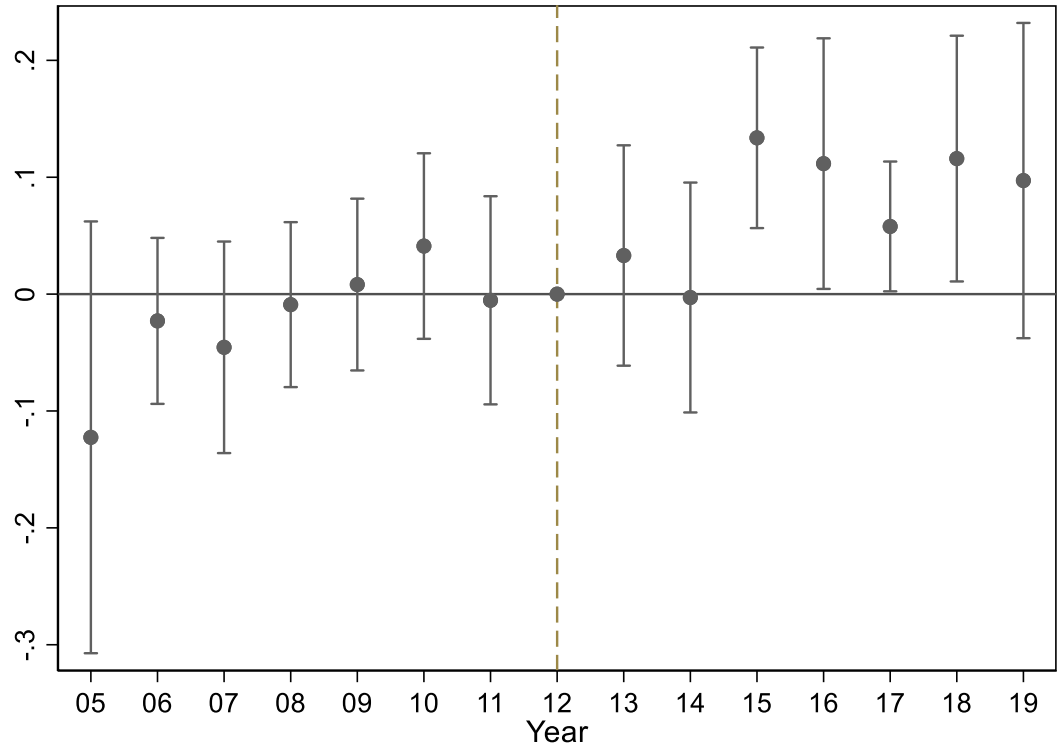
Notes: Table A.4 estimates DACA effects following Eq.(1) controlling for additional labor market channels. Columns [1]-[3] add indicators for whether the person is working, in the labor force, or unemployed, respectively. Columns [4]-[5] add real total personal income and hours of work per week, respectively. Columns [6]-[9] control for whether the person worked last year, is self-employed, attending school, or obtained a GED, while Column [10] includes all labor market controls except unemployed (to avoid multicollinearity). All regressions include state-by-year fixed effects and additional individual controls (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white). Standard errors are in parentheses and clustered at the state level. $*$ $p < 0.1$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Figure 1
Trends in homeownership for DACA-eligible and DACA-ineligible groups.



Notes: Figure 1 illustrates the evolution of homeownership rates for DACA-eligible Mexican non-citizen householders and their ineligible counterparts. The sample includes Mexican-born non-citizen household heads ages 23-40 who entered the US under the age of 16 before 2007 and have at least a high school degree (or equivalent) or are a veteran. DACA eligible householders were age 30 or younger in 2012; ineligible householders were 32 or older in 2012. Thus, 2005 includes only one DACA eligible cohort, householders age 23 in 2005 and age 30 in 2012. Similarly, 2019 includes only two ineligible cohorts, householders ages 39-40 in 2019 and ages 32-33 in 2012. The dotted vertical line indicates DACA implementation in 2012. Results from a simple DID estimation (without control variables) are presented in the bottom right. The standard error is in parentheses.

Figure 2
Event study analysis of the homeownership effect.



Notes: Figure 2 presents coefficient estimates and 95% confidence intervals of the homeownership effect using an event study analysis. The outcome is an indicator for being a homeowner. We interact DACA eligibility indicator with each year (2012 being the base year). The analysis additionally controls for individual characteristics (sex, age fixed effects, age-at-arrival fixed effects, and an indicator for race being white) and state-by-year fixed effects. Standard errors are clustered at the state level.