DACA, Mobility Investments, and Economic Outcomes of Immigrants and Natives*

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Abstract

Exploiting variation created by Deferred Action for Childhood Arrivals (DACA), we document the effects of immigrant legalization on immigrant mobility investments and economic outcomes. We provide new evidence that DACA increased both geographic and job mobility of young immigrants, often leading them to high paying labor markets. We then examine whether these gains to immigrants spillover and affect labor market outcomes of US-born workers. Exploiting Mexican state migration networks and natural disasters that induce migration, we show that US-born workers in labor markets that are more exposed to the DACA-induced shock see little to no change in employment rates and actually observe increases in wage earnings after DACA's implementation, suggesting immigrant workers complement native workers and immigrant legalization generates broader local labor market benefits.

Keywords: Legal Status, DACA, immigration, natives geographic mobility, job mobility, local labor markets
JEL Codes: J15, K37, R23

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1 Introduction

A large literature shows that "mobility" human capital investments like geographic mobility and job mobility improve economic outcomes like employment and earnings.[1] For example, by moving, individuals can encounter communities and labor markets that more closely match their skills or preferences, and improve outcomes in expectation. Similarly, job, occupation, and industry mobility can provide an opportunity for workers to match with better suited jobs and move up the wage ladder. Workers might be less willing to make these mobility investments when there is uncertainty that they will reap the return on these investments. This is particularly relevant for the more than 11.4 million unauthorized immigrants in the United States who lack legal work status and face uncertainty about their future residency (Baker, 2021). The risk of deportation might discourage individuals from engaging in costly geographic or occupational moves, as it decreases the probability the worker experiences the return to the costly adjustment. Although we have evidence of how legalization affects educational attainment (Amuedo-Dorantes and Antman, 2016; Hsin and Ortega, 2018; Kuka et al., 2020), we do not know how it affects mobility human capital investments. These investments might not only increase individual productivity, but could affect local labor market dynamisms, leading to aggregate effects. With a large unauthorized population, as in the US, this could generate large externalities that affect citizens and other legal residents.

We want to understand how providing legal status and reducing the risk of deportation affects immigrants' geographic and occupational mobility. This can help contextualize the effects of legalization on recipients' economics outcomes. We also want to understand how providing legal status to unauthorized immigrants affects the labor market outcomes of native workers. These externalities could be both positive and negative, making it un-

[1] For geographic mobility examples, see (Briggs and Kuhn, 2008; Deryugina et al., 2018; Groen et al., 2020; Jia et al., forthcoming; Nakamura et al., forthcoming; Sjaastad, 1962). For job mobility examples see (Bartel and Borjas, 1981; Topel and Ward, 1992).

clear how granting legal status affects overall social welfare. Removing uncertainty could increase unauthorized immigrants' willingness to engage in costly investments (like moving), leading to increased individual productivity and potential positive spillovers on aggregate productivity. Alternatively, increased human capital and employment opportunities among immigrants could hurt native workers if they become substitutes in the production process. As a policy, legalization might provide economic benefits for immigrants, but we are also interested in understanding to what extent US-born workers bear the cost of immigrant legalization. To understand both the individual-level and aggregate effects of providing legal status to unauthorized immigrants, we examine variation created by Deferred Action for Childhood Arrivals, or DACA.

After years of debate and failed legislation in Congress, DACA was suddenly enacted by executive order of President Barack Obama in 2012. DACA provides temporary work authorization and deferment of deportation to foreign born immigrants that came to the United States without legal status as children. To be eligible, an individual must have arrived in the United States before age 16, must have been under age 31 when the policy went into place, June 15, 2012, had to be enrolled in school or have a high school diploma or equivalent, and have not been convicted of a felony or a significant misdemeanor. To avoid encouraging new unauthorized immigration, individuals were also required to have arrived by 2007 and have continually resided in the U.S. since then. As such, eligibility status was predetermined, 5 years before the policy was implemented.

Using microdata from the 2007-2019 American Community Survey, we examine geographic and occupational mobility of foreign born, Hispanic individuals that meet all of the eligibility criteria (arrived before 2007, were under 31 as of June 2012, meet the education requirements, and arrived before age 16), relative to similar individuals who met all of the eligibility criteria, but arrived after their 16th birthday. As such, we are able to compare outcomes for individuals in the same birth cohorts, with similar characteristics, some of whom were eligible, while others were ineligible. In an event study, we show that the propensity to move trends similarly for eligible and ineligible individuals from 2007 to 2011.[2] Then, in 2012, there is a discrete 4.2 percentage point increase in the probability of moving among DACA-eligible individuals. This increase is significant, and persists through the end of the sample in 2019. This is accompanied by a 1.4 percentage point (23 percent) increase in moving out of the local area, and a 0.6 percentage point (20 percent) increase in moving out of state. After the policy, DACA-eligible individuals are more likely to move to areas with higher average wages, consistent with more moves to economic opportunity.

We also see significant changes in the industry and occupational composition of jobs DACA-eligible individuals hold. After DACA implementation, DACA-eligible individuals shift from working in wholesale and retail trade and finance to working in manufacturing. The shift from trade and finance is concentrated among service occupations like cashiers, clerks, bank tellers, and customer service representatives. DACA-eligible individuals are more likely to be production workers, but also personal care providers, teachers, and nurses (defined broadly). DACA-eligible individuals move into industries and occupations with higher median earnings and skill-oriented occupations which require occupational licenses. These mobility investments can help explain the economic outcomes of beneficiaries.

Consistent with existing work (Amuedo-Dorantes and Antman, 2016; Pope, 2016), we find that this DACA-elgible group is 3.5 percentage points more likely to be employed and earns over $1,300 more per year. Although we cannot link this causally to the human capital investments, this is consistent with legal status improving labor market outcomes in part by increasing mobility across places and jobs. The DACA-eligible are less likely to live with their parents, get married, or have children, consistent with them becoming less tied to a local area, and potentially more responsive to local economic conditions. This could increase the dynamism of local labor markets (Blanchard and Katz, 1992; Molloy et al., 2016). Legalization as a policy yields large benefits to immigrant recipients, but we also want to know if the policy has unintended spillover effects on native born residents in the

[2]Treatment timing is the same for everyone, so we avoid recent concerns about twoway fixed effects models with staggered treatment timing (Callaway and Sant'Anna, 2020; Goodman-Bacon, 2020).

local labor market.

US-born individuals in local labor markets that had a higher share of the adult, working age population that was DACA-eligible in 2007 did not experience declines in employment after the 2012 policy, but if anything experienced slight gains. They also saw significant increases in wage earnings after 2012. However, we might be concerned that places with high DACA-eligible shares are different in other ways that correlate with employment and earngings trends. These patterns are robust to controlling for potential confounding factors, such as the 2007 Hispanic share or urbanicity interacted with year effects. The overall patterns are similar when we isolate plausibly exogenous variation in the share of the adult population that is DACA-eligible, due to pre-existing migration networks between Mexican states and US commuting zones and natural disasters in Mexican states that push immigrants to the United States when the DACA-eligible were children. We see no evidence of native-born workers experiencing worse outcomes, but overall gains in earnings, which are mostly concentrated among older, and more educated workers, consistent with legalized immigrants complementing native workers in the production process.

Previous work has documented that DACA increases employment (Amuedo-Dorantes and Antman, 2016; Pope, 2016). There is evidence DACA increases high school completion (Kuka et al., 2020), but the evidence on college attendance is more mixed with some suggestive, positive evidence (Kuka et al., 2020), but, other evidence suggesting college attendance falls after DACA, as the outside employment option improves (Amuedo-Dorantes and Antman, 2016; Hsin and Ortega, 2018). DACA reduces teen birth rates (Kuka et al., 2016) and overall fertility rates (Gihleb et al., 2021), but might increase marriage rates (Soriano, 2022). Perhaps the closest existing work finds that DACA recipients become more likely to live independently and less likely to live with a parent (Gihleb et al., 2021). We add to this literature by showing how DACA affects unauthorized immigrants' geographic and job mobility. Like education, this is another human capital investment legalization might affect. This increased mobility potentially improves these young immigrants' economic mobility, by providing access to better employment opportunities. Although there is a growing literature exploring how DACA affects outcomes of immigrants, we do not have a good understanding of externalities it might impose on native workers. We provide new evidence that the benefits of legalization extend beyond the immigrants' outcomes.

A well established literature explores the effects of immigration on natives' outcomes (Abramitzky et al., 2022; Borjas, 1999; Card, 2005, 2009; Dustmann et al., 2016; Kerr and Kerr, 2011; Lewis and Peri, 2015; Peri, 2016; Price et al., 2020; Tabellini, 2020), but this is often focused on an influx of immigrants, not a mass change in legal status. DACA allows us to examine this type of scenario. Documenting the effect of DACA on immigrant mobility investments can help us understand any spillover effects. Cadena and Kovak (2016) show that less-educated, Mexican-born immigrants' migration behavior respond more strongly to local labor market conditions than less-educated natives. They also show that this responsiveness allows local labor markets to adjust more quickly to negative shocks, leading to improved outcomes for natives. Our estimates suggests that providing legal status does not undo the local labor market insurance effect of immigrants. If anything, providing legal status could increase the positive externality of immigrants. Overall, our results suggest that a path to legal status would benefit not only immigrants, but the economy more broadly.

2 The Origins of DACA

Deferred Action for Childhood Arrivals (DACA) was designed to provide legal status to unauthorized immigrants who had arrived in the United States as children. DACA was enacted by President Barack Obama on June 15, 2012 through executive order after the failed Development, Relief and Education for Alien Minors (DREAM) Act. The DREAM Act was first proposed in 2001, as an attempt to provide a path to legalization for unauthorized immigrants, but did not receive sufficient support in congress. It languished in congress for 10 years and eventually failed to pass in the 2011 legislative session. Only after this uncertainty was it enacted through executive order.

DACA provided two key benefits for recipients. First, deportation was deferred, meaning recipients could live in the US without risk of deportation as long as they were approved for DACA. Second, recipients were able to obtain an Employment Authorization Document, which allowed them to legally work in the US.[3] The first applications were accepted in August 2012 (see Figure 1 for a timeline of DACA events and eligibility.)

Unauthorized immigrants were eligible for DACA if they met five criteria: (1) they must have arrived in the US before their 16th birthday, (2) they must have continuously lived in the US since June 15, 2007, (3) they must have been under age 31 by June 15, 2012, (4) they must be currently enrolled in school or have a high school diploma or equivalent,[4] and (5) they have not been convicted of a felony, significant misdemeanor, or three or more other misdemeanors. There was also an application fee of $465 (2012 dollars), which has gradually increased to $495. Individuals who met all of the criteria also had to be at least 15 to apply, so many individuals had to wait past the June 2012 date until they were 15. This legal protection lasts two years, but under current rules can be renewed indefinitely. Empirically, virtually all recipients receive extensions through a renewal process, allowing them to maintain legal protections.

DACA has been the subject of extensive litigation that challenged its legality and continuance, affecting the application process. On September 5, 2017, Donald Trump's administration issued a memorandum rescinding DACA. This memo prohibited all first-time applications, and allowed for renewals only until October 5, 2017, after which the program was to phase out. In January 2018, California challenged Trump's rescission of DACA temporarily allowing for renewals. On December 4, 2020 the Supreme Court overruled the rescission of DACA resulting in USCIS accepting new applications and granting renewals. On July 16, 2021 the U.S. District Court for Texas challenged the legality of DACA, limiting

[3]There were other, smaller benefits attached to DACA. For example, recipients could obtain a social security number which allowed them to open a bank account, build a credit history, and in most states obtain a driver's license (Pope, 2016) and access in-state college tuition.

[4]This requirement was waived for individuals who were honorably discharged from the Armed Forces or Coast Guard.

the program to renewals and prohibiting USCIS from granting initial DACA requests. As of July 2022, there is no decision regarding the future of DACA.

Take-up of DACA was high and immediate. Between August 15, 2012 and the end of the fiscal year just one and a half months later (September 30, 2012), 157,826 individuals had applied for DACA. Within the next year, another 443,967 had applied and by September 30, 2013 472,287 individuals had already obtained DACA protections. By December 31, 2019, over 825,000 individuals had received DACA, and nearly 1.76 million renewals had occurred. Of the 2.58 million total approvals, 94 percent were from Latin America, with 79 percent from Mexico alone. Almost 29 percent of approved applicants were living in California, with another 16 percent in Texas, and the remainder spread across the other states with a higher concentration in the Southwest.

The program eligibility features make this a good setting to explore the effect of DACA on the mobility of immigrants. Because individuals had to arrive by June 15, 2007, a full five years before the policy, migrants were not able to move to the US and gain eligibility in response to the program, thus shutting down immigrant selection. Maximum age thresholds and education requirements provide settings for us to estimate placebo effects and verify we are not just capturing secular trends. Importantly, the age of entry requirement allows us to compare individuals of a similar age, and at a similar point in the life-cycle, but some will be eligible and some will not.

3 Data and Identification Strategy

To estimate the effect of DACA on mobility, we use microdata from the 2007-2019 American Community Survey (ACS), obtained through the Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2022). The ACS is a repeated cross-section one percent, annual survey of households in the United States and covers topics including demographics, origins, household structure, employment, income, education, and migration. Although the ACS asks about place of birth and citizenship status, it does not ask about legal status among non-citizens. As such, we are not able to perfectly isolate the population treated by DACA. Following existing work, we will use information on birth year, birth quarter, education, immigration status, and immigration timing to identify a sample of likely DACA-eligible individuals. Using foreign born status and year of immigration to the US, we can identify immigrants who moved to the US prior to 2007.[5] Using birth year and quarter of birth, we can determine how old immigrants were when the policy was enacted and if they meet the "under 31 by June 2012" requirement.[6] Using educational attainment and schooling measures, we can identify individuals that meet the education requirement. By combining year, year of immigration, and birth year we can identify immigrants' age when they arrived in the US. Our main specification isolates individuals who meet the previously described education, age, and arrival date requirements, and then compares individuals who arrived before they turned 16 (and were thus DACA-eligible) relative to a counterfactual group that arrived after their 16th birthday. In our analysis we will describe these groups as the DACA-eligible (treatment) and ineligible (counterfactual) groups.

The ACS includes an individual's current state of residence. Smaller geographic entities, like county, are suppressed for privacy purposes and only available if over 100,000 people reside in a county. The ACS does, however, provide individuals' Public Use Micro Area (PUMA) of residence, which is a small, geographic entity that contains at least 100,000 people. In some cases these are smaller than counties. The ACS also asks individuals if they have moved in the past 12 months, and if so, where they were living. From this, the state and Migration PUMA (MIGPUMA) is provided for anyone who moved. MIGPUMAs do not correspond to PUMAs one-to-one. MIGPUMAs must contain entire counties, and are thus sometimes the union of multiple contiguous PUMA. A PUMA must be completely

[5]We do not observe date of immigration, so we cannot use the sharp June 15, 2007 cutoff. For this reason we limit the sample to individuals arriving in 2007 or earlier. Estimates are robust to excluding 2007 arrivals.

[6]Because we do not observe exact date of birth, we can only determine if individuals are under 31 by the end of June in 2012, not the 15th of June. As such, there will be a small number of people in our sample that turned 31 after June 15, 2012 and are not eligible for the program. Results are unaffected if we omit individuals who turn 31 in 2012.

contained within a MIGPUMA. As such, we can aggregate up to observe an individual's state and MIGPUMA of residence in the current year, as well as in the previous year. Using these measures, we construct our main outcome of interest, whether or not an individual moved residencies in the past 12 months. We will also examine whether or not they move to a different MIGPUMA or to a different state in the past 12 months as well as the types of places individuals move to. For example, we rank MIGPUMA according on their placement in the distribution of average prime-age (18-40) wages and employment rates then create binary measures that indicate a move to MIGPUMA with above median or below median wages or employment. This will help us understand how DACA affects total geographic mobility, moves out of the local area, and long distance moves across states, and the types of places they move.

The ACS also provides detailed 3-digit industry and 4-digit occupation codes for workers. From these measures, we can examine how DACA affects the occupation and industrial distribution of workers. Unfortunately, unlike migration we do not observe an individual's occupation or industry from the previous year so we cannot examine industry-to-industry specific gross flows, only the net compositional change. From this, we can identify industries and occupations that DACA recipients were more likely to shift into, after the policy change. We focus on the 11 course industry delineations (natural resources, construction, manufacturing, trade/transportation, information, finance, professional and business services, education and health, leisure and hospitality, other services, and the public sector).

We will also explore the types of occupations that DACA-eligible individuals move into. Using data on US-born workers in the ACS, we construct median wage earnings by 4-digit occupation. This will allow us to see if DACA-eligible individuals shift into higher or lower paying occupations. Using questions about occupational licensing that were recently added to the Current Population Survey (CPS) we can crosswalk individuals in the ACS to occupations that require licensure.[7] Occupational licenses have been shown to boost wages,

[7]The presence of occupational licenses for workers in the CPS is self-reported, introducing measurement error. We will treat an occupation as licensed in a state if over 10 percent of people in the occupation in the

by as much as 18 percent (Kleiner and Krueger, 2013). Because of licensure requirements, unauthorized immigrants are often unable to work in these occupations so we might observe shifting into these occupations once they obtain legal status. Using measures from Autor et al. (2003) and Deming (2017) we also explore how the routine, math-skill, and social skill task composition of occupations of DACA-eligible workers. Because formal employment is restricted to unauthorized immigrants, we also look at self-employment rates after DACA is enacted.

In our analysis we will make several data restrictions to isolate potentially eligible individuals and identify treatment and counterfactual individuals who are more similar. We restrict the sample to Hispanic, non-citizens who meet the following criteria: (1) they were under age 31 by the end of June 2012, (2) they entered the US before 2007, and (3) they are either currently enrolled in school or have a high school degree or equivalent. We will further restrict the sample to individuals who were 18 or older in 2007 and 30 or younger by July 2012, or in other words individuals who were born between the third quarter of 1981 and the end of 1989. By imposing this restriction, we are following the same birth cohorts over time, some of whom are DACA-eligible and "treated" while others are untreated, because they moved to the US after their 16th birthday. This restriction also means we will only be examining their mobility decisions as adults. We focus on Hispanics, as over 94 percent of DACA recipients were from Latin America, with 79 percent from Mexico alone. We focus on non-citizens, because citizens do not need the protections of DACA.[8]

As seen in Table 1, the observations from 2007-2011, prior to the enactment of DACA, are similar on average.[9] In the pre-period, the treated group is about 1.5 years younger, 4

state report that a license is required.

[8]Importantly, we find that citizenship status among the treatment group does not respond to the policy change.

[9]In principle, we could extend our sample to include data from 2005 and 2006. However, since DACA requires that individuals arrive prior to 2007, these years would be included under different selection criteria. People in the 2005 data would have to arrive prior to 2005 (or they wouldn't be in the data) rather than 2007. Also, this means that we might have individuals who just arrived this year, meaning we cannot explore their internal migration behavior over the past 12 months. This problem is minimized if we start the sample in 2007.

percentage points less likely to be male, and less likely to be married than similar individuals who arrived after their 16th birthday. The two groups are similar along employment dimensions. In columns (3) and (4) of Table 1 we also report means for the post-2012 period. The same differences from the pre-period persist, and don't seem to be trending differently for the two groups. However, the treated group is now more attached to the labor force, with higher employment and wage income than the counterfactual group, consistent with existing work documenting the labor market effects of DACA (Pope, 2016).

4 Estimation Equation and Identification

We estimate the following event study specification on the analysis sample described above

$$Move_{it} = \sum_{\tau=2007}^{2019} \beta_\tau \textit{Entered Under 16}_i * (Year = \tau) + \delta \textit{Entered Under 16}_i + \phi_s + \theta_t + \alpha_a + \varepsilon_{it} \quad (1)$$

To explore geographic mobility, our outcome of interest will be a binary variable that equals one if the individual moved during the last twelve months. We will also look at whether they moved to a different MIGPUMA (proxy for a different local labor market), to a different state, or the industry and occupation they end up in. The explanatory variables of interest are the interactions between *Entered Under 16* and the year indicators. The β_τ coefficients trace out changes in migration propensities for individuals who entered the US before their sixteenth birthday (and were therefore DACA-eligible) relative to individuals who entered after their sixteenth birthday. We omit the 2011 year interaction, so all of the β_τ are relative to this year. By looking at the coefficients from 2007-2010 we can evaluate pre-trends, and by looking at the 2012-2019 coefficients we can explore the treatment effect over time. Because DACA became law across the entire country at the same time, we avoid some of the recent concerns about twoway fixed effects models and staggered treatment timing (Callaway and

Sant'Anna, 2020; Goodman-Bacon, 2020). We also include fixed effects for year, state of residence (in the previous year), and single year of age. We correct standard errors for clustering at the state of residence in the previous year.

Recall that the sample is restricted to those who were 18 or older in 2007 and 30 or younger in 2012 and met the 2007 arrival, 31-year-old age cap, and education requirements. As such, we are estimating the effect of DACA among similarly aged individuals and compare outcomes for those who are eligible relative to those who meet all other criteria but are just barely ineligible because they arrived older than 16.[10] Our identifying assumption is that if DACA had not been enacted, individuals who met all of the DACA eligibility criteria and arrived before age 16 would have behaved like the similarly aged individuals who met all of the other DACA-eligibility, but arrived after their 16th birthday. As we saw in Table 1, these groups appear similar on many dimensions prior to the enactment of DACA, but we can further probe our identifying assumption by examining pre-trends in the event study specification.

We will also estimate difference-in-differences specifications, where the *Entered Under 16* by year interactions are replaced with a single $\textit{Entered Under 16}_i * Post_t$ interaction, as follows

$$Move_{it} = \beta \textit{Entered Under 16}_i * Post_t + \delta \textit{Entered Under 16}_i + \phi_s + \theta_t + \alpha_a + \varepsilon_{it} \qquad (2)$$

This allows us to estimate the average post-2012 treatment effect of DACA. Post indicates observations in 2012 or later, with all other variables as described above. In addition to examining the probability of moving, we will look at moves to places with certain characteristics (e.g., above/below median average wages) and the probability of being in a certain industry or occupation to understand industry and occupational mobility. From equation

[10] If we were to use one of the other criteria (enter before 2007 or under age 31 in 2012) and enforce the other eligibility criteria we would not observe treatment and counterfactual individuals at the same age. We would not be able to separate treatment effects from life-cycle differences in mobility. For this reason we focus on arrival age to determine treatment.

(2) we can succinctly identify the average effects of legal eligibility on mobility as well as other outcomes, such as labor market outcomes and family related behaviors. If geographic mobility and job mobility do respond to DACA, this could provide a potential mechanism through which other economic outcomes and behaviors adjust.

5 Results

5.1 Impact of DACA on Geographic Mobility

We first explore the effect of DACA on geographic mobility. As seen in Figure 2, the difference in the probability of moving is low, and close to zero between 2007 and 2011 consistent with the parallel trends assumption. In 2012, when DACA is authorized, there is a discrete, 4.0 percentage point (19 percent) increase in the annual move rate. This increase is significant and persistent, with a slight upward trend, through the end of the sample in 2019. DACA has an average effect of 4.2 percentage points on moving during the post period (Table 2). DACA also leads to a 1.4 percentage point increase in moves out of the local PUMA among eligible individuals, suggesting one third (0.014/0.042) of the DACA-induced moves were not local, but moves to a different labor market. DACA is also associated with a 0.6 percentage point increase in out-of-state moves. Relative to the mean, this would suggest that the legal protections associated with DACA increased cross-state moves of young Hispanic non-citizens by 20 percent.[11]

We also examine what types of labor markets DACA-eligible immigrants begin moving to, relative to those who are barely ineligible. At the PUMA-level we calculate the average wages and employment rates for individuals 18-40, to correspond to the age distribution of the sample. We then rank PUMAs and identify whether they are above or below the median for average wages and employment to population rates. We then construct binary outcomes

[11] Event study plots for out of PUMA and out of state moves can be found in Appendix Figure 2. Once again, pre-trends are flat, with discrete increases in 2012. However, given the rarity of these moves, the single year standard errors are less precise.

for whether the individual moved and if the destination is in the appropriate bin. DACA-eligibility increases moves to PUMAs with above median average wages by one percentage point, while migration to below median wage PUMAs only increase by 0.4 percentage points (Table 2). DACA affects moves to PUMAs with above median and below median employment rates about equally. Taken together, this would suggest that DACA induces individuals to move to higher wage labor markets, but not places with better employment rates. After DACA, eligible individuals are more likely to move to places with higher wages for non-college workers, larger Hispanic populations, and more urban areas, but not necessarily to states that border Mexico (Appendix Table A1).

5.2 Impact of DACA on Occupational Mobility

We next explore how DACA affects DACA-eligible individuals' occupational and industrial distribution relative to those who are barely ineligible. In Figure 3 we plot the β coefficients from equation (2), where the outcome is the binary variable that equals one if the person works in the given industry or occupation. DACA-eligible individuals shift out of wholesale and retail trade, and finance (concentrated in bank branch employment as bank tellers and customer service representatives) and into manufacturing.[12] Industries are plotted from lowest median wages to highest, suggesting some moving into higher wage industries. The occupational distribution suggests that these individuals are shifting out of unemployment and jobs as cashiers and clerks into roles in childcare, personal care aides (both labeled as personal care in the figure), production workers, teachers, and nurses (broadly defined). DACA shifts immigrants into employment and from entry-level service jobs to more skills-based occupations.

Given this shift in industry and occupation, we next explore how the characteristics of workers' occupations change after DACA is enacted (Table 3). After 2012, DACA-eligible immigrants are in occupations with median earnings that are over one thousand dollars

[12]Event study plots for each industry outcome are provided in Appendix Figure A2.

higher. Even when we condition on being employed, we observe an increase in occupation median earnings. This is not all drive by extensive margin employment effects, with some DACA-eligible individuals moving up the occupation ladder. Consistent with the shift to teaching and nursing seen previously, they are also more likely to be in occupations that require an occupational license. This is also consistent with shifts to higher paying occupations (Kleiner and Krueger, 2013). We do not see significant changes in the percentile rank of routine, math-skill, or social skill task composition of immigrants' occupations. There is a marginally significant, 0.7 percentage point decrease in the probability of being self-employed, consistent with immigrants moving into formal employment.

5.3 Robustness

We next verify that the impacts of DACA on mobility are robust. As seen in Figure 2 and Appendix Figure A1, pre-period trends are flat, supportive of our identifying assumption. In Appendix Figures A2 and A3 we also document the event study effects for each industry and for being in a licensed occupation. In Appendix Table A2 we show that the mobility effects are insensitive to sample restrictions. Estimates are similar if we broaden the sample to include Non-Hispanic immigrants and or immigrants who have since gained citizenship (potentially endogenous to the policy). Immigrants must have arrived before June 15, 2007 to be eligible, but since we only observe the year of immigration, our baseline estimates might include some 2007 arrivals that are ineligible. Results are similar if we exclude all 2007 arrivals. Including observations from 2005-2006, even though everyone in 2005 and 2006 would meet the arrival by 2007 eligibility requirement, or excluding 2007 observations to avoid first year migrants does not significantly affect the coefficient. The point estimate is also insensitive to excluding 2017-2019, to avoid Trump-era DACA changes. Limiting the sample to individuals who came to the US as teens (excluding those who came as young children) makes the treatment and counterfactual group more similar, but does not significantly affect the estimate. The migration results are also insensitive to specification choice, including adding state specific linear trends, adding state-by-year fixed effects, to explicitly compare eligible and ineligible immigrants in the same state and year, adding age at entry fixed effects (essentially flexibly controlling for the running variable in the analogous regression discontinuity), or adding age-by-year fixed effects to compare mobility of people who are the same age in the same year (Appendix Table A3).

To further verify that these patterns are not driven by aggregate trends, we estimate two separate placebo specifications. First, we re-estimate equation (1) but restrict the sample to individuals who arrived at the same ages as our main analysis sample (between zero and 26) but who were between the ages of 33 and 42 in 2012 and thus all ineligible. Some of these individuals arrived before their 16th birthday, but this will not affect eligibility allowing us to estimate placebo effects. As seen in Figure A4 there is no trend break after the 2012 policy and post period estimates are in fact negative and insignificant. Next, we re-estimate equation (1) but restrict the sample to individuals who meet the age (under 31 before July, 2012) and arrival (arrive before 2007) criteria for DACA, but do not meet the education requirement. Once again, none of the individuals in this sample are eligible for DACA. As expected, the event study is flat with no trend break or higher levels after the 2012 policy. These placebo estimates would suggest that we are not just capturing an aggregate trend in mobility among young Hispanic immigrants but a response to the policy (Figure A4).[13]

One concern, is that the implementation of DACA could lead to differential attrition from the treatment and counterfactual groups. Unauthorized immigrants who arrived to the US after their 16th birthday and were ineligible for DACA might differentially emigrate from the US at higher rates and no longer show up in the ACS sample. This would be problematic if these were precisely the types of individuals who were more likely to make mobility investments, like moving or changing occupations. In Appendix Figure A6 we document how the fixed characteristics of the treatment and counterfactual sample, such as gender, age, year of arrival, and years in the US, change over time. If these average

[13]Placebo estimates for employment in licensed occupations for these same groups are provided in Appendix Figure A5 and show no evidence of effects.

measures start to trend differently after the implementation of DACA, this could indicate that DACA led to differential attrition. Because of our sample criteria, measures like age, and years in the US will mechanically trend over time. However, the trends between the treatment and counterfactual groups are by and large parallel. There is some convergence in gender, but this mostly occurs before DACA is enacted. In Appendix Figure A7, we also plot our treatment and counterfactual groups as a share of the full foreign born ACS sample that are in the 1981-1989 birth cohorts, meet the education requirements, and are Hispanic, with no restriction on citizenship status. As expected, the analysis sample shrinks as a share of the full sample over time, since it is composed of immigrants from a certain time period and many immigrants eventually return home. However, the trend in both the treatment and counterfactual groups are similar, suggesting we are not missing additional ineligible individuals who leave after the policy is implemented.[14] Since we do not observe sharp changes in sample composition in 2012, but we do observe sharp changes in mobility outcomes, it is unlikely that the estimated effects are driven by differential sample attrition.

5.4 Contextualizing Effects on Economic Outcomes

Given the robust effects on geographic and occupational mobility, we next document how DACA affects recipients economic outcomes in Table 4. Some of these outcomes, like employment (Pope, 2016) have been examined before, but we include them here for completeness. Consistent with DACA providing work authorization, we estimate that DACA increased employment rates among the eligible population by 3.5 percentage points, and increased wage income by nearly $1,350. This effect is large economically, increasing income by 8.5 percent at the mean. These effects are consistent with estimates from Pope (2016). Estimates on the inverse hyperbolic sine of wage income suggest even larger gains in wage earnings. Despite the small reduction in self-employment, DACA does not change self-employment income.

[14]The trend for the ineligible does become steeper between 2016 and 2017 when President Trump took office. However, as we show in Appendix Table A2, the estimates are insensitive to excluding the Trump presidency.

Transfer income also does not change, but this is perhaps not surprising since DACA does not give eligibility to means-test safety net programs. DACA led to large economic gains for those immigrants who were eligible for the program.

The effect of DACA on these mobility investments can also shed light on how DACA has affected the living arrangements of recipients (Gihleb et al., 2021). As seen in Appendix Table A4, DACA-eligible individuals are more likely to be never married and less likely to marry after DACA is implemented. However, they are slightly more likely to be married to a US citizen. DACA also leads to reductions in fertility. DACA-eligible individuals are 5 percentage points (11 percent) less likely to have any children and have 0.12 fewer children.[15] DACA-eligible individuals are also less likely to be living with a parent after 2012. Taken as a whole, these outcomes would suggest that DACA leads to more independent living arrangements. These patterns are consistent with DACA increasing mobility and reducing rootedness to a local area. This increased mobility responsiveness could increase the insurance value that immigrants generate for native workers (Cadena and Kovak, 2016).[16]

6 Spillover Effects on the US-Born

6.1 Estimating Spillover Effects

Legal protections through DACA lead to more geographic mobility, job mobility, employment, and earnings among Hispanic immigrants who are likely to be eligible. This provides large economic benefits for immigrant recipients, but are there economic costs of the policy? Perhaps the most salient potential economic cost would be displacement of US-born work-

[15] Because the ACS only provided repeated cross-sections, we are not able to determine if this reduction in the number of children is due to a reduction in lifetime fertility or delay. As seen in Appendix Table A5 the post-DACA effects on marriage and fertility are largest in the later years, when the cohorts are older. Because the effects do not fade, we cannot rule out either delays or lifetime reductions.

[16] For completeness, we also document effects on educational attainment in Appendix Table A6. We find that DACA-eligible individuals are less likely to be attending college after 2012 consistent with (Amuedo-Dorantes and Antman, 2016) who use an almost identical strategy, but are more likely to have either a two year degree or a four year degree, consistent with (Kuka et al., 2020). We have also estimated models looking at English speaking ability, the difference in difference estimates suggest they are less likely to speak English well, but there are significant pre-trends when examining English speaking.

ers. If immigrant workers provide a substitute for US-born workers, gains in employment and earnings among immigrants could be offset by losses among the US-born. There is a large, mixed literature exploring the impact of immigrant arrival on natives' labor market outcomes.[17] However, DACA introduces a unique setting. DACA provides legal status and work authorization, but only for a subset of immigrants already living within the United States. The DACA eligibility criteria explicitly excludes new arrivals and does not create direct incentives for new, potential immigrants. Rather than examine how the arrival of immigrants affects US-born workers labor market outcomes, we can estimate how the authorization of undocumented immigrants who are already here affects the labor market outcomes of US-born workers.

There are several reasons we might expect the effects of legalization to differ from the effects of immigrant arrival. An influx of new immigrants means there are more people, leading to more potential competition in the labor market, but also an increase in local demand for goods and services. Depending on their legal status, newly arriving immigrants might seek informal employment, or employment in sectors that native workers are unlikely to consider (such as agriculture). As such, the aggregate spillover effects on US-born workers might be minimal. Granting legal status to a pre-existing set of immigrants does not result in a population increase, so changes in local demand might be less pronounced. Legal work authorization could also drive them to jobs where they are more likely to compete with US-born workers for jobs. However, by increasing mobility investments among immigrants, DACA could lead to more productive workers and more dynamic labor markets. This could spillover to benefit US-born workers who are not directly affected by the policy. The net effect is an empirical question.

To understand the spillover effects of DACA on US-born workers we will exploit local labor market level variation in exposure to the population that received eligiblity through DACA. As seen in Figure 4, the share of the population that met DACA-eligibility require-

[17] See, for example, (Abramitzky et al., 2022; Borjas, 1999; Card, 2005, 2009; Dustmann et al., 2016; Kerr and Kerr, 2011; Lewis and Peri, 2015; Peri, 2016; Price et al., 2020; Tabellini, 2020).

ments in 2007 varies across local labor markets (as captured by commuting zones). The DACA share is high along the border with Mexico, but there is also substantial variation, even within a close proximity. We will exploit this variation to identify local labor markets that are more exposed to the DACA legalization shock as follows

$$Y_{ict} = \sum_{\tau=2007}^{2019} \gamma_\tau DACA\ Share_{c,2007} * (Year = \tau) + X_{ict}\Gamma\phi_c + \theta_t + \alpha_a + \varepsilon_{it} \tag{3}$$

In this specification we are interested in two main outcomes, employment and income, for individual i (who is US-born) in commuting zone c in year t. The event study interacts the fixed, 2007 level of exposure for commuting zone c with year indicators, leaving 2011 as the omitted year. We standardize the DACA share, so the γ_τ coefficients can be interpreted as the effect associated with a one standard deviation increase in share of the population that is DACA eligible. We include commuting zone, year, and age fixed effects, with the standard errors corrected for clustering at the commuting zone level.

With this specification, our identifying assumption is that US-born workers in commuting zones with higher DACA shares would have experienced a similar trend in outcomes as US-born workers in commuting zones with lower DACA shares. This assumption depends on the cross-sectional variation in DACA shares, much like the variation of a shift-share instrument. There are two primary threats to identification with this specification. First, there is an endogeneity concern if, for example, immigrant families with DACA age-eligible children move to labor markets that are experiencing economic growth. This would result in positive correlations that are not causal. A second, related threat facing all shift-share strategies, is that the DACA-eligible share might be correlated with trends in other characteristics that also affect the outcomes of interest (Goldsmith-Pinkham et al., 2020). For example, if DACA-eligible individuals tend to locate in areas that are becoming more urban, any changes in US-born worker outcomes could be due to the increasing urbanicity, not exposure to DACA legalization in the local labor market.[18]

[18] As seen in Appendix Table A7, the DACA share is correlated with some trends in local characteristics

For this reason, we also provide results from an instrumental variables strategy in addition to the OLS event study estimates in equation (3). We want to isolate variation in the DACA-eligible share of the population that is not due to economic conditions or trends in the local labor market or other characteristics of the local population that might correlate with economic trends. To do this, we are going to exploit variation that arises from historic migration networks and enclaves, interacted with migration "push" factors in the form of natural disasters. Approximately 80 percent of DACA-eligible individuals come from Mexico. As noted by (Munshi, 2003), there are strong geographic migration networks from Mexico to the United States. People from the same communities and regions in Mexico tend to locate in the same areas in the United States. This is similar to the literature exploiting national, immigrant enclaves, but exploits variation within the source country.

The Matrícula Consular is a form of Mexican ID for nationals living abroad. This document has a cost around $35, depending on the Mexican consulate, and needs to be renewed every 5 years. It requires proof of Mexican nationality and of residence within the boundaries of the Mexican Consulate of emission. This ID can be used as a substitute for a passport in several U.S. institutions, for instance for opening bank accounts, obtaining an Individual Taxpayer Identification Number (ITIN), or obtaining a driver's license in some states. Massey et al. Massey et al. (2010) argues that those applying for this ID are most likely undocumented immigrants as a legal status would provide them with another form of ID; however, not all undocumented immigrants may be interested in applying for a Matrícula Consular.

Using annual reports of IDs (Matrículas Consulares) from 2005 to 2009 from the Instituto de los Mexicanos en el Exterior (IME), we are able to identify the number of immigrants with Mexican IDs in each county in the United States by their origin state of residence in Mexico that applied or renew their ID. From this, we construct the share of immigrants to

between 2007 and 2011, but these changes are often small. In the appendix we also estimate the results including a vector of Hispanic share by year interactions and population by year interactions in equation (3) and verify that the results are robust.

the US from each Mexican state that reside in each commuting zone in the US as follows.

$$share_{mct} = \frac{Number\ of\ Immigrants_{mct}}{\sum_{i=1}^{I} Number\ of\ Immigrants_{mit}} \tag{4}$$

Where m indicates the Mexican state, c indicates the US commuting zone, and t indicates year (2005-2009). We then take the average of this measure between 2005 and 2009 to remove year-to-year noise from the measure to construct $share_{mc}$. As seen in the top panel of Figure 5, immigrants from different Mexican states concentrated in different areas. Puebla and Tabasco are two states in Southern Mexico. While both states have strong ties to Southern California, immigrants from Puebla are more likely to locate in New York City and Minneapolis, while immigrants from Tabasco are more likely to locate in Houston, Dallas, Denver, and Orlando. As such, if something pushes residents in Puebla to immigrate to the US, they will likely end up in different places than if residents of Tabasco were prompted to move. If the timing and location of these "push" events are somewhat random, this could generate variation in the size of the population that is DACA-eligible across commuting zones in the US. The geographic variation in migrant networks is not only present for these two states, but across all 32 Mexican states (Appendix Figure A8). Places like Southern California draw immigrants from many Mexican states, while other areas, including large urban areas like Houston, Dallas, Chicago, Phoenix, New York City, Seattle, Raleigh, Indianapolis, Denver, Salt Lake City, and Atlanta tend to draw from particular Mexican states. A heat plot in Appendix Figure A9 shows that although the Los Angeles area draws a large share from most Mexican states, there are a lot of enclaves across the country from different Mexican states. Some commuting zones have a high share of immigrants from a particular state, but virtually none from other states.

We next identify potential "push" factors that might induce residents to move out of the local area by focusing on large, natural disasters in Mexican states. Using US Geological Survey data on earthquake timing and coordinates, we link all earthquakes between 1995 and 2006 in Mexico that were above a 6.0 magnitude to the Mexican state that they occur in (see the map in Figure A10). Earthquakes above a 6.0 magnitude are expected to produce significant structural damage, displacing individuals and families. We focus on earthquakes between 1995 and 2006, as these are the years when children coming to the US would have met the DACA age and arrival eligibility criteria. In the ACS, these are the years when the bulk of the DACA age-eligible immigrants came to the US. As seen in Figure A10, there were 19 magnitude 6.1 or larger earthquakes that affected 6 separate states. For example, in 1999, there was a magnitude 7.0 earthquake in the state of Puebla, while there were no significant earthquakes in Tabasco. As such, if natural disasters like earthquakes push families to immigrate to the US, we would expect to see increases in the population of immigrant children who will eventually be DACA eligible in places like Minneapolis and New York City, but to a lesser extent in Dallas, Houston, Orlando, or Denver. Although there is no data on the total, annual migration flows to the US by Mexican state, using a survey of migrants leaving Mexico for the US, we are able to see how large, destructive earthquakes affect migrant flows from the affected states. Using an event study set up, we see that undocumented immigrant flows from the affected state increase by approximately one hundred percent after the earthquake (see bottom left panel of 5.[19] Uncertainty about the timing and exact location of major earthquakes helps to generate variation in DACA-eligible shares that is not driven by local economic conditions in the commuting zone. We interact the enclave share measure and the earthquake measure to construct our instrument as follows

$$Migrant\ Shock_c = \frac{1}{Pop_{c,2007}} * \sum_{m=1}^{M} share_{mc} * \mathbf{1}\{Any\ Earthquake_m\} \tag{5}$$

This measure will be larger in US commuting zones that have a large share of immigrants from Mexican states that experienced a massive earthquake between 1995 and 2006. We then scale this measure by the population of the commuting zone in 2007, to make this consistent

[19]The outcome is the inverse hyperbolic sine of migrants, to approximate percentage effects and include zeros. The pattern is similar if $ln(migrants)$, $ln(migrants + 1)$, or migrants is used. I provide the percent effects since this is only capturing migrants that are surveyed, not the total flows.

with the population share measure. So, places like Los Angeles that draw a lot of migrants will be down weighted by its large population because each additional migrant represents a smaller share of the potential workforce. We will measure this migrant shock per 100,000 people.[20]

This measure is highly predictive of the 2007, commuting zone population share that is DACA-eligible, as seen in the bottom left panel of Figure 5. We limit the sample to the US-born that are 18-64 in the ACS in 2007 and estimate the cross-sectional relationship between the DACA share and the migrant shock, controlling for age effects (ϕ_a) as follows

$$DACA\ Share_{c,2007} = \beta Migrant\ Shock_c + \alpha_a + \varepsilon_c \tag{6}$$

As seen in Appendix Table A8, *Migrant Shock* is highly predictive of *DACA Share*. When correcting the standard errors for clustering at the commuting zone level, the F-statistic is 82. To estimate the effect of DACA on US-born workers' outcomes, we will interact this measure with year indicators and instrument for the each of the $DACA\ Share_{c,2007} * (Year = \tau)$ interactions as follows

$$Y_{ict} = \sum_{\tau=2007}^{2019} \gamma_\tau DACA\ \widehat{Share_{c,2007}} * (Year = \tau) + X_{ict}\Gamma + \phi_c + \theta_t + \alpha_a + \varepsilon_{it} \tag{7}$$

The exclusion restriction for this instrument to be valid is that *Migrant Shock* only affects native worker outcomes through its affect on the share of the working age population that is DACA-eligible.[21] It seems plausible that the geographic spread of the migration network would affect native worker outcomes through its impact on the share of Mexican immigrants, some of whom are DACA-eligible. However, it is possible, that this increases the

[20]We use an indicator for whether or not there were any earthquakes over magnitude 6.0 in the state between 1995 and 2006. We have also constructed the measure using the maximum earthquake magnitude or the relative magnitude (correcting for the log base 10 measurement system) and the measures are very highly correlated.

[21]One potential concern is that events like earthquakes might affect remittances and income in the local economy. However, since we are interested in how shocks betwen 1995 and 2006 affect outcomes after 2012 the short-term remittances channel is less relevant.

total number of immigrants, not just those that are DACA-eligible. This is why we interact it with shocks between 1995 and 2006, when DACA-elgible children would be arriving. If we re-estimate first stage equation (6), but look at the share of the working age population that is Hispanic, but does not meet DACA eligibility criteria, the effect is about two thirds as large, suggesting this instrument is more predictive of DACA-eligible immigrant flows than general flows from Mexico. (Table A8). However, as the instrument might generate variation in the overall Mexican immigrant share of the population this changes slightly the interpretation of the estimates. However, by interacting this with the time variation of when DACA was enacted, this should still proxy for exposure to people affected by the DACA.

In addition to estimating the spillover effects for all native workers, 18-64, we also examine effects on employment and earnings for subgroups. In particular, we look at effects by age and education level. During the sample period, DACA-eligible immigrants are young, all under the age of 38. As such, we might expect the effects to be different for younger workers (18-34) than for prime age workers (35-54) and older workers (55-64). It is possible that DACA-eligible workers provide a substitute for younger US-born workers, but might complement the productivity of older workers. The same is also true when looking by education. Although DACA does appear to increase educational attainment, the majority of DACA recipients attended some college but do not have a college degree after 2012. As with age, DACA-eligible workers might provide a reasonable substitute for moderately educated US-born workers, but potentially complement the productivity of more and less educated workers. We will explore employment and wage income for these groups separately.

6.2 Spillover Results

Event study results are provided in Figure 6. When looking at employment, we see that the 2007 DACA share predicts slight, downward pre-trends between 2007 and 2011. If anything, local labor markets with higher DACA shares saw small, relative decreases in employment between 2007 and 2011. However, after 2012 when DACA is implemented that trend starts to reverse. Overall, the implied effects are quite small. At most, a large, one standard deviation increase in the share of the population that meets DACA's eligibility criteria only increases employment rates by less than half of a percentage point. When we instrument for the DACA share patterns are similar. The downward sloping pre-trend is more pronounced (but less precise) and the gains in employment after DACA is implemented are slightly larger. DACA does not appear to reduce total employment among US-born workers, and if any thing there were very small gains. When looking at US-born workers wage income, the 2007 DACA share does not predict changes between 2007 and 2011. Once again, after 2012 when DACA is implemented, US-born workers in labor markets with higher DACA shares in 2007 start to observe steady increases in wage income. By the end of the sample in 2019, a one standard deviation increase in the share of the population that was DACA eligible in 2007 is associated with a significant, nearly $1,000 increase in average wage income. Point estimates from the instrumental variables specification are remarkably similar, but estimated with much less precision.

As seen in Figure 6, the trends in employment and wage income between commuting zones with high and low exposure to the DACA-eligible population only start to diverge in 2012 after DACA begins. As such, it seems unlikely that these effects are driven by other local characteristic that are trending over time, unless those characteristics also experience a trend break after DACA was implemented. Furthermore, controlling for characteristic by year interactions to account for trends in potentially concerning characteristics, such as the Hispanic share or the size of the population, does not explain the effects. If anything, the pattern of effects becomes stronger (Appendix Figure A11).

We next explore effects for different subgroups based on age and education. As seen in Figure 7, the exposure to more people legalized through DACA has very little, or no effect on employment among 18-34 year-old US-born workers. There do appear to be significant gains in employment after DACA is enacted among prime age workers, 35 to 54 with a 0.5 (OLS) or 1 (IV) percentage point increase. There are no significant employment effects for older workers, age 55 or older. When looking at wage incomes, prime age and older workers in labor markets with higher DACA shares start to experience income growth after 2012, resulting in average wages that are $1,500-2,000 higher for a one standard deviation increase in the share of the population that is DACA eligible. Meanwhile, the evidence on wage income for younger workers is much more mixed, with some downward sloping trends in the pre-period, and perhaps some reversal after the policy, although this is imprecisely estimated. These patterns do not provide compelling evidence that DACA recipients displace US-born workers. Rather, the patterns are consistent with DACA recipients complementing prime age and older workers in the production process, leading to higher income and slightly more employment among the prime age.

We explore the spillover effects on US-born workers by educational attainment in Figure 8. As seen in Table 1, the vast majority of DACA recipients have attended at least some college. We see significant 1 percentage point gains in employment for US-born workers with a high school degree or less in labor markets with higher DACA shares after 2012. Employment effects for US-born workers with some college or at least a four year degree follow a similar pattern, but observe smaller gains in the post period, and more pronounced downward sloping pre-trends. Less-educated US-born workers in more DACA-exposed areas experience small gains in wage income after 2012, while the most educated workers experience gains that are about three times as large. US-born workers with only some college, the same education group as most DACA-eligible individuals, do not experience wage gains when the DACA share is higher, although there are larger negative pre-trends here as well. Once again, these patterns are consistent with DACA recipients complementing other workers in the production process, leading to higher income. Workers with some college do not experience the same gains in wage income, which might be indicative of substitution in the production process, but we do not find evidence that they are displacing US-born workers or that US-born workers experience losses as a result of exposure to more immigrants receiving legal status.[22]

7 Conclusion

There are nearly 11.4 million unauthorized immigrants in the United States. in this paper, we examine the effects of immigrant legalization on both immigrant outcomes and outcomes for natives. Exploiting variation in legal authorization generated by DACA, we show that gaining legal status and work authorization increases both geographic and job mobility among the eligible, immigrant population. This is consistent with immigrants making more mobility investments when legal status removes the risk surrounding these investments. Immigrants are more likely to move to different labor markets and more likely to move to labor markets with higher wages. After legalization immigrants move into manufacturing and other skill-based industries and out of trade, specifically employment as cashiers and clerks in stores. They move into occupations with higher median wages and more licensing restrictions. DACA eligible immigrants experience better labor market outcomes.

From the immigrants' perspective, legalization brings large economic benefits. These benefits do not appear to be offset by added costs borne by US-born workers. Exploiting local-level variation in the share of the population that is affected by DACA, we show that employment-levels of US-workers respond very little when the labor market is exposed to more people that receive legal status through DACA. If anything, there are slight employment gains. The wage income of US-born workers rise steadily in labor markets that are exposed to the DACA legalization shock, but only after the 2012 implementation of DACA. Estimates for subgroups find similar patterns although the employment and wage gains are concentrated among workers that are more likely to be complements to immigrant workers in the production process (older workers, or workers with different education levels). These patterns of results do not indicate that native workers bear the cost of unauthorized im-

[22]Patterns are similar when we estimate OLS, but control for the Hispanic share-by-year interactions and population-by-year interactions (Appendix Figures A12 and A13.

migrant legalization and suggest that there could be large positive, local externalities to legalization policy.

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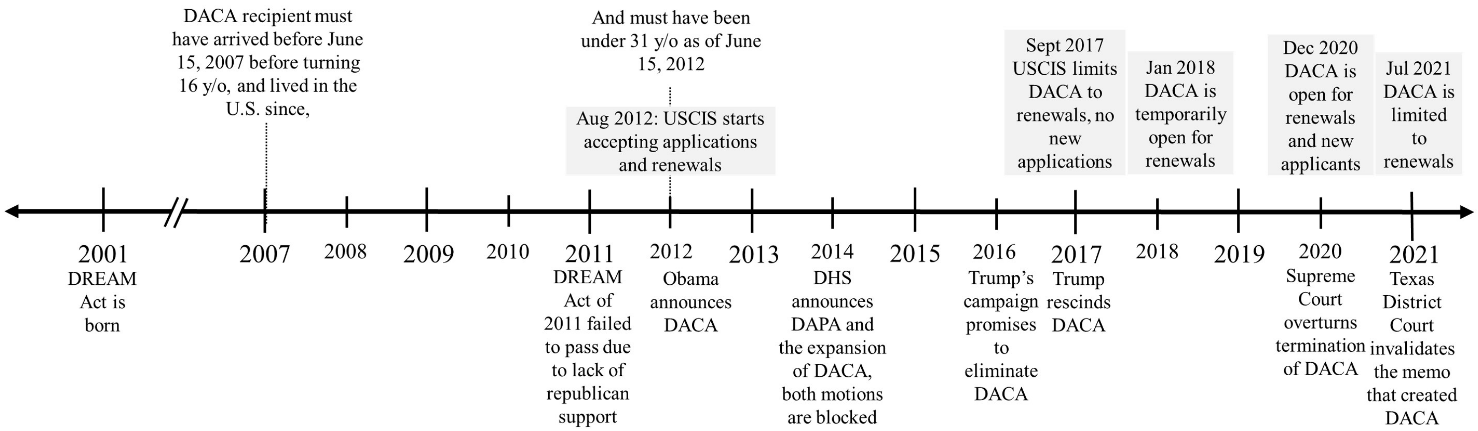
8 Tables and Figures

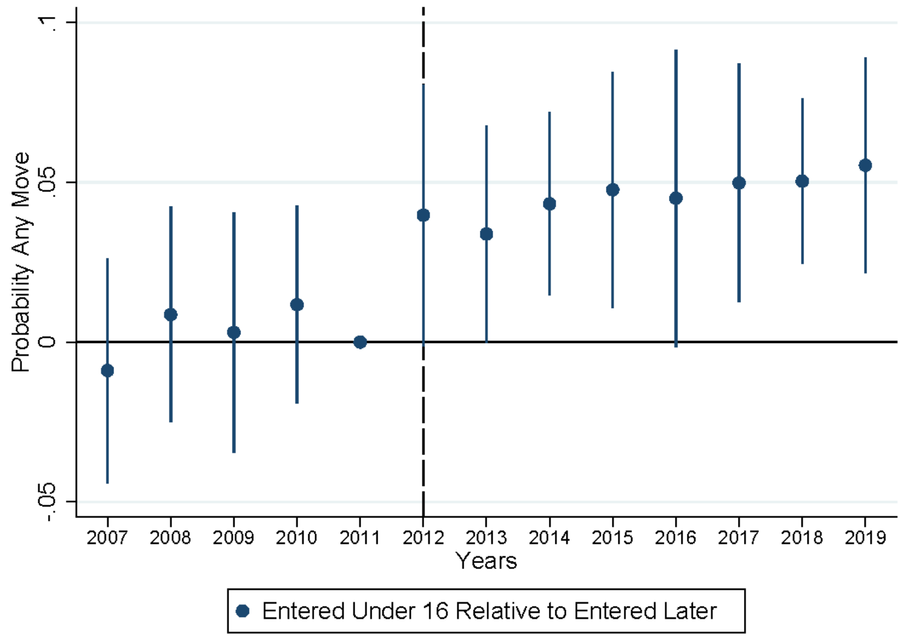
Figure 1: Timeline of DACA Legislation



NOTE: DACA was enacted June 15, 2012 by executive order. Applications began begin accepted August 15, 2012. Individuals had to continuously reside in the US since June 15, 2007, be under age 31 by June 15, 2012, and arrive in the US under the age of 16.

SOURCE: Author's own construction based on DACA related legislation.

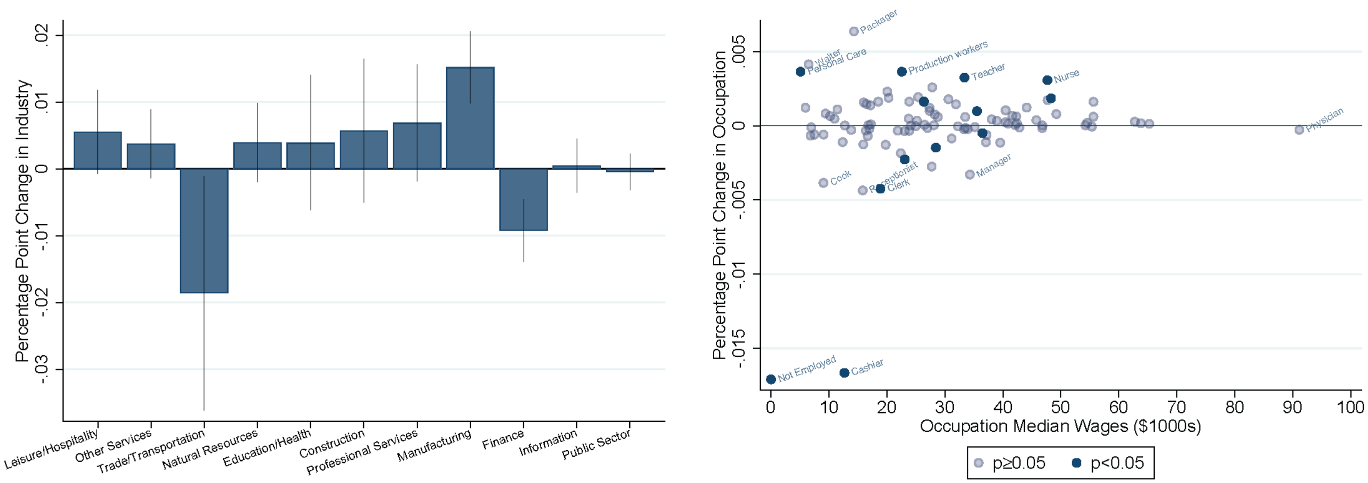
Figure 2: Probability of Moving Among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. These individuals must also meet the DACA education requirements. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. The coefficients from equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

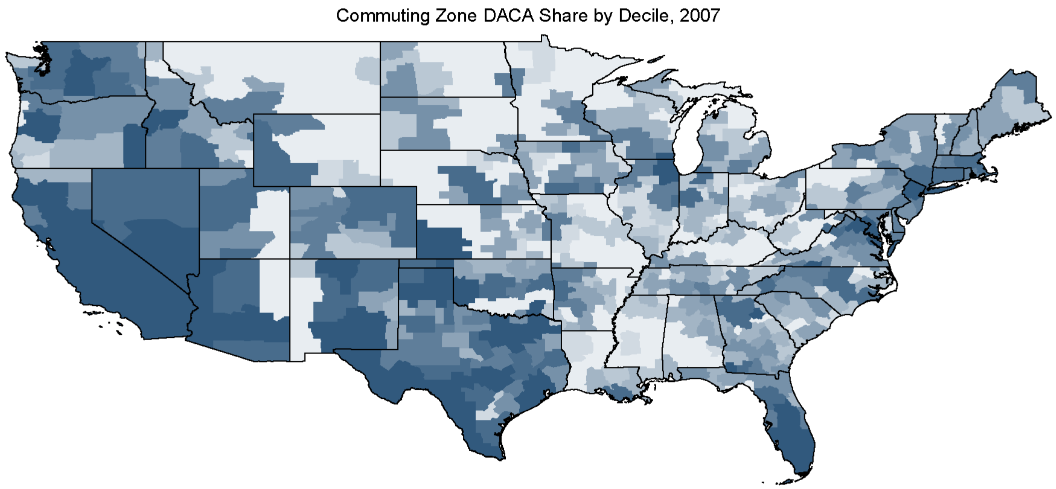
Figure 3: Occupation and Industry Mobility Among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. These individuals must also meet the DACA education requirements. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. The coefficients from equation (2) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year, where each bar/point represents a separate industry or occupation. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

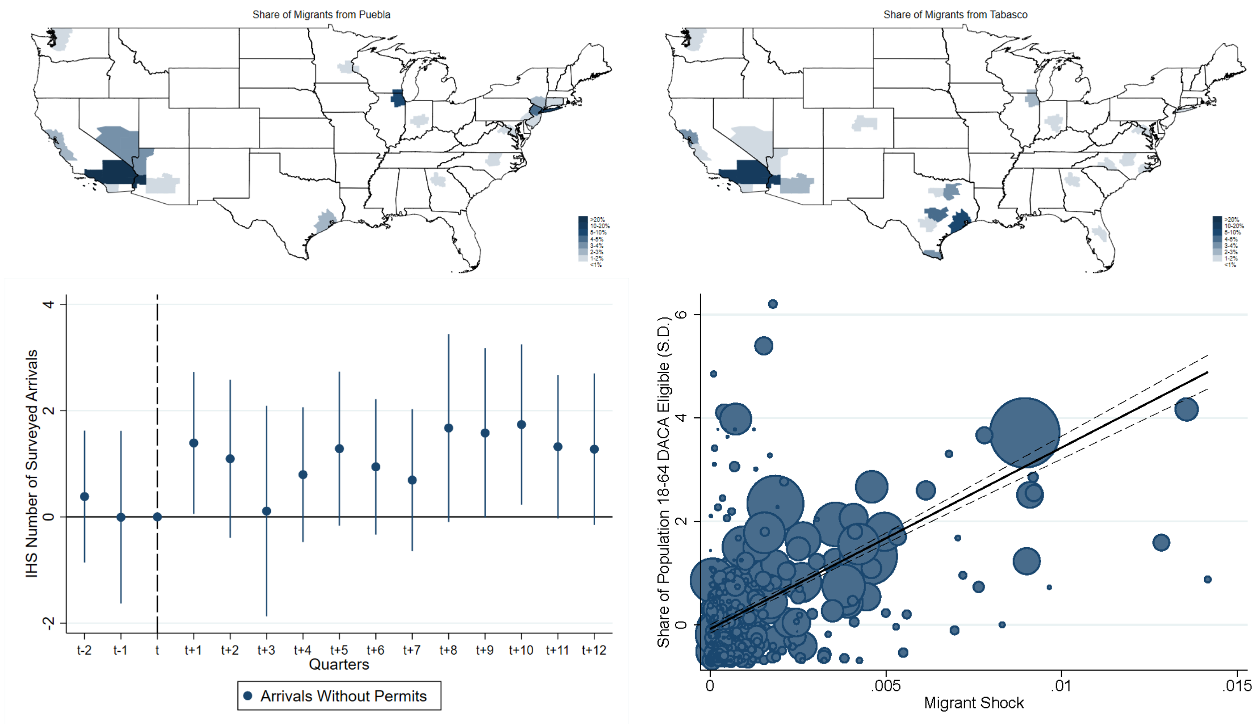
Figure 4: Share of Population 18-64 that is DACA-Eligible



NOTE: Figure plots the share of individuals 18-64 in the 2007 ACS that meet the DACA eligibility criteria, including arrived in the US by 2007, under the age of 31 by July 2012, arrived in the US before their 16th birthday, meet the DACA education criteria, and are not citizens. Estimates shaded according to Commuting Zone level decile.

SOURCE: Author's own calculations using 2007 ACS microdata.

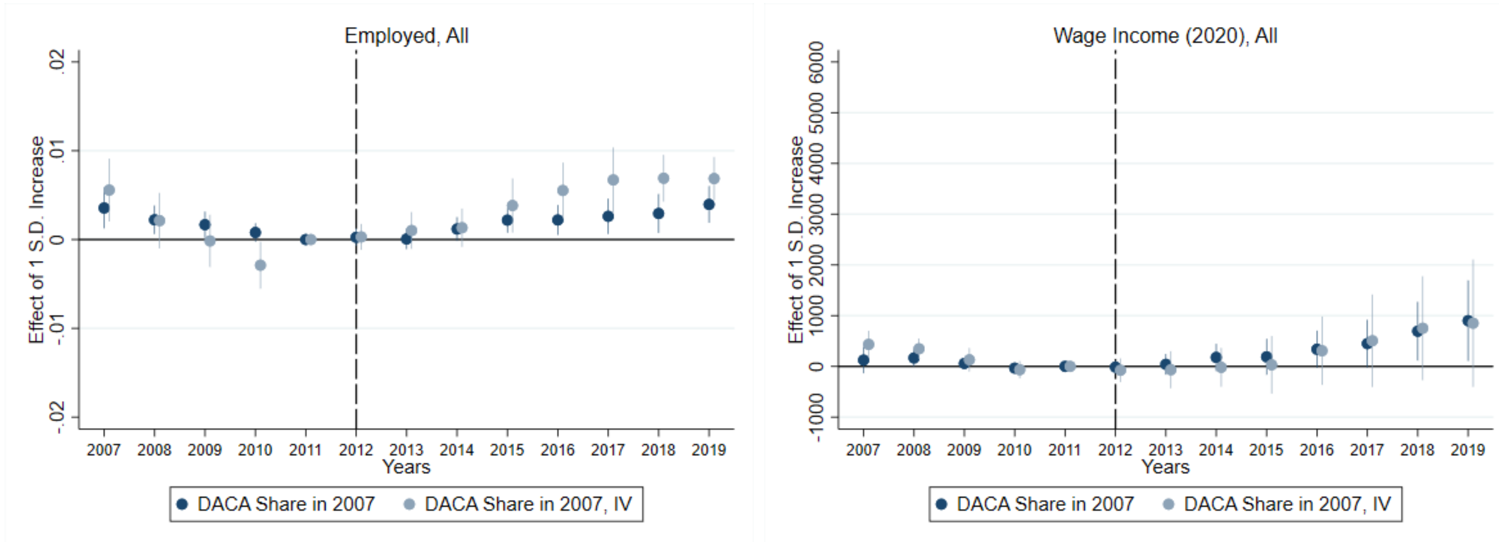
Figure 5: Instrumental Variation: Mexican State Networks and Natural Disaster Shocks



NOTE: The top panel plots the migration network from two states in Southern Mexico states, Puebla and Tabasco. Puebla experienced a magnitude 7 earthquake in 1999 while Tabasco did not experience any earthquakes larger than 6.0 between 1995 and 2006. The bottom panel shows how the earthquakes affected migrant flows of individuals without work or residency permits and the first stage relationship between the predicted migrant shock from migrant networks interacted with earthquake shocks between 1995-2006 and the share of the working age population that was DACA eligible.

SOURCE: Author's own calculations using 2007 ACS microdata and 2005-2009 Mexico Consulate Mexican ID data.

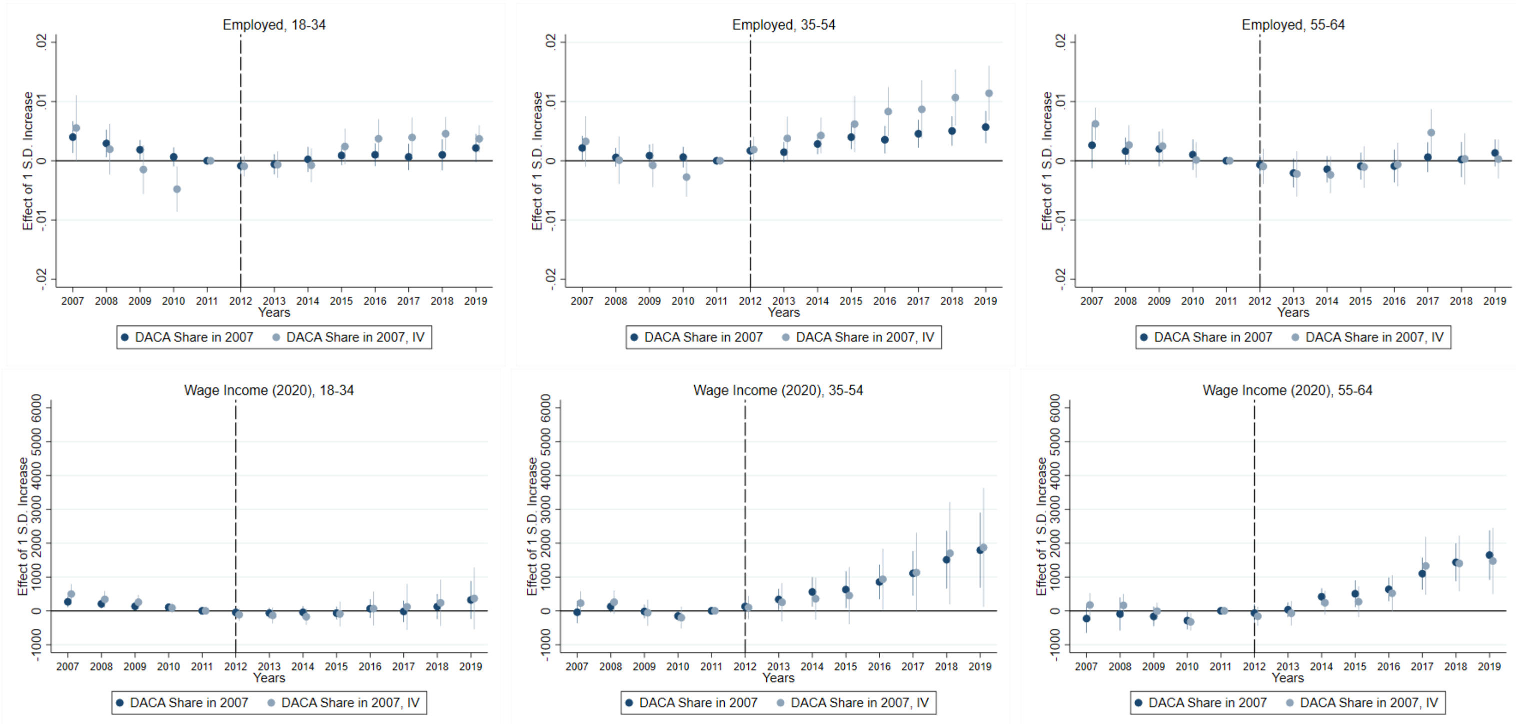
Figure 6: Spillover Impact of DACA on Labor Market Outcomes of Non-Foreign Born in the Commuting Zone



NOTE: Sample restricted to US-born respondents of the 2007-2019 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 that meet the DACA criteria used in the main analysis to identify the treated sample. The coefficients from equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the commuting zone level. Fixed effects for age, year, and commuting zone are included. The lighter plotted coefficients are obtained from equation (5), where historic migration networks, interacted with Mexican state earthquake shocks are used to instrument for the 2007 DACA eligible share. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013).

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

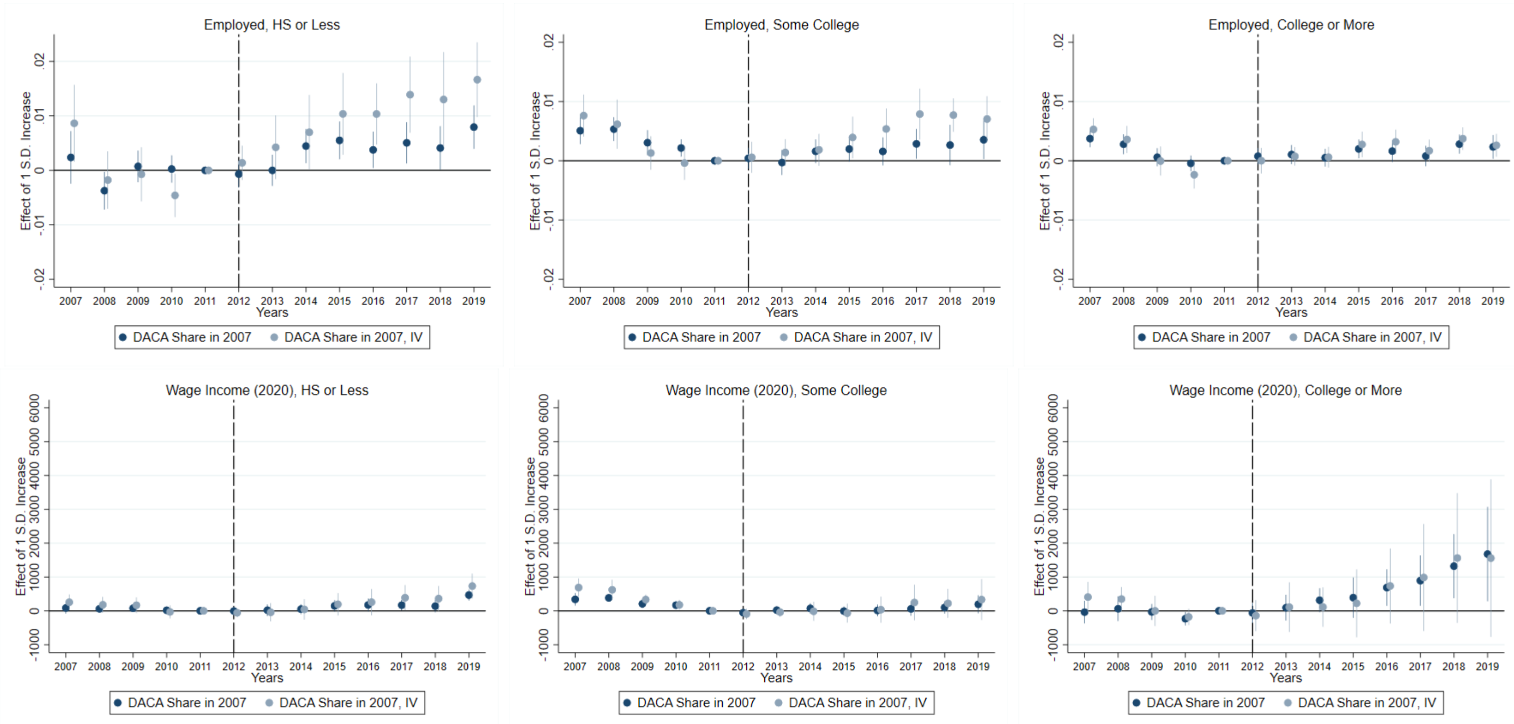
Figure 7: Spillover Impact of DACA on Labor Market Outcomes of Non-Foreign Born in the Commuting Zone, by Age



NOTE: Sample restricted to US-born respondents of the 2007-2019 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 that meet the DACA criteria used in the main analysis to identify the treated sample. The coefficients from equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the commuting zone level. Fixed effects for age, year, and commuting zone are included. The lighter plotted coefficients in the top panel control for 2007 Hispanic share by year interactions and 2007 population by year interactions. The lighter plotted coefficients in the bottom panel are obtained from equation (5), where historic migration networks, interacted with Mexican state earthquake shocks are used to instrument for the 2007 DACA eligible share. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013).

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

Figure 8: Spillover Impact of DACA on Labor Market Outcomes of Non-Foreign Born in the Commuting Zone, by Education



NOTE: Sample restricted to US-born respondents of the 2007-2019 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 that meet the DACA criteria used in the main analysis to identify the treated sample. The coefficients from equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the commuting zone level. Fixed effects for age, year, and commuting zone are included. The lighter plotted coefficients in the top panel control for 2007 Hispanic share by year interactions and 2007 population by year interactions. The lighter plotted coefficients in the bottom panel are obtained from equation (5), where historic migration networks, interacted with Mexican state earthquake shocks are used to instrument for the 2007 DACA eligible share. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013).

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

Table 1: Summary Statistics for Eligible and Ineligible Hispanic Immigrants that Meet DACA's Age and Education Requirements

	Pre-DACA (2007-2011)		Post-DACA (2012-2019)	
	Entered After 16	Entered Under 16	Entered After 16	Entered Under 16
	(1)	(2)	(3)	(4)
Male	0.56	0.52	0.54	0.52
Age	24.55	22.97	30.74	29.28
Never Married	0.58	0.71	0.38	0.48
Married	0.39	0.26	0.55	0.45
Divorced/Separated	0.03	0.04	0.07	0.07
Own Home as Head	0.09	0.08	0.19	0.20
Any College	0.84	0.82	0.98	0.98
4 Year Degree or More	0.09	0.05	0.10	0.10
Employed	0.68	0.65	0.72	0.75
Worked 26 Weeks or Less	0.14	0.17	0.08	0.09
Worked 27-49 Weeks	0.19	0.18	0.14	0.13
Worked 50 Weeks or More	0.67	0.64	0.79	0.79
Usual Hours Worked	29.18	27.63	30.11	31.44
Wage Income (2020)	11561.27	11201.26	18458.42	20421.73
Business Income (2020)	505.87	393.89	1342.66	1090.54
Transfer Income (2020)	44.75	64.06	110.97	134.24
Observations	20,665	21,830	26,001	25,683

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. All individuals meet the DACA age, education, and year of arrival requirements, but vary in whether or not they arrived before their 16th birthday, which determines eligibility. The group that entered under age 16 is eligible for DACA.

Table 2: Impact of DACA on Mobility of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

				Move to PUMA with Average Wages		Move to PUMA with Average E-POP	
	Any Move (1)	Move out of PUMA (2)	Move out of State (3)	Above Median (4)	Below Median (5)	Above Median (6)	Below Median (7)
Entered Under 16*Post-DACA	0.042***	0.014***	0.006***	0.010***	0.004**	0.007**	0.006***
	(0.007)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)
Entered Under 16	-0.052***	-0.007**	-0.005***	-0.006**	-0.002	-0.005*	-0.003
	(0.006)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)
Dependent Mean	0.21	0.06	0.03	0.05	0.02	0.03	0.03
Observations	94,179	94,179	94,179	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table 3: Impact of DACA on Occupational Choice of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

	Occupation Median Income (2020) (1)	Employed: Occupation Median Income (2020) (2)	Licensed Occupation (3)	Occ. Routine Percentile (4)	Occ. Math Percentile (5)	Occ. Social Skill Percentile (6)	Self Employed (7)
Entered Under 16*Post-DACA	1005.076***	501.798**	0.020***	0.036	-0.055	-0.043	-0.007*
	(189.529)	(222.509)	(0.006)	(0.036)	(0.049)	(0.035)	(0.004)
Entered Under 16	2973.472***	3294.352***	0.085***	-0.158***	0.915***	0.768***	-0.005***
	(188.062)	(216.314)	(0.011)	(0.040)	(0.056)	(0.034)	(0.002)
Dependent Mean	14792.92	18084.31	0.28	4.90	3.54	3.79	0.06
Observations	94,179	66,023	94,179	73,104	73,104	73,104	94,179

NOTE: Occupational percentiles in Math, Routine, and Social Skills taken from (Deming, 2017) and capture the relative task composition of occupations based on the 1998 O*net dictionary. Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. $p< 0.01$ ***, $p< 0.05$ **, $p<0.1$ *.

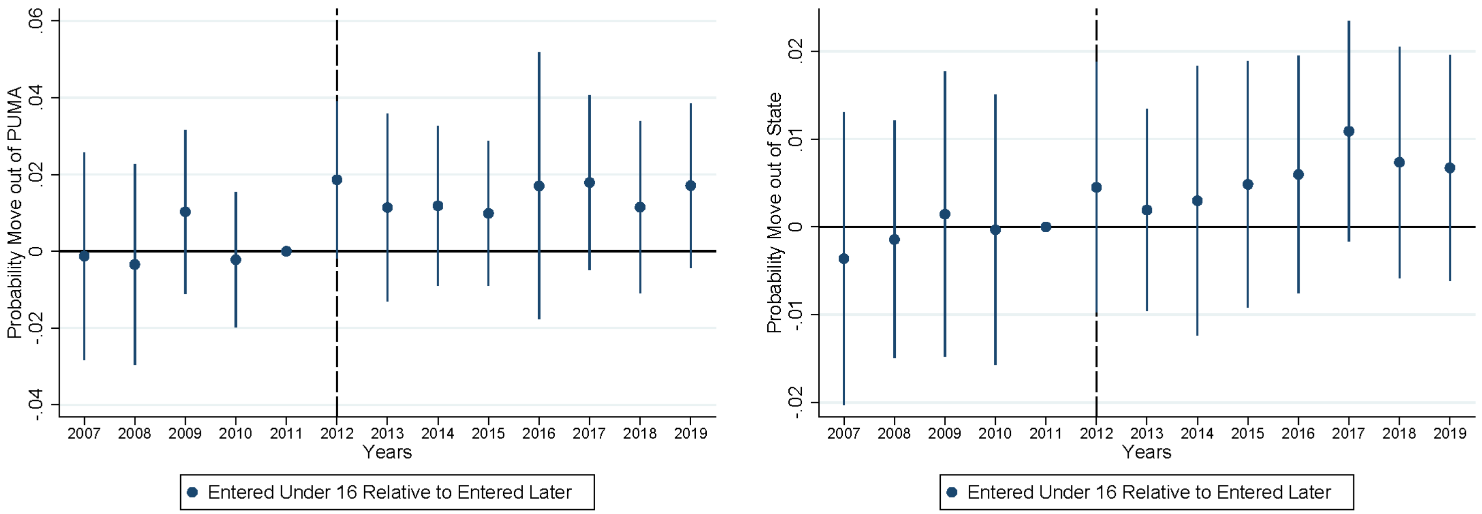
Table 4: Impact of DACA on Labor Market Outcomes of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

	Worked	Usual Hours Worked, Workers	Usual Hours Income (2020)	IHS Wage Income (2020)	Wage Income (2020)	Business Income (2020)	Transfer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Entered Under 16*Post-DACA	0.035***	1.366***	0.454**	0.349***	1347.925***	-43.297	-5.553
	(0.008)	(0.294)	(0.201)	(0.083)	(235.434)	(82.738)	(11.515)
Entered Under 16	0.001	0.285	-0.430**	0.267***	1637.392***	-38.265	30.341***
	(0.006)	(0.272)	(0.180)	(0.056)	(172.838)	(32.934)	(5.064)
Dependent Mean	0.70	29.69	38.28	7.56	15798.27	870.38	91.91
Observations	94,179	94,179	73,045	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Appendix A. Supplementary Analyses (For Online Publication)

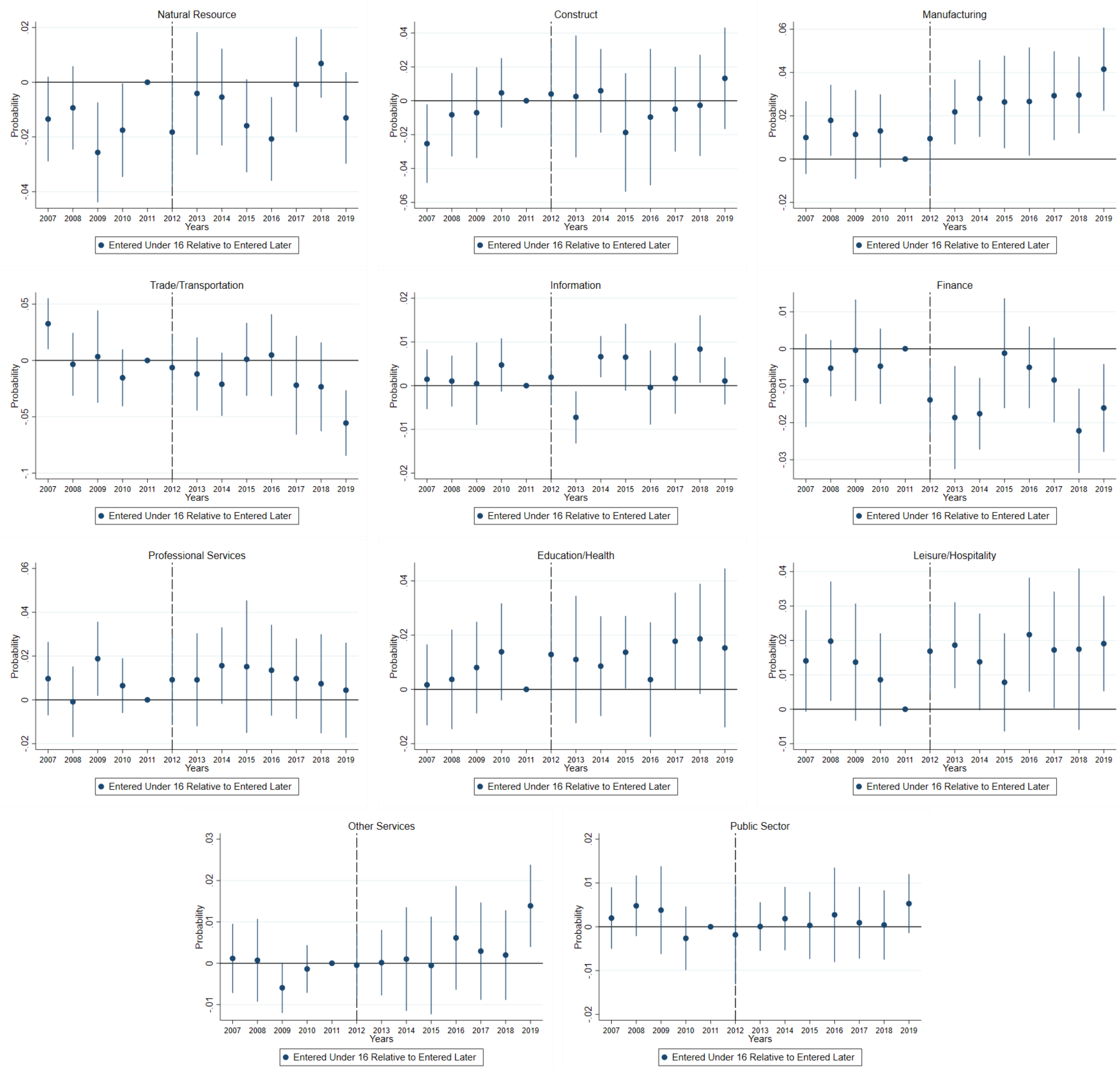
Figure A1: Probability of Moving Among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. These individuals must also meet the DACA education requirements. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. The coefficients from equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

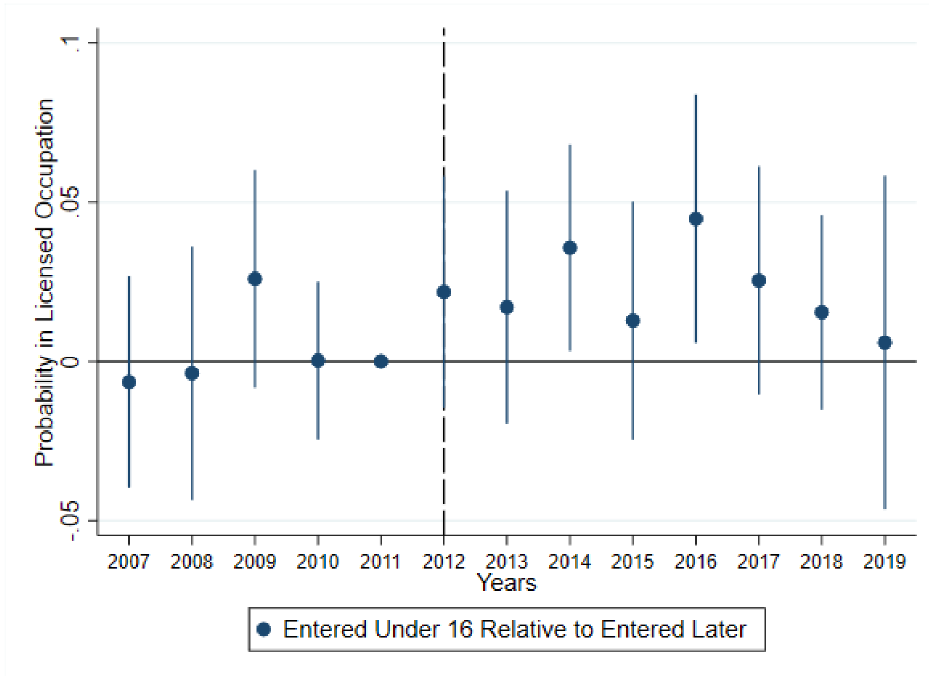
Figure A2: Industry-Specific Event Study Estimates



NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. These individuals must also meet the DACA education requirements. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. The coefficients from equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year where the outcome is a binary for being in the designated industry. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

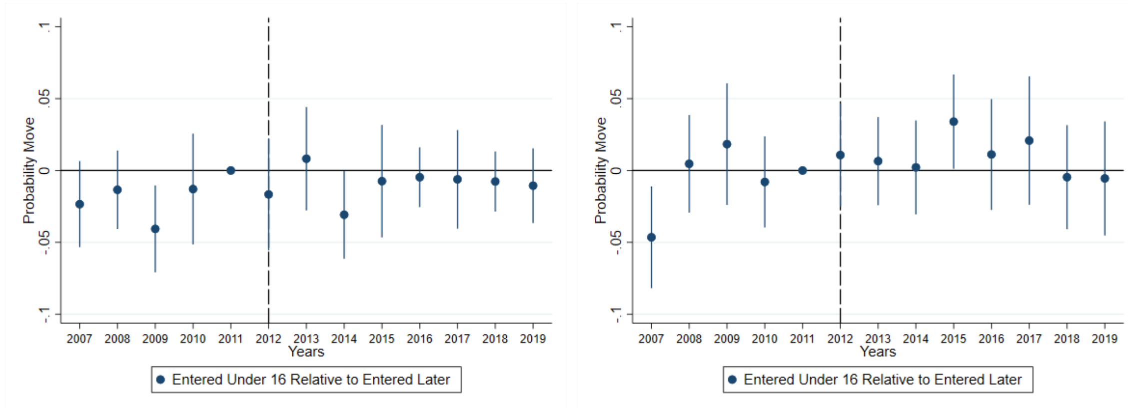
Figure A3: Probability of Being In Licensed Occupation Among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. These individuals must also meet the DACA education requirements. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. The coefficients from equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

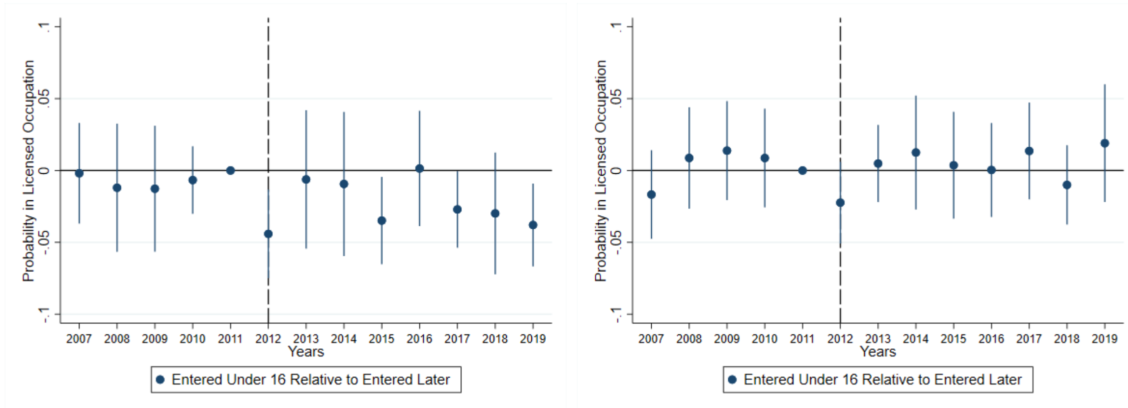
Figure A4: Placebo Impact of DACA on Geographic Mobility Among Ineligible Immigrants



NOTE: In the left panel, sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS who arrived to the US between the ages of 0-26 (consistent with the main analysis sample) and who were age 33-42 in 2012 and thus ineligible. We restrict birth cohorts to keep a similar age distribution in the treatment and counterfactual group, as in the main analysis sample. In right panel, sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989, and do not meet the DACA education requirements. The coefficients from equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

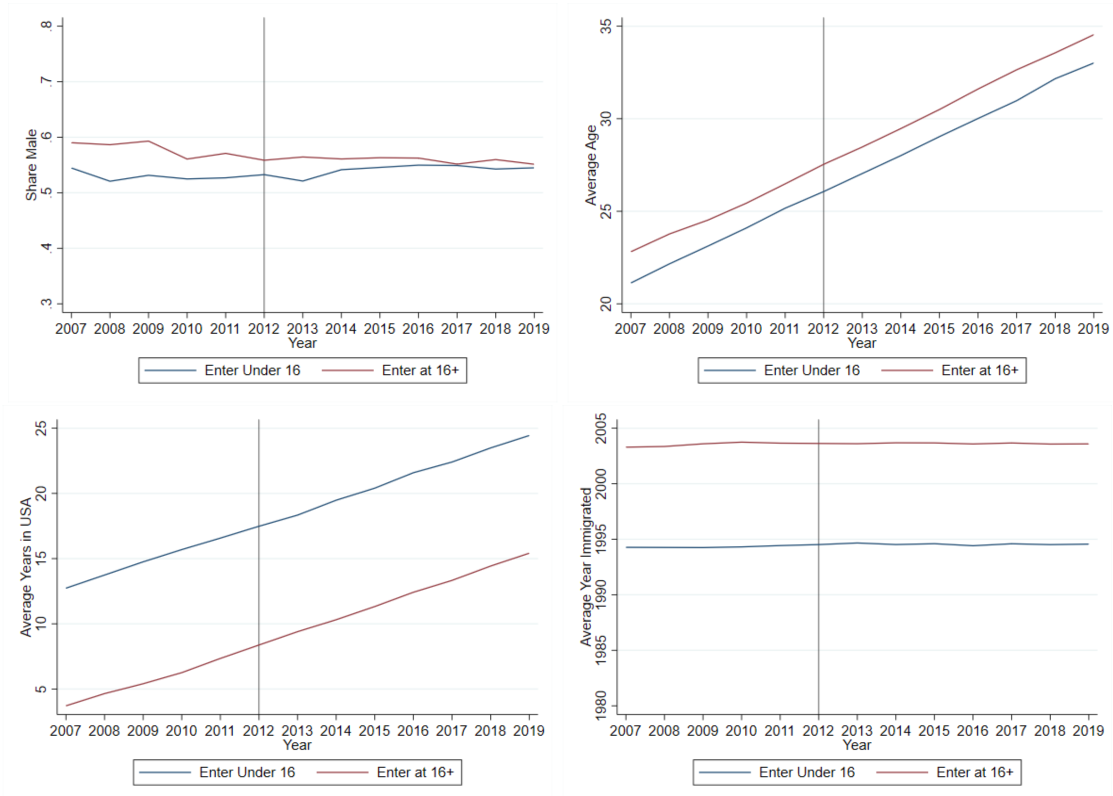
Figure A5: Placebo Impact of DACA on Occupational Mobility Among Ineligible Immigrants



NOTE: In the left panel, sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS who arrived to the US between the ages of 0-26 (consistent with the main analysis sample) and who were age 33-42 in 2012 and thus ineligible. We restrict birth cohorts to keep a similar age distribution in the treatment and counterfactual group, as in the main analysis sample. In right panel, sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989, and do not meet the DACA education requirements. The coefficients from equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

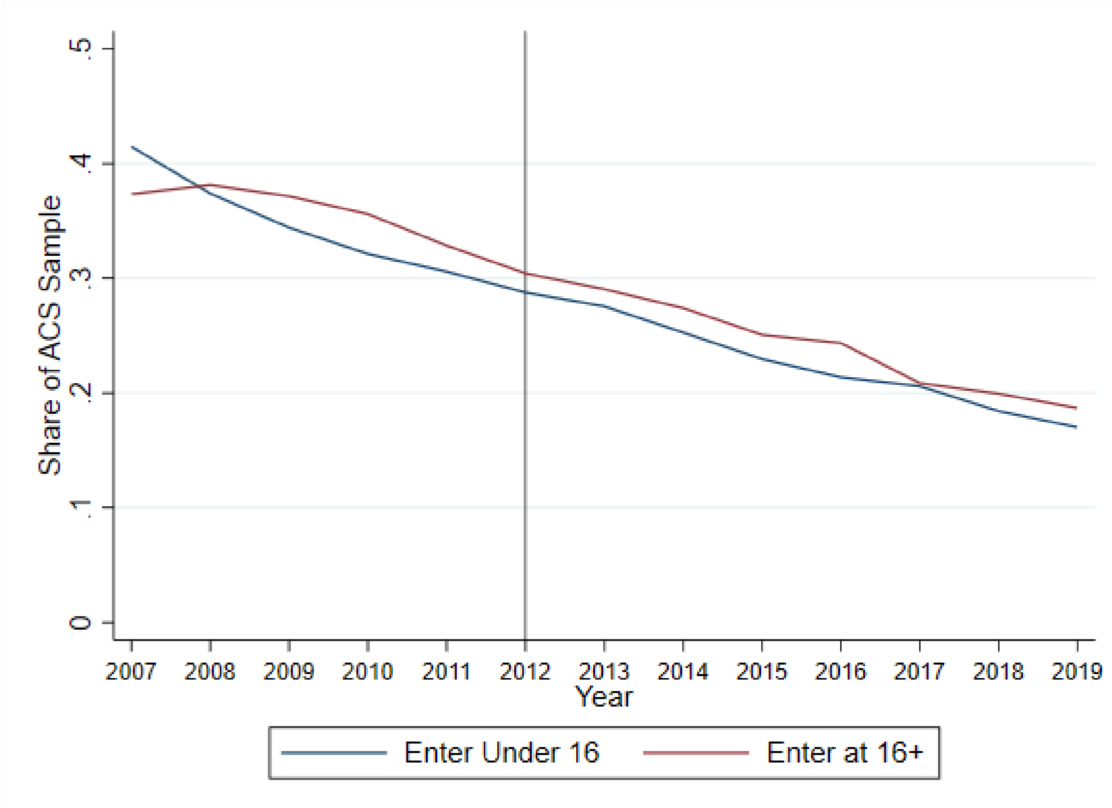
Figure A6: Differential Attrition: Trends in Average Characteristics of Treatment and Counterfactual Groups Over Time



NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989, unless otherwise specified. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Average characteristics are then calculated for individuals that arrived before their 16th birthday (treated) and after (counterfactual), using survey weights. If the policy led to differential attrition, we would expect the averages to diverge after the 2012 implementation of DACA.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

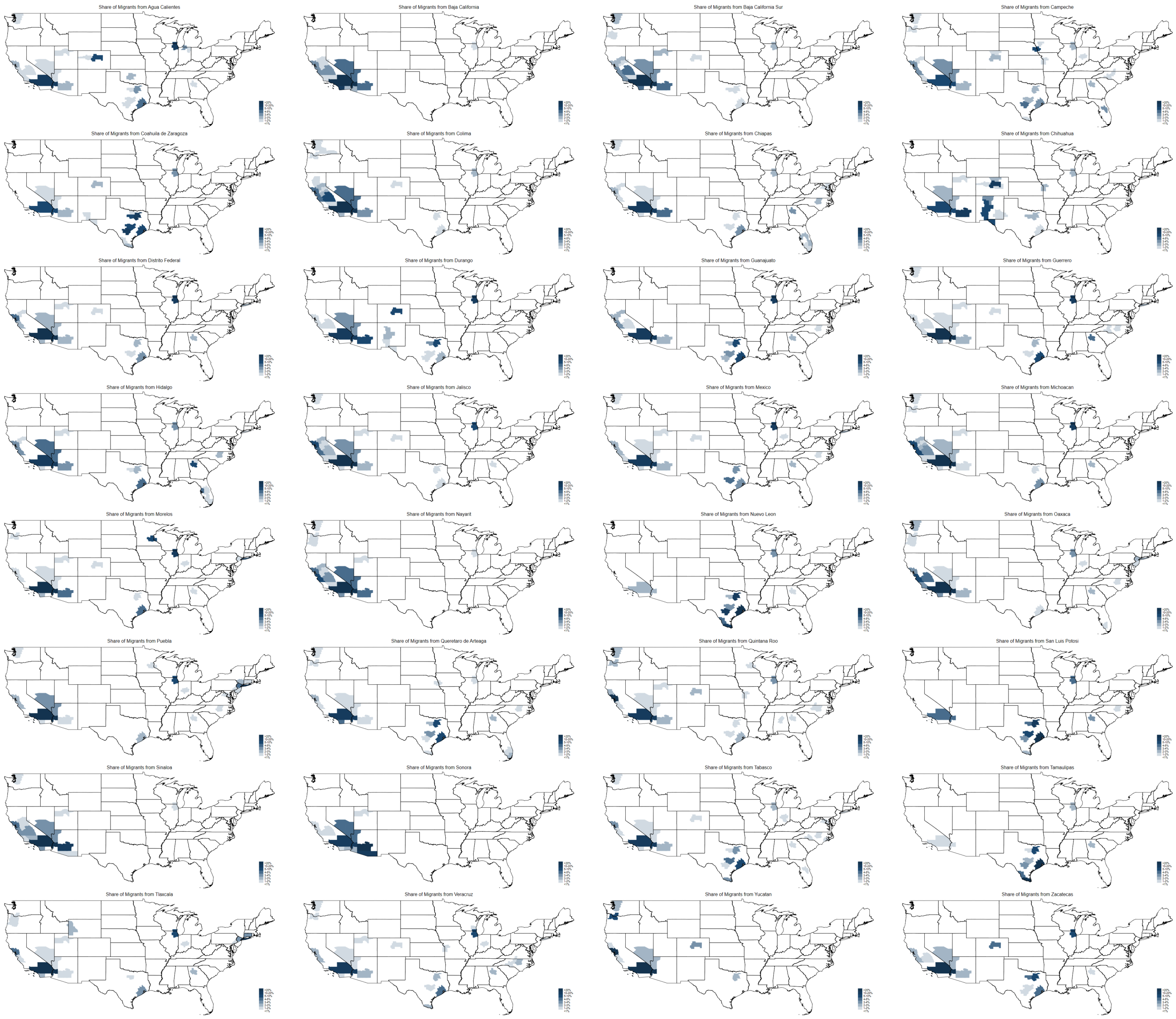
Figure A7: Differential Attrition: Share of ACS Sample in Treatment and Counterfactual Groups Over Time



NOTE: We construct the share of the ACS sample that was born in the last half of 1981, or 1982-1989, foreign born, Hispanic, and meets the DACA education requirements that fall in the analysis sample treatment and counterfactual groups in each year, using survey weights. Because the analysis sample conditions on arrival by 2007 the share of the total sample in the analysis sample will naturally decline over time as some immigrants eventually return home. If the policy led to differential attrition, we would expect the shares to diverge after the 2012 implementation of DACA.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

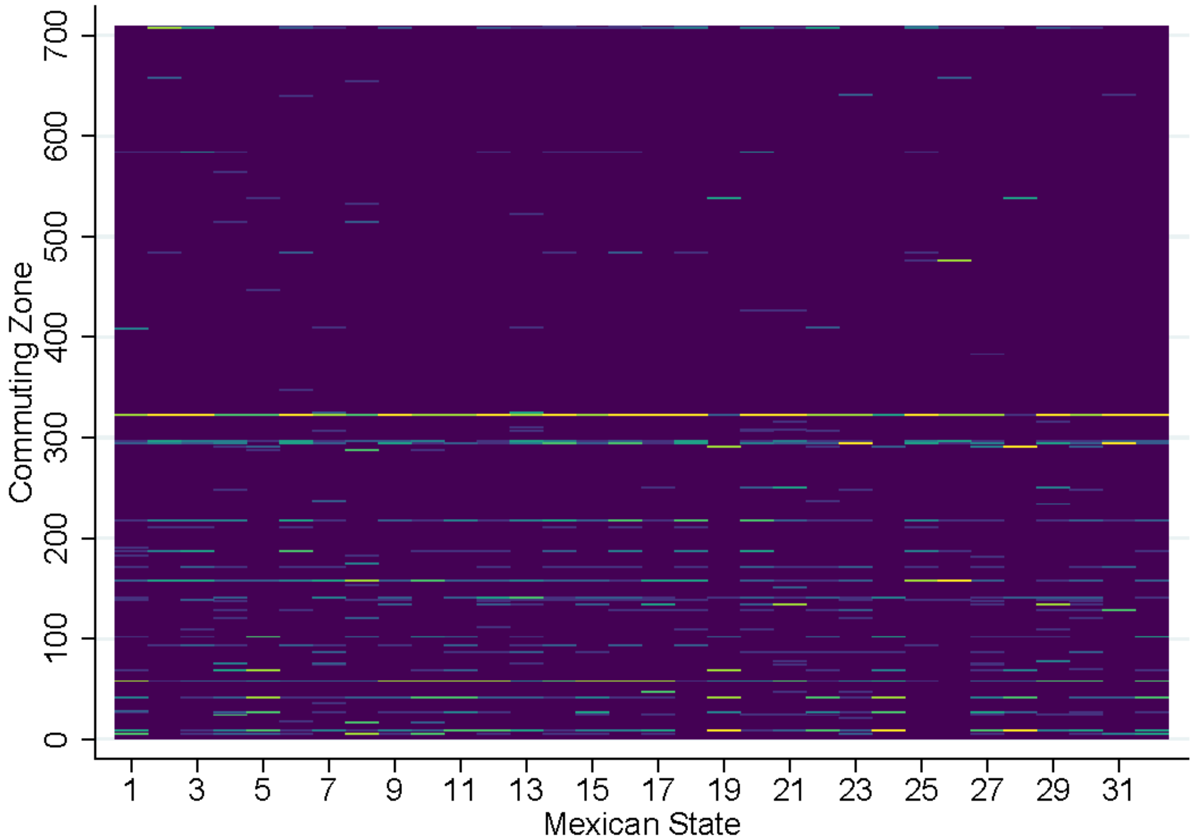
Figure A8: Mexico State to US Commuting Zone Migration Networks, 2005-2009 Mexican ID Data



NOTE:

SOURCE: Author's own calculations using 2005-2009 Mexican Consulate data on Mexico citizen IDs.

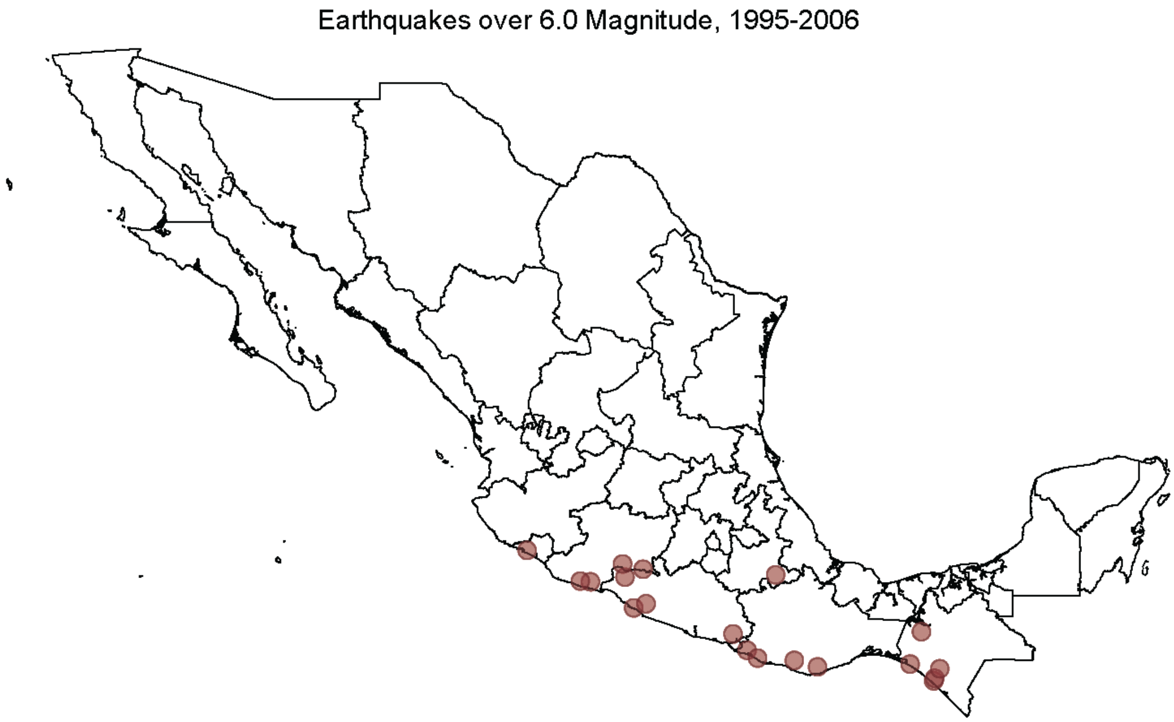
Figure A9: Mexican State US Commuting Zone Migrant Share Matrix



NOTE: The measure $share_{mc}$, which is the average share from equation (4) between 2005 and 2009 is plotted for each Mexican state and US Commuting Zone pair. Lighter colors represent a larger share of immigrants from the given Mexican state. The brightest line near the middle is the commuting zone that includes Los Angeles.

SOURCE: Author's own calculations using 2005-2009 Mexican Consulate data on Mexico citizen IDs.

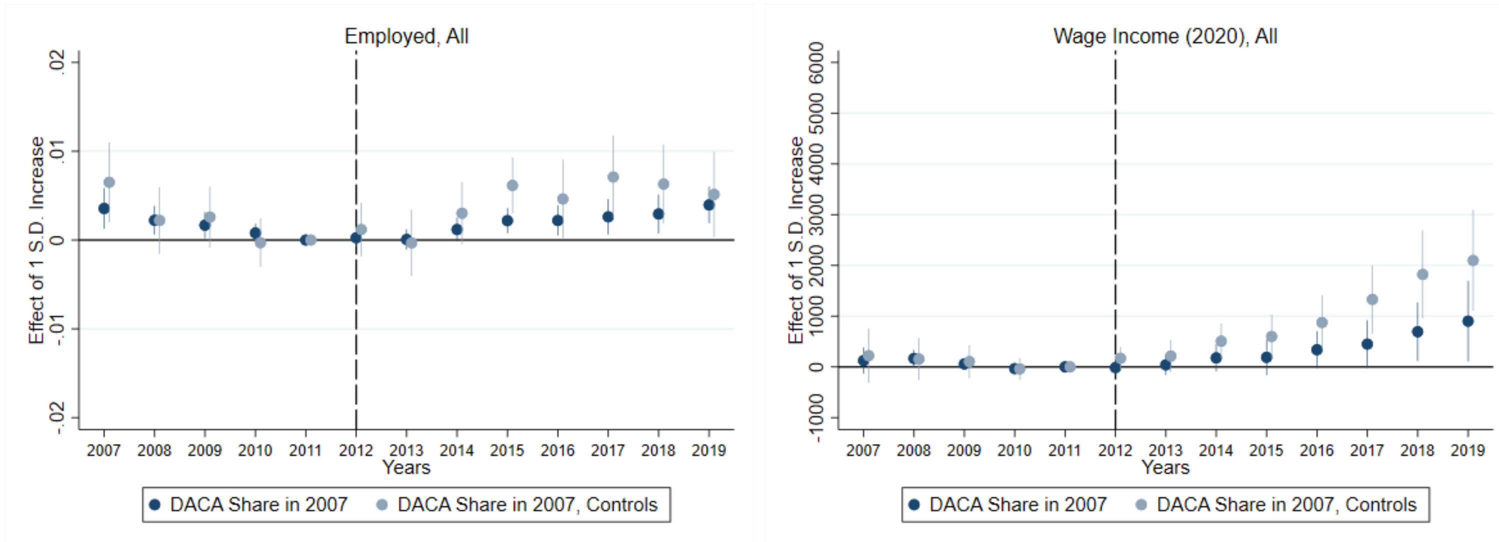
Figure A10: Location of Earthquakes Over 6.0 in Magnitude between 1995 and 2006



NOTE: The location of earthquakes over magnitude 6.0 between 1995 and 2006 are plotted.

SOURCE: Author's own calculations using US Geological Survey data on earthquake timing and coordinates.

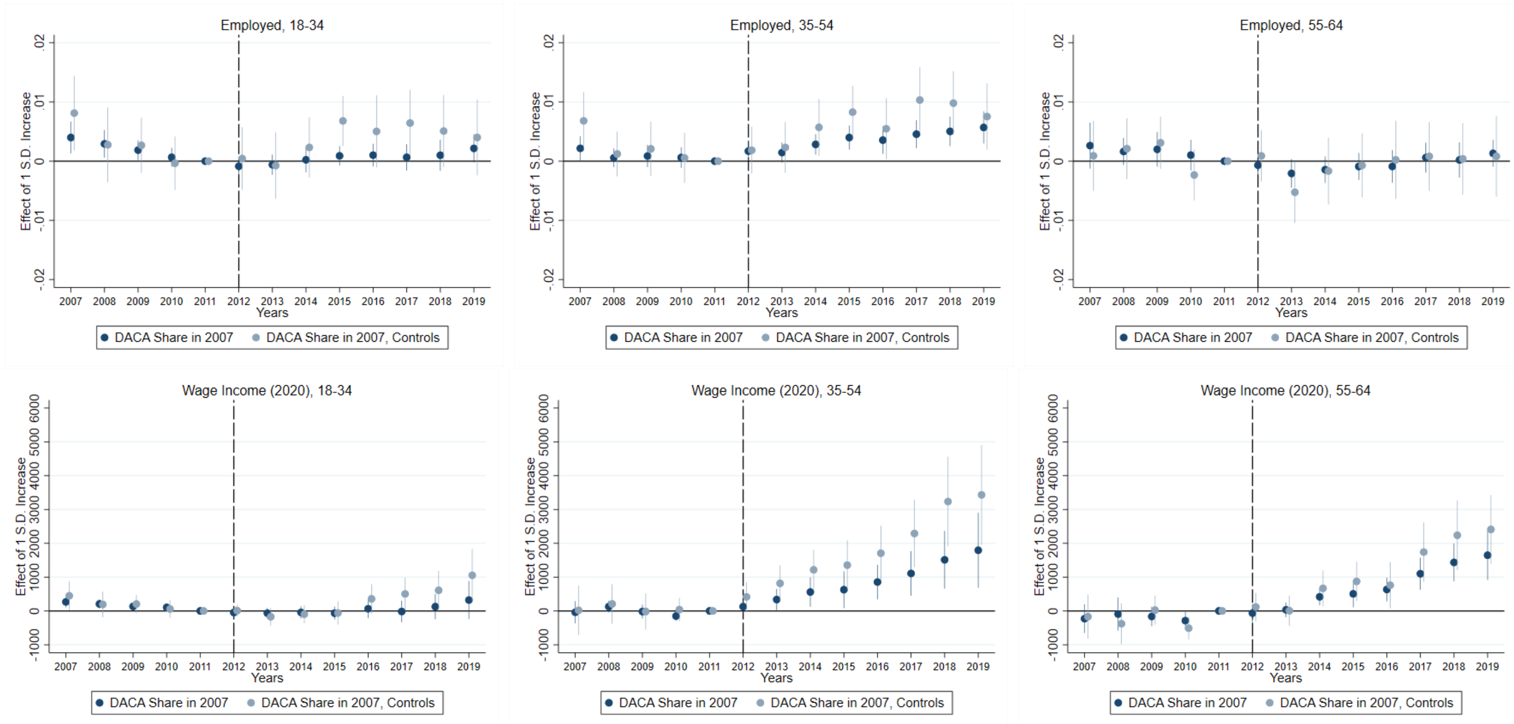
Figure A11: Robustness to Trend Controls: Spillover Impact of DACA on Labor Market Outcomes of Non-Foreign Born in the Commuting Zone



NOTE: Sample restricted to US-born respondents of the 2007-2019 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 that meet the DACA criteria used in the main analysis to identify the treated sample. The coefficients from equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the commuting zone level. Fixed effects for age, year, and commuting zone are included. The lighter plotted coefficients control for 2007 Hispanic share by year interactions and 2007 population by year interactions. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013).

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

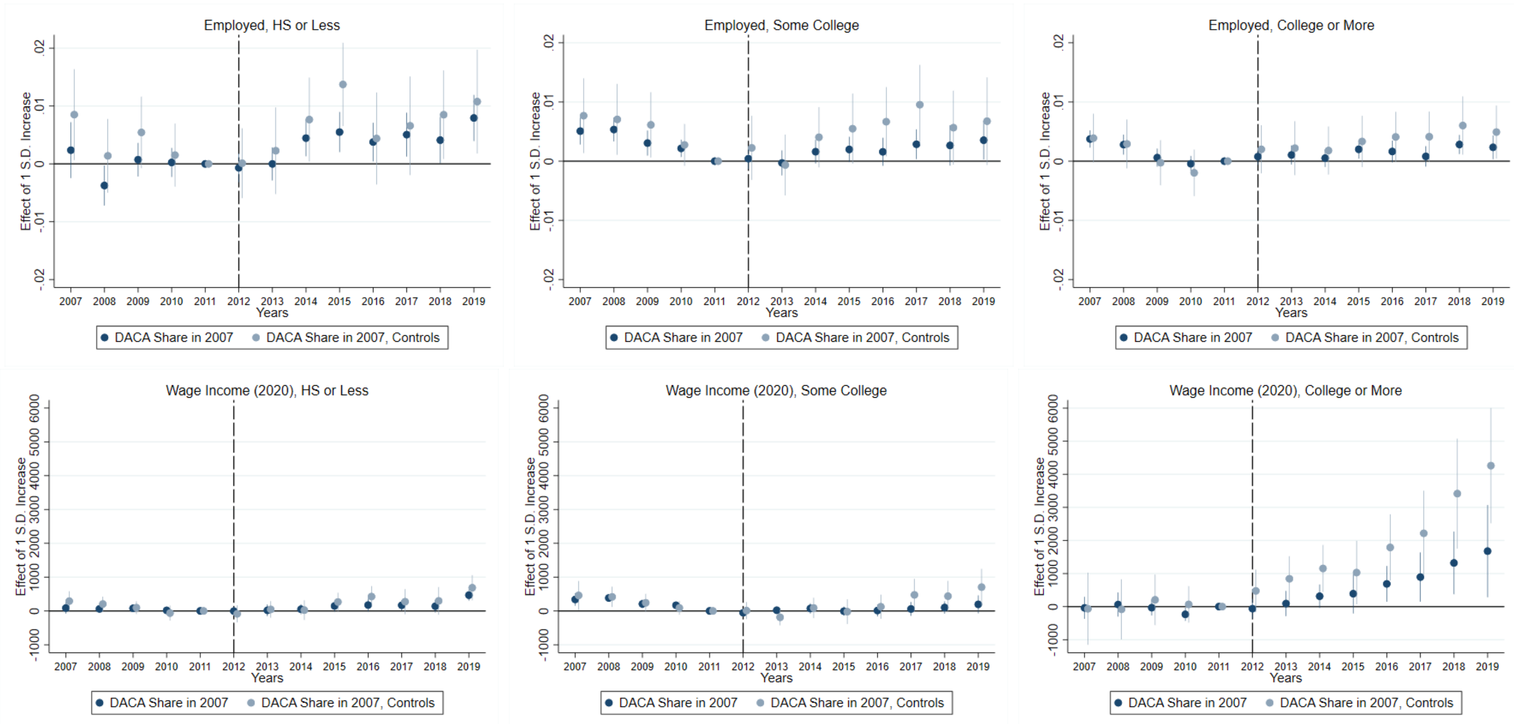
Figure A12: Robustness to Trend Controls: Spillover Impact of DACA on Labor Market Outcomes of Non-Foreign Born in the Commuting Zone, by Age



NOTE: Sample restricted to US-born respondents of the 2007-2019 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 that meet the DACA criteria used in the main analysis to identify the treated sample. The coefficients from equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the commuting zone level. Fixed effects for age, year, and commuting zone are included. The lighter plotted coefficients in the top panel control for 2007 Hispanic share by year interactions and 2007 population by year interactions. The lighter plotted coefficients in the bottom panel are obtained from equation (5), where historic migration networks, interacted with Mexican state earthquake shocks are used to instrument for the 2007 DACA eligible share. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013).

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

Figure A13: Robustness to Trend Controls: Spillover Impact of DACA on Labor Market Outcomes of Non-Foreign Born in the Commuting Zone, by Education



NOTE: Sample restricted to US-born respondents of the 2007-2019 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 that meet the DACA criteria used in the main analysis to identify the treated sample. The coefficients from equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the commuting zone level. Fixed effects for age, year, and commuting zone are included. The lighter plotted coefficients in the top panel control for 2007 Hispanic share by year interactions and 2007 population by year interactions. The lighter plotted coefficients in the bottom panel are obtained from equation (5), where historic migration networks, interacted with Mexican state earthquake shocks are used to instrument for the 2007 DACA eligible share. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013).

SOURCE: Author's own calculations using 2007-2019 ACS microdata.

Table A1: Impact of DACA on Where DACA-Eligible Immigrants Move Relative to Barely Ineligible Immigrants

	Move to PUMA with Average, Non-College Wages		Move to PUMA with Average Test Scores		Move to PUMA in State with Average Violent Crime		Move to PUMA with Share Hispanic		Move to PUMA	Move to PUMA
	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	in MSA	in Border State
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Entered Under 16*Post-DACA	0.009***	0.004**	0.005*	0.009***	0.011***	0.002	0.012***	0.002	0.010***	0.003
	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Entered Under 16	-0.005	-0.003	-0.003	-0.004**	-0.005*	-0.002	-0.007**	-0.001	-0.007**	0.001
	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)
Dependent Mean	0.04	0.02	0.03	0.04	0.05	0.01	0.05	0.01	0.05	0.02
Observations	94,179	94,179	89,181	89,181	94,179	94,179	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Violent crime rate measured at the state level. Border states include California, Arizona, New Mexico, and Texas. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A2: Robustness of Impact of DACA on Mobility of DACA-Eligible Immigrants to Sample Restrictions

	Outcome: Move in the Past 12 Months						
	Include Non-Hispanic	Include Citizens	Exclude 2007 Arrivals	Include 2005-2006	Exclude 2007	Exclude 2017-2019	Teen Arrivals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Entered Under 16*Post-DACA	0.044***	0.038***	0.044***	0.049***	0.037***	0.039***	0.025**
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)	(0.008)	(0.011)
Entered Under 16	-0.067***	-0.042***	-0.050***	-0.059***	-0.047***	-0.052***	-0.041***
	(0.005)	(0.006)	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)
Dependent Mean	0.24	0.20	0.20	0.22	0.20	0.22	0.21
Observations	165,429	147,679	88,735	111,912	84,802	77,422	47,996

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989, unless otherwise specified. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. Column (1) no longer restricts the sample to Hispanics. Column (2) no longer restricts the sample to non-citizens. Column (3) excludes individuals that arrived in 2007, as DACA requires arrival by June 15, 2007. Column (4) includes observations from 2005 and 2006, even though the arrive before 2007 requirement affects them differently. Column (5) excludes observations from 2007, as these are potentially new arrivals. Column (6) excludes the years affected by Trump-era uncertainty and changes to DACA in 2017-2019. Column (7) restricts the sample to only include individuals who came to the US between ages 11 and 19, in an attempt to identify a more similar sample. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A3: Robustness of Impact of DACA on Mobility of DACA-Eligible Immigrants to Specification

	Outcome: Move in the Past 12 Months			
	State Trends	State-by-Year F.E.	Entry Age F.E.	Age-by-Year F.E.
	(1)	(2)	(3)	(4)
Entered Under 16*Post-DACA	0.044***	0.045***	0.042***	0.041***
	(0.007)	(0.006)	(0.007)	(0.007)
Entered Under 16	-0.053***	-0.054***	0.000	-0.051***
	(0.006)	(0.006)	(0.000)	(0.006)
Dependent Mean	0.21	0.21	0.21	0.21
Observations	94,179	94,087	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989, unless otherwise specified. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. Column (1) included state-specific time trends, as in previous work (Pope, 2016). Column (2) includes state-by-year fixed effects, to control for state level shocks and policy and make this a comparison between immigrants in the same state. Column (3) includes age at entry fixed effects. Controlling for the age at entry makes this similar to a regression discontinuity. Column (4) includes age-by-year fixed effects, making this a comparison between treated and counterfactual people of the same age in the same year. excludes individuals that arrived in 2007, as DACA requires arrival by June 15, 2007. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A4: Impact of DACA on Family Setting of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

	Never Married (1)	Divorced or Separated (2)	Married (3)	Married to Citizen (4)	Any Children (5)	Number of Children (6)	Live with Parent (7)
Entered Under 16*Post-DACA	0.018**	0.003	-0.021**	0.013*	-0.050***	-0.124***	-0.081***
	(0.007)	(0.004)	(0.008)	(0.007)	(0.008)	(0.015)	(0.006)
Entered Under 16	0.039***	0.017***	-0.056***	0.025***	-0.012	0.015	0.236***
	(0.007)	(0.003)	(0.007)	(0.006)	(0.007)	(0.014)	(0.013)
Dependent Mean	0.53	0.05	0.42	0.17	0.46	0.93	0.26
Observations	94,179	94,179	94,179	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A5: Differential Impact of DACA Over Time on Family Setting Outcomes of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

	Never Married (1)	Divorced or Separated (2)	Married (3)	Married to Citizen (4)	Any Children (5)	Number of Children (6)	Live with Parent (7)
Entered Under 16*Post-DACA (2012-2015)	0.009	0.002	-0.011	0.011**	-0.037***	-0.081***	-0.064***
	(0.009)	(0.004)	(0.011)	(0.005)	(0.008)	(0.017)	(0.007)
Entered Under 16*Post-DACA (2016-2019)	0.030***	0.004	-0.034***	0.016	-0.067***	-0.181***	-0.103***
	(0.008)	(0.006)	(0.008)	(0.013)	(0.010)	(0.021)	(0.007)
Entered Under 16	0.039***	0.017***	-0.056***	0.025***	-0.011	0.016	0.237***
	(0.006)	(0.003)	(0.007)	(0.006)	(0.007)	(0.014)	(0.014)
Dependent Mean	0.53	0.05	0.42	0.17	0.46	0.93	0.26
Observations	94,179	94,179	94,179	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

Table A6: Impact of DACA on Educational Attainment of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

	Attend College (1)	Some College (No Degree) (2)	Two-Year Degree (3)	Four-Year Degree (4)	Advanced Degree (5)	Bachelors in STEM (6)	Median Wages of Major (7)
Entered Under 16*Post-DACA	-0.045***	-0.022**	0.008**	0.017***	0.001	-0.001	266.793
	(0.007)	(0.009)	(0.003)	(0.003)	(0.001)	(0.002)	(162.349)
Entered Under 16	0.079***	0.081***	0.023***	-0.004	-0.004**	-0.002**	-171.260*
	(0.011)	(0.012)	(0.003)	(0.003)	(0.002)	(0.001)	(99.992)
Dependent Mean	0.13	0.24	0.05	0.07	0.02	0.02	15327.20
Observations	94,179	94,179	94,179	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign born, non-citizens respondents of the 2007-2019 ACS born in the last half of 1981, or 1982-1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. $p< 0.01$ ***, $p< 0.05$ **, $p<0.1$ *.

Table A7: Predictive Power of 2007 DACA Eligible Share on Commuting Zone Characteristics and Trends

	Predictive Power of DACA Share	
	Level in 2007	Δ 2007-2011
	(1)	(2)
Total Population (1,000s)	337.252***	13.786***
	(106.110)	(2.832)
Share Male	0.001***	-0.001*
	(0.000)	(0.000)
Age	-0.732***	-0.051*
	(0.067)	(0.030)
Share NH White	-0.086***	-0.005***
	(0.006)	(0.001)
Share NH Black	-0.009***	-0.000
	(0.003)	(0.000)
Share NH Other	-0.004***	-0.001***
	(0.001)	(0.000)
Share Hispanic	0.093***	0.005***
	(0.006)	(0.001)
Share Foreign Born	0.042***	-0.001
	(0.003)	(0.001)
Share Married	-0.006***	-0.001
	(0.001)	(0.001)
Share Never Married	0.010***	0.002***
	(0.001)	(0.001)
Share Divorce/Separated	-0.004***	-0.002***
	(0.001)	(0.001)
Share Less than HS	0.019***	0.001
	(0.002)	(0.001)
Share HS	-0.019***	0.019***
	(0.001)	(0.001)
Share Some College	-0.004***	-0.020***
	(0.001)	(0.001)
Share 4 Year Degree	0.002	0.000
	(0.001)	(0.000)
Share Advanced Degree	0.002**	-0.000
	(0.001)	(0.000)
Share Not in LF	-0.002	-0.002**
	(0.002)	(0.001)
Share Employed	0.002	-0.000
	(0.002)	(0.001)
Share Unemployed	0.000	0.002***
	(0.000)	(0.001)
Average Wage Income	358.082**	-80.984*
	(141.384)	(42.162)

NOTE: Sample collapsed to the commuting zone by year level, including all respondents 18-64 in the 2007-2019 ACS. Standard errors corrected for clustering at the commuting zone level. p< 0.01 ***, p< 0.05 **, p<0.1 *.

Table A8: First Stage: Relationship between Mexico State Migrant Shocks and Share of Population DACA Eligible in 2007

	Share of 2007 Population DACA Eligible (S.D.)			Share of 2007 Population Hispanic, Not DACA Eligible (S.D.)		
	Any Event	Max Magnitude	Relative Max Magnitude	Any Event	Max Magnitude	Relative Max Magnitude
	(1)	(2)	(3)	(4)	(5)	(6)
Migrant Shock	350.844***	46.886***	7.641***	256.039***	34.252***	5.673***
	(38.620)	(5.232)	(1.258)	(25.115)	(3.389)	(0.814)
F Statistic	82.5	80.3	36.9	103.9	102.2	48.6
Dependent Mean	0.34	0.34	0.34	0.45	0.45	0.45
Observations	2,383,193	2,383,193	2,383,193	2,383,193	2,383,193	2,383,193

NOTE: Sample restricted to US-born respondents of the 2007 ACS, ages 18 to 65. The *Share DACA Eligible* captures the share of the population 18 to 65 in 2007 that meet the DACA age, education, and arrival criteria and were not citizens. The *Share DACA Age-ineligible* captures the share of the population that meet age, education, and arrival criteria and were not citizens but did not arrive in the US before age 16. The coefficients from equation (6) are provided with standard errors corrected for clustering at the commuting zone level. Individuals are mapped from PUMA to commuting zone using a population weighted crosswalk. The mapping is not one-to-one. As such, individuals in PUMAs that intersect multiple commuting zones are assigned one observation for each of these commuting zones, and their survey weights are scaled down by the share of the PUMA population in the given commuting zone, following Autor and Dorn (2013). p< 0.01 ***, p< 0.05 **, p<0.1 *.

SOURCE: Author's own calculations using 2007-2019 ACS microdata.