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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 and licensed occupations. We then examine whether these gains to immigrants spill over and affect labor market outcomes of U.S.-born workers. Exploiting immigrant enclaves and source-country flows of DACA-eligible immigrants to isolate plausibly exogenous variation in the concentration of DACA recipients, we show that in labor markets where more of the working-age population can access legal protection through DACA, U.S.-born workers see little-to-no change in employment rates and actually observe increases in wage earnings after DACA's implementation. These gains are concentrated among older and more educated workers, suggesting immigrant workers complement U.S.-born workers and immigrant legalization generates broader local labor market benefits.

JEL Classification Codes: J15, K37, R23

Key Words: Legal states, DACA, immigration, geographic mobility, job mobility, occupational licensing, local labor markets

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1 Introduction

A large literature shows that "mobility" human-capital investments such as 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 whether 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 that the worker experiences a return on the costly adjustment. Although we have evidence of how legalization affects educational attainment (Amuedo-Dorantes and Antman, 2016; Ballis, 2023; Hsin and Ortega, 2018; Kuka et al., 2020), we do not know how it affects mobility human-capital investments. These investments not only might increase individual productivity but could affect local labor market dynamisms, leading to aggregate effects. With a large unauthorized population, as in the U.S., this could generate large externalities that affect citizens and other legal residents.

We explore 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' economic outcomes. We also explore how providing legal status to unauthorized immigrants affects the labor market outcomes of U.S.-born workers. These externalities could be both positive and negative. Understanding how granting legal status

[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).

affects overall social welfare is an empirical question. 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 U.S.-born 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 U.S.-born workers would be affected by 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. Recent legal decisions regarding DACA highlight the need to understand the overall welfare effects of immigrant legalization.

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 who came to the United States as children without legal status. 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 on June 15, 2012, had to be enrolled in school or have a high school diploma or the equivalent, and must not have 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 to have continually resided in the U.S. since then. As such, eligibility status was predetermined at five 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 who 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 sixteenth birthday. As such, we are able to compare outcomes for individuals in the same birth cohorts and with similar characteristics, some of whom were eligible, while others were ineligible. In an event study, we show that the propensity to move follows a similar trend for eligible and ineligible individuals from 2007 to 2011.[2] Then, in 2012, there was a discrete 4.2 percentage point increase in the probability of moving among DACA-eligible individuals.[3] 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, as is consistent with more moves to economic opportunity. Changes in the living arrangements of the DACA-eligible are consistent with their becoming less tied to a local area (Gihleb et al., 2021), which could increase the dynamism of local labor markets (Blanchard and Katz, 1992; Molloy et al., 2016).

We also see significant changes in the occupational composition of jobs DACA-eligible individuals hold. After DACA implementation, DACA-eligible individuals shift from occupations like cashiers, clerks, and mechanics to occupations like child-care worker, production worker, manager, teacher, nurse, and engineer. DACA-eligible individuals move into occupations with higher median earnings and skill-oriented occupations that require occupational licenses. DACA gives immigrants new access to occupations that require legal credentials. 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-eligible group is 3.5 percentage points more likely to be employed and earns over $1,300 more per year relative to the barely ineligible. There is also an increase in wage income among the working population, suggesting that this is not all driven by the extensive margin. We estimate that 85 percent of the gain in wage income, conditional on employment, associated with DACA can be explained by endogenous human-capital investment responses, with most of this being explained by changes in the occupations that

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

[3]Calculated based on the respondent's residence information from previous years, as provided by ACS.

DACA-eligible individuals work in. Legalization as a policy yields large benefits to immigrant recipients, but we also want to know whether the policy has unintended spillover effects on U.S.-born residents.

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 responds more strongly to local labor market conditions than the behavior of 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. However, increased access to jobs could create more competition for U.S.-born workers, potentially leading to detrimental effects.

We explore the possibility of both within-occupation and within-local-labor-market spillovers. At the occupation level, there is not a strong correlation between DACA-eligible employment growth and U.S.-born employment rates. Using the existing distribution of pre-1981 DACA-ineligible immigrants, we combine information on source-country enclaves and DACA-eligible-specific immigrant flows to isolate plausibly exogenous variation in the share of the population that was DACA eligible in 2007. We find that U.S.-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 did see significant increases in wage earnings after 2012. These gains are concentrated among college-educated workers and workers over 34, while workers without a college degree or under 34 do not, as a whole, experience the same wage gains. This pattern is consistent with a pattern of production substitutes and complements, with legalized immigrants complementing older, more-educated U.S.-born workers but potentially competing with similar-aged, less-educated workers in the production process. We see no evidence of U.S.-born workers experiencing negative spillovers from immigrant legalization in the local labor market.

Previous work has documented that DACA increases employment (Amuedo-Dorantes and Antman, 2016; Pope, 2016). There is evidence that DACA increases high school completion (Kuka et al., 2020), while the evidence on college attendance is more mixed: some evidence suggests a positive influence from DACA (Kuka et al., 2020), but other evidence suggests that 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 to this paper 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 that 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 the externalities it might impose on native workers. Ballis (2023) shows that DACA produced positive spillovers in the classroom among Los Angeles County students, but we do not know how this affected labor markets. We provide new evidence that the benefits of legalization extend beyond immigrants' outcomes; they also create positive labor market spillovers for U.S.-born workers. 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 an executive order after the

Development, Relief and Education for Alien Minors (DREAM) Act failed to pass Congress. The DREAM Act had first been proposed in 2001 as an attempt to provide a path to legalization for unauthorized immigrants, but it did not garner sufficient support in Congress. It languished there 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 U.S. 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 U.S.[4] The first applications were accepted in August 2012 (see Appendix Figure A1 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 U.S. before their sixteenth birthday; 2) they must have lived continuously in the U.S. since June 15, 2007; 3) they must have been under the age of 31 by June 15, 2012; 4) they must currently be enrolled in school or have a high school diploma or equivalent;[5] 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 (in 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 both initial and current rules it can be renewed indefinitely. Empirically, a majority of 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, President Donald Trump's administration issued a memorandum rescinding DACA. This memo prohibited all first-time

[4] 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.

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

applications and allowed for renewals only until October 5, 2017, after which the program was to be phased out. In January 2018, the State of 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, a federal district court judge in Texas challenged the legality of DACA, limiting the program to renewals and prohibiting U.S. Citizenship and Immigration Services (USCIS) from granting initial DACA requests. As of October 2022, USCIS still approves renewals but is not permitted to approve new applications. On September 13, 2023, this same federal district court judge sided with the 2021 challenge, ruling DACA illegal and leaving the program in uncertainty.

The take-up of DACA was both large 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 residing in Texas. The remainder were 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 have arrived by June 15, 2007, a full five years before the policy, migrants were not able to move to the U.S. 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 that 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 of 1 percent in an 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 noncitizens. 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 United States, we can identify immigrants who moved to the U.S. prior to 2007.[6] Using birth year and quarter of birth, we can determine how old immigrants were when the policy was enacted and whether they meet the "under 31 by June 2012" requirement.[7] Using educational attainment and schooling measures, we can identify individuals who meet the education requirement. By combining year, year of immigration, and birth year, we can identify immigrants' age when they arrived in the U.S. 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 sixteenth birthday. In our analysis, we will describe these groups as the DACA-eligible (treatment) and ineligible (counterfactual) groups.

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

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

The ACS includes an individual's current state of residence. Identification of smaller geographic entities, like counties, is suppressed for privacy purposes and only available if more than 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 whether they have moved in the past 12 months, and if so, where they were living before the move. From this, the individual's state and Migration PUMA (MIGPUMA) are provided for anyone who moved. MIGPUMAs do not correspond one-to-one to PUMAs. MIGPUMAs must contain entire counties and are thus sometimes the union of multiple contiguous PUMAs.

A PUMA must be completely 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 that individual moved 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 MIGPUMAs according to 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 from individuals out of the local area, and long-distance moves across states, and the types of places individuals move to.

The ACS also provides detailed three-digit industry and occupation codes for workers. From these measures, we can examine how DACA affects the occupation and industrial distribution of workers. Unfortunately, unlike as with migration, we do not observe an individual's occupation or industry from the previous year, so we cannot examine occupation-to-occupation specific gross flows, only the net compositional change. From this, we can identify occupations and industries that DACA recipients were more likely to shift into after the policy change. We focus on 2-digit occupations and 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).

Using data on U.S.-born workers in the ACS, we construct median wage earnings by three-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.[8] Occupational licenses have been shown to boost wages 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 the routine, math-skill, and social-skill task composition of occupations of DACA-eligible workers. Because unauthorized immigrants are excluded from formal employment, 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 noncitizens who meet the following criteria: 1) they were under the age of 31 by the end of June 2012, 2) they entered the U.S. 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 co-

[8]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 that occupation in the state report that a license is required.

horts over time, some of whom are DACA-eligible and "treated," while others are untreated because they moved to the U.S. after their sixteenth 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 noncitizens because citizens do not need the protections of DACA.[9]

As seen in Table 1, the observations from 2007 to 2011, prior to the enactment of DACA, are similar on average.[10] In the pre-period, the treated group is about 1.5 years younger, 4 percentage points less likely to be male, and less likely to be married than similar individuals who arrived after their sixteenth 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, as is consistent with existing work documenting the labor market effects of DACA (Pope, 2016).

One concern is that the implementation of DACA could lead to differential attrition from the treatment and counterfactual groups. Unauthorized immigrants who arrived in the United States after their sixteenth birthday and were ineligible for DACA might differentially emigrate from the U.S. 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 A2, we document how the fixed characteristics of the treatment and counterfactual sample, such as gender, age, year of arrival, and years in the U.S., change over time. If these average measures start to trend differently after the implementation of DACA, this could indicate

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

[10]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 prior to 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.

that DACA led to differential attrition. Because of our sample criteria, measures like age and years in the United States 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 A3, 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 trends in both the treatment and counterfactual groups are similar, suggesting we are not missing additional ineligible individuals who leave after the policy is implemented.[11] 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.

4 Estimation Equation and Identification

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

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

To explore geographic mobility, our outcome of interest will be a binary variable that equals 1 if the individual moved during the past 12 months. We will also look at whether the individual moved to a different MIGPUMA (proxy for a different local labor market), to a different state, or at the industry and occupation they ended up in. The explanatory

[11]The trend for the ineligible does become steeper between 2016 and 2017, when President Trump took office. However, as we show in Appendix Table A3, the estimates are insensitive to excluding the Trump presidency.

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 U.S. 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 to 2010, we can evaluate pretrends, 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 two-way fixed-effects models and staggered treatment timing (Callaway and Sant'Anna, 2020; de Chaisemartin and D'Haultfœuille, 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 who met the 2007 arrival, 31-year-old age cap and the education requirements. As such, we are estimating the effect of DACA among similarly aged individuals and comparing outcomes for those who are eligible, relative to those who meet all other criteria but are just barely ineligible because they arrived when they were older than 16. This allows us to observe both treated and counterfactual individuals at the same point in the life cycle.[12] 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 sixteenth 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 pretrends in the event study specification.

We will also estimate difference-in-differences specifications, where the *Entered Under*

[12] If we were to use one of the other criteria (entered 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.

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 (2), we can succinctly identify the average effects of legal eligibility on mobility as well as other outcomes, such as labor market outcomes. 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 1, 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 that one-third (0.014/0.042) of the DACA-induced moves were not local, but were 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 noncitizens by 20 percent.[13]

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 for whether the individual moved and whether the destination is in the appropriate bin. DACA eligibility increases moves to PUMAs with above-median-average wages by 1.0 percentage point, while migration to below-median-wage PUMAs only increases such moves 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 to places with better employment rates. We explore where people move in more detail in Appendix Table A1 and find that after DACA, eligible individuals are more likely to move to places with higher wages for noncollege workers, larger Hispanic populations, more urban areas, and "sanctuary cities," but not to places with better schools or lower crime, and not necessarily to states that border Mexico.

The increase in mobility due to DACA suggests that DACA-eligible immigrants have become more mobile and less rooted, potentially leading to more dynamic labor markets. Consistent with Gihleb et al. (2021), we find that DACA affects the living arrangements of recipients. As seen in Appendix Table A5, DACA-eligible women are more likely to be married, and more likely to be married to a citizen, while DACA-eligible men are more likely never to have been married. Both sexes see a reduction in fertility from DACA eligibility, and both are less likely to be living with a parent.[14] These patterns are consistent with

[13]Event study plots for out-of-PUMA and out-of-state moves can be found in Appendix Figure A4. Once again, pretrends are flat, with discrete increases in 2012. However, given the rarity of these moves, the single-year standard errors are less precise.

[14]Because the ACS only provided repeated cross-sections, we are not able to determine whether this

DACA increasing mobility and reducing rootedness in a local area. This increased mobility responsiveness could increase the insurance value that immigrants generate for native workers (Cadena and Kovak, 2016).

5.2 Impact of DACA on Occupational Mobility

We next explore how DACA affects DACA-eligible individuals' occupational distribution relative to those who are barely ineligible. In Figure 2, we plot the β coefficients from Equation (2), where the outcome is a binary variable that equals "1" if the person works in the given two-digit occupation. DACA-eligible individuals shift out of unemployment and jobs like cashier, customer service representative, or food services manager and into jobs like child-care worker, production worker, teacher, nurse, engineer, and manager in an office setting. DACA shifts immigrants into employment and from entry-level service jobs to more skills-based occupations. Often these jobs require some formal training or credential.

Given this shift in 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 more than $1,000$ higher. Even when we condition on being employed, we observe an increase in occupation median earnings. This is not all driven by extensive margin employment effects, with some DACA-eligible individuals moving up the occupation ladder. Consistent with the shift to skills-based occupations seen above, these individuals 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). Once immigrants are granted work authorization, they enter licensed occupations that were previously unavailable to them.

We do not see significant changes in the percentile rank of routine, math-skill, or social-skill task composition of immigrants' occupations, although these are estimated imprecisely.

reduction in the number of children is due to a reduction in lifetime fertility or delay. As seen in Appendix Table A6, 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.

There is a marginally significant 0.7 percentage point decrease in the probability of being self-employed, consistent with immigrants moving into formal employment. As documented in Appendix Table A2, access to DACA does not significantly change job characteristics like the number of hours worked or working the night shift, but it does lead to small, significant increases in commute time.[15] As seen in Appendix Figure A6, there is a corresponding industry shift as well. The occupation and industry patterns are similar for men and women, although the shift among women is more concentrated in education and health services, while the increase among men is more concentrated in manufacturing and natural resources (Appendix Figure A7).

5.3 Robustness

We next verify that the impacts of DACA on mobility investments are robust. As seen in Figure 1 and Appendix Figure A4, pre-period trends are flat, supportive of our identifying assumption. In Appendix Figure A5, we also document the event study effects for being in a licensed occupation. In Appendix Table A3, we show that the effects on mobility, median occupation wages, and being in a licensed occupation are insensitive to sample restrictions. Estimates are similar if we broaden the sample to include 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 U.S. as teens (excluding those who came as young children) makes the treatment and counterfactual

[15] DACA does increase the probability of having health insurance through one's employer, but this is driven by the extensive margin increase in formal employment.

groups more similar, but does not significantly affect the estimate. The only time estimates are sensitive (occupation median wages and being in a licensed occupation) is if we expand the sample to include non-Hispanic immigrants. Based on administrative application data, many of these non-Hispanic immigrants did not participate in the program. The 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 A4).

To further verify that these patterns are not driven by aggregate trends, we estimate two separate placebo specifications looking at geographic mobility and being in a licensed occupation. First, we reestimate Equation (1) but restrict the sample to individuals who arrived at the same ages as our main analysis sample (between 0 and 26) but who were between the ages of 33 and 42 in 2012, and thus all of them were ineligible. Some of these individuals arrived before their sixteenth birthday, but this will not affect eligibility, allowing us to estimate placebo effects. As seen in Figure A8, there is no trend break after the 2012 policy, and postperiod estimates are in fact negative and insignificant. Next, we reestimate Equation (1) but restrict the sample to individuals who meet the age (under 31 before July 2012) and arrival (having arrived 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 A8).

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, such as employment, have been examined before (Pope, 2016), 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.[16] We estimate smaller, but still significant, increases in wage income, conditional on working. Among the employed, DACA increases the wage income of the eligible by $445.

Since DACA-eligible individuals move to better labor markets and higher-paying occupations, part of these labor market improvements could be the result of workers' mobility investments. If this is the case, controlling for location or occupation fixed effects will capture changes in the outcome due to this. In general, we do not want to control for location or occupation when looking at the causal effect on income, as this is a potential outcome. As such, we view columns (5)–(8) of Table 4 as a descriptive decomposition to understand to what extent the wage gains from DACA come from human capital investments. In column (5), we include PUMA fixed effects, and the effect on wage income drops to $425, suggesting location choice explains only 4.5 percent of the wage gains. In column (6), we include occupation fixed effects, and the wage income effect falls to $125, suggesting that 72 percent of the wage effect can be explained by occupation choice. Given the existing work documenting DACA's impact on educational attainment, we include education-bin fixed effects (less than high school; high school; some college; four-year degree; advanced degree) in column (7), and the wage income effect drops to $308, or 31 percent.

As noted above, many DACA-eligible individuals moved into skilled occupations that

[16]Estimating effects on the inverse hyperbolic sine of wage income suggests even larger gains in wage earnings. DACA does not change self-employment income. Transfer income also does not change, but this is perhaps not surprising, since DACA does not give eligibility to means test safety-net programs.

require education or a credential, so occupation switching and education investments are likely to be correlated. As such, if we include location-, occupation-, and education-bin fixed effects, controlling for all of these human capital investments explains 85.4 percent of the wage income effect. A large fraction of the gains in wage income for the employed can be explained by human-capital investment responses to the program.

6 Spillover Effects on the U.S.-Born

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 U.S.-born workers. If immigrant workers provide a substitute for U.S.-born workers, gains in employment and earnings among immigrants could be offset by losses among the U.S.-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 exclude new arrivals and do not create direct incentives for new, potential immigrants. Rather than examine how the arrival of immigrants affects U.S.-born workers' labor market outcomes, we can estimate how the authorization of undocumented immigrants who are already here affects the labor market outcomes of U.S.-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 in a locality, leading to more potential competition in the labor market, but also producing an increase in local demand for goods and services. Depending on their legal status, newly

[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).

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 U.S.-born workers might be minimal. Granting legal status to a preexisting 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 U.S.-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 spill over to benefit U.S.-born workers who are not directly affected by the policy. The net effect is an empirical question.

We explore the spillover effects of DACA on U.S.-born workers in two ways. First, we explore how changes in U.S.-born employment, before and after DACA, relate to the occupation switching of the DACA-eligible from Figure 2. Second, we exploit local labor-market-level variation in exposure to the population that received eligibility through DACA to account for potential cross-occupation spillovers (either positive or negative).

6.1 Estimating Within-Occupation Spillovers on U.S.-Born Workers

As seen in Figure 2, after 2012, DACA-eligible individuals shift into occupations like production worker, child-care worker, teacher, nurse, engineer, and office manager, and out of occupations like cashier, customer service representative, and food services manager. If DACA recipients are displacing U.S.-born workers, we would expect to see offsetting changes among U.S.-born workers. Using the ACS, we can examine self-reported employment for the U.S.-born by occupation before and after 2012. However, it is plausible that the occupations DACA recipients entered were growing because of increased demand or other secular trends. To account for underlying trends in occupation growth, we estimate changes in employment for a given occupation, relative to other occupations in the same general classification. For example, we explore the employment of U.S.-born nurses compared to the employment of other health-care practitioners, technicians, and support staff before and after 2012. We do this by estimating the following equation for each occupation, o

$$Employed_{iot} = \beta_o Focal\ Occupation_o * Post + \beta_1 Focal\ Occupation_o + X_{iot}\Gamma + \phi_{ct} + \varepsilon_{iot} \quad (3)$$

The outcome of interest is an indicator for whether the individual i in occupation o in year t is employed. The main coefficient of interest is β_o, which captures changes in the U.S.-born employment rate in occupation o relative to other U.S.-born workers in similar occupations. We control for worker characteristics, including age, gender, and race, as well as for occupation and commuting-zone-by-year fixed effects, to compare employment outcomes for individuals that are observably similar.

For each occupation, we restrict the sample to individuals in occupations in the same general occupation classification as provided by IPUMS. There are 26 different occupation groups. Some of the groups are small and include only two or three 2-digit occupations. In these cases, we combine neighboring classification groups that are similar (e.g., health-care practitioners and health-care support). This results in 15 classification groups. The identifying assumption in this specification is that employment for individuals in the focal occupation would have evolved similarly to employment for individuals in the comparison occupations after 2012 if DACA had not been implemented. We restrict the sample to similar occupations to identify settings where this is more likely to hold.

This specification is only suited to explore within-occupation spillovers. If DACA recipients displace U.S.-born workers, the U.S.-born worker might move to a different job or occupation. This would not be captured by β_o, since treatment is based on reported occupation and individuals only report their current or latest occupation if unemployed. We also saw that DACA recipients shifted into occupations like child-care worker, which could facilitate more U.S.-born workers—mothers in particular—to enter the labor market. We need a different approach to consider more general spillovers.

6.2 Estimating Local Labor Market Spillovers on U.S.-Born Workers

As seen in Figure A9, the share of the population aged 18 to 64 that met DACA-eligibility requirements 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 across commuting zones, even within close proximity. This variation can identify labor markets that are more exposed to the "shock" of legalization, and we can estimate the local labor market spillover effects of DACA legalization by comparing markets with higher and lower DACA shares before and after DACA is implemented. However, there is an endogeneity concern. If, for example, immigrant families with DACA age-eligible children move to labor markets that are experiencing economic growth, our estimates would be biased by reverse causality.

We overcome this endogeneity concern by isolating variation in the 2007 DACA-eligible share of the population that is driven by historic immigrant settlement patterns and aggregate, country-specific flows of eventually DACA-eligible children prior to 2007. This immigrant enclave "shift share" will allow us to capture a component of the legalization shock that is not correlated with economic conditions or local labor market trends that affect the settlement of immigrants and are correlated with our outcomes of interest.

Across the country, there are varying concentrations of immigrants from different source countries. Using the 2000 census 5 percent sample, we can map out the location of immigrants who arrived prior to 1981, the first year that the oldest DACA-eligible individuals were born.[18] Although some places, like Southern California, draw immigrants from lots of countries, other local labor markets experience source-specific concentrations. For example, if we focus on the four countries that produced the most DACA applicants, we see that

[18] Even though we are interested in the pre-1981 immigrant population, we construct immigrant networks based on the 2000 census, because PUMA is only available starting in 2000. We are able to map individuals to 2000 commuting zones using the same crosswalk that we use for the ACS data and to construct the DACA-eligible flows, which we cannot do with the 1980 five percent census sample, since only "country group" is reported, and these groups do not align with commuting zones. If we use the 1980 sample to construct the pre-1981 network, we end up with fewer matches to DACA-eligible immigrant flows, because the base geographies differ.

the pre-1981 concentration of immigrants from Mexico is high in California, the Southwest, Chicago, and Texas; immigrants from El Salvador are much more concentrated in the Washington, D.C., area, San Francisco, Los Angeles, and Houston; immigrants from Guatemala are concentrated in San Francisco, Chicago, Miami, and the Mid-Atlantic; while immigrants from Honduras are more concentrated in New Orleans, Miami, and along the East Coast (see Figure 3, with the full source country-by-commuting-zone matrix in Appendix Figure A10).

From the 2000 census 5 percent sample, we calculate the aggregate flow of individuals that were born in 1981 or later, arrived between 1981 and 2000, and were under the age of 16 at arrival from each source country. We do this to capture the flow of *DACA-eligible individuals*. We combine the preexisting migration networks with the aggregate source-country flows to create a shift-share prediction of the DACA-eligible population prior to DACA, as follows:

$$\textit{Predicted DACA Share}_c = \frac{1}{\textit{Population}_c} * \sum_{o=1}^{o} \textit{Pre-1981 share}_{oc} * \textit{DACA-eligible Flow}_{o,1981-2000} \tag{4}$$

DACA-eligible Flow is the total number of immigrants who meet the DACA age and arrival criteria, and who came to the U.S. from origin country o between 1981 and 2000. *Pre-1981 share* is the share of the pre-1981 immigrant population from country o that was living in commuting zone c. *Population* is the commuting-zone population, captured in 2000.[19] This measure is larger in U.S. commuting zones that have a large share of pre-1981 immigrants from source countries that experienced large inflows of DACA-eligible individuals between 1981 and 2000.[20]

This measure is highly predictive of the 2007 commuting-zone population share that is

[19]We use the 2000 population to be consistent with the immigrant flows, which we only observe through 2000 as well.

[20]In theory, we could use information from the 2005–2007 ACS to capture people who immigrated to the U.S. from 2000 to 2007. However, since the ACS is only a 1 percent sample, it undercounts flows from smaller immigration groups relative to bigger groups. The relationship between *Predicted DACA Share* and the 2007 *DACA Share* is similar, but less precise when we include these flows.

DACA-eligible, as seen in Figure 4. In the cross section, a 1 percentage point increase in the predicted DACA share is associated with a 0.42 percentage point increase in the 2007 DACA-eligible share, with an F-statistic of 159. Using this measure, we will estimate the effect of DACA legalization on U.S.-born workers' labor market outcomes as follows:

$$Y_{ict} = \sum_{\tau=2007}^{2019} \gamma_\tau \textit{Predicted DACA Share}_{c,2007} * (Year = \tau) + X_{ict}\Gamma + \phi_c + \theta_t + \alpha_a + \varepsilon_{it} \quad (5)$$

We are interested in two main outcomes, 1) employment and 2) income, for individual i (who is U.S.-born) in commuting zone c in year t. The event study interacts the fixed, predicted DACA share for commuting zone c with year indicators, leaving 2011 as the omitted year. Commuting-zone fixed effect (ϕ_c) will account for geographic-level differences in labor markets and any time-invariant unobservables that correlate with the predicted DACA share and our outcomes. Year fixed effects (θ_t) will account for secular trends in outcomes, and age fixed effects (α_a) will absorb differences in labor market outcomes throughout the life cycle. We correct the standard errors for clustering at the commuting-zone level.

With this specification, our identifying assumption is that U.S.-born workers in commuting zones with higher predicted DACA shares would have experienced a similar trend in outcomes as U.S.-born workers in commuting zones with lower predicted DACA shares. As with all shift-share strategies, a potential threat to identification is that the predicted 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, in this particular case, a plausible concern is that the predicted DACA share might be correlated with the share of the population that is Hispanic or with larger populations (as immigrants tend to be located in more populous areas). The circumstances that pushed DACA-eligible minors from their home countries to the United States could also push other immigrants from those countries. As the majority of DACA-eligible individuals come from Mexico and Latin

America, the predicted DACA share might simply proxy for the Hispanic share, which could affect labor market outcomes of the U.S.-born. As seen in Appendix Table A7, the predicted DACA share is correlated with measures like population and race/ethnicity shares. For this reason, we verify that our point estimates from Equation (5) are robust to controlling for commuting-zone-level measures, including total population, share male, average age, race/ethnicity shares (including share Hispanic), share foreign-born, marital-status shares, and education shares, which might correlate with the 2007 DACA share and affect the outcomes of interest.[21]

The event study structure of Equation (5) will also help probe the validity of this concern. If there are spillover effects on the U.S.-born, we would expect them to show up in more intensely treated labor markets, *but only after* DACA is implemented in 2012. If the estimated effects are driven by trends in other characteristics, like the Hispanic share, we would observe "treatment effects" before treatment occurs.[22]

In addition to estimating the spillover effects for all U.S.-born 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 U.S.-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 U.S.-born workers, but might also potentially complement the productivity of more

[21]The predicted DACA share is also correlated with changes in labor market outcomes, but we do not control for these, as this is our outcome of interest.

[22]Because there are possible violations of the exclusion restriction (as in the example listed here), we focus on the reduced-form relationship. For completeness, we also provide estimates of the OLS relationship between the observed DACA share and employment/earnings and the two-stage least squares relationship in the appendix.

educated workers. We will explore employment and wage income for these groups separately.

6.3 Spillover Results

6.3.1 Within-Occupation Spillover Results

We present the occupation-specific spillover results from Equation (3) in Figure 5. The estimated β from Figure 2 (capturing the degree of occupational shifting among DACA recipients) is plotted along the x-axis, and the β_o coefficient from Equation (3) is plotted along the y-axis. Each point represents a two-digit occupation. Only occupations that experienced a significant change in employment among the DACA-eligible are plotted, while confidence intervals are associated with the employment change among U.S.-born workers.[23]

There is no strong pattern or correlation in the data. In many of the occupations, individuals experienced employment declines after 2012 relative to people in similar occupations, but there are also occupations that experienced employment gains. Occupations that the DACA-eligible left also experienced declines in employment. Only nine of the occupation-specific effects are statistically different from zero at the 5 percent level. Of the 11 occupations that experienced a significant increase in DACA-eligible employment, only 4 experienced a significant decline in U.S.-born employment, while two experienced a significant increase. The within-occupation evidence suggests that in some cases legalization might have resulted in DACA-eligible works being substituted, but in other cases they were complements.

6.3.2 Geographic Spillover Results

Local labor-market-event study results are provided in Figure 6. The predicted 2007 DACA share has virtually no effect on U.S.-born worker employment or wage income during the pre-period between 2007 and 2011. After 2012, we see no significant changes in employment, but there is a marginally significant *increase* in wage income. By 2019, a 1 percentage point increase in the predicted 2007 DACA share is associated with a $1,277 increase in average

[23] Plots with all of the occupations can be found in Appendix Figure A11.

wage income (2020 dollars). When we control for commuting-zone characteristics that might be correlated with the 2007 predicted DACA share, the pretrends are still flat prior to 2012, and the estimated employment effects are similar. The post-DACA increase in wage income is unchanged by the controls but becomes more precise. The data suggest that, on average, DACA legalization did not lead to displacement in the local labor market and, if anything, led to increases in wage income.[24] 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. It is unlikely that these effects are driven by other local characteristics that are trending over time, unless those characteristics also experience a trend break after DACA was implemented. Furthermore, controlling for local characteristics does not explain the effects. If anything, the pattern of effects becomes stronger.[25]

We next explore effects for different subgroups based on age and education. As seen in the top panel of Figure 7, the exposure to more people legalized through DACA has very little or no effect on employment across the different age groups. There are distinct differences in wage income by age. Younger workers (18–34 years old) in more intensely treated labor markets see little-to-no increase in wage income after 2012 relative to young workers in less intensely treated labor markets. However, prime-age (35–54) and older (55–64) workers in labor markets with higher predicted DACA shares observe large, significant gains in wage income after 2012. A 1 percentage point increase in the predicted DACA share is associated with an additional $2,200 of wage income for older workers.

The pattern of effects by education is plotted in the bottom panel of Figure 7. Pretrends are once again flat, but the predicted DACA share is associated with small, marginally significant gains in employment among the U.S.-born population with a high school degree

[24]In Appendix Figure A12, we document spillover effects on log wage income. By taking the log, we are only estimating effects among workers with positive earnings. We see little evidence that the predicted DACA share affects log wage income for any group, although there are slight gains for workers with a high school degree or less. This would suggest that most of the increases in wage income are due to small, extensive margin changes.

[25]For completeness, we include the corresponding OLS and two-stage-least-squares estimates in Appendix Figures A13 and A14.

or less. The predicted DACA share does *not* affect wage income for U.S.-born workers with a high school degree or less or with some college, but, as with older adults, it does increase wage income among those with a college degree. A 1 percentage point increase in the predicted DACA share results in wage income that is \$2,700 higher in 2019.[26] These patterns do not provide compelling evidence that DACA recipients displace U.S.-born workers. Rather, the patterns are consistent with DACA recipients complementing prime-age and older workers and college-educated workers in the production process, leading to higher income for these workers.[27]

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 U.S.-born workers. 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. In such a case, 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 occupations with higher median wages and more licensing restrictions. Providing legal status allows them to undertake these costly and risky mobility investments. As a result, DACA-eligible immigrants experience better labor market outcomes. DACA increases employment among the eligible but also increases earnings at the intensive margin. We find that approximately 85 percent of the gain in earnings at the intensive margin can be attributed to human capital investments and mobility investments such as occupational

[26]We examine potential differences by gender in Appendix Figure A15 but do not see significant differences, except for one year when looking at log wages.

[27]We also do not observe negative effects among the foreign-born population that arrived before 2007 but were not DACA eligible. However, they do not exhibit the same gains in earnings as U.S.-born workers did (Figure A16).

switching, seeking more education, and moving.

From the immigrants' perspective, legalization brings large economic benefits. These benefits do not appear to be offset by added costs borne by U.S.-born workers. When we look within occupations, there are patterns consistent with DACA-eligible workers either substituting or complementing U.S.-born workers, depending on the occupation. Exploiting plausibly exogenous local-level variation in the share of the population that is affected by DACA, we show that employment levels of U.S. workers respond very little when the labor market is exposed to more people that receive legal status through DACA. The wage income of U.S.-born workers rises 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 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 higher education levels). These patterns of results do not indicate that native workers bear the cost of unauthorized immigrant legalization, but rather suggest that there could be large positive, local externalities to legalization policy.

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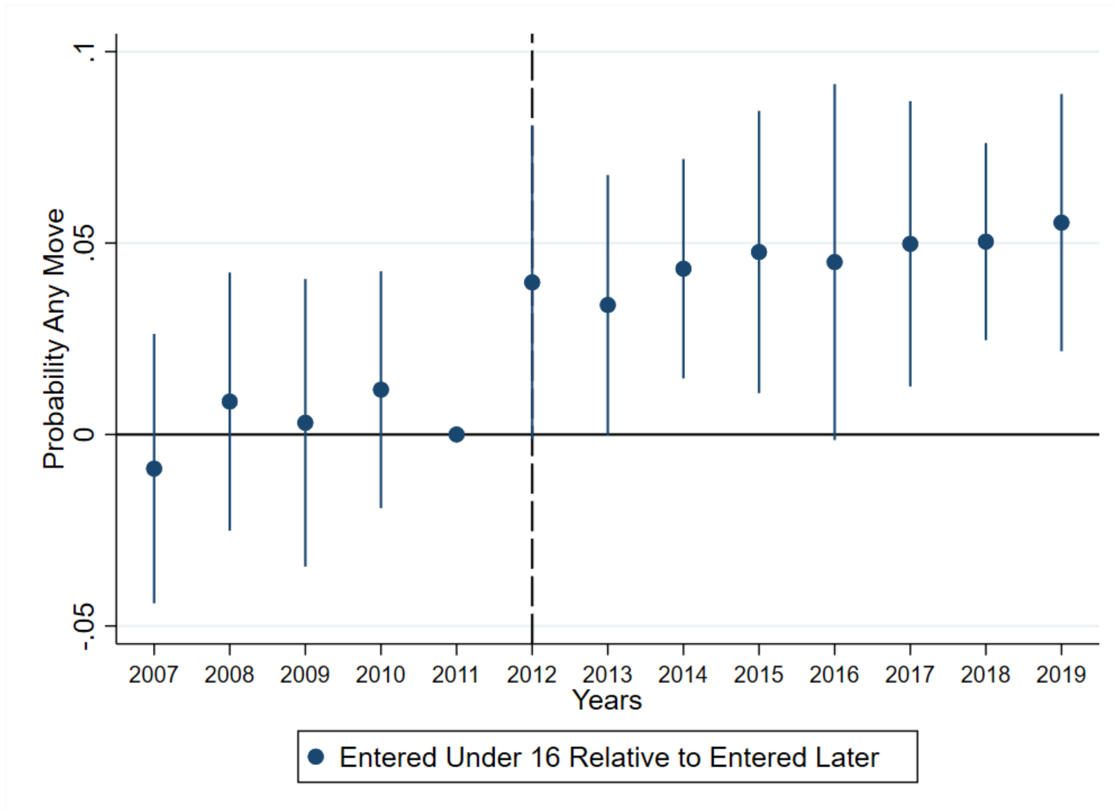
8 Tables and Figures

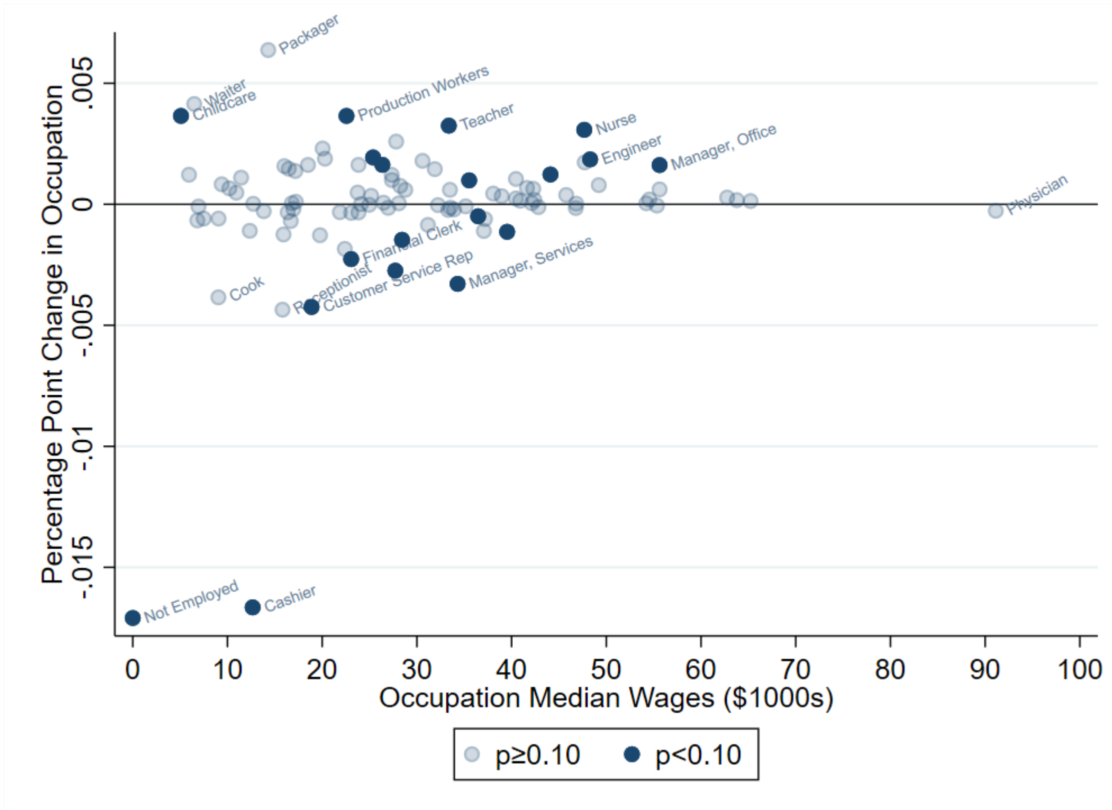
Figure 1: Probability of Moving among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 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: Authors' own calculations using 2007–2019 American Community Survey (ACS) microdata.

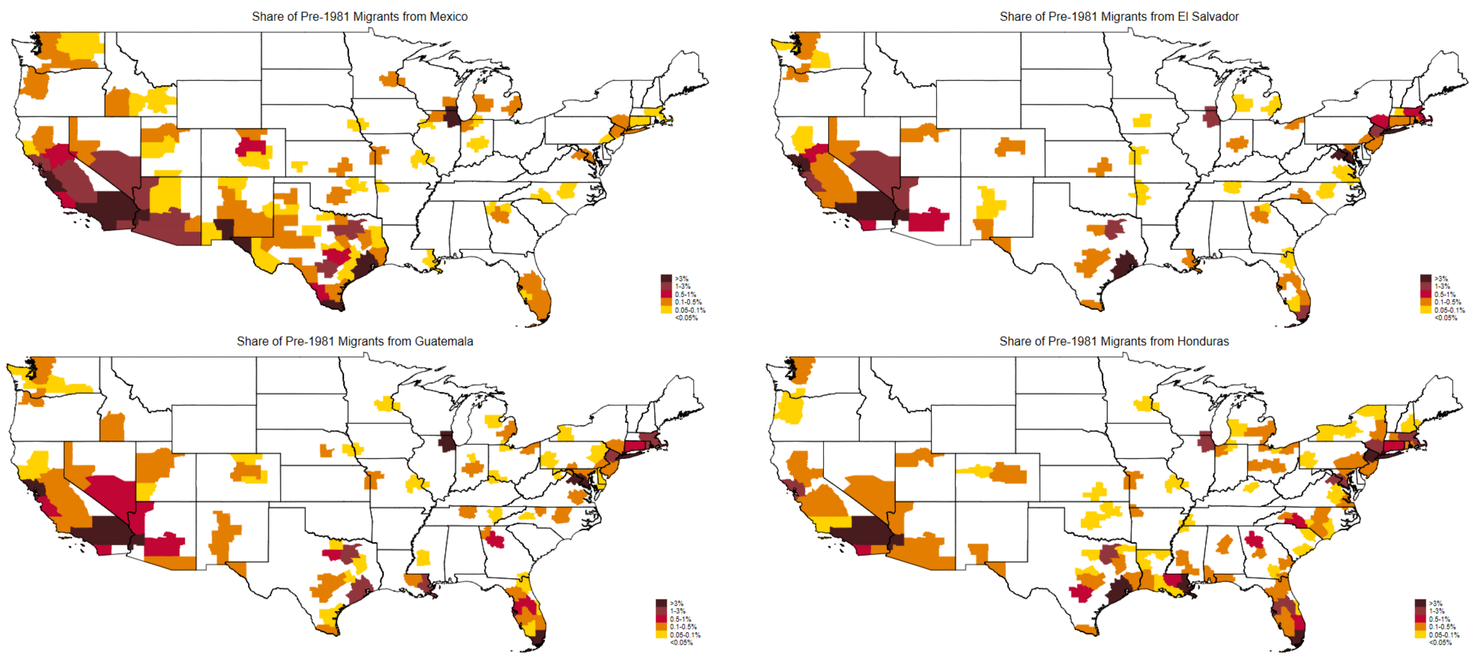
Figure 2: Occupational Mobility among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. These individuals must also meet the DACA education requirements. The birth-cohort restriction ensures that individuals were under the age of 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: Authors' own calculations using 2007–2019 ACS microdata.

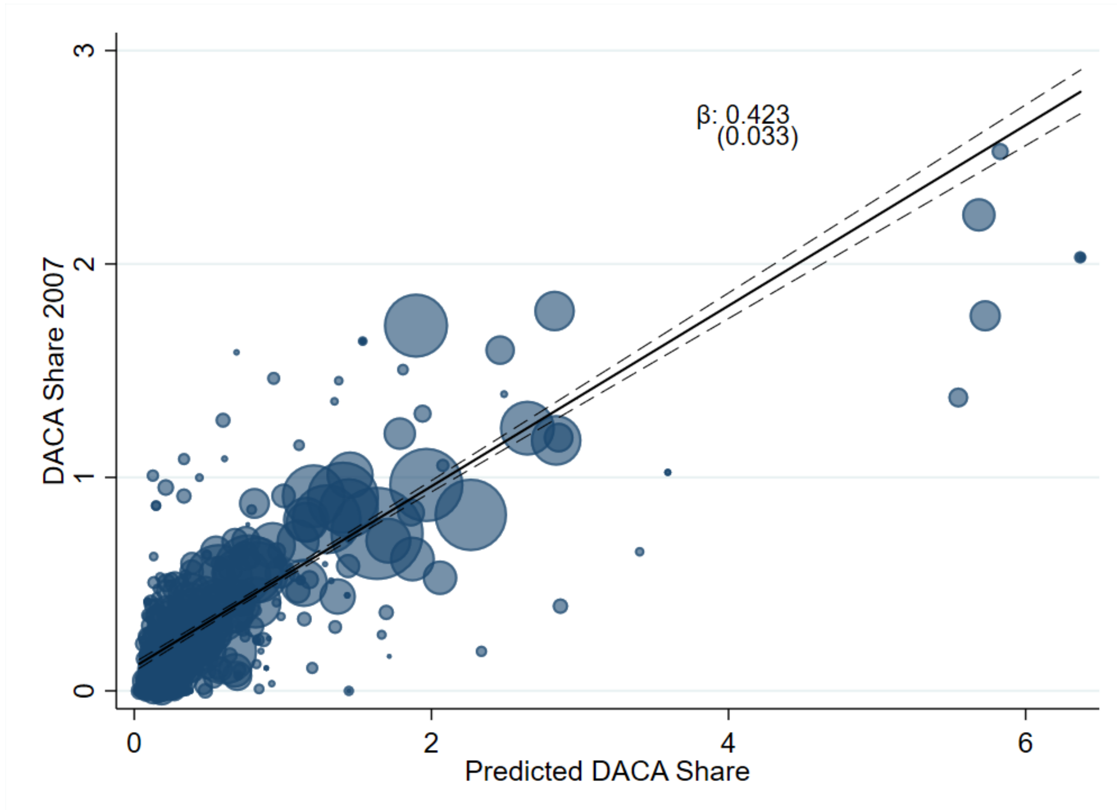
Figure 3: Source Country to U.S. Commuting Zone Migration Networks, Pre-1981 Arrivals from Top Four DACA-Recipient Countries



NOTE: The share of pre-1981 immigrants from each of the top four DACA-recipient source countries are plotted by commuting zone. We plot the pre-1981 share to capture settlement patterns of pre-DACA immigrants. We use the measure from 2000 as it allows us to crosswalk back to the same commuting zones as the ACS.

SOURCE: Authors' own calculations using data from the 2000 census on pre-1981 immigrants.

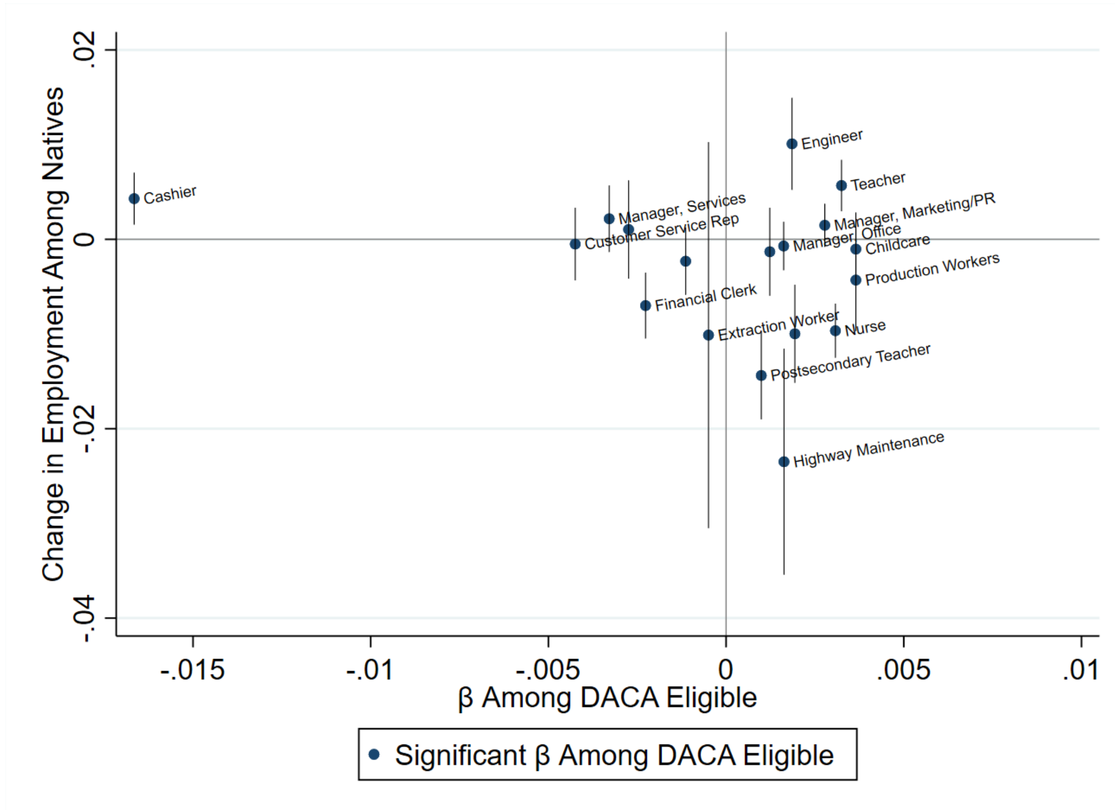
Figure 4: Relationship between 2007 DACA-Eligible Share and Predicted 2007 DACA-Eligible Share Based on Source Country Enclaves and Age-Specific Aggregate Flows



NOTE: The x-axis plots the commuting-zone-predicted 2007 DACA-eligible share based on source-country enclaves (from pre-1981 immigrants) and 1981–2000 aggregate flows of immigrants that would meet the eventual DACA age-and-arrival-eligibility criteria. The y-axis is the actual 2007 commuting-zone DACA-eligible share, as measured in the 2007 ACS. The reported coefficient and standard error are from the bivariate regression, weighted by commuting-zone 2007 population. Robust standard errors are included.

SOURCE: Authors' own calculations using 2007 ACS microdata and 2000 census data.

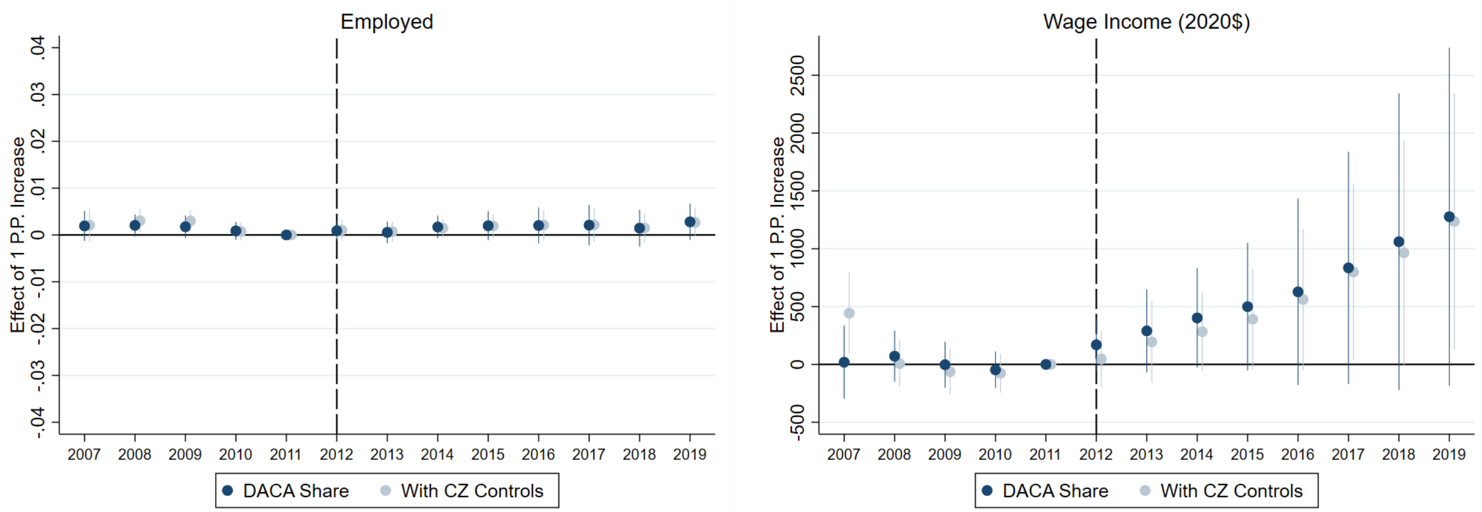
Figure 5: Within-Occupation Spillover of DACA on Employment of U.S.-Born



NOTE: For each point, sample restricted to U.S.-born respondents to the 2007–2019 ACS, ages 18 to 65, in the same general classification group as the focal occupation. The point represents the interaction point estimate from the shifting of DACA-eligible individuals into the occupation (on the x-axis) and the employment of U.S.-born workers in the occupation (relative to other individuals in similar occupations) on the y-axis. Only occupations where the shifting of DACA-eligible individuals is statistically significant are plotted. We do not estimate effects for the military, as DACA recipients are not eligible to serve in the armed forces and there is not a similar counterfactual occupation. Ninety-five percent confidence intervals, with standard errors corrected for clustering at the commuting-zone level, are included along the y-axis dimension. Fixed effects for commuting zone by year, age, gender, race, and occupation are included.

SOURCE: Authors' own calculations using 2007–2019 ACS microdata.

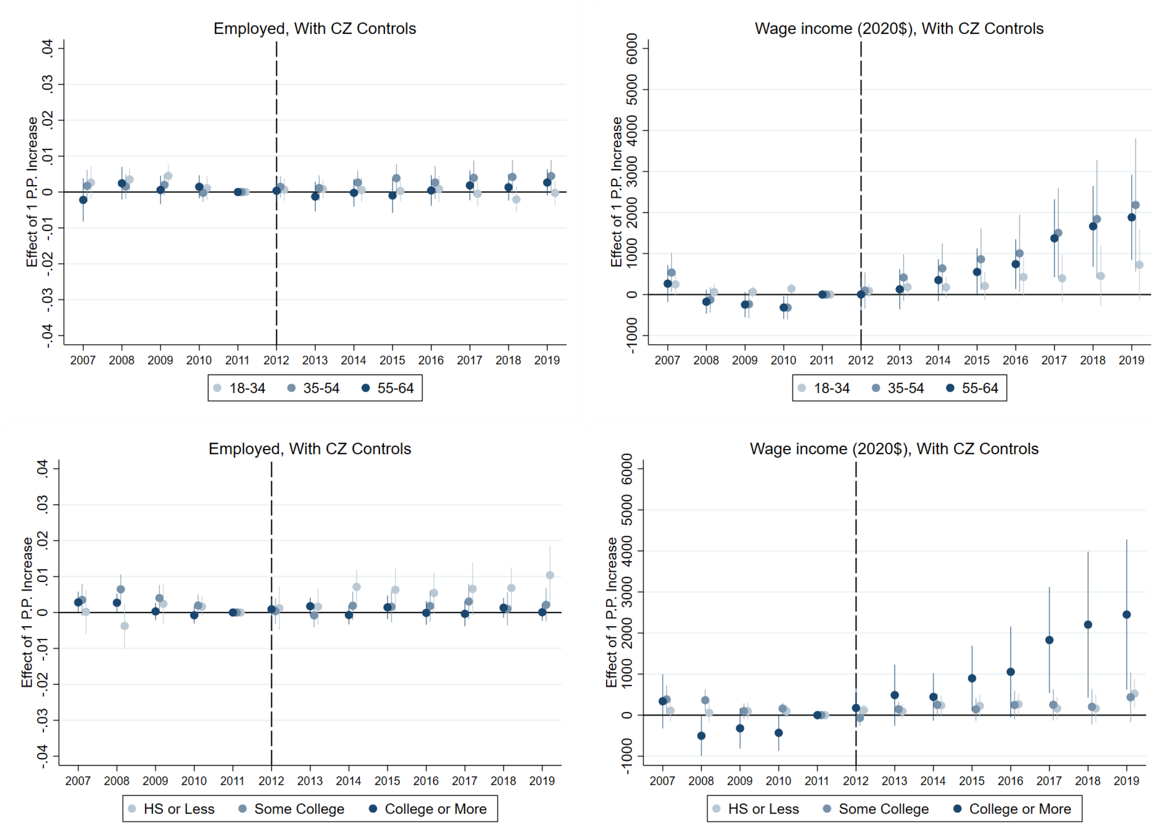
Figure 6: Spillover Impact of DACA on Labor Market Outcomes of U.S.-Born Respondents in the Commuting Zone



NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 65. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. Commuting-zone controls include total population, share male, average age, race/ethnicity shares (including share Hispanic), share foreign-born, marital-status shares, and education shares. 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: Authors' own calculations using 2007–2019 ACS microdata.

Figure 7: Heterogeneity: Spillover Impact of DACA on Labor Market Outcomes of U.S.-Born in the Commuting Zone, by Age and Education



NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 65. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. Commuting-zone controls include total population, share male, average age, race/ethnicity shares (including share Hispanic), share foreign-born, marital-status shares, and education shares. 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: Authors' 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
High School	0.03	0.03	0.06	0.07
Some College	0.24	0.35	0.23	0.33
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, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were 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 sixteenth birthday, which determines eligibility. The group that entered at under age 16 is eligible for DACA.

SOURCE: Authors' calculations.

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	Move out of PUMA	Move out of State	Above Median	Below Median	Above Median	Below Median
	(1)	(2)	(3)	(4)	(5)	(6)	(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, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

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***	0.364	-0.548	-0.430	-0.007*
	(189.529)	(222.509)	(0.006)	(0.356)	(0.489)	(0.351)	(0.004)
Entered Under 16	2973.472***	3294.352***	0.085***	-1.579***	9.148***	7.680***	-0.005***
	(188.062)	(216.314)	(0.011)	(0.403)	(0.564)	(0.336)	(0.002)
Dependent Mean	14792.92	18084.31	0.28	48.96	35.44	37.90	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, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

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

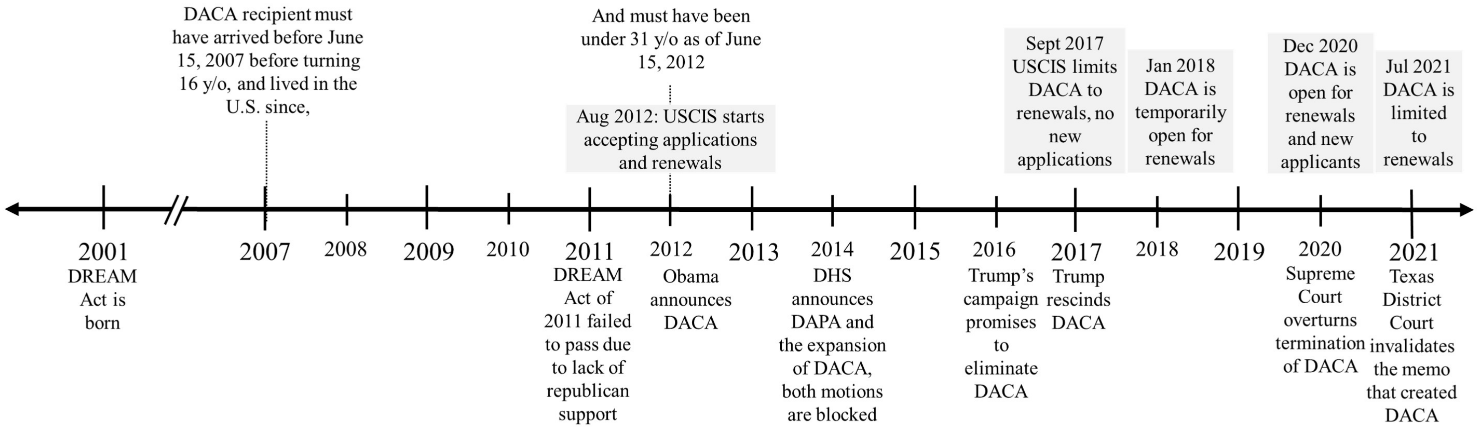
				Wage Income (2020) Among Working				
	Employed	Usual Hours Worked	Wage Income (2020)	Baseline	MIGPUMA F.E.	Occupation F.E.	Education F.E.	All Investment F.E.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Entered Under 16*Post-DACA	0.035***	1.366***	1347.925***	445.201**	424.952*	125.044	308.240	65.013
	(0.008)	(0.294)	(235.434)	(211.184)	(239.821)	(197.024)	(195.090)	(220.436)
Entered Under 16	0.001	0.285	1637.392***	1608.915***	1735.716***	614.427***	1529.903***	907.156***
	(0.006)	(0.272)	(172.838)	(167.549)	(171.420)	(161.319)	(190.565)	(210.310)
Percent Explained					4.5	71.9	30.8	85.4
Dependent Mean	0.70	29.69	15798.27	21569.22	21569.22	21569.22	21569.22	21569.22
Observations	94,179	94,179	94,179	68,937	68,937	68,937	68,937	68,937

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth cohort restriction ensures that individuals were under the age of 31 as of 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

Appendix A. Supplementary Analyses (for online publication)

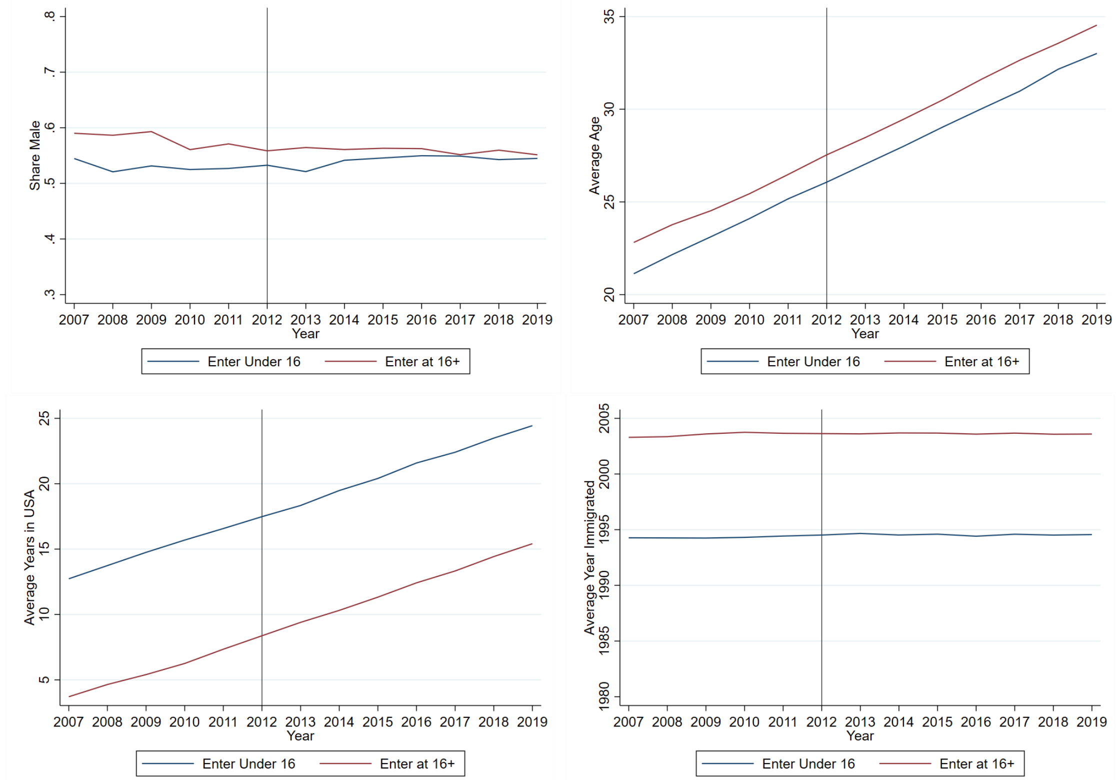
Figure A1: Timeline of DACA Legislation



NOTE: DACA was enacted on June 15, 2012, through an executive order. Applications were first accepted on August 15, 2012. Individuals had to have continuously resided in the U.S. since June 15, 2007, be under the age of 31 by June 15, 2012, and have arrived in the U.S. when under the age of 16.

SOURCE: Authors' own construction based on DACA-related legislation.

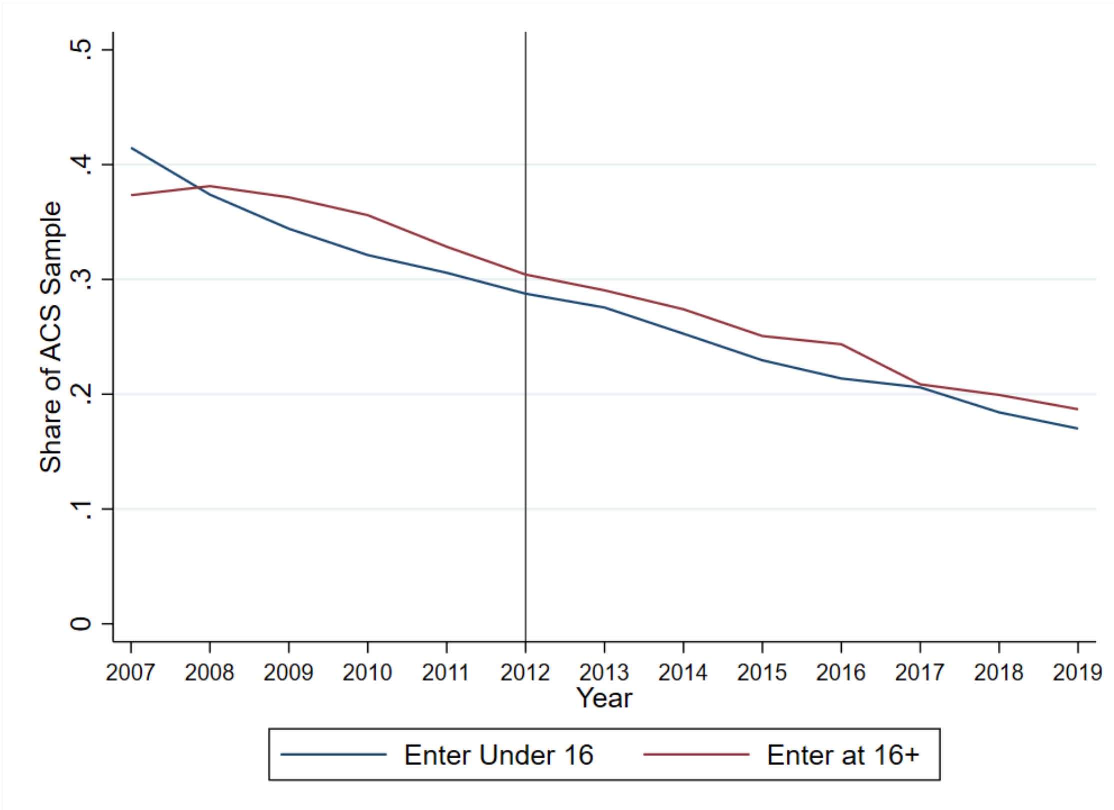
Figure A2: Exploring Differential Attrition: Trends in Average Characteristics of Treatment and Counterfactual Groups over Time



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989, unless otherwise specified. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were at least 18 in 2007. Average characteristics are then calculated for individuals that arrived before their sixteenth birthday (treated) and after that (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: Authors' own calculations using 2007–2019 ACS microdata.

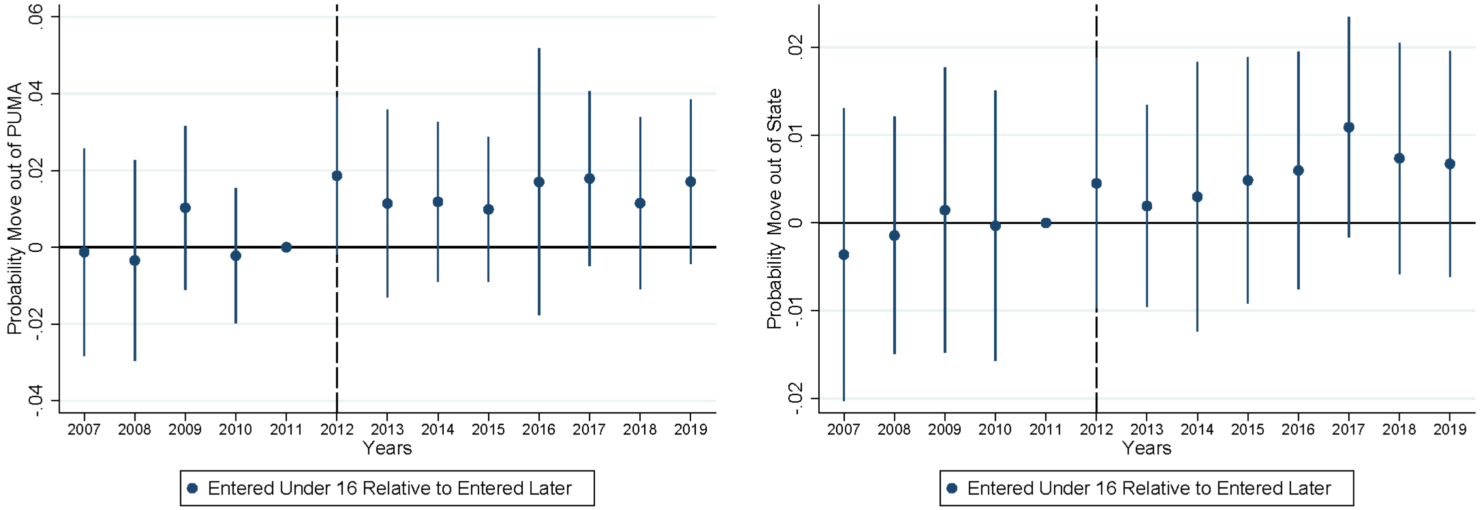
Figure A3: Exploring Differential Attrition: Share of ACS Sample in Treatment and Counterfactual Groups over Time



NOTE: We construct the share of the ACS sample that was born in the latter half of 1981 or in 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: Authors' own calculations using 2007–2019 ACS microdata.

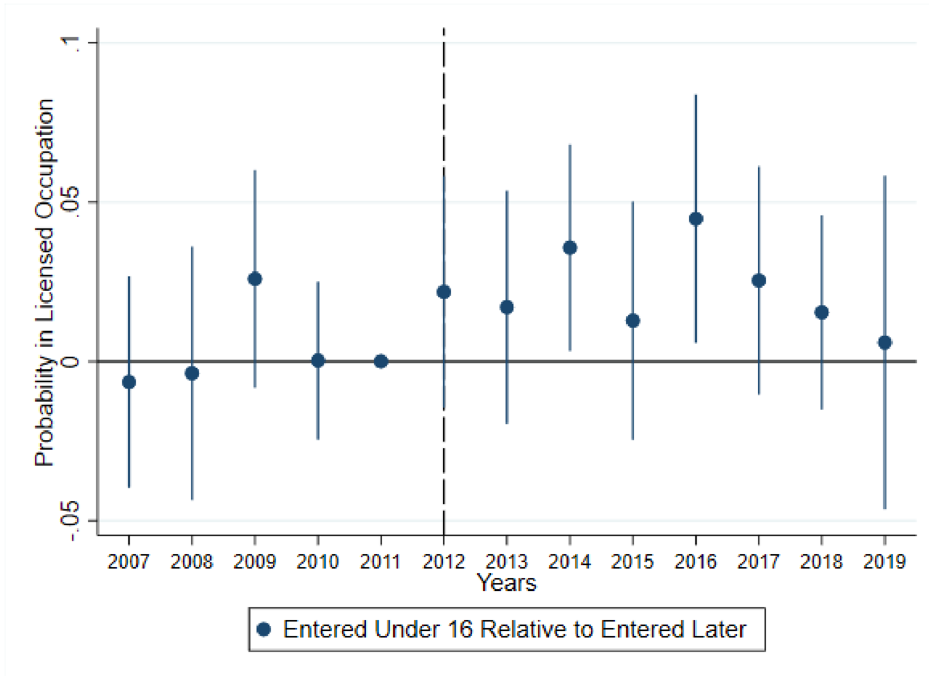
Figure A4: Probability of Moving among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. These individuals must also meet the DACA education requirements. The birth-cohort restriction ensures that individuals were under the age of 31 as of June 30, 2012, as required for DACA eligibility, and were 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: Authors' own calculations using 2007–2019 ACS microdata.

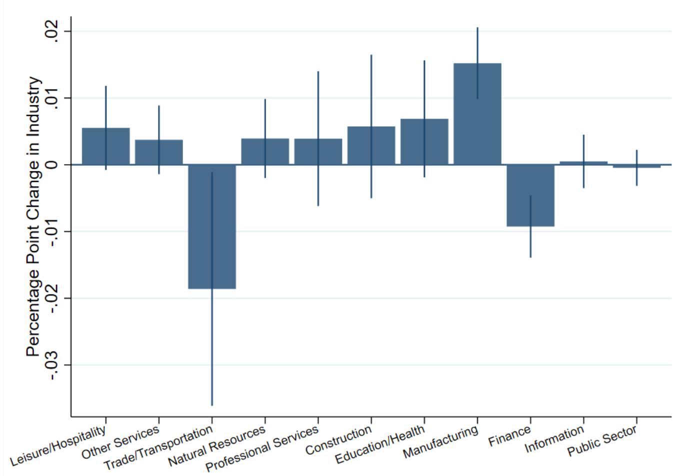
Figure A5: Probability of Being in a Licensed Occupation among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. These individuals must also meet the DACA education requirements. The birth-cohort restriction ensures that individuals were under the age of 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: Authors' own calculations using 2007-2019 ACS microdata.

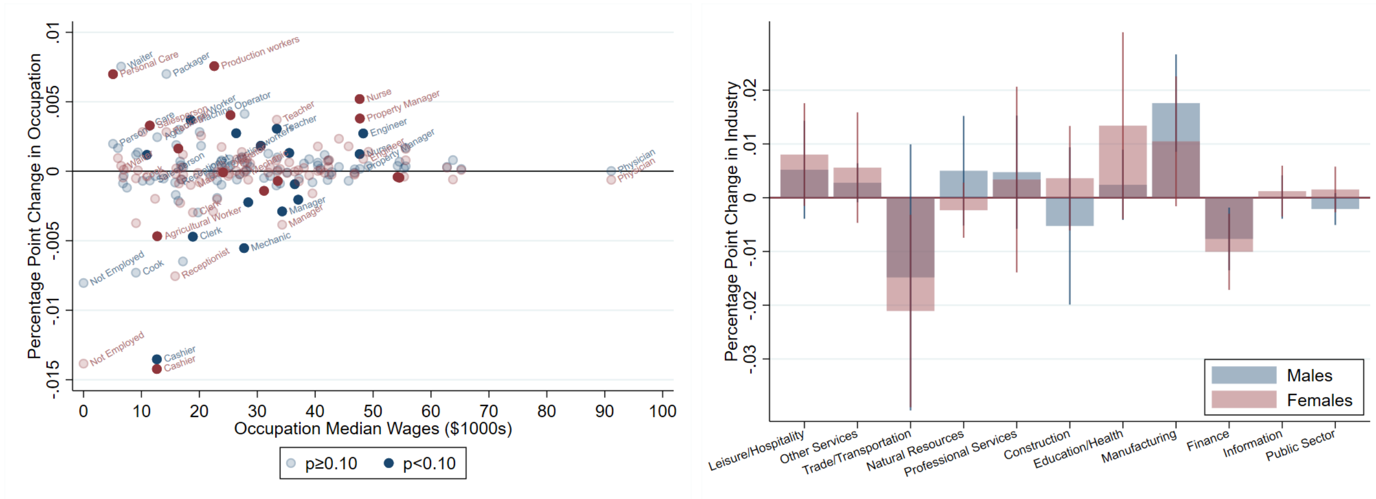
Figure A6: Industry Mobility among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. These individuals must also meet the DACA education requirements. The birth cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were 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: Authors' own calculations using 2007–2019 ACS microdata.

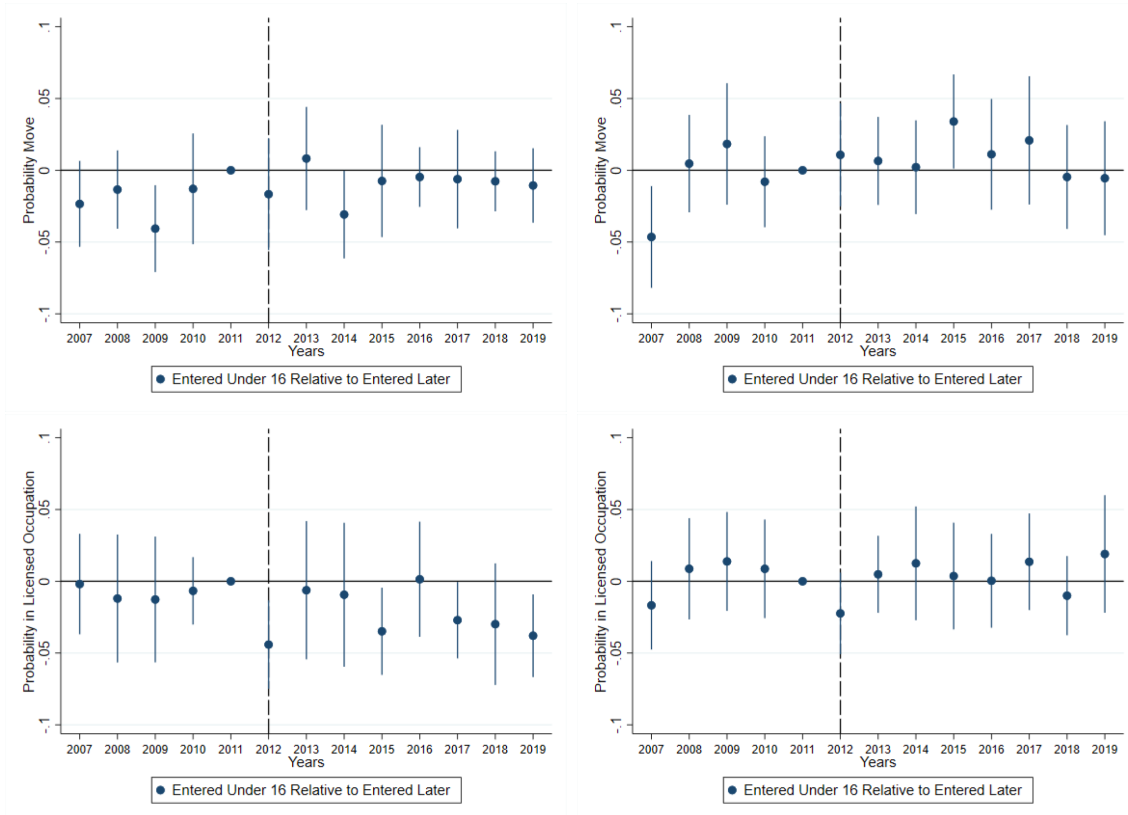
Figure A7: Occupation and Industry Mobility among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants, by Gender



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. These individuals must also meet the DACA education requirements. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were 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: Authors' own calculations using 2007–2019 ACS microdata.

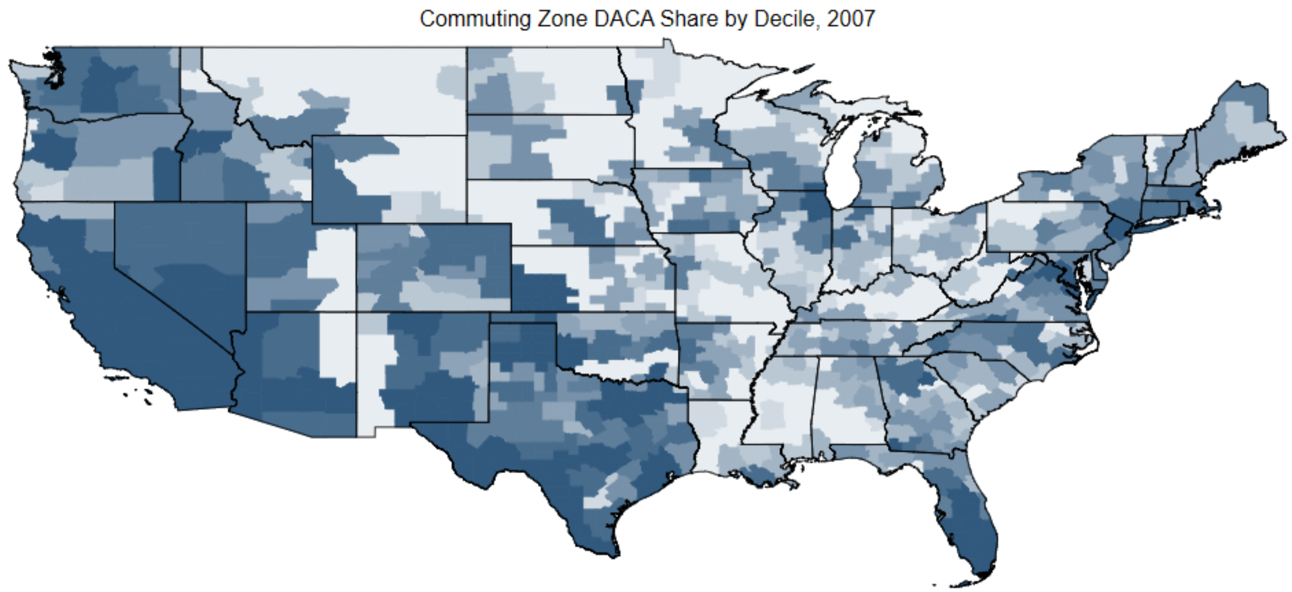
Figure A8: Placebo Impact of DACA on Geographic Mobility and Occupational Credentialing among Ineligible Immigrants



NOTE: In the left panel, sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS who arrived in the U.S. between the ages of 0 and 26 (consistent with the main analysis sample) and who were aged 33–42 in 2012 and thus ineligible. We restrict birth cohorts to keep a similar age distribution in the treatment and counterfactual groups, as in the main analysis sample. In the right panel, the sample is restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989, and who 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: Authors' own calculations using 2007–2019 ACS microdata.

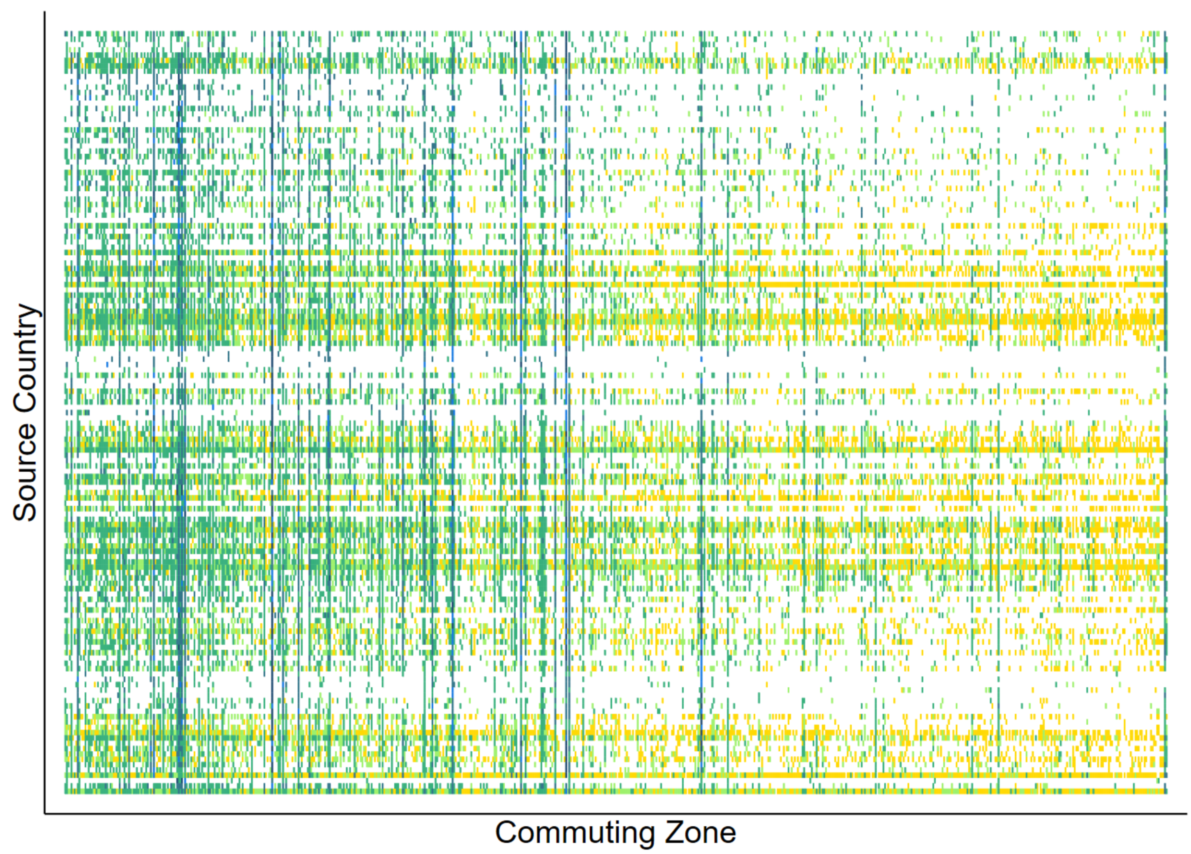
Figure A9: Share of Population 18–64 That Is DACA-Eligible



NOTE: Figure plots the share of individuals 18–64 in the 2007 ACS that meet the DACA eligibility criteria, including having arrived in the U.S. by 2007, having been under the age of 31 by July 2012, having arrived in the U.S. before their sixteenth birthday, having met the DACA education criteria, and not being citizens. Estimates are shaded according to commuting-zone-level decile, with darker shades indicating greater DACA eligibility.

SOURCE: Authors' own calculations using 2007 ACS microdata.

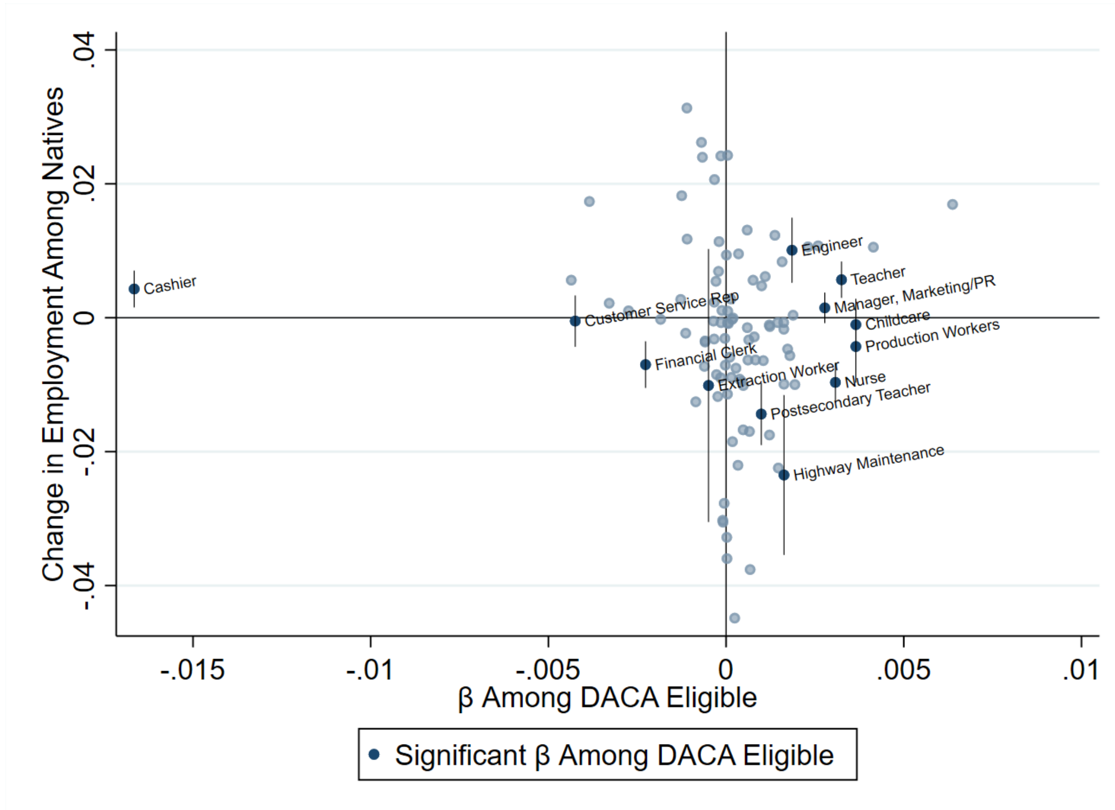
Figure A10: Source Country to U.S. Commuting Zone Migrant Share Matrix



NOTE: The measure $share_{mc}$, which is the average share from Equation (4) prior to 1981, is plotted for each Source Country and U.S. Commuting Zone pair. Lighter colors represent a smaller share of immigrants from the given source country.

SOURCE: Authors' own calculations using data from the 2000 census on pre-1981 immigrants.

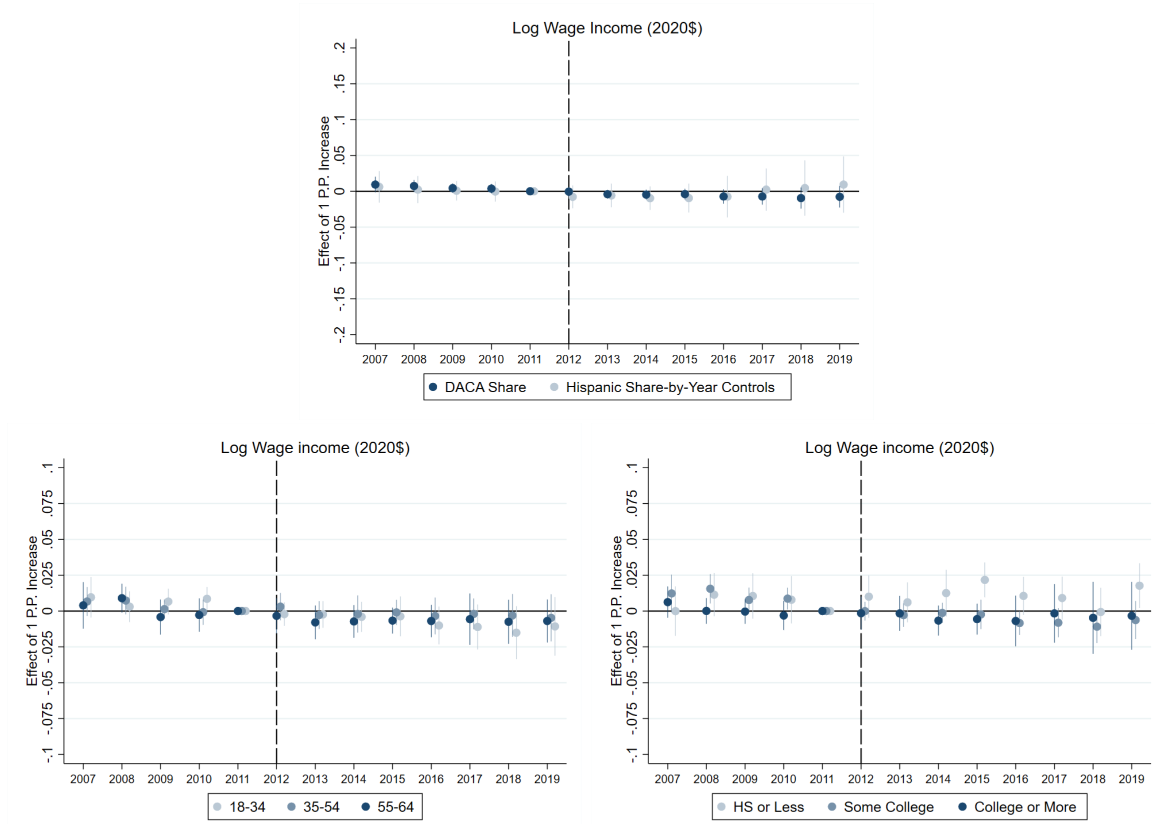
Figure A11: Within-Occupation Spillover of DACA on Employment of U.S.-Born Respondents, All Occupations



NOTE: For each point, sample is restricted to U.S.-born respondents to the 2007–2019 ACS, ages 18 to 65, in the same general classification group as the focal occupation. The point represents the interaction point estimate from the shifting of DACA-eligible individuals into the occupation (on the x-axis) and the employment of U.S.-born workers in the occupation (relative to other individuals in similar occupations) on the y-axis. All two-digit occupations are plotted, except for military occupations. We do not estimate effects for the military, as DACA recipients are not eligible to serve in the armed forces and there is not a similar counterfactual occupation. The 95 percent confidence intervals, with standard errors corrected for clustering at the commuting-zone level, are included along the y-axis dimension. Fixed effects for commuting zone by year, age, gender, race, and occupation are included.

SOURCE: Authors' own calculations using 2007–2019 ACS microdata.

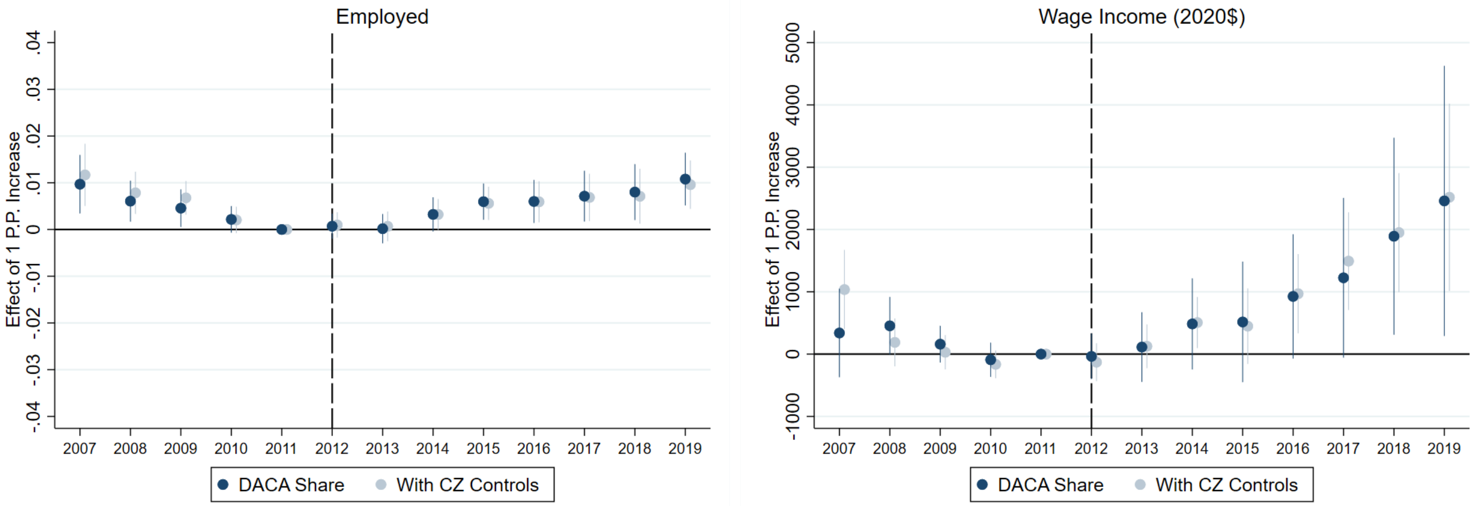
Figure A12: Spillover Impact of DACA on Log Wage Income of U.S.-Born in the Commuting Zone by Age and Education



NOTE: Sample restricted to U.S.-born respondents to the 2007–2019 ACS, ages 18 to 65. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. 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: Authors' own calculations using 2007–2019 ACS microdata.

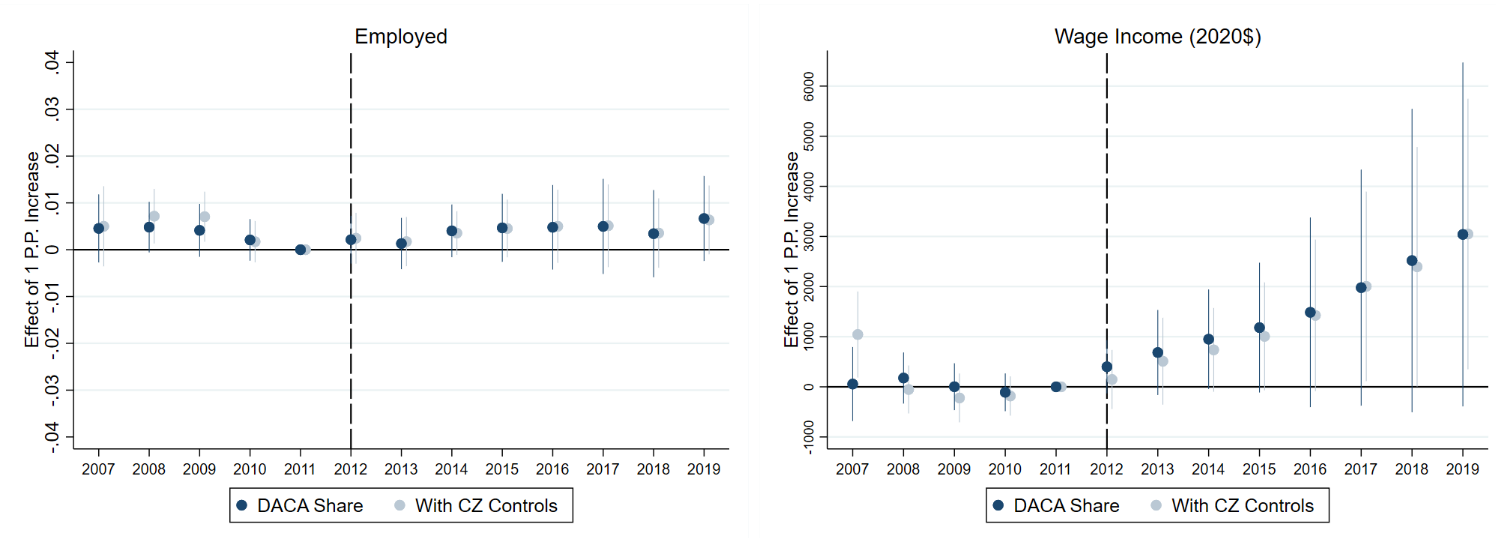
Figure A13: OLS: Spillover Impact of DACA on Labor Market Outcomes of U.S.-Born in the Commuting Zone



NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 65. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. Commuting-zone controls include total population, share male, average age, race/ethnicity shares (including share Hispanic), share foreign-born, marital- status shares, and education shares. 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: Authors' own calculations using 2007–2019 ACS microdata.

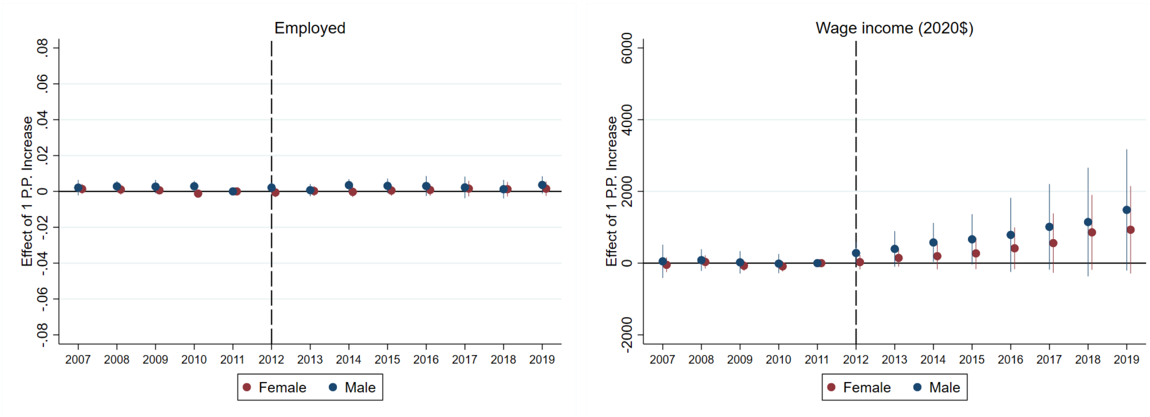
Figure A14: Two-Stage Least Squares: Spillover Impact of DACA on Labor Market Outcomes of U.S.-Born in the Commuting Zone



NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 65. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. Commuting-zone controls include total population, share male, average age, race/ethnicity shares (including share Hispanic), share foreign-born, marital-status shares, and education shares. 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: Authors' own calculations using 2007–2019 ACS microdata.

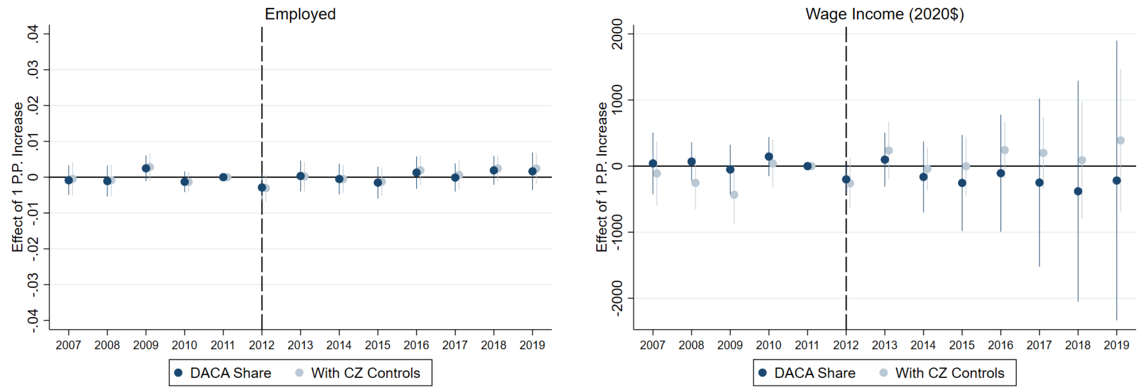
Figure A15: Spillover Impact of DACA on U.S.-Born in the Commuting Zone by Gender



NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 65. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. 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: Authors' own calculations using 2007–2019 ACS microdata.

Figure A16: Spillover Impact of DACA on Preexisting Foreign-Born in the Commuting Zone



NOTE: Sample restricted to foreign-born respondents of the 2007–2019 ACS, ages 18 to 65, who entered before 2007 but do not meet DACA eligibility criteria. The *Predicted DACA Share* captures the predicted share of the commuting-zone population that meets the DACA criteria used in the main analysis to identify the treated sample. The coefficients from Equation (5) 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. 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: Authors' 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		With Average Test Scores		In State with Average Violent Crime		With Hispanic Hispanic				
	Above Median (1)	Below Median (2)	Above Median (3)	Below Median (4)	Above Median (5)	Below Median (6)	Above Median (7)	Below Median (8)	In MSA (9)	In Border State (10)	In Sanctuary City
Entered Under 16*Post-DACA	0.011***	0.003	0.005*	0.009***	0.011***	0.002	0.012***	0.002	0.010***	0.003	0.007**
	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Entered Under 16	-0.006**	-0.001	-0.003	-0.004**	-0.005*	-0.002	-0.007**	-0.001	-0.007**	0.001	-0.004*
	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
Dependent Mean	0.04	0.02	0.02	0.04	0.05	0.01	0.05	0.01	0.05	0.02	0.03
Observations	94,179	94,179	94,179	94,179	94,179	94,179	94,179	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were at least 18 in 2007. Violent crime rate is measured at the state level. Border states include California, Arizona, New Mexico, and Texas. Standard errors are 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

Table A2: Impact of DACA on Job Characteristics of DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants

	Employed Individuals				All Individuals
	Work Over 40 Hours per Week (1)	Depart for Work After 7 PM (2)	Commute Time (in Minutes) (3)	Health Insurance Through Employer (4)	Health Insurance Throught Employer (5)
Entered Under 16*Post-DACA	0.009	0.003	0.979***	0.006	0.020*
	(0.007)	(0.003)	(0.361)	(0.010)	(0.010)
Entered Under 16	-0.009	0.001	-1.303***	0.132***	0.097***
	(0.006)	(0.002)	(0.262)	(0.008)	(0.007)
Dependent Mean	0.18	0.02	27.42	0.34	0.28
Observations	66,023	63,313	63,313	60,112	84,802

NOTE: Occupational percentiles in Math, Routine, and Social Skills are taken from (Deming, 2017) and capture the relative task composition of occupations based on the 1998 O*net dictionary. Sample is restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were at least 18 in 2007. Standard errors are 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

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

	Include Non-Hispanic (1)	Include Citizens (2)	No Education Restriction (3)	Exclude 2007 Arrivals (4)	Include 2005-2006 (5)	Exclude 2007 (6)	Exclude 2017-2019 (7)	Teen Arrivals (8)
Outcome: Move in the Past 12 Months								
Entered Under 16*Post-DACA	0.044***	0.038***	0.036***	0.044***	0.049***	0.037***	0.039***	0.025**
	(0.005)	(0.007)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)	(0.011)
Entered Under 16	-0.067***	-0.042***	-0.037***	-0.050***	-0.059***	-0.047***	-0.052***	-0.041***
	(0.005)	(0.006)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)	(0.008)
Dependent Mean	0.24	0.20	0.21	0.20	0.22	0.20	0.22	0.21
Observations	165,429	147,679	166,508	88,735	111,912	84,802	77,422	47,996
Outcome: Occupation Median Income (2020)								
Entered Under 16*Post-DACA	-2.4e+03***	1285.738***	879.936***	1050.666***	1267.323***	973.782***	971.335***	1318.317***
	(330.650)	(228.311)	(152.995)	(204.459)	(198.802)	(215.622)	(220.894)	(239.410)
Entered Under 16	1191.609***	4714.531***	2482.896***	2969.144***	2716.317***	2992.575***	2989.407***	1315.763***
	(230.914)	(164.009)	(126.713)	(195.492)	(160.470)	(209.425)	(189.297)	(151.964)
Dependent Mean	19371.81	18080.49	12876.39	14864.23	14015.32	15095.73	14291.63	14247.57
Observations	165,429	147,679	166,508	88,735	111,912	84,802	77,422	47,996
Outcome: Licensed Occupation								
Entered Under 16*Post-DACA	0.001	0.012	0.017***	0.020***	0.024***	0.016**	0.022***	0.023***
	(0.005)	(0.009)	(0.005)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Entered Under 16	0.049***	0.125***	0.074***	0.084***	0.080***	0.088***	0.085***	0.042***
	(0.006)	(0.010)	(0.009)	(0.011)	(0.010)	(0.010)	(0.011)	(0.007)
Dependent Mean	0.34	0.35	0.22	0.28	0.26	0.28	0.26	0.26
Observations	165,429	147,679	166,508	88,735	111,912	84,802	77,422	47,996

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989, unless otherwise specified. The birth-cohort restriction ensures that individuals were under 31 years of age by June 30, 2012, as required for DACA eligibility, and were at least 18 in 2007. Standard errors are 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 noncitizens. Column (3) removes the education restriction. Column (4) excludes individuals who arrived in 2007, as DACA requires arrival by June 15, 2007. Column (5) includes observations from 2005 and 2006, even though the arrived-before-2007 requirement affects them differently. Column (6) excludes observations from 2007, as these are potentially new arrivals. Column (7) excludes the years affected by Trump-era uncertainty and changes to DACA in 2017–2019. Column (8) restricts the sample to include only individuals who came to the U.S. between ages 11 and 19, in an attempt to identify a more similar sample. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

Table A4: Robustness of Impact of DACA on Mobility of DACA-Eligible Immigrants to Specification

	State Trends (1)	State-by-Year F.E. (2)	Entry Age F.E. (3)	Age-by-year F.E. (4)
	Outcome: Move in the Past 12 Months			
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
	Outcome: Occupation Median Income (2020)			
Entered Under 16*Post-DACA	1146.824***	927.714***	1006.519***	1037.247***
	(183.660)	(179.355)	(193.216)	(196.953)
Entered Under 16	2908.546***	3032.410***	0.000	2950.222***
	(187.815)	(182.967)	(0.000)	(176.640)
Dependent Mean	14792.92	14790.32	14792.92	14792.92
Observations	94,179	94,087	94,179	94,179
	Outcome: Licensed Occupation			
Entered Under 16*Post-DACA	0.024***	0.019***	0.020***	0.019***
	(0.006)	(0.006)	(0.007)	(0.007)
Entered Under 16	0.083***	0.086***	0.000	0.085***
	(0.010)	(0.010)	(0.000)	(0.010)
Dependent Mean	0.28	0.28	0.28	0.28
Observations	94,179	94,087	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 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 were at least 18 in 2007. Standard errors are 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) includes 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, and excludes individuals that arrived in 2007, as DACA requires arrival by June 15, 2007. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

Table A5: 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)
				Women			
Entered Under 16*Post-DACA	-0.026**	0.006	0.020*	0.037***	-0.018	-0.068***	-0.079***
	(0.011)	(0.006)	(0.011)	(0.007)	(0.013)	(0.025)	(0.011)
Entered Under 16	0.091***	0.020***	-0.111***	0.014**	-0.042***	-0.009	0.213***
	(0.011)	(0.004)	(0.012)	(0.006)	(0.014)	(0.024)	(0.019)
Dependent Mean	0.46	0.07	0.48	0.17	0.60	1.21	0.25
Observations	43,722	43,722	43,722	43,722	43,722	43,722	43,722
				Men			
Entered Under 16*Post-DACA	0.042***	0.000	-0.043***	-0.003	-0.063***	-0.154***	-0.082***
	(0.009)	(0.004)	(0.011)	(0.010)	(0.013)	(0.024)	(0.007)
Entered Under 16	0.011	0.013***	-0.024***	0.032***	-0.006	0.006	0.256***
	(0.008)	(0.003)	(0.009)	(0.009)	(0.009)	(0.019)	(0.013)
Dependent Mean	0.59	0.04	0.37	0.16	0.35	0.70	0.26
Observations	50,446	50,446	50,446	50,446	50,446	50,446	50,446

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were at least 18 in 2007. Standard errors are 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

Table A6: 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)
				Women			
Entered Under 16*Post-DACA	-0.026**	0.006	0.020	0.037***	-0.018	-0.068**	-0.079***
	(0.010)	(0.007)	(0.012)	(0.007)	(0.014)	(0.029)	(0.011)
Entered Under 16	0.091***	0.020***	-0.111***	0.014**	-0.042***	-0.009	0.213***
	(0.011)	(0.004)	(0.012)	(0.006)	(0.014)	(0.024)	(0.019)
Dependent Mean	0.46	0.07	0.48	0.17	0.60	1.21	0.25
Observations	43,722	43,722	43,722	43,722	43,722	43,722	43,722
				Men			
Entered Under 16*Post-DACA	0.024**	-0.001	-0.024**	0.002	-0.049***	-0.117***	-0.067***
	(0.010)	(0.003)	(0.011)	(0.007)	(0.013)	(0.019)	(0.010)
Entered Under 16	0.011	0.013***	-0.024**	0.032***	-0.006	0.006	0.256***
	(0.008)	(0.003)	(0.009)	(0.009)	(0.009)	(0.019)	(0.013)
Dependent Mean	0.59	0.04	0.37	0.16	0.35	0.70	0.26
Observations	50,446	50,446	50,446	50,446	50,446	50,446	50,446

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born in the latter half of 1981, or in 1982–1989. The birth-cohort restriction ensures that individuals were under the age of 31 by June 30, 2012, as required for DACA eligibility, and were at least 18 in 2007. Standard errors are 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. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.

Table A7: Relationship between Predicted DACA Share and Commuting-Zone Characteristics and Trends

	Predictive Power of Predicted 2007 DACA Share		
	Level in 2007	Δ 2007-2011	Δ 2007-2019
	(1)	(2)	(3)
Total Population (1,000s)	227.500***	14.430***	40.701**
	(72.105)	(3.823)	(16.882)
Share Male	0.002**	0.001***	0.002***
	(0.001)	(0.000)	(0.001)
Age	-0.899***	-0.132***	-0.205***
	(0.101)	(0.038)	(0.067)
Share NH White	-0.135***	-0.005***	-0.009***
	(0.008)	(0.001)	(0.003)
Share NH Black	-0.020***	-0.001**	-0.001**
	(0.003)	(0.000)	(0.001)
Share NH Other	-0.004***	-0.002***	-0.003***
	(0.001)	(0.000)	(0.001)
Share Hispanic	0.152***	0.008***	0.012***
	(0.007)	(0.001)	(0.003)
Share Foreign Born	0.054***	-0.001	-0.003
	(0.005)	(0.001)	(0.002)
Share Married	-0.009***	0.000	-0.001
	(0.001)	(0.001)	(0.001)
Share Never Married	0.014***	0.002***	0.004***
	(0.002)	(0.001)	(0.001)
Share Divorce/Separated	-0.004***	-0.002***	-0.003**
	(0.001)	(0.001)	(0.001)
Share Less than HS	0.031***	-0.001	0.000
	(0.002)	(0.001)	(0.002)
Share HS	-0.025***	0.025***	0.025***
	(0.002)	(0.002)	(0.002)
Share Some College	-0.007***	-0.024***	-0.022***
	(0.001)	(0.003)	(0.003)
Share 4 Year Degree	-0.000	0.001**	-0.001*
	(0.001)	(0.000)	(0.001)
Share Advanced Degree	0.000	-0.001**	-0.001**
	(0.001)	(0.000)	(0.001)
Share Not in LF	0.004*	-0.004***	-0.007***
	(0.002)	(0.001)	(0.001)
Share Employed	-0.006**	0.004***	0.007***
	(0.003)	(0.001)	(0.001)
Share Unemployed	0.001**	0.001	-0.000
	(0.000)	(0.001)	(0.001)
Average Wage Income	59.965	61.043	258.052
	(209.920)	(49.870)	(175.810)

NOTE: Observation at the commuting-zone level, with one observation per commuting zone. Robust standard errors are provided. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

SOURCE: Authors' calculations.