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

Jimena Villanueva Kiser† Riley Wilson‡

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Abstract

Exploiting variation created by Deferred Action for Childhood Arrivals (DACA), we document the effects of immigrant legalization on mobility investments and economic outcomes. DACA increased both geographic and job mobility of young immigrants, leading them to high paying labor markets and licensed occupations. Employing these shifts, we examine whether these gains to immigrants are offset by losses among U.S.-born workers. Employment of U.S.-born workers grows in the occupations that DACA recipients shifted into after DACA is implemented, even when controlling for local demand. Spillover estimates are consistent with worker complementarities and suggest that immigrant legalization generates broader economic benefits.

Key words: Legal status, DACA, immigration, geographic mobility, job mobility, occupational licensing, local labor markets

JEL classification codes: J15, K37, R23

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†Brigham Young University

‡Corresponding Author: Brigham Young University, Department of Economics, 2146 West View Building, Provo UT, 84602. Email: riley_wilson@byu.edu.

1 Introduction

A large literature shows that "mobility" investments, like geographic mobility and job mobility, are human-capital investments that improve economic outcomes like employment and earnings.[1] For example, by moving, individuals can encounter labor markets that more closely match their skills or preferences and improve outcomes in expectation. Similarly, job, occupation, and industry mobility can provide an opportunity for workers to match with better-suited jobs and move up the wage ladder. Workers might be less willing to make these mobility investments when there is uncertainty that they will reap the return on these investments. This is particularly relevant for the more than 11.4 million unauthorized immigrants in the United States (Baker, 2021) who lack legal work status and face uncertainty about their future residency. The risk of deportation might discourage individuals from engaging in costly geographic or occupational moves, as it decreases the probability 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 might not only increase individual productivity but could affect local labor market dynamisms, leading to aggregate effects. With a large unauthorized population, as in the 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 for a renewable period of two years. 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 observable 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, 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 average 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 high average wages and higher employment rates, 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 service managers to occupations like child-care worker, production worker, business operations 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

[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.

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 DACA-eligible individuals work in and changes in educational attainment, while very little is explained by geographic mobility.

Legalization as a policy yields large benefits to immigrant recipients, but we also want to know if 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 legalization.[4] 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 potential employment spillovers among the U.S.-born. Using the estimated occupation shifts among DACA-eligible workers after the 2012 policy, we estimate how exposure to more DACA workers with legal work status affects U.S.-born occupation-specific employment and hourly wages in the local labor market. We find that U.S.-born individuals in occupations that saw a larger influx of DACA-eligible workers experienced small, but significant *increases* in employment rates after 2012 relative to other occupations in the same labor market. Estimates are similiar if we account for local, occupation-specific demand shocks. These gains are concentrated among workers with some college or more and prime

[4]There is work that exploits mass changes in legal status, such as Cascio and Lewis (2019), but this work focuses on outcomes of immigrants.

age workers, while workers with only a high school degree or less experience smaller employment gains. We observe little change in occupation-specific hourly wages after the influx of DACA-eligible workers. These results are 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, only a lack of gains for groups similar in age or education to the immigrants.

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 one of the closest existing works 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 complete understanding of the externalities it might impose on native workers. Related work by Battaglia (2023) finds that having a larger share of the foreign born population that are DACA-eligible in a local area is associated with increases in U.S.-born employment. But if the DACA-eligible sort into better performing labor markets (which we find), this could reflect local economic growth. Ballis

(2023) shows that DACA produced positive spillovers in the classroom among Los Angeles County students. Similar to Battaglia (2023) we are interested in understanding how legalization affects native-born workers. To account for potential immigrant sorting we exploit changes in the occupation composition of DACA-eligible workers to document within occupation spillovers to U.S.-born workers, controlling for local occupation demand. 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 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 US.[5] 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

[5]With the Employment Authorization Document (EAD card) 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.

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;[6] 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 $555 (in 2024 dollars). 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, Donald Trump's administration issued a memorandum rescinding DACA. This memo prohibited all first-time applications and allowed for renewals only until October 5, 2017, after which the program was to be phased out. In January 2018, California challenged Trump's rescission of DACA, temporarily allowing for renewals. On December 4, 2020, the Supreme Court overruled the rescission of DACA, resulting in USCIS accepting new applications and granting renewals. On July 16, 2021, the U.S. District Court of 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 April 2024, USCIS still approves renewals with an initial filling status prior to July 16 of 2021 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

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

by September 30, 2013, 472,287 individuals had already obtained DACA protections. By December 31, 2019, over 825,000 individuals had received DACA, and nearly 1.76 million renewals had occurred. Of the 2.58 million total approvals, 94 percent were from Latin America, with 79 percent from Mexico alone. Almost 29 percent of approved applicants were living in California, with another 16 percent in Texas. 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, annual survey of 1 percent 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, only intent to treat effects. 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.[7] 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.[8] Using educational attainment and schooling measures, we can identify individuals who are currently in school or have a high school diploma or equivalent and 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.

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 over 100,000 people reside in a county. The ACS does, however, provide individuals' Public Use Micro Area (PUMA) of residence, which is a small geographic entity that contains at least 100,000 people. In some cases, these are smaller than counties. The ACS also asks individuals if they have moved in the past 12 months, and if so, where they were living before the move. From this, the 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,

[7]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.

[8]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.

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, a different commuting zone, 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 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 coarse 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.[9] Occupational licenses have been shown to boost wages by as much as 18 percent (Kleiner and Krueger, 2013). Because of licensure requirements,

[9]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.

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 July 1981 and the end of 1989. By imposing this restriction, we are following the same birth cohorts over time, some of whom are DACA-eligible and "treated," while others are untreated because they moved to the 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.[10]

As seen in Table 1, the observations from 2007 to 2011, prior to the enactment of DACA, are similar on average.[11] 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

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

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

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 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.[12] Since we do not

[12]The trend for the ineligible does become steeper between 2016 and 2017, when President Trump took office. However, as we show in Appendix Table A4, the estimates are insensitive to excluding the Trump presidency.

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:

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

To explore geographic mobility, our outcome of interest will be a binary variable that equals 1 if the individual moved during the past twelve months. We will also look at whether the individual moved to a different MIGPUMA, to a different commuting zone (proxy for a different local labor market), to a different state, or at the industry and occupation they ended up in. The explanatory variables of interest are the interactions between *Entered Under 16* and the year indicators. The β_τ coefficients trace out changes in migration propensities for individuals who entered the 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.[13] 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 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

[13] 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.

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. We find a significant 0.9 percentage point (18 percent) increase in moves out of the commuting zone.[14] 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.[15] For reference, Cadena and Kovak (2016) find that for a one percent change in employment rates, the less-educated Mexican-born population adjusts by 0.6 percent.

We also examine what types of labor markets DACA-eligible immigrants are 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

[14]Since PUMA do not map one-to-one into commuting zones, we examine moves out of the PUMA's primary commuting zone based on population. Nearly 63 percent of PUMA lie completely within one commuting zone. On average 62 percent of a PUMA's population lies within the primary commuting zone.

[15]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.

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. We look at moves to above/below median PUMA, but also whether the move was to a PUMA with relatively higher or lower wages or employment rates than the origin. DACA eligibility increases moves to PUMAs with above-median-average wages by one percentage point, while migration to below-median-wage PUMAs only increases such moves by 0.4 percentage points (Table 2). However, DACA increased moves to PUMA with relatively lower wages. Taken together, this would suggest that DACA induces individuals to move to higher-wage labor markets, but that they also came from labor markets with relatively high wages. DACA affects moves to PUMAs with above-median and below-median employment rates about equally, but it significantly increased moves to PUMA with relatively higher employment rates by about twice as much as moves to relatively lower employment rates. DACA eligibility induced individuals to move to places with better employment opportunities, that also had high wages.

Temporary legal status might not only enable moves to different labor markets, but also facilitate moves to other local amenities and living arrangements. 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," [16] 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. In particular they are less likely to be living with a parent (Appendix Table A2). These patterns are consistent with DACA increasing mobility and reducing rootedness in a local area. This increased mobility responsiveness could increase the insurance value that immigrants generate for native

[16] As defined by the Center of Immigration Studies.

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. Workers need not move geographically to encounter better occupations. 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 service managers and into jobs like child-care worker, production worker, teacher, nurse, engineer, and business operations managers. 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 a $500 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, which tends to increase pay (Kleiner and Krueger, 2013). Once immigrants are granted work authorization, they enter licensed occupations that were previously unavailable to them.

This shift in occupations could also lead to a change in the types of tasks immigrants do. 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. 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 A3, access to DACA does not significantly change non-wage job amenities like the number of hours worked or the need to work the night shift, but it does lead to small, significant increases in commute time.[17] 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 A4, 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.[18] 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 group more similar, but does not significantly affect the estimate. The only time estimates are sensitive (occupation median wages

[17]DACA also increases the probability of having health insurance through your employer. This is driven by the extensive margin increase in formal employment.

[18]Since the sample is limited to birth cohorts who were adults by 2007, these estimates do not reflect potential changes among younger cohorts who aged into eligibility during Trump's presidency.

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 [19]. 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 A5).

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 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

[19] See Migration Policy Institute.

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.[20] We estimate smaller, but still significant, increases in wage income, conditional on working. Among the employed, DACA increases 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. We estimate the share of the wage income gains associated with DACA that are due to these investment switches by controlling for these intermediate outcomes. If the gains are due to changes in location or occupation, controlling for PUMA of residence or occupation fixed effects will absorb changes in the outcome due to this, allowing us to determine how much of the estimated effect of DACA can be explained by these switches. 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 or mediation analysis 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 ((445-425)/445) 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

[20]Estimating effects on the inverse hyperbolic sine of wage income suggest 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 tested safety-net programs.

individuals moved into skilled occupations that 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 led 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.[21] However, DACA introduces a unique setting. DACA provides temporary legal status and work authorization, but only for a subset of immigrants already living within the United States. The DACA eligibility criteria explicitly excludes new arrivals and does not create direct incentives for new, potential immigrants. Rather than examine how the arrival of immigrants affects 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

[21] 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).

an increase in local demand for goods and services. Depending on their legal status, newly arriving immigrants might seek informal employment or employment in sectors that native workers are unlikely to consider (such as agriculture). As such, the aggregate spillover effects on 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. Battaglia (2023) exploits the variation in exposure to DACA-eligible immigrants by geographic region and finds evidence of the policy increasing U.S. born employment; however, these results may be driven by self-selection as DACA recipients may sort into more prosperous areas (Table 2). We explore spillovers of DACA on employment of U.S.-born workers by exploiting the changes in the occupational composition of DACA-eligible workers documented in Figure 2, this approach allows us to control for shifts in local occupation demand.

6.1 Estimating Spillovers on U.S.-Born Workers

As seen in Figure 2, the legal protections of DACA shifted DACA-eligible individuals into some occupations and out of others. If immigrants displace U.S. born workers we would expect to see lower native employment rates in the occupations where the DACA-eligible ended up after DACA was implemented in 2012. If immigrants augment U.S.-born worker productivity we might expect growth in these occupations after 2012. To examine these potential impacts, we construct local labor market, occupation-specific employment rates of U.S.-born adults 18-64 using the 2007-2019 ACS. First, we collapse the data to the two digit occupation by commuting zone by year level, using survey weights. We then construct the commuting zone occupation specific employment rate in a given year by dividing the occupation employment by the total population in the commuting zone, and multiplying by 100. We do this for the full adult population, but also for subgroups by education, age, and sex. We also calculate occupation-specific hourly wages in the labor market for full-time, full-year U.S.-born workers in the occupation. We then examine how an influx of immigrants in an occupation affects U.S.-born, occupation-specific employment and wages with the following event study estimation

$$Emp.\ Rate_{oct} = \sum_{\tau=2007}^{2019} \gamma_\tau Shift\ Among\ DACA_o * (t = \tau) + \phi_o + \delta_{ct} + \varepsilon_{oct} \tag{3}$$

The outcome of interest is the employment rate in occupation o in commuting zone c of the U.S.-born population 18-64 in year t. We also examine effects on log average hourly wages for full-time, full-year employees by occupation. *Shift Among DACA* equals the point estimates from Figure 2 for each two digit occupation o. These point estimates capture the change in the probability of being in occupation o among DACA-eligible individuals after 2012 relative to barely ineligible individuals from equation (2). Occupations with larger values of *Shift Among DACA* saw more DACA-eligible inflows and are thus more treated. Commuting zone-by-year fixed effects are also included, making this a comparison between occupation-specific employment rates in the same local labor market. This will control for the fact that some labor markets might be growing more than others. We also include occupation fixed effects to control for time invariant occupation characteristics. The coefficients of interest are the γ_τ, which represent the change in the U.S.-born employment rate in more treated occupations relative to less treated occupations in the same local labor market. One concern is that DACA-eligible individuals just shifted into growing occupations. If this is the case we might also see growth in U.S.-born employment in these occupations. To see if this is driving the effects we construct occupation-level Bartik shock demand measures to capture local occupation-specific demand.[22] Standard errors are corrected for clustering

[22]We construct the share of the national employment in each two digit occupation in each commuting zone in 2006. We then interact this with the leave-out change in the national employment in that occupation from one year to the next, where the employment in the focal commuting zone is omitted, as follows

at the occupation level.

With this specification the identifying assumption is that employment rates in high DACA concentration occupations would have evolved like employment rates in low DACA concentration occupations in the same commuting zone if DACA had not been enacted. To probe the validity of this assumption we examine pre-trends to see if high and low DACA concentration occupations in the same commuting zone trended similarly as well as verify that estimates are robust to controlling for local occupation-specific demand, to ensure that we are not simply capturing occupational growth.

6.2 Spillover to U.S.-born Workers: Results

Event study results for occupation-level employment rates and log hourly wages are provided in Figure 3. The shift of the DACA-eligible after 2012 has virtually no effect on U.S.-born worker employment during the pre-period between 2007 and 2011, suggesting that occupation-level employment rates of U.S.-born workers were not correlated with future shifts among the DACA-eligible. After 2012, we see a delayed increase in employment, beginning in 2014 and gradually increasing through the end of the sample in 2019. This delay is consistent with the fact that it took time for DACA recipients to shift into more skilled, licensed occupations upon receiving a Social Security Number. For a one point percentage point increase in the share of DACA-eligible individuals in an occupation, employment of U.S.-born adults in that occupation increases by 0.2 percentage points by 2019, suggesting a small positive spillover. This is not driven by the fact that DACA-eligible immigrants chose to enter growing occupations. The pattern is the same if we include Bartik measures of local occupation demand. When examining log average hourly wages there is a positive pre-trend between 2007 and 2009. However after 2012 this levels off or even reverts slightly. There is little evidence that U.S.-born workers in occupations that DACA recipients shifted into experienced a change in hourly wages. This is consistent with DACA recipients shifting into

$Bartik\ Demand_{oct} = (\frac{Emp_{oc2006}}{Emp_{o2006}}) * \Delta(Emp_{o,t,-c} - Emp_{o,t+1,-c})$.

occupations that were experiencing wage growth during the Great Recession, but that were growing similar to other occupations after 2012. Once again, the patterns are similar if we include occupation demand measures (Table 5). After DACA is implemented, we observe a small, but significant increase in U.S.-born employment in the same occupations that DACA-eligible workers shifted into. This is consistent with a small, positive employment spillover with no effect on hourly wages.

We next explore effects by education-level, age, and sex in Table 5 using the difference-in-differences equation analogous to equation (3). For the full U.S.-born population, a one percentage point increase in the share of DACA-eligible individuals in an occupation, increased employment in that occupation by 0.1 percentage points. The positive employment spillover is concentrated among workers with some college or more and among all age groups. The effects are largest for middle-aged workers, followed by effects for older workers and then younger workers, who are approximately the same age as the DACA-eligible. The employment effects are larger for women, but not statistically distinguishable. Controlling for local occupation demand does not change these estimates. We see weak evidence that the shift of DACA recipients into an occupation affects the hourly wages in that occupation after 2012, and consistent with the event studies, these estimated effects are small (0.4 percent increase for the full population). 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 workers and workers with some college education in the production process, leading to increased employment in these occupations for these workers, while workers who were more similar (younger workers and less educated workers) did not experience the same gains. The human capital investments among unauthorized immigrants create positive externalities to U.S.-born workers. These patterns are consistent with theoretical predictions of the effect of legalization on native-born workers (Chassamboulli and Peri, 2015), and with previous literature exploring the effect of geographic exposure to DACA-recipients (Battaglia, 2023).

7 Conclusion

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 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. Exploiting occupation-level changes in the concentration of DACA-eligible individuals, we show that if anything occupation-specific employment levels of U.S. workers increase when the occupation is exposed to more DACA recipients, while average hourly wages are unchanged. Estimates for subgroups find similar patterns, although employment gains are concentrated among workers that are more likely to be complements to immigrant workers in the production process (prime-age workers, or workers with higher education levels). These patterns of results do not indicate that native workers bear the cost of unauthorized immigrant legalization and suggest that there could be large positive, local externalities to legalization policy.

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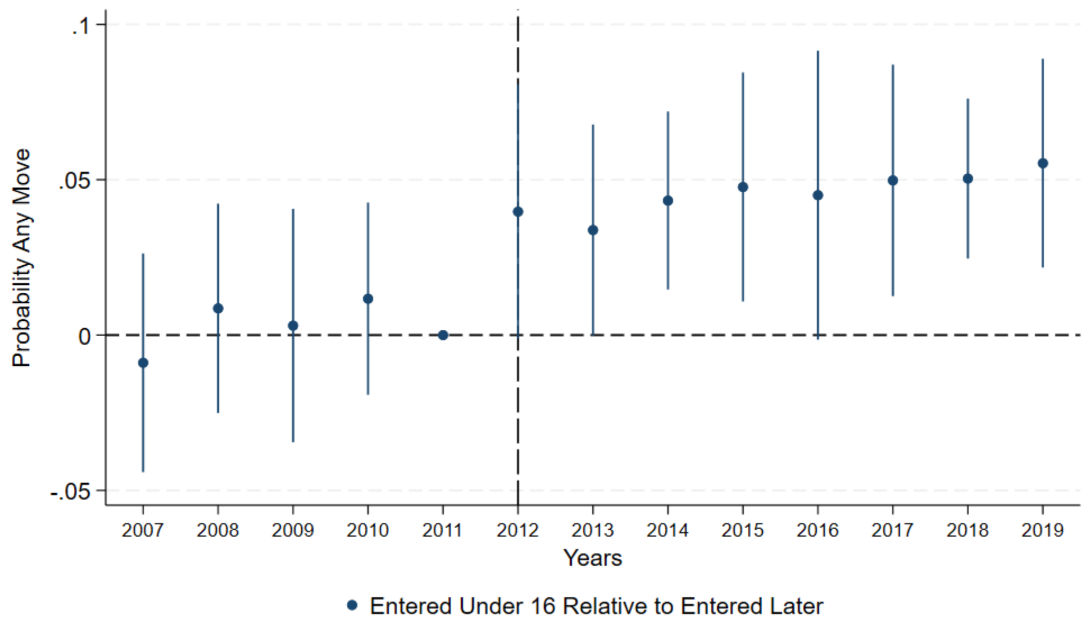
8 Tables and Figures

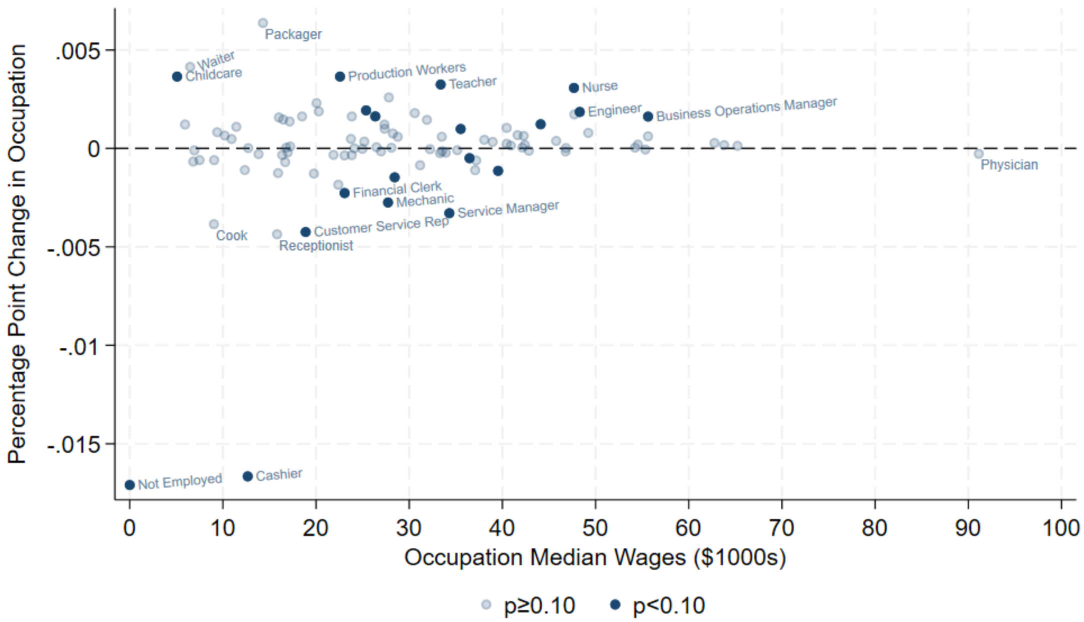
Figure 1: Probability of Moving among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



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

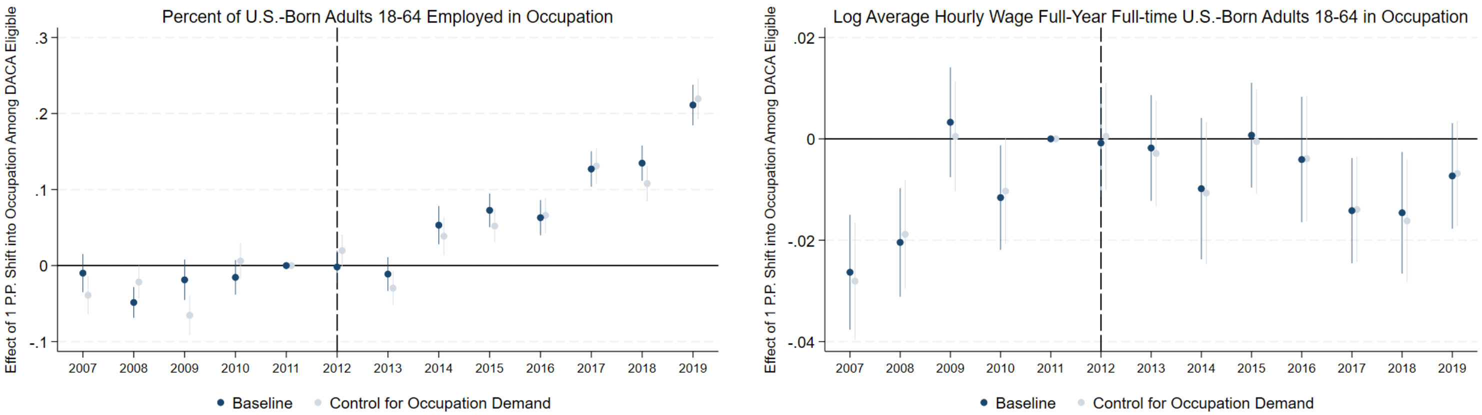
Figure 2: Occupational Mobility among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



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

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

Figure 3: Spillover Impact of DACA on Occupation-Specific Labor Market Outcomes of U.S.-Born



NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 64, then collapsed to the two-digit occupation by commuting zone by year level. Log average hourly wages are for full-year, full-time workers in the given occupation. The coefficients from Equation (3) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the occupation level. This represents the effect of a one percentage point increase in the share of DACA-eligible in a given occupation. Fixed effects for occupation and commuting zone by year are included. Bartik Demand Measures are shift share measures where the commuting zone share of national employment in the occupation in 2006 is interacted with the year-to-year change in national employment in the occupation, leaving out employment in the commuting zone. 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). This is then collapsed to the occupation by year level.

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

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

	Pre-DACA (2007-2011)		Post-DACA (2012-2019)	
	Entered After 16	Entered Under 16	Entered After 16	Entered Under 16
	(1)	(2)	(3)	(4)
Male	0.56	0.52	0.54	0.52
Age	24.55	22.97	30.74	29.28
Never Married	0.58	0.71	0.38	0.48
Married	0.39	0.26	0.55	0.45
Divorced/Separated	0.03	0.04	0.07	0.07
Own Home as Head	0.09	0.08	0.19	0.20
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 of the 2007–2019 ACS born between July 1981 and December 1989. The birth-cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. All individuals meet the DACA age, education, and year-of-arrival requirements, but vary in whether or not they arrived before their sixteenth birthday, which determines eligibility. The group that entered under age 16 is eligible for DACA.

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

	Any Move	Move out of PUMA	Move out of CZ	Move out of State
	(1)	(2)	(3)	(4)
Entered Under 16*Post-DACA	0.042***	0.014***	0.009***	0.006***
	(0.007)	(0.004)	(0.003)	(0.002)
Entered Under 16	-0.052***	-0.007**	-0.004*	-0.005***
	(0.006)	(0.003)	(0.002)	(0.002)
Dependent Mean	0.21	0.06	0.05	0.03
Observations	94,179	94,179	94,179	94,179
Move to PUMA with	Average Wages		Relative Wages	
	Above Median	Below Median	Higher	Lower
	(5)	(6)	(7)	(8)
Entered Under 16*Post-DACA	0.010***	0.004**	0.004	0.009***
	(0.003)	(0.002)	(0.003)	(0.002)
Entered Under 16	-0.006**	-0.002	-0.003*	-0.004*
	(0.003)	(0.001)	(0.002)	(0.002)
Dependent Mean	0.05	0.02	0.02	0.04
Observations	94,179	94,179	94,179	94,179
Move to PUMA with	Average E-POP		Relative E-POP	
	Above Median	Below Median	Higher	Lower
	(9)	(10)	(11)	(12)
Entered Under 16*Post-DACA	0.007**	0.006***	0.009***	0.005**
	(0.004)	(0.002)	(0.003)	(0.002)
Entered Under 16	-0.005*	-0.003	-0.005**	-0.002
	(0.003)	(0.002)	(0.002)	(0.002)
Dependent Mean	0.03	0.03	0.02	0.04
Observations	94,179	94,179	94,179	94,179

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born between July 1981 and December 1989. 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 corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. $p< 0.01$ ***, $p< 0.05$ **, $p<0.1$ *.

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

	Occupation Median Income (2020) (1)	Employed: Occupation Median Income (2020) (2)	Licensed Occupation (3)	Occ. Routine Percentile (4)	Occ. Math Percentile (5)	Occ. Social Skill Percentile (6)	Self Employed (7)
Entered Under 16*Post-DACA	1005.076***	501.798**	0.020***	0.364	-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 between July 1981 and December 1989. 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 corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. $p < 0.01$ ***; $p < 0.05$ **; $p < 0.1$ *.

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

				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 between July 1981 and December 1989. The birth cohort restriction ensures that individuals were under 31 by June 30, 2012, as required for DACA eligibility, and at least 18 in 2007. Standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included. Columns (5)-(8) add location, occupation, and education fixed effects, to determine what share of the effect on wage income can be explained by these intermediate human capital investments. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

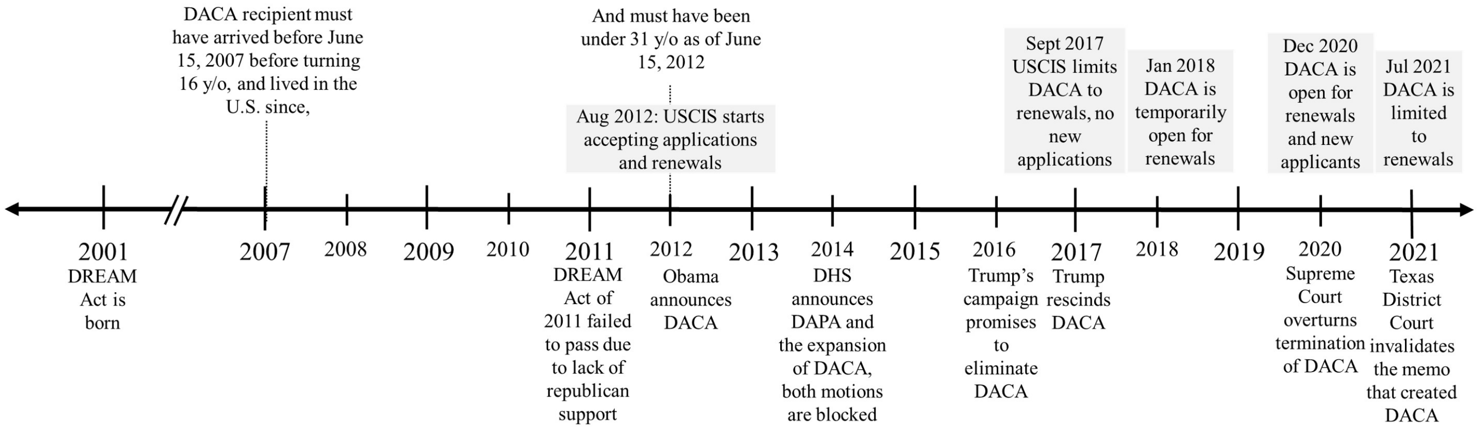
Table 5: Heterogeneous Spillover Impact of DACA on Occupation-Specific Labor Market Outcomes of U.S.-Born

	All (1)	HS or Less (2)	Some College (3)	College (4)	Age 18-34 (5)	Age 35-54 (6)	Age 55-64 (7)	Male (8)	Female (9)
Occupation-Level Employment per 100									
Shift Among DACA*Post-DACA	0.100***	0.023*	0.068***	0.067***	0.061***	0.175***	0.092***	0.087***	0.111***
	(0.007)	(0.014)	(0.011)	(0.009)	(0.012)	(0.007)	(0.008)	(0.008)	(0.010)
Dependent Mean	0.75	0.52	0.78	0.88	0.74	0.81	0.64	0.77	0.72
Observations	842,496	842,496	842,496	842,496	842,496	842,496	842,496	842,496	842,496
Occupation-Level Employment per 100, Bartik Demand Controls									
Shift Among DACA*Post-DACA	0.100***	0.023*	0.068***	0.067***	0.062***	0.175***	0.092***	0.087***	0.111***
	(0.007)	(0.014)	(0.011)	(0.009)	(0.012)	(0.007)	(0.008)	(0.008)	(0.010)
Bartik Demand Measure	20.537***	9.943***	19.559***	19.700***	24.100***	21.024***	19.027***	20.375***	20.626***
	(2.447)	(1.550)	(1.871)	(2.863)	(2.467)	(2.878)	(2.195)	(2.561)	(2.442)
Dependent Mean	0.75	0.52	0.78	0.88	0.74	0.81	0.64	0.77	0.72
Observations	842,496	842,496	842,496	842,496	842,496	842,496	842,496	842,496	842,496
Log Occupation-Level Full-time Full-Year Hourly Wages									
Shift Among DACA*Post-DACA	0.004*	-0.002	0.011***	-0.001	0.007*	-0.003	-0.004	-0.002	0.005
	(0.002)	(0.005)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)	(0.003)	(0.004)
Dependent Mean	2.76	2.47	2.68	2.96	2.54	2.82	2.82	2.84	2.63
Observations	802,572	373,732	659,232	522,493	634,741	718,333	590,158	720,564	589,068
Log Occupation-Level Full-time Full-Year Hourly Wages, Bartik Demand Controls									
Shift Among DACA*Post-DACA	0.004*	-0.002	0.011***	-0.001	0.007*	-0.003	-0.004	-0.002	0.005
	(0.002)	(0.005)	(0.003)	(0.005)	(0.004)	(0.003)	(0.005)	(0.003)	(0.004)
Bartik Demand Measure	1.227***	2.354**	0.639	1.098	1.141**	1.785***	-0.352	1.643***	0.808
	(0.358)	(1.080)	(0.517)	(0.688)	(0.507)	(0.509)	(0.643)	(0.427)	(0.584)
Dependent Mean	2.76	2.47	2.68	2.96	2.54	2.82	2.82	2.84	2.63
Observations	802,572	373,732	659,232	522,493	634,741	718,333	590,158	720,564	589,068

NOTE: Sample restricted to U.S.-born respondents of the 2007–2019 ACS, ages 18 to 64, then collapsed to the two-digit occupation by commuting zone by year level, for each demographic group. Log average hourly wages are for full-year, full-time workers in the given occupation. The coefficients from difference-in-differences specification analogous to Equation (3) are provided, with standard errors corrected for clustering at the occupation level. Fixed effects for occupation and commuting zone by year are included. Bartik Demand Measures are shift share measures where the commuting zone share of national employment in the occupation in 2006 is interacted with the year-to-year change in national employment in the occupation, leaving out employment in the commuting zone. 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). This is then collapsed to the occupation by year level. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

Appendix A. Supplementary Analyses (for online publication)

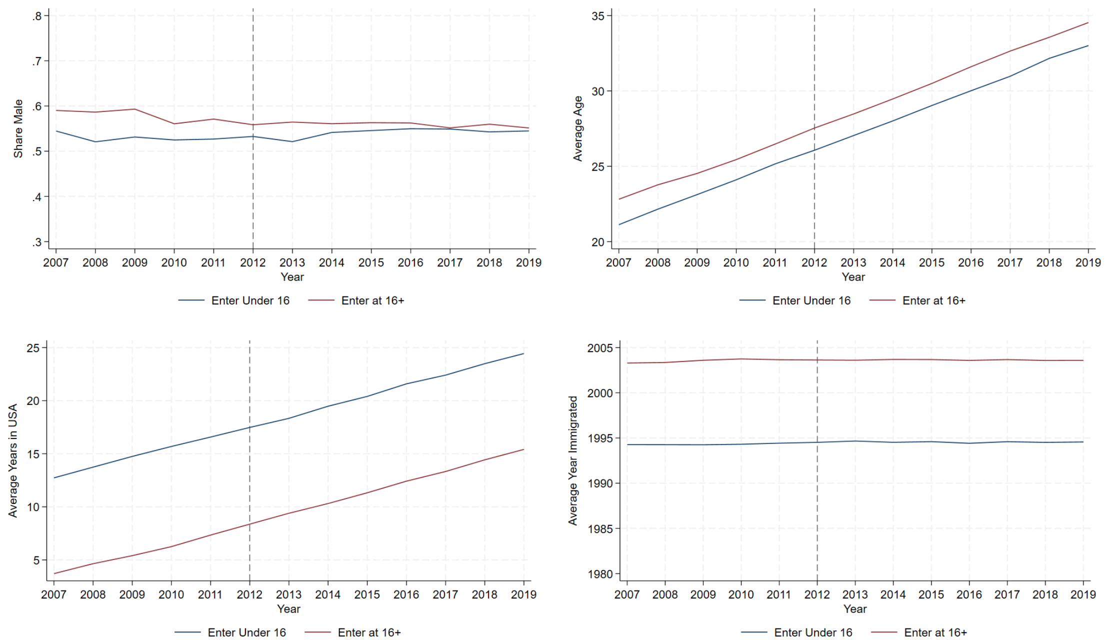
Figure A1: Timeline of DACA Legislation



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

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

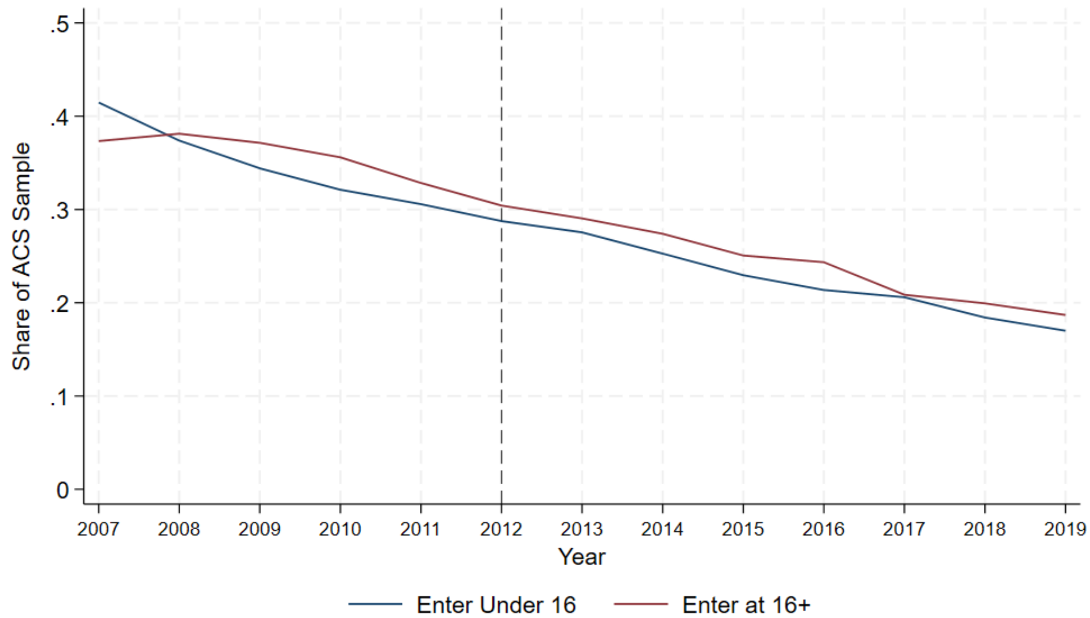
Figure A2: Exploring Differential Attrition: Trends in Average Characteristics of Treatment and Counterfactual Groups over Time



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents to the 2007–2019 ACS born between July 1981 and December 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. Average characteristics are then calculated for individuals that arrived before their sixteenth birthday (treated) and after (counterfactual), using survey weights. If the policy led to differential attrition, we would expect the averages to diverge after the 2012 implementation of DACA.

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

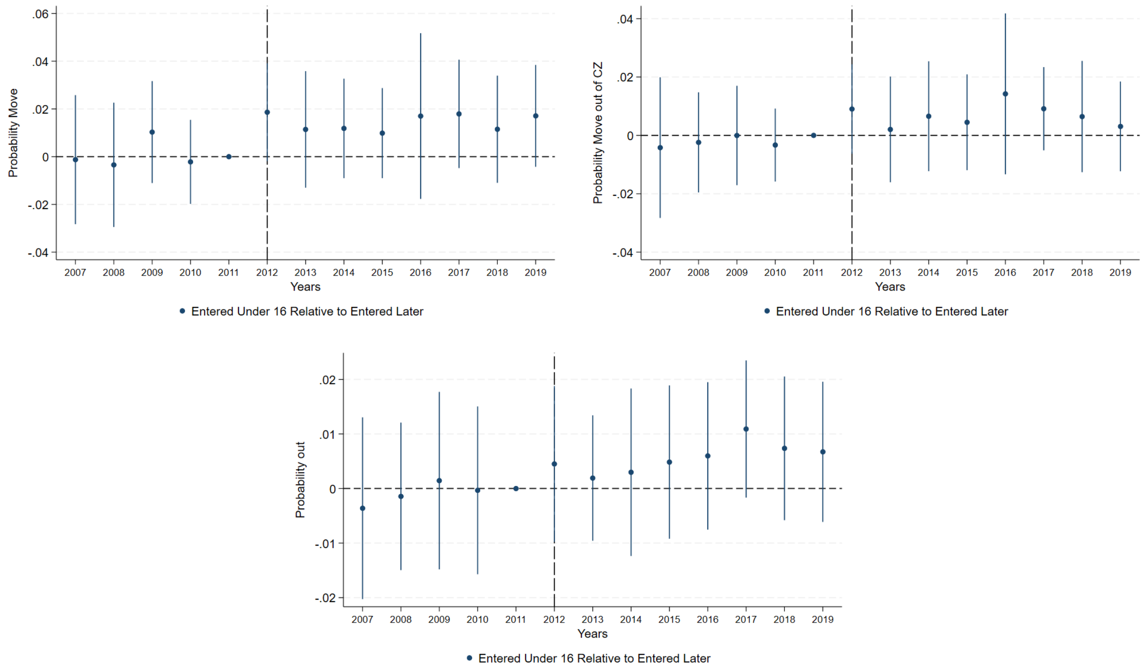
Figure A3: Exploring Differential Attrition: Share of ACS Sample in Treatment and Counterfactual Groups Over Time



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

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

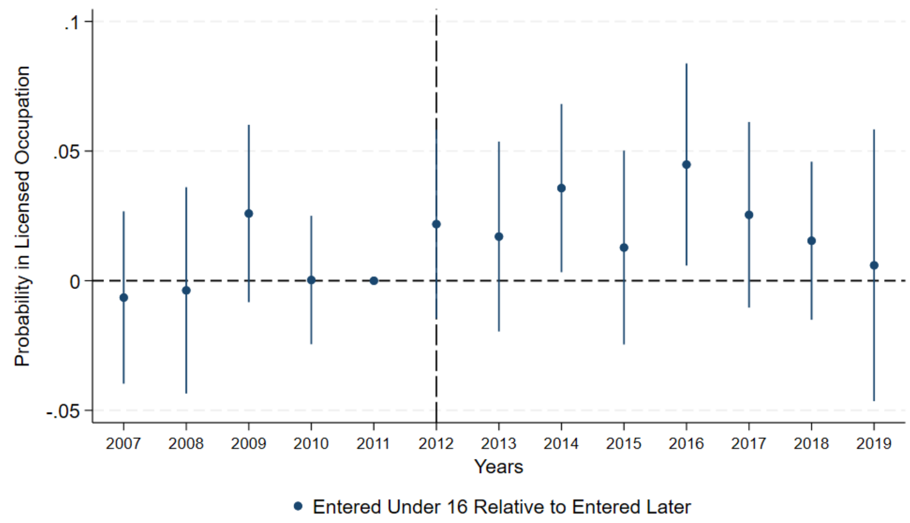
Figure A4: Probability of Moving Among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born between July 1981 and December 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 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: Author's own calculations using 2007–2019 ACS microdata.

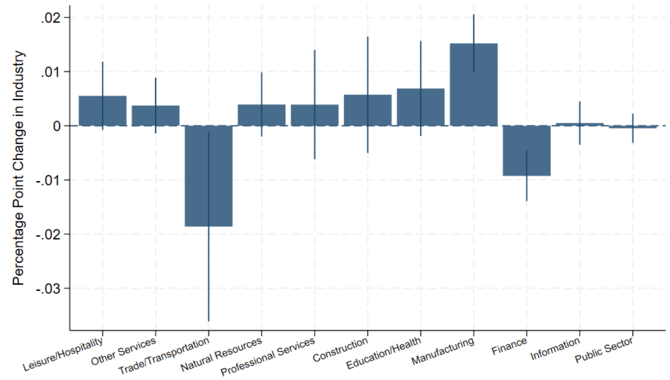
Figure A5: Probability of Being In a Licensed Occupation among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



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

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

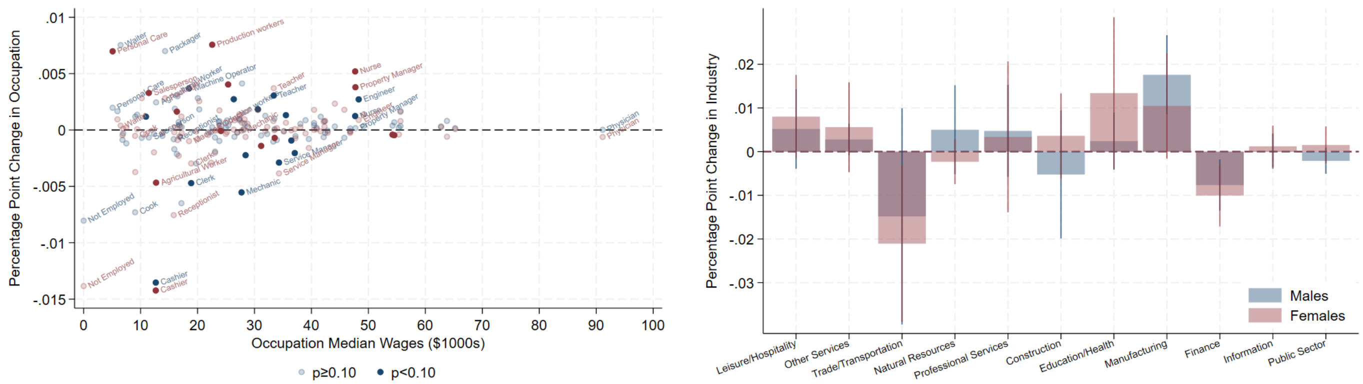
Figure A6: Industry Mobility among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born between July 1981 and December 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 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: Author's own calculations using 2007–2019 ACS microdata.

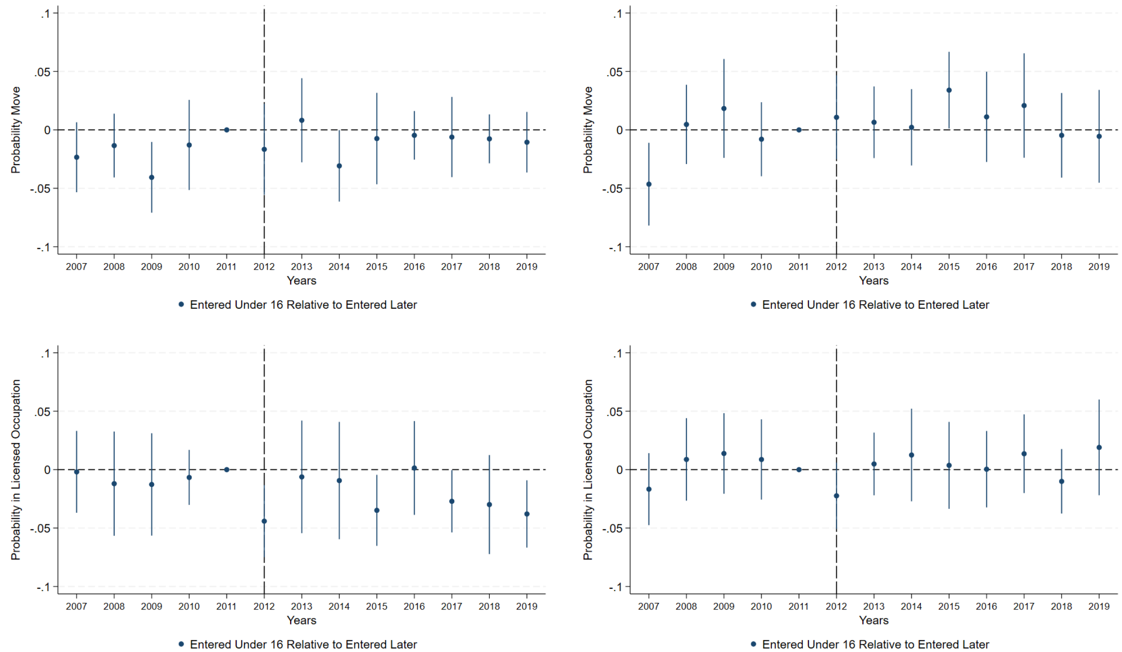
Figure A7: Occupation and Industry Mobility among DACA-Eligible Immigrants Relative to Barely Ineligible Immigrants, by Gender



NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born between July 1981 and December 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 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: Author's 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 between July 1981 and December 1989, and do not meet the DACA education requirements. The coefficients from Equation (1) are provided with 95 percent confidence intervals, with standard errors corrected for clustering at the state of residence in the previous year. Fixed effects for age, year, and state of residence in the previous year are included.

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

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			In Border	In Sanctuary
	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	Above Median	Below Median	In MSA	State	City
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
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 between July 1981 and December 1989. 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. Violent crime rate is measured at the state level. Border states include California, Arizona, New Mexico, and Texas. Sanctuary cities follow the definition of the Center for Immigration Studies (Vaughan and Griffith, 2024). 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. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

Table A2: Impact of DACA on Living Arrangements 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 to the 2007–2019 ACS born between July 1981 and December 1989. 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. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

Table A3: 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 Through Employer (5)
Entered Under 16*Post-DACA	0.009	0.003	0.921***	0.006	0.020*
	(0.007)	(0.002)	(0.317)	(0.010)	(0.010)
Entered Under 16	-0.009	0.001	-1.334***	0.132***	0.097***
	(0.006)	(0.002)	(0.253)	(0.008)	(0.007)
Dependent Mean	0.18	0.02	26.29	0.34	0.28
Observations	66,023	66,023	66,023	60,112	84,802

NOTE: Sample restricted to Hispanic, foreign-born, noncitizen respondents of the 2007–2019 ACS born between July 1981 and December 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. Health insurance data only becomes available in 2008, leading to a smaller sample. 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. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

Table A4: 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 of the 2007–2019 ACS born between July 1981 and December 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) excludes individuals who arrived in 2007, as DACA requires arrival by June 15, 2007. Column (4) includes observations from 2005 and 2006, even though the arrived-before-2007 requirement affects them differently. Column (5) excludes observations from 2007, as these are potentially new arrivals. Column (6) excludes the years affected by Trump-era uncertainty and changes to DACA in 2017–2019. Column (7) restricts the sample to include only individuals who came to the U.S. between ages 11 and 19, in an attempt to identify a more similar sample. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.

Table A5: 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 between July 1981 and December 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) included state-specific time trends, as in previous work (Pope, 2016). Column (2) includes state-by-year fixed effects, to control for state-level shocks and policy and make this a comparison between immigrants in the same state. Column (3) includes age at entry fixed effects. Controlling for the age at entry makes this similar to a regression discontinuity. Column (4) includes age-by-year fixed effects, making this a comparison between treated and counterfactual people of the same age in the same year, and excludes individuals that arrived in 2007, as DACA requires arrival by June 15, 2007. $p < 0.01$ ***, $p < 0.05$ **, $p < 0.1$ *.