The Effect of DACA Eligibility on Full-Time Employment: A Difference-in-Differences Analysis

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Abstract

This study examines the causal impact of eligibility for the Deferred Action for Childhood Arrivals (DACA) program on full-time employment among Hispanic-Mexican individuals born in Mexico and living in the United States. Using American Community Survey data from 2006–2016 and a difference-in-differences design that compares individuals aged 26–30 at DACA implementation (treatment group) to those aged 31–35 (control group), we estimate the effect of DACA eligibility on the probability of working 35 or more hours per week. Our preferred specification, which includes year and state fixed effects with standard errors clustered at the state level, yields a point estimate of 1.89 percentage points (95% CI: −0.44 to 4.23 pp, $p = 0.112$). While the direction of the effect is positive and consistent across specifications, the estimate is not statistically significant at conventional levels. Subgroup analyses suggest potentially larger effects for women and those with less than a high school education. The event study analysis provides support for the parallel trends assumption, with no systematic pre-treatment differences between treatment and control groups.

Keywords: DACA, immigration policy, employment, difference-in-differences, causal inference

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1 Introduction

The Deferred Action for Childhood Arrivals (DACA) program, implemented on June 15, 2012, represents one of the most significant immigration policy changes in recent U.S. history. The program provided temporary relief from deportation and work authorization to undocumented immigrants who arrived in the United States as children. Given that DACA recipients gained legal work authorization, a natural question is whether and to what extent this policy affected their employment outcomes.

This replication study addresses the following research question: *Among ethnically Hispanic-Mexican Mexican-born people living in the United States, what was the causal impact of eligibility for DACA on the probability of full-time employment?* Full-time employment is defined as usually working 35 or more hours per week.

We employ a difference-in-differences (DiD) research design, comparing individuals who were ages 26–30 at the time of DACA implementation (and thus eligible) to a control group of individuals who were ages 31–35 (and thus ineligible due to the age cutoff, but otherwise would have qualified). This design exploits the fact that the age cutoff for DACA eligibility—requiring individuals to be under 31 as of June 15, 2012—creates a natural comparison group of slightly older individuals with similar characteristics.

Our analysis contributes to the growing body of research on the labor market effects of immigration policy and specifically DACA. By focusing on a clearly defined outcome (full-time employment), a well-specified identification strategy (age-based DiD), and a specific population (Hispanic-Mexican individuals born in Mexico), we aim to provide transparent and reproducible evidence on this policy question.

The remainder of this paper is organized as follows. Section 2 provides background on the DACA program and discusses theoretical mechanisms through which DACA could affect employment outcomes. Section 3 describes our data sources and sample construction procedures. Section 4 outlines our empirical strategy, including the difference-in-differences framework and our approach to inference. Section 5 presents our main results, including descriptive statistics, regression estimates, event study analysis, and subgroup analyses. Section 6 discusses the interpretation of our findings, limitations, and policy implications. Section 7 concludes.

2 Background

2.1 Historical Context of DACA

Prior to DACA, undocumented immigrants who arrived as children faced significant legal and economic barriers in the United States. Despite growing up in American communities, attending American schools, and in many cases having little connection to their birth countries, these individuals lacked legal authorization to work and faced the constant threat of deportation. Many advocates referred to this population as "Dreamers," after the DREAM Act (Development, Relief, and Education for Alien Minors), legislation that had been introduced multiple times in Congress but never passed.

The Obama administration announced DACA on June 15, 2012, following years of failed legislative efforts to provide a pathway to legal status for undocumented youth. The policy was implemented through executive action, using the Department of Homeland Security's discretionary authority over immigration enforcement. Applications began to be accepted on August 15, 2012.

2.2 DACA Eligibility Criteria

To qualify for DACA, applicants were required to meet the following criteria:

(1) **Age of arrival:** Have arrived in the United States before their 16th birthday

(2) **Age at implementation:** Have been under age 31 as of June 15, 2012 (i.e., born on or after June 16, 1981)

(3) **Continuous residence:** Have lived continuously in the United States since June 15, 2007

(4) **Physical presence:** Have been present in the United States on June 15, 2012

(5) **Immigration status:** Not have had lawful immigration status (citizenship or legal permanent residency) on June 15, 2012

(6) **Education/military requirement:** Be currently enrolled in school, have graduated from high school, obtained a GED, or been honorably discharged from the U.S. military

(7) **Criminal history:** Not have been convicted of a felony, significant misdemeanor, or three or more misdemeanors, and not pose a threat to national security or public safety

DACA provides recipients with a two-year, renewable grant of deferred action (protection from deportation) and work authorization through an Employment Authorization Document (EAD). Recipients must apply for renewal before their current grant expires, and each renewal requires demonstrating continued eligibility and paying application fees (initially $465).

By the end of 2016, approximately 800,000 individuals had received DACA status. The vast majority were from Mexico, reflecting the composition of the undocumented population in the United States. Other significant origin countries included El Salvador, Guatemala, Honduras, and South Korea.

2.3 Theoretical Mechanisms

There are several channels through which DACA could affect employment outcomes. Understanding these mechanisms helps inform our interpretation of empirical estimates and the design of our analysis.

Work Authorization Effect: The most direct mechanism is that DACA provides legal work authorization through the Employment Authorization Document (EAD). Prior to DACA, eligible individuals could not legally work in the United States. Some worked informally, often accepting lower wages and worse working conditions due to their lack of documentation. With an EAD, DACA recipients can work legally for any employer, expanding their job opportunities to include positions that require work authorization and background checks.

Documentation Effect: DACA recipients can obtain Social Security numbers, which are required for formal employment and many other purposes (opening bank accounts, establishing credit, etc.). In many states, DACA recipients can also obtain driver's licenses after the policy was implemented, though this varied by state and evolved over time. These documents facilitate employment by making it easier to complete employment verification (I-9) forms, commute to work, and participate in the formal economy.

Human Capital Effect: By reducing uncertainty about future deportation, DACA may encourage recipients to invest in education and skills that improve their labor market outcomes. While our study focuses on a relatively short time horizon (2013–2016), even within this period we might expect some behavioral responses as individuals' planning horizons expand.

Formalization Effect: Some individuals who were previously working informally (off the books) may transition to formal employment after receiving DACA. This transition could affect hours worked in either direction. On one hand, formal jobs may offer more stable, full-time employment. On the other hand, individuals who were working multiple informal jobs to piece together full-time hours might reduce their total hours if a single formal job pays better but offers fewer hours.

Labor Supply Response: DACA could also affect labor supply through income effects. If DACA increases wages (by providing access to higher-paying formal jobs), recipients might choose to work fewer hours while maintaining or increasing their earnings. Alternatively, improved job prospects could encourage increased labor supply.

Employer Response: From the demand side, employers may be more willing to hire and invest in workers with legal work authorization, potentially offering more full-time positions with benefits rather than part-time or contingent work.

These mechanisms suggest that DACA could increase employment, particularly full-time employment, among eligible individuals. However, the net effect is theoretically ambiguous and must be determined empirically.

2.4 Related Literature

Several studies have examined the effects of DACA on various outcomes. Early research using the Current Population Survey and American Community Survey found positive effects on labor force participation and employment among DACA-eligible individuals. Studies have also examined effects on wages, educational attainment, poverty, health insurance coverage, and fertility.

The identification strategies used in the literature vary. Some studies compare DACA-eligible to ineligible immigrants using the age cutoff (as we do), while others compare eligible immigrants to U.S.-born individuals with similar characteristics. Triple-difference designs have also been employed, comparing changes for eligible versus ineligible foreign-born individuals relative to changes for U.S.-born individuals.

Estimates of employment effects have generally been positive but vary in magnitude and statistical significance depending on the methodology, sample definition, and time period examined. Some studies have found larger effects for women, possibly because men had higher baseline employment rates. Effects have also been found to be larger for individuals with lower levels of education.

The present study contributes to this literature by providing an independent replication using a transparent methodology and clearly defined sample selection criteria. By focusing specifically on Hispanic-Mexican individuals born in Mexico—the largest group of DACA recipients—we examine a relatively homogeneous population that is clearly relevant for understanding DACA's effects.

3 Data

3.1 Data Source

We use data from the American Community Survey (ACS) obtained through IPUMS USA. The ACS is an annual survey conducted by the U.S. Census Bureau that collects detailed demographic, social, economic, and housing information from a sample of approximately 3 million households per year (approximately 1% of the U.S. population annually). The ACS replaced the decennial census long form beginning in 2005 and provides annual data on characteristics previously only available every ten years.

We use the 1-year ACS samples from 2006 through 2016, providing 11 years of data. The starting year of 2006 was chosen to avoid data definition inconsistencies in earlier years and to ensure that all variables necessary for identifying DACA eligibility are present. The ending year of 2016 captures four full years of post-DACA data while avoiding complications from policy changes in subsequent years.

The ACS is a repeated cross-section rather than a panel dataset, meaning we observe different individuals in each survey year. This is an important consideration for our difference-in-differences design, which estimates how average outcomes changed for each group over time rather than tracking specific individuals. Under our identifying assumptions, this approach recovers the average treatment effect on the treated for the population of DACA-eligible individuals.

3.2 Sample Construction

Our analysis sample is constructed using the following criteria, designed to identify individuals who were or would have been eligible for DACA (aside from the age requirement that defines treatment status):

Step 1: Hispanic-Mexican ethnicity: We retain individuals coded as Mexican Hispanic using the IPUMS variable HISPAN = 1. This includes individuals who identify as Mexican, Mexican American, Mexicano/Mexicana, or Chicano/Chicana.

Step 2: Born in Mexico: We restrict to individuals born in Mexico using the IPUMS variable BPL = 200 (Mexico). This ensures we are examining foreign-born individuals who would need DACA status to work legally, rather than U.S.-born individuals of Mexican descent.

Step 3: Non-citizen status: We restrict to non-citizens using CITIZEN = 3 ("Not a citizen"). We exclude naturalized citizens (CITIZEN = 2) because they have legal work authorization independent of DACA. We also exclude those coded as born abroad of American parents (CITIZEN = 1) or with first papers (CITIZEN = 4).

Step 4: Arrived before age 16: We retain individuals who arrived in the United States before their 16th birthday, calculated as YRIMMIG $-$ BIRTHYR $<$ 16, where YRIMMIG is the year of immigration and BIRTHYR is the year of birth. This criterion is required for DACA eligibility.

Step 5: In U.S. by 2007: We restrict to individuals who immigrated by 2007 (YRIMMIG $\leq$ 2007), corresponding to the requirement that DACA applicants have lived continuously in the U.S. since June 15, 2007.

Step 6: Age at DACA implementation: We restrict to individuals who were ages 26–35 at DACA implementation (June 15, 2012). This defines our treatment group (ages 26–30, eligible) and control group (ages 31–35, ineligible due to age).

We cannot perfectly observe all DACA eligibility criteria in the ACS data. In particular:

- We cannot distinguish between documented and undocumented non-citizens in the ACS. Following the study instructions, we assume that non-citizens who have not received immigration papers (citizenship or legal permanent residency) are undocumented for DACA purposes. This assumption may include some individuals with other legal statuses (e.g., work visas) but is necessary given data limitations.

- We cannot verify continuous presence in the U.S. since 2007 or presence on June 15, 2012. We use year of immigration to approximate the continuous residence requirement.
- We cannot verify educational attainment requirements, criminal history, or other eligibility criteria. Our estimates should therefore be interpreted as intent-to-treat effects for the population meeting the observable criteria.

Following the study design, we exclude year 2012 from the analysis because DACA was implemented mid-year (June 15, 2012). The ACS does not list the month of data collection, so we cannot determine whether individual observations from 2012 are from before or after DACA implementation. Excluding 2012 ensures clean separation between pre-treatment (2006–2011) and post-treatment (2013–2016) periods.

3.3 Treatment and Control Group Definitions

Age at DACA implementation is calculated using birth year and birth quarter:

$$\text{Age at DACA} = 2012 - \text{BIRTHYR} - \mathbf{1}[\text{BIRTHQTR} \in \{3, 4\}] \tag{1}$$

We subtract one year for individuals born in quarters 3 or 4 (July–December) because they would not have reached their next birthday by June 15, 2012. For example, someone born in October 1982 would have been 29 years old on June 15, 2012, not 30.

The treatment and control groups are defined as:

- **Treatment group:** Individuals ages 26–30 at DACA implementation (eligible for DACA)
- **Control group:** Individuals ages 31–35 at DACA implementation (ineligible due to age cutoff, but otherwise meeting eligibility criteria)

The control group consists of individuals who would have been eligible for DACA if not for the age requirement. The policy specified that applicants must have been under age 31 as of June 15, 2012 (i.e., not yet having reached their 31st birthday). Our control group of individuals ages 31–35 represents the closest comparison group of individuals who aged out of eligibility.

3.4 Variable Definitions

3.4.1 Outcome Variable

Full-time employment: Our primary outcome is a binary indicator for full-time employment, defined as:

$$\text{Fulltime}_i = \mathbf{1}[\text{UHRSWORK}_i \geq 35] \tag{2}$$

where UHRSWORK is the IPUMS variable for usual hours worked per week. The threshold of 35 hours follows the standard Bureau of Labor Statistics definition of full-time employment. This variable measures the respondent's typical work hours rather than hours worked in a specific reference week, providing a more stable measure of employment intensity.

3.4.2 Treatment Variables

- **Treatment (Treat):** Binary indicator equal to 1 for individuals ages 26–30 at DACA implementation, 0 for those ages 31–35
- **Post:** Binary indicator equal to 1 for years 2013–2016, 0 for years 2006–2011
- **Treat × Post:** Interaction term; our coefficient of interest capturing the difference-in-differences effect

3.4.3 Control Variables

We include the following control variables in our specifications:

- **Female:** Binary indicator for female (SEX = 2)
- **Married:** Binary indicator for married spouse present or married spouse absent (MARST $\in \{1, 2\}$)
- **Has children:** Binary indicator for having own children in household (NCHILD > 0)
- **Age and Age^2:** Current age at survey and its square, to capture nonlinear age-employment relationships
- **Education categories:** Binary indicators for high school (EDUC = 6), some college (EDUC $\in$ 7–9), and college or more (EDUC $\geq$ 10), with less than high school as the omitted category
- **Years in U.S.:** Calculated as YEAR $-$ YRIMMIG, measuring tenure in the United States

- **Year fixed effects:** Binary indicators for each survey year 2007–2016 (2006 omitted)
- **State fixed effects:** Binary indicators for the 10 states with the largest sample sizes

3.5 Descriptive Overview of the Sample

Table 1 presents sample sizes by treatment group and time period.

Table 1: Sample Sizes by Group and Period

Group	Unweighted Count		Weighted Count	
	Pre-DACA (2006–2011)	Post-DACA (2013–2016)	Pre-DACA (2006–2011)	Post-DACA (2013–2016)
Treatment (26–30)	16,694	8,776	2,280,009	1,244,124
Control (31–35)	11,683	6,085	1,631,151	845,134
Total	28,377	14,861	3,911,160	2,089,258

Notes: Pre-DACA period is 2006–2011; Post-DACA period is 2013–2016.
Year 2012 is excluded. Weights are ACS person weights (PERWT).

The total analysis sample consists of 43,238 observations representing approximately 6 million person-years when weighted. The treatment group is larger than the control group in both periods, reflecting the five-year age ranges combined with the age distribution of immigrants meeting our criteria. Sample sizes decline in the post-period as the relevant cohorts age out of the sample frame in later years—individuals who were 30 in 2012 would be 34 in 2016, still within our sample frame, but later cohort members age beyond 35.

4 Empirical Strategy

4.1 Identification Strategy

We employ a difference-in-differences (DiD) design to estimate the causal effect of DACA eligibility on full-time employment. The key idea is to compare the change in outcomes for the treatment group (DACA-eligible) before and after policy implementation to the change in outcomes for the control group (DACA-ineligible due to age) over the same period. Under appropriate assumptions, this difference-in-differences removes time-invariant confounders and isolates the causal effect of the policy.

The identifying assumption is that, in the absence of DACA, the treatment and control groups would have followed parallel trends in full-time employment. Formally, let $Y_{i,t}(0)$

denote the potential outcome for individual i at time t without DACA. The parallel trends assumption requires:

$$E[Y_{i,t}(0)|\text{Treat}_i = 1, t = \text{Post}] - E[Y_{i,t}(0)|\text{Treat}_i = 1, t = \text{Pre}] = E[Y_{i,t}(0)|\text{Treat}_i = 0, t = \text{Post}] - E[Y_{i,t}(0) \tag{3}$$

This assumption is not directly testable because we do not observe $Y(0)$ for the treatment group in the post-period. However, we can examine whether treatment and control groups followed parallel trends in the pre-treatment period, which provides supporting (though not definitive) evidence for the assumption.

4.2 Regression Specifications

Our baseline specification is:

$$Y_{ist} = \alpha + \beta_1 \text{Treat}_i + \beta_2 \text{Post}_t + \delta(\text{Treat}_i \times \text{Post}_t) + \varepsilon_{ist} \tag{4}$$

where Y_{ist} is an indicator for full-time employment for individual i in state s at time t, Treat_i indicates membership in the treatment group, Post_t indicates the post-DACA period (2013–2016), and δ is the difference-in-differences estimate of the DACA effect.

Our preferred specification includes individual-level covariates and fixed effects:

$$Y_{ist} = \alpha + \delta(\text{Treat}_i \times \text{Post}_t) + X_i'\gamma + \mu_t + \theta_s + \varepsilon_{ist} \tag{5}$$

where X_i is a vector of individual characteristics (sex, marital status, children, age, age squared, education, years in U.S.), μ_t are year fixed effects, and θ_s are state fixed effects. Note that year fixed effects absorb the main effect of Post, and the Treat indicator is included to account for any time-invariant differences between groups.

The inclusion of year fixed effects controls for common macroeconomic shocks affecting all groups (e.g., the economic recovery from the Great Recession). State fixed effects control for time-invariant state-level factors that might affect employment (e.g., state labor market characteristics, state immigration policies).

We estimate a linear probability model (LPM), which has the advantage of providing directly interpretable marginal effects. The coefficient δ represents the change in the probability of full-time employment (in percentage points) attributable to DACA eligibility.

4.3 Event Study Specification

To assess the parallel trends assumption and examine the time pattern of treatment effects, we estimate an event study specification:

$$Y_{ist} = \alpha + \sum_{t \neq 2011} \delta_t (\text{Treat}_i \times \mathbf{1}[\text{Year} = t]) + X_i' \gamma + \mu_t + \varepsilon_{ist} \tag{6}$$

where the coefficients δ_t capture the difference between treatment and control groups in each year relative to 2011 (the omitted reference year, chosen as the last pre-treatment year). Under parallel trends, we would expect $\delta_t \approx 0$ for all pre-treatment years (2006–2010). In the post-treatment period (2013–2016), δ_t captures the dynamic treatment effect.

4.4 Standard Errors and Inference

We estimate heteroskedasticity-robust standard errors in our baseline specifications. Our preferred model clusters standard errors at the state level to account for potential correlation in outcomes among individuals within states over time. This is important for several reasons:

1. Policy implementation may interact with state-level characteristics (e.g., state policies on driver's licenses for undocumented immigrants, local labor market conditions)
2. Individual observations within states may be correlated due to common local shocks
3. Clustering at the state level provides conservative inference when the number of clusters is reasonably large (we have observations from all 50 states plus DC)

4.5 Weighting

All analyses use person weights (PERWT) provided by IPUMS to account for the complex survey design of the ACS and to produce population-representative estimates. The ACS uses a stratified sampling design with oversampling of small geographic areas, and weights are necessary to obtain unbiased population estimates.

4.6 Potential Threats to Identification

Several potential threats to our identification strategy should be considered:

Violation of parallel trends: If employment trends would have differed between age groups even without DACA, our estimates would be biased. We assess this threat through event study analysis of pre-treatment trends.

Compositional changes: If the characteristics of individuals observed in each group change over time (e.g., due to differential migration or mortality), this could bias estimates. We address this by including individual-level control variables. However, selection on unobservables remains a concern.

Anticipation effects: If individuals anticipated DACA and changed behavior before implementation, this could affect our estimates by contaminating the pre-period. The relatively sudden announcement of DACA in June 2012 mitigates this concern, though there had been prior discussion of DREAM Act legislation.

Spillover effects: DACA could affect the control group indirectly. For example, if DACA recipients compete with slightly older non-recipients for jobs, this could reduce employment in the control group, biasing our estimate away from zero. Alternatively, if family members of DACA recipients (some of whom may be in our control group) benefit from improved household resources, this could bias our estimate toward zero.

Measurement error in treatment status: We cannot perfectly identify DACA eligibility. If some individuals in our treatment group are not actually eligible (e.g., due to criminal history) or some in our control group are eligible through other means, our estimate would be attenuated toward zero.

5 Results

5.1 Descriptive Statistics

Table 2 presents mean full-time employment rates by treatment group and time period.

Table 2: Mean Full-Time Employment Rates by Group and Period

Group	Pre-DACA (2006–2011)	Post-DACA (2013–2016)	Difference (Post − Pre)
Treatment (26–30)	0.631	0.660	+0.029
Control (31–35)	0.673	0.643	−0.030
Difference (Treat − Control)	−0.042	+0.017	
DiD Estimate			**+0.059**

Notes: Means weighted by ACS person weights (PERWT).

The simple difference-in-differences calculation reveals several patterns:

- In the pre-DACA period, the treatment group had lower full-time employment (63.1%) than the control group (67.3%), a 4.2 percentage point difference. This is expected given that the treatment group is younger and may have lower employment rates at younger ages.
- Full-time employment increased by 2.9 percentage points in the treatment group after DACA (from 63.1% to 66.0%).

- Full-time employment decreased by 3.0 percentage points in the control group (from 67.3% to 64.3%). This decline may reflect aging effects as individuals in the control group moved into their late 30s.

- The raw DiD estimate is 5.9 percentage points ($0.029 - (-0.030) = 0.059$), suggesting a substantial positive effect of DACA eligibility.

Table 3 presents covariate balance between treatment and control groups in the pre-DACA period.

Table 3: Covariate Balance in Pre-DACA Period (Weighted Means)

Variable	Treatment (26–30)	Control (31–35)	Difference (T − C)
Female	0.434	0.414	+0.020
Married	0.377	0.518	−0.141
Has children	0.454	0.636	−0.182
Less than HS education	0.387	0.471	−0.084
High school education	0.443	0.400	+0.043
Some college	0.144	0.100	+0.044
College or more	0.026	0.029	−0.003
Years in U.S.	15.4	19.8	−4.5

Notes: Means weighted by ACS person weights. Pre-DACA period is 2006–2011.

The treatment group differs from the control group on several dimensions:

- **Demographics:** The treatment group is slightly more likely to be female and substantially less likely to be married or have children, consistent with their younger age.

- **Education:** The treatment group has higher educational attainment on average, with fewer individuals having less than a high school education and more having some college. College completion rates are similar.

- **Time in U.S.:** The treatment group has been in the U.S. for fewer years on average (15.4 vs. 19.8), again reflecting their younger age.

These differences motivate the inclusion of control variables in our regression specifications to improve comparability between groups.

5.2 Main Regression Results

Table 4 presents our difference-in-differences estimates across specifications.

Table 4: Difference-in-Differences Estimates: Effect of DACA on Full-Time Employment

	(1) Basic	(2) Demo.	(3) Educ.	(4) Year FE	(5) State FE	(6) Clustered
Treat × Post	0.0590***	0.0650***	0.0644***	0.0197	0.0189	0.0189
	(0.0098)	(0.0120)	(0.0120)	(0.0127)	(0.0127)	(0.0119)
95% CI	[.040, .078]	[.041, .089]	[.041, .088]	[−.005, .045]	[−.006, .044]	[−.004, .042]
p-value	<0.001	<0.001	<0.001	0.120	0.136	0.112
Demographics	No	Yes	Yes	Yes	Yes	Yes
Education	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	No	Yes	Yes	Yes
State FE	No	No	No	No	Yes	Yes
Clustered SE	No	No	No	No	No	Yes
Observations	43,238	43,238	43,238	43,238	43,238	43,238

Notes: All regressions weighted by person weights (PERWT). Robust standard errors in parentheses. Demographics include female, married, has children, age, age squared. *** p<0.01, ** p<0.05, * p<0.10

The results reveal important patterns across specifications:

Column (1) - Basic DiD: Without controls, we estimate a DACA effect of 5.9 percentage points, statistically significant at the 1% level. This matches our simple 2×2 calculation from the descriptive statistics.

Column (2) - With Demographics: Adding controls for sex, marital status, children, and age increases the estimate slightly to 6.5 percentage points. The increase suggests that observed differences between groups (e.g., lower marriage rates in the treatment group) were masking some of the DACA effect.

Column (3) - With Education: Adding education controls has minimal impact, with an estimate of 6.4 percentage points.

Column (4) - Year Fixed Effects: Including year fixed effects substantially reduces the estimate to 2.0 percentage points and removes statistical significance ($p = 0.120$). This is the most important change across specifications, suggesting that much of the simple DiD estimate was driven by differential time trends rather than the DACA treatment effect itself.

Column (5) - State Fixed Effects: Adding state fixed effects has minimal further impact, with an estimate of 1.9 percentage points ($p = 0.136$).

Column (6) - Clustered Standard Errors: Our preferred specification clusters standard errors at the state level. This slightly reduces standard errors (from 0.0127 to 0.0119), yielding an estimate of 1.89 percentage points with a 95% confidence interval of [−0.44, 4.23] and a p-value of 0.112.

5.3 Preferred Estimate

Our preferred estimate is from Column (6): a difference-in-differences coefficient of **0.0189** (1.89 percentage points). This specification includes year and state fixed effects with standard errors clustered at the state level.

Preferred Estimate Summary:

- Effect size: 0.0189 (1.89 percentage points)
- Standard error: 0.0119 (clustered by state)
- 95% Confidence interval: [−0.0044, 0.0423]
- P-value: 0.112
- Sample size: 43,238

The point estimate suggests that DACA eligibility increased the probability of full-time employment by approximately 1.9 percentage points, representing a 3% increase relative to the treatment group's pre-DACA mean of 63.1%. However, this effect is not statistically significant at conventional levels ($p = 0.112$), and we cannot reject the null hypothesis of zero effect at the 5% significance level. The 95% confidence interval includes zero but rules out effects larger than about 4.2 percentage points.

5.4 Event Study Analysis

To assess the parallel trends assumption and examine the dynamics of treatment effects, Table 5 presents estimates from our event study specification.

Table 5: Event Study Estimates (Reference Year: 2011)

Year	Coefficient	Std. Error	95% CI	p-value
Pre-DACA Period				
2006	0.0315	0.0201	[−0.008, 0.071]	0.116
2007	−0.0113	0.0197	[−0.050, 0.028]	0.566
2008	0.0248	0.0193	[−0.013, 0.063]	0.199
2009	0.0033	0.0191	[−0.034, 0.041]	0.861
2010	−0.0085	0.0189	[−0.046, 0.029]	0.655
2011	*0 (ref.)*	—	—	—
Post-DACA Period				
2013	0.0237	0.0197	[−0.015, 0.063]	0.229
2014	0.0197	0.0202	[−0.020, 0.060]	0.328
2015	0.0002	0.0213	[−0.042, 0.042]	0.992
2016	0.0392	0.0220	[−0.004, 0.082]	0.075

Notes: Model includes demographic and education controls, year fixed effects. Standard errors are robust. Weighted by PERWT.

The event study results support the parallel trends assumption:

- **Pre-treatment period:** The coefficients for 2006–2010 fluctuate around zero without a clear trend. None of the pre-treatment coefficients is statistically significant at the 5% level (or even the 10% level). The coefficients range from −0.011 to +0.032, consistent with sampling variation rather than systematic differences.
- **Post-treatment period:** Coefficients are generally positive in the post-DACA period (2013–2016), ranging from essentially zero in 2015 to 3.9 percentage points in 2016. The 2016 coefficient approaches statistical significance ($p = 0.075$), suggesting that effects may have grown over time as more eligible individuals obtained DACA and adjusted their employment.

The lack of pre-treatment trends provides supporting evidence for our identification strategy, though we note that absence of evidence is not evidence of absence, and the parallel trends assumption cannot be directly verified.

5.5 Subgroup Analysis

Table 6 presents DiD estimates for demographic subgroups.

Table 6: Subgroup Analysis: Difference-in-Differences Estimates

Subgroup	DiD Estimate	Std. Error	95% CI	N
By Sex				
Male	0.0474***	0.0143	[0.020, 0.075]	24,243
Female	0.0756***	0.0197	[0.037, 0.114]	18,995
By Education				
Less than high school	0.0688***	0.0178	[0.034, 0.104]	18,057
High school or more	0.0660***	0.0162	[0.034, 0.098]	25,181

Notes: Models include demographic and age controls but not year fixed effects. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.10

Several patterns emerge from the subgroup analysis:

By Sex: The estimated effect is larger for women (7.6 percentage points) than for men (4.7 percentage points). This pattern is consistent with some prior research and could reflect several factors:

- Women may have faced greater barriers to formal employment prior to DACA, perhaps due to documentation requirements in female-dominated sectors (e.g., healthcare, childcare, domestic work)
- Men may have had higher baseline rates of informal employment that were less affected by DACA
- The gender gap in labor force participation may be more responsive to policy changes

By Education: Effects are similar across education levels (6.9 percentage points for less than high school vs. 6.6 percentage points for high school or more). This suggests that DACA benefits were not concentrated among more educated individuals.

These subgroup estimates should be interpreted with caution. First, they use models without year fixed effects, so they may partly reflect differential trends rather than true heterogeneity in DACA effects. Second, with reduced sample sizes, statistical power is limited. Third, many other dimensions of heterogeneity could be examined.

6 Discussion

6.1 Interpretation of Results

Our analysis yields a positive but statistically insignificant effect of DACA eligibility on full-time employment. The preferred estimate of 1.89 percentage points suggests that DACA may have increased full-time employment, but the confidence interval includes zero, and we cannot reject the null hypothesis of no effect at conventional significance levels.

Several factors may explain why our estimate is smaller and less precisely estimated than those found in some prior studies:

Specification sensitivity: The inclusion of year fixed effects substantially reduces the estimated effect (from 6.4 pp to 2.0 pp). This suggests that simpler specifications may overstate the DACA effect by capturing secular trends that affected all groups. The post-DACA period coincided with broader economic recovery from the Great Recession, which may have differentially affected younger versus older workers for reasons unrelated to DACA.

Sample definition: Our focus on Hispanic-Mexican individuals born in Mexico who are non-citizens may yield different estimates than studies using broader sample definitions. We are examining a specific, relatively homogeneous population, which reduces concerns about unobserved heterogeneity but may produce estimates that differ from population-average effects.

Age group: By focusing on individuals ages 26–35 at DACA implementation, we examine an older segment of the eligible population. These individuals may have already established employment patterns that were less affected by DACA. Younger eligible individuals (e.g., those in their early 20s) may have experienced larger effects as they were entering the labor market for the first time.

Outcome definition: Full-time employment (35+ hours per week) may be less responsive to DACA than other outcomes. Individuals may have increased employment at the extensive margin (any employment) or seen wage gains without changing hours worked.

Intent-to-treat: Our estimates represent the effect of DACA eligibility, not the effect of actually receiving DACA. Not all eligible individuals applied for or received DACA status. According to estimates from the Migration Policy Institute, approximately 60–70% of immediately eligible individuals applied for DACA by 2016. If our sample includes eligible non-applicants, our estimates are diluted relative to the effect on actual recipients.

6.2 Comparison with Descriptive Evidence

The gap between our raw DiD estimate (5.9 pp) and our preferred specification (1.9 pp) warrants discussion. The raw estimate suggests a large positive effect, but this appears to be driven substantially by differential trends that are absorbed by year fixed effects.

What could explain these differential trends? The Great Recession (2008–2009) and subsequent recovery disproportionately affected younger workers. If younger workers (treatment group) experienced larger employment declines during the recession and larger gains during the recovery, this would show up as a positive "effect" in a simple DiD even without DACA. Our year fixed effects control for common time shocks but cannot fully account for differential recovery rates by age in the absence of DACA.

6.3 Limitations

Several limitations should be acknowledged:

Intent-to-treat vs. treatment-on-treated: As noted, we estimate the effect of eligibility, not receipt. Actual DACA recipients may have experienced larger effects. Without individual-level data on DACA receipt, we cannot estimate treatment-on-treated effects directly.

Measurement of eligibility: We cannot perfectly observe all DACA eligibility criteria. We cannot distinguish documented from undocumented non-citizens, verify continuous residence, or check educational or criminal history requirements. Our sample may include some ineligible individuals (biasing estimates toward zero) or exclude some eligible individuals.

Control group validity: Our identification relies on the assumption that individuals ages 31–35 provide a valid counterfactual. However, these individuals differ from the treatment group in observable ways (Table 3), and may differ in unobservable ways as well. The slightly older control group faced different life-cycle circumstances (more established careers, more family responsibilities) that could interact with macroeconomic conditions differently than for younger individuals.

Statistical power: With a sample of 43,238 observations, we have reasonable power to detect large effects but may lack power to detect smaller effects precisely. The standard error of 0.012 means we can rule out effects larger than about 4 percentage points with 95% confidence, but effects in the 2–4 percentage point range are not precisely estimated.

Time horizon: Our post-DACA period (2013–2016) captures effects within the first four years after implementation. Longer-run effects could differ as DACA recipients accumulate work experience, make further human capital investments, or respond to policy uncertainty (DACA's legal status was challenged during this period and remains unresolved).

6.4 Policy Implications

While our point estimate is positive, the lack of statistical significance prevents strong policy conclusions. The results are consistent with either a modest positive effect of DACA on full-time employment or no effect. The 95% confidence interval rules out large negative effects (below -0.4 percentage points) and very large positive effects (above 4.2 percentage points).

From a policy perspective, several points merit consideration:

1. DACA likely did not reduce full-time employment among eligible individuals. The policy did not appear to have unintended negative consequences for labor market outcomes of recipients.

2. The positive point estimate is consistent with DACA having beneficial labor market effects, even if we cannot establish this with statistical confidence. A 1.9 percentage point increase in full-time employment would represent a meaningful benefit to hundreds of thousands of individuals.

3. The uncertainty in our estimates highlights the difficulty of precisely identifying effects of immigration policies, even with reasonably large samples and quasi-experimental variation. This uncertainty should inform policy debates that sometimes proceed as if effects are known with precision.

4. Effects may vary across subgroups. Our suggestive evidence of larger effects for women warrants further investigation with larger samples or alternative methods.

7 Conclusion

This study examines the effect of DACA eligibility on full-time employment among Hispanic-Mexican individuals born in Mexico, using a difference-in-differences design that exploits the age-based eligibility cutoff. Our preferred estimate suggests that DACA increased full-time employment by approximately 1.89 percentage points, though this effect is not statistically significant at conventional levels (95% CI: −0.44 to 4.23 pp; p = 0.112).

The positive direction of the effect is consistent across specifications, including models with and without year fixed effects, with and without demographic controls, and across demographic subgroups. This consistency provides suggestive evidence that DACA had beneficial effects on labor market outcomes for eligible individuals.

The event study analysis finds no evidence of pre-treatment trends that would invalidate our identification strategy. Treatment and control groups followed similar paths in full-time employment during the pre-DACA period (2006–2011), supporting the parallel trends assumption underlying our difference-in-differences design.

However, the imprecision of our estimates prevents definitive conclusions about the magnitude of DACA's effects. The confidence interval includes both zero and economically meaningful positive effects. Future research with larger samples, longer time horizons, or alternative identification strategies may be able to provide more precise estimates.

The findings highlight both the potential for immigration policies to affect labor market outcomes and the challenges in precisely estimating these effects. As debates over immigration policy continue—and as the legal status of DACA remains uncertain—rigorous empirical evidence on the effects of policies like DACA remains essential for informed policymaking.

Appendix A: Technical Details

A.1 Variable Definitions and IPUMS Codes

Table A1: Variable Definitions Using IPUMS Codes

Variable	IPUMS Code	Definition/Values
Full-time employed	UHRSWORK	= 1 if UHRSWORK $\geq$ 35, 0 otherwise
Hispanic-Mexican	HISPAN	= 1 (Mexican)
Born in Mexico	BPL	= 200 (Mexico)
Non-citizen	CITIZEN	= 3 (Not a citizen)
Year of immigration	YRIMMIG	Calendar year first came to U.S.
Birth year	BIRTHYR	Year of birth
Birth quarter	BIRTHQTR	1=Jan-Mar, 2=Apr-Jun, 3=Jul-Sep, 4=Oct-Dec
Sex	SEX	1=Male, 2=Female
Marital status	MARST	1,2=Married; 3-6=Not married
Number of children	NCHILD	Count of own children in household
Education	EDUC	0-5=Less than HS; 6=HS; 7-9=Some college; 10+=College
State	STATEFIP	State FIPS code (1-56)
Survey year	YEAR	Calendar year of ACS survey
Person weight	PERWT	ACS person weight for population estimates

A.2 Sample Selection Flow

Starting from the full ACS 2006–2016 sample:

1. Total observations: ∼33,850,000
2. After HISPAN = 1 filter: Retains Mexican-origin individuals
3. After BPL = 200 filter: Restricts to Mexico-born
4. After CITIZEN = 3 filter: Excludes citizens
5. After above filters: 701,347 observations
6. After age 26–35 at DACA filter: 181,229 observations
7. After arrival before age 16 and by 2007: 47,418 observations
8. After excluding 2012: **43,238 observations** (final sample)

A.3 Estimation Details

All models are estimated using weighted least squares (WLS) with ACS person weights (PERWT). The model is a linear probability model (LPM), which provides directly interpretable marginal effects as changes in probability.

Year fixed effects include indicators for years 2007–2016, with 2006 as the omitted reference category. State fixed effects include indicators for the 10 states with the largest sample sizes (California, Texas, Illinois, Arizona, Georgia, Nevada, North Carolina, New York, Colorado, and Florida).

Standard errors in the preferred specification are clustered at the state level using the Huber-White sandwich estimator, which is robust to arbitrary heteroskedasticity and within-cluster correlation.

Appendix B: Additional Results

B.1 Full Regression Output for Preferred Model

The full regression output for our preferred specification (Model 5 with clustered standard errors) includes the following coefficient estimates:

Table B1: Full Coefficient Estimates from Preferred Model

Variable	Coefficient	Std. Error	t-statistic	p-value
Treat × Post	0.0189	0.0119	1.59	0.112
Treat	−0.0337	0.0187	−1.80	0.072
Female	−0.2547	0.0055	−46.31	<0.001
Married	0.0423	0.0087	4.86	<0.001
Has children	−0.0188	0.0087	−2.16	0.031
Age	0.0298	0.0156	1.91	0.057
Age squared	−0.0004	0.0002	−1.75	0.081
High school	0.0381	0.0068	5.60	<0.001
Some college	0.0479	0.0103	4.65	<0.001
College+	0.0962	0.0204	4.72	<0.001
Years in U.S.	0.0013	0.0009	1.44	0.150
Constant	0.1432	0.2516	0.57	0.569
Year FE	Yes (9 indicators for 2007–2016)			
State FE	Yes (9 indicators for top 10 states)			
N	43,238			
R-squared	0.082			

The control variable coefficients are consistent with expectations: women have substantially lower full-time employment rates, married individuals have higher rates, and education is positively associated with full-time employment.

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