

ABSTRACT

Does Naturalization Increase Years of Education?
Effect of IRCA on Educational Attainment of Undocumented Immigrant Children

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This paper studies the effect of the 1986 Immigration Reform and Control Act (“IRCA”) on the educational attainment of undocumented immigrant children. IRCA provided a path to citizenship for over 3 million undocumented immigrants who arrived in the United States. I use data from the American Community Survey 5-Year Public Use Sample for the period 2013 through 2017 to estimate the effect on completed educational attainment. Using a differences-in-differences model, I compare childhood immigrants who arrived before the 1982 cutoff to those who arrived after. The second difference compares those who were born in countries with high applications rates (“high take-up”) and with low application rates (“low take-up”). Unexpectedly, I find no evidence that IRCA improved education outcomes. I even find partial evidence that IRCA decreased educational attainment, but this effect is primarily driven by Mexico. After dropping Mexico, I find a “zero” effect, making it unclear whether the relationship is due to unobservable changes in Mexican immigration or to the policy.

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DOES NATURALIZATION INCREASE YEARS OF EDUCATION?
EFFECT OF IRCA ON EDUCATIONAL ATTAINMENT OF UNDOCUMENTED
IMMIGRANT CHILDREN

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CHAPTER ONE

Introduction

From 1986 to 2012, the undocumented population of the United States grew from 3.2 million to 11.5 million people (Chishti and Kamasaki, 2014). This large number of undocumented immigrants has raised concerns about their economic outcomes since this group does not currently have a path to citizenship. To deter potential migrants from entering the country, the Trump administration has increased Border Patrol hours and funding. The U.S. Immigration and Customs Enforcement (ICE) removed 256,085 undocumented immigrants in 2018, a 13 percent increase from 2017 (Figure 1).

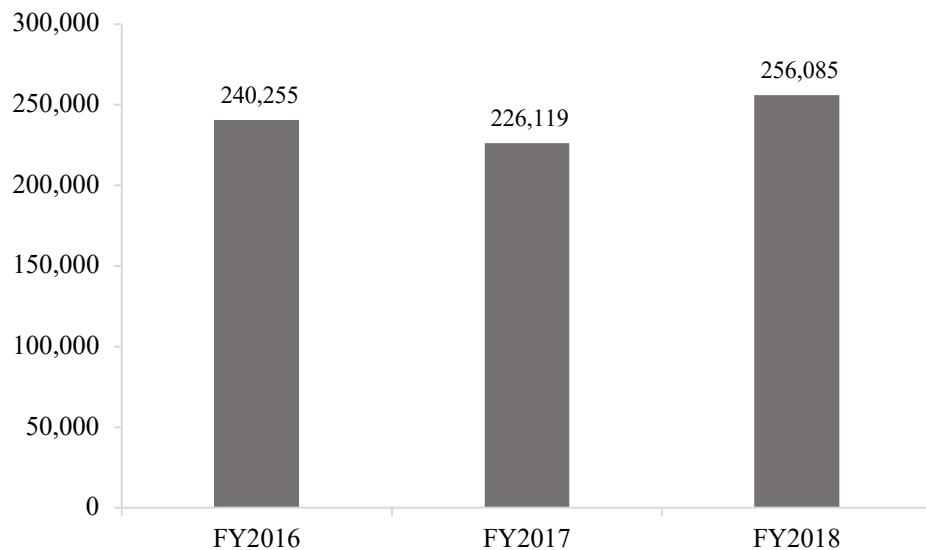


Figure 1. FY2016 - FY2018 ICE Removals

Though ICE has increased removals, another possible solution to a large number of immigrants is to grant amnesty and offer a path to citizenship. Amnesty reduces deportation risk and therefore might increase investment into human capital that helps undocumented immigrants to economically assimilate. Without amnesty, undocumented immigrants may not make such investments if they did not expect to stay in the United States and earn a return. However, the impact of deportation policy on the educational outcomes of immigrants is unclear.

In order to observe the effect of amnesty on educational outcomes of undocumented immigrants, I look at the largest amnesty program in American history: the Immigration Reform and Control Act of 1986. The Immigration Reform and Control Act (IRCA) distributed documents to more than 3 million people. The undocumented population was 3.2 million in 1986 (Woodrow, 1992), which suggests that nearly every eligible individual applied and therefore entered a path to citizenship. I estimate the effect of IRCA on educational attainment by exploiting discontinuities in who was eligible for the amnesty. Importantly, the policy's announcement in late 1985 protects the integrity of my study given that immigrants did not know that the policy would be put into effect. The bulk of undocumented immigrants applied for citizenship under the Legally Authorized Workers (LAWs) program, while a smaller set applied under the Special Agricultural Worker (SAWs) program. I focus on the larger LAW program.

In order to be eligible for amnesty under IRCA's LAW program, applicants needed to arrive in the United States before January 1, 1982, reside there continuously and show evidence of their residency. Therefore, I can compare individuals who arrived before and after the cutoff date. Second, undocumented immigrants from specific

countries were more likely to apply for IRCA than other countries, which I call “high-take-up” countries. According to the Yearbook of Immigration Statistics from the Immigration and Customs Enforcement, formerly the Immigration and Naturalization Service, these countries include Mexico, El Salvador, Haiti, Guatemala, and Colombia (Table 1). Together, these countries account for nearly 86 percent of total applicants. Most applicants came from Mexico, accounting for about 75 of all applicants. All other immigrants will be considered as “low take-up” countries. Therefore, the second difference will be from comparing immigrants from high take-up countries to immigrants from low take-up countries. This empirical strategy is related to Kuka et al., (2019), who estimate the effect of DACA (Deferred Action on Childhood Arrivals) on educational outcomes.

Table 1. IRCA Applicants by Country of Birth

| Country of Birth | IRCA Applicants | IRCA Weight |
|------------------|-----------------|-------------|
| Mexico | 2,266,577 | 74.8% |
| Haiti | 59,800 | 2.0% |
| El Salvador | 167,952 | 5.5% |
| Guatemala | 70,953 | 2.3% |
| Colombia | 34,727 | 1.1% |
| Subtotal | 2,600,009 | 85.8% |
| All countries | 3,031,848 | 100.0% |

I use a difference-in-difference model to analyze the effect of IRCA on years of educational attainment of children by using the immigrants' year of arrival into the United States and their country of birth. I examine the long-run effect on educational attainment in the American Community Survey 5% Public Use Sample from 2013 – 2017. The trends in education rates are similar for both high-take up and low-take up countries before the cut-off date, which lends support to using the difference-in-difference specification to estimate the effect of IRCA. Unexpectedly, after controlling for individual variation in race and ethnicity, age of arrival, sex, and family size, my findings indicate that there is no evidence of a positive effect on educational attainment from IRCA. In fact, those who were *not* eligible for IRCA experience *more* years of education. I find that the entire effect is attributed to Mexican immigrants and discuss reasons for why I find this effect.

This paper contributes to the literature on the impact of IRCA on the economic outcomes of migrants. Some studies have estimated how IRCA influenced naturalized workers' wages relative to those who remained undocumented. For instance, Phillips and Massey (1999) find that IRCA encouraged greater discrimination against undocumented migrants who did not naturalize under IRCA, with employers passing the costs of unauthorized hiring on to the workers. Kossoudji and Clark (2002) analyze the wages of legalized men and found that wage determinants are structurally different after amnesty due to the elimination of the wage penalty associated with undocumented status. Cascio and Lewis (2018) use data on personal income taxes in California to show that Earned Income Tax Credits raised the incomes of IRCA-applicant families with children. The study most related to this thesis is that of Cortes (2013), which examines the effect of

IRCA on college enrollment. In contrast to my findings, Cortes finds an increase in post-secondary educational enrollment. Though I find no evidence of a positive effect in my model for years of educational attainment, this thesis contributes to the existing literature on the impact of the Immigration Reform and Control Act and its implications.

My research is also related to the literature on the Deferred Action for Childhood Arrivals (DACA) initiative in 2012. DACA is an immigration policy targeted toward individuals who arrived as children with undocumented status. Unlike IRCA which provided a path to citizenship, DACA granted applicants temporary protection from deportation (for a renewable two-year period) and also allows applicants to gain work permits. In contrary to my estimates a negative effect of IRCA on education, Kuka, Shenhav, and Shih (2019) find that DACA increased high school attendance and high school graduation rates. Additionally, they found positive impacts on college attendance. Others have shown that DACA improves health among children and adults (Hainmuller et al., 2017; Giuntella and Lonsky, 2018), reduces pregnancy rates among teens (Kuka, Shenhav and Shih, 2019), and improves labor market outcomes for adults (Amuedo-Dorantes and Antman, 2017).

The improved educational outcomes for DACA recipients suggest that DACA was a successful program for immigrant assimilation in contrast to my findings from IRCA. In other words, IRCA may have been a less effective policy, relative to DACA, in encouraging educational attainment.

CHAPTER TWO

Institutional Background and Take-Up of IRCA

After 15 years of failed attempts at immigration reform, the Immigration Reform and Control Act (IRCA) was signed into law by President Reagan on November 6, 1986. Its passage was many years in the making, and it won final approval in the closing days of the 99th Congress. The legislation was an important milestone in United States immigration history because it was the first and most comprehensive policy to take on the issue of illegal immigration to the United States. The primary objective of the legislation was to decrease unauthorized entries and legalize unauthorized immigrants already in the country.

A common descriptor of IRCA is a “three-legged stool.” Advocates of the policy saw all three components as being necessary for solving the problem of undocumented immigrants. The first leg included increasing border control and immigration enforcement. This portion of the law introduced criminal penalties for the use of fraudulent identity documents and transporting unauthorized immigrants. It also increased funding for the Immigration and Naturalization Service (INS) to carry out this objective.

The second leg was to establish federal civil and criminal penalties for employers who knowingly hired unauthorized immigrants. The portion of the law created a new employment verification regime, the I-9 process. The process requires employers to

confirm and document the lawful status and authorization to work of all new employees, including U.S. citizens. Along with the new process, IRCA made it illegal for an employer to discriminate against an applicant to his or her place of employment based on national origin or citizenship status. This portion of the law also created the office of Special Counsel for Immigration Related Unfair Employment Practices in the Department of Justice to handle claims of discrimination.

The third leg was the legalization program for unauthorized immigrants who had been living or working in the United States for extended periods. This leg will be the primary focus of this thesis given that it contains the natural experiment necessary to test the effect of naturalization on educational attainment. In order to grant eligibility to as many undocumented immigrants as possible, the third leg of the legislation included two distinct paths to legalization. The path that an applicant would choose depended primarily on the occupation of the applicant. President Reagan, in signing the bill, famously said that this piece of the legislation would permit unauthorized immigrants to “come out of the shadows.”

The first of the two legalization paths was for Special Agricultural Workers, or SAWs. Under this path, approximately 1.1 million applicants were granted permanent residence and about 250,000 were ultimately naturalized by 2001 (Rytina, 2002). Under this program, applicants must have been employed in a qualifying agricultural occupation in the United States for 90 days in the aggregate in 1984, 1985, and 1986. Additionally, they must have resided in the United States for six months, in the aggregate, in each of those years. Applicants under this program would be granted legal permanent residence status almost automatically one to two years after gaining temporary admission.

The second of the two legalization programs was a program to become a Legally Authorized Worker, or LAW. This was a more general legalization program that granted permanent residence to approximately 1.6 million applicants and about 635,000 were ultimately naturalized (Rytina, 2002). Under the program, applicants must document continuous residence in the United States since before January 1, 1982 to apply for temporary legal status. After obtaining approval or temporary status, applicants were able to apply for Legal Permanent Residence status contingent upon learning English and passing a civics test.

Freedman, Owens, and Bohn (2018) discuss a problematic circumstance in which immigrants may have fraudulently obtained documentation of continuous residence. Under each of the amnesty programs, any applicant who could provide minimal evidence of qualification was issued a work authorization card upon leaving the legalization office. The authorization was effective immediately and lasted until the INS made a final decision for the specific applicant's approval. In Mexico alone, over 70 percent of LAW applications and 40 percent of SAW applications were likely fraudulent (Donato and Carter, 1999). This situation opens concerns regarding my empirical strategy.

The demand for any evidence of residency from before January 1, 1982 created a black market or the documents needed to prove the date of entry into the United States. One federal employee recalls, "rent receipts, food receipts ... anything needed was for sale on Los Angeles streets ... there were document vendors all over the place and fraud was rampant" (Oltman, 2011). In response to an influx of applications and a heavy administrative burden, Baker (1990) notes that the initial amnesty applications would

generally be accepted if there was “anything with 1981 in the file” as sufficient evidence of LAW eligibility.

Freedman, Owens, and Bohn (2018) also compare census and INS data to find the degree of systematic misrepresentation of immigrants’ date of entry into the Bexar County, containing San Antonio, Texas. The census data suggests that annual immigration increased by 85 percent in the first two years of the 1980s, and the INS data point to a 300 percent increase during that period. Immigration then fell by 50 percent and 70 percent according to census and INS data, respectively, in the years following 1982. Given these high rates of fraudulent activity in one county of the United States, I can reasonably assume that other applicants in areas across the country may have engaged in some type of fraudulent misrepresentation.

The LAW program created a multistage path to citizenship. In phase one, applicants could obtain temporary legal status once they demonstrated they had been in the United States since the Jan. 1, 1982 cutoff date and paid a filing fee of \$185. After a minimum of 18 months in approved temporary status, they could apply for permanent legal status if they met provided proof of English language ability and passed an examination over U.S. civics and history.

Additionally, the Office of Immigration Statistics Statistical Yearbook from 1991 provides data on the countries of origin for IRCA applicants. Table 1 shows the top five countries from which IRCA applicants originated. Specifically, it shows the number of applicants from each country and divides the number of applicants from a specific country by the total number of applicants for IRCA. These five countries account for almost 86 percent of total IRCA applicants. Importantly, Mexico accounts for nearly 75

percent of all IRCA applicants, with the remaining four countries are Latin American.

Because of the large take up of IRCA from Mexican immigrants, I test to see if the large applicant base Mexico is impacting the results by excluding it in one regression. I find that Mexico is positively influencing the results.

Though over three million immigrants applied for IRCA, the legislation has been widely criticized for its ineffectiveness in decreasing the unauthorized immigrant population in the United States. According to the Migration Policy Institute, the unauthorized immigrant population in the United States has grown almost considerably from approximately 4 million in 1986 to the most recent estimate of 11.5 million in 2012. A quarter-century after the implementation of IRCA, the United States faced an undocumented population almost three times larger than the original one they sought to naturalize. I utilize the natural experiment of IRCA to test its effectiveness in improving educational attainment over thirty years after its implementation.

CHAPTER THREE

Data

I use data from the American Community Survey 5-Year Public Use Sample for the period 2013 through 2017 to examine the educational attainment of eligible and ineligible individuals under IRCA (Ruggles et al., 2019). I focus on child arrivals, or those who arrived less than 17, since they were the most likely to invest more in education rather than older adults. Since I use data from 2013-2017, enough years have passed since the policy for individuals to complete their total educational attainment. The American Community Survey samples information from households in the United States. Further, the survey includes households consisting of citizens and non-citizens. Importantly, the Census Bureau takes several precautionary steps to ensure the confidentiality of the responses from households (Liscow and Woolston, 2016). Personal information recorded on the ACS will not be distributed to other government agencies. Therefore, undocumented citizens should be included in the survey, though the extent of undercounting is unclear. For Spanish-speaking households, the Bureau partners with Hispanic organizations and distributes the survey in Spanish and English.

I use the year of immigration variable from the ACS as the first indicator of eligibility for IRCA. Individuals who reported a year of arrival before 1982 are considered eligible to receive amnesty under IRCA. The use of year of immigration may be a problem if there are large, unexplained differences between the number of arrivals in

high take-up countries and low take-up countries. Nevertheless, the reported year of immigration is not differential across the treated group (i.e., high take-up countries) and untreated group (i.e. low take-up countries). Naturally, there may be some error in the year of arrival variable as seen in the spikes in the year of arrival in Figure 2. In 1975, 1980, and 1985, there are considerable spikes in the reported year of immigration. The common tendency to round to years ending in zero or five may explain this finding.

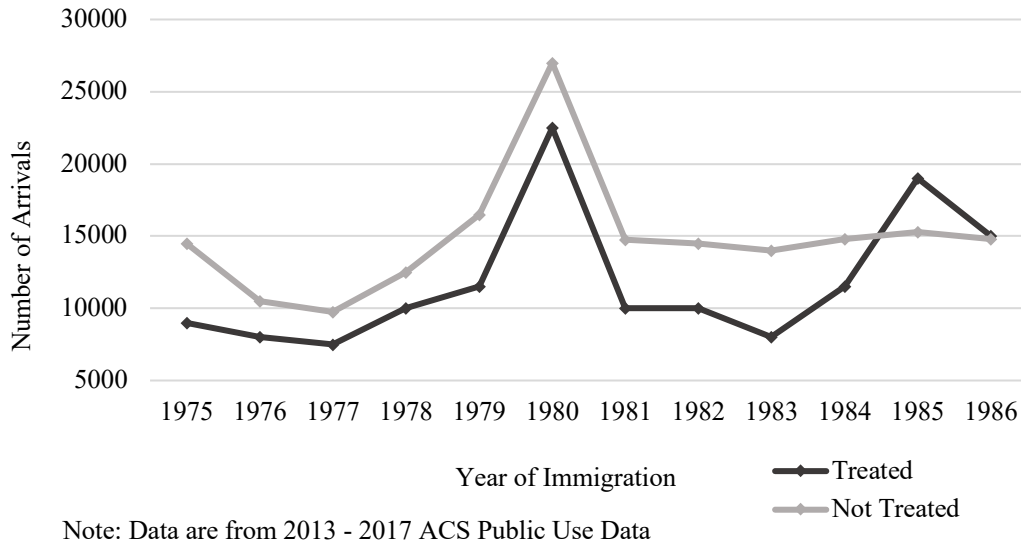


Figure 2. ACS 2013-2017 Reported Year of Immigration

Additionally, the increased number of arrivals around 1980 may be a result of individuals fraudulently reporting an earlier year of arrival to be eligible for amnesty under IRCA. Freedman, Owens, and Bohn (2018) discuss this fraudulent reporting. As a result, the number of individuals who are eligible, by year of arrival, for IRCA may be overstated. At the same time, if an immigrant lied to the government that he arrived in 1980 while actually arriving in 1983, then this would not be problematic since the

immigrant was treated by the policy. This could also help explain the negative effect I find if those who lied about arriving before 1982 had lower educational attainment in general.

Finally, the year of arrival variable may also be subject to misunderstanding for individuals who migrated in and out of the United States regularly. Confusion may arise in reporting only one year of immigration. Specifically, individuals could report their first entry into the United States, a subsequent entry, or their most recent year of entry. The Census Bureau instructs individuals to report their first year of entry in an attempt to correct this situation. Despite the instruction, I consider this situation as relatively common for individuals from Mexico due to geographic proximity. Mexico also accounts for approximately 75% of all IRCA applicants. The magnitude of imprecision in the year of arrival variable may be large for Mexican applicants.

For the second difference, I compare high-applicant countries to low-applicant countries. I use the Yearbook of Immigration Statistics from 1991 to find countries whose migrant stock was among the top five on a percentage-of-total-applicants basis (Table 1). These countries include Mexico, Haiti, El Salvador, Guatemala, and Colombia. Individuals who indicate their birthplace as one of these countries in the ACS are considered to be from “high take-up” countries. Those who arrived before 1982 are eligible for IRCA. Together, those who are from “high take-up” countries and arrived after 1982 are the difference-in-difference estimate. While those who arrived before 1982 are subject to the policy, I opt to use those who arrived after 1982 as the indicator in my regression to provide results that are easier to interpret.

For this paper, individuals who are citizens or were born abroad of American parents are excluded from the data. I focus on individuals who are not citizens or are naturalized citizens during the survey year. I recode the citizen variable to be a bivariate variable where “0” is not a citizen and “1” is a naturalized citizen. Table 2 shows the expected value of the citizen variable for each of the four groups with higher values indicating higher naturalization rates.

As mentioned before, this paper focused on the outcomes of childhood arrivals since they were the ones most likely to change their educational attainment. The American Community Survey does not provide information regarding the age of arrival for those who reported a year of immigration. I generate an “age of arrival” variable by using the year of immigration and subtracting the difference between the year of the observation and age. Naturally, age of arrival may contain errors if year of immigration was reported imprecisely. I intentionally limit the age of arrival variable from age zero to 18 to include individuals who would still be likely to continue obtaining education in the United States after their arrival.

In order to include individuals on both sides of IRCA’s eligibility date of January 1, 1982, I keep observations with years of immigration between 1975 and 1986. I do not include arrivals after 1986 since this is when IRCA was passed and it could change the type of immigrant who entered the country. I exclude individuals who did not report a year of immigration or recorded “not applicable.”

Table 2 shows the summary statistics of the year of immigration and other key variables. The education variable is recoded to reflect the true number of years of education that an individual has attained. For example, a high school graduate would

have an education level of 12 years and a college graduate with a bachelor's degree would have 16 years of education. Educational attainment among individuals from high take-up countries and low take-up countries show notable differences from around 12 years of education for those from high take-up countries and 16 years of education for those from low take-up countries. The predominant reason for this discrepancy comes primarily from low educational attainment among individuals from Mexico, a "high take-up" country. However, the difference in levels between high take-up and low take-up countries is not necessarily problematic for a difference-in-differences strategy. Rather the trend in educational attainment must be similar for those not treated by the policy, something I will test for later.

Interestingly, the table suggests that education levels were *higher* for high take-up countries after 1982 than before 1982. For high take-up countries, education levels after 1982 were 11.88 years compared to the pre-1982 level of 11.51 years. The increase for low take-up countries was smaller, changing from 15.98 years before 1982 and 16.05 years after 1982. It is surprising to see an increase in educational attainment for those who lacked access to citizenship in the United States.

Table 2. Summary Statistics of Variables

| High Take-up | Year of Arrival Pre-1982 | | | | Year of Arrival Post-1982 | | | |
|-----------------|--------------------------|--------------------|------|------|---------------------------|--------------------|------|------|
| | Mean | Standard Deviation | Min | Max | Mean | Standard Deviation | Min | Max |
| Education | 11.51 | 4.92 | 0 | 22 | 11.88 | 4.62 | 0 | 22 |
| Age | 49.81 | 5.65 | 36 | 60 | 44.50 | 5.39 | 31 | 53 |
| Citizen | 0.63 | 0.48 | 0 | 1 | 0.53 | 0.49 | 0 | 1 |
| Year of Arrival | 1978.45 | 1.95 | 1975 | 1981 | 1984.30 | 1.38 | 1982 | 1986 |
| Age of Arrival | 11.26 | 5.39 | 0 | 18 | 11.80 | 5.26 | 0 | 18 |
| | n = 33,324 | | | | n = 25,375 | | | |
| Low Take-up | Mean | Standard Deviation | Min | Max | Mean | Standard Deviation | Min | Max |
| Education | 15.98 | 3.78 | 0 | 22 | 16.05 | 3.65 | 0 | 22 |
| Age | 48.82 | 5.98 | 36 | 60 | 43.38 | 5.80 | 31 | 53 |
| Citizen | 0.87 | 0.33 | 0 | 1 | 0.83 | 0.37 | 0 | 1 |
| Year of Arrival | 1978.33 | 2.00 | 1975 | 1981 | 1984.04 | 1.41 | 1982 | 1986 |
| Age of Arrival | 10.15 | 5.66 | 0 | 18 | 10.42 | 5.61 | 0 | 18 |
| | n = 47,805 | | | | n = 31,152 | | | |

Additional educational attainment would include cases in which individuals go back to finish high school or obtain a post-secondary degree such as vocational schooling, an associate degree, or a bachelor's degree. Any additional educational attainment completed in the United States would increase an individual's United States specific human capital. However, an investment in additional human capital, in the form of education, requires some degree of certainty in an individual's ability to continuously reside in the United States. The certainty of continuous residence in the United States comes from amnesty policy or traditional naturalization. I consider the effect of a specific

amnesty policy, the Immigration Reform and Control Act, on educational attainment for those who were eligible by arriving before 1982 and born in high take-up countries.

CHAPTER FOUR

Empirical Strategy

The implementation of the Immigration Reform and Control Act of 1986 provides a natural experiment in which an objective measurement, year of arrival, though subject to some error, is used for determining eligibility. The first difference captures individuals who are eligible for amnesty under IRCA by their year of arrival. The second difference captures individuals who are more likely to apply for amnesty given their country of birth or residence. The top five countries represent approximately 86% of all IRCA applicants. These countries provided the highest number of applicants based on total applicants from the country divided by the total applicants for IRCA from all countries. These two differences create the difference-in-difference framework in which I interact the two differences to find the group of individuals who were from a “high take-up” country and were eligible by their year of arrival. The interaction between the two indicator variables generates the difference-in-difference estimator.

A key assumption of this framework is that there are no time-varying unobservable characteristics associated with the 1982 cutoff in year of arrival and high-take up countries. I will test this assumption by analyzing pre-trends among educational attainment.

There is one key distinction in my difference-in-difference set up versus a standard set up. In a standard difference-in-difference, the “before” period is untreated by

the policy and the “after” period is treated. However, since my time variable is year of arrival, the “before” period (1975-1981 arrivals) are treated while the “after” period (1982-1986 arrivals) are untreated. Because of this difference, I will estimate the effect of *not having access* to IRCA rather having access to IRCA.

Through an Ordinary Least Squares estimation method, I analyze the effect of IRCA on educational attainment on immigrant youth with:

$$Y_{it} = \alpha + \beta_1 (hightakeup_{it} \cdot post1982_{it}) + \beta_2 hightakeup_{it} + \beta_3 post1982_{it} + \delta X_{it} + \varepsilon_{it}$$

where Y_{it} is the dependent variable measuring the years of education that an individual immigrant youth i has received in cohort t . $Hightakeup_{it}$ is an indicator variable for the country of birth of an individual. $Hightakeup_{it}$ is equal to one if an individual is from a country considered to be “high take-up” and zero if an individual is from a country considered to be “low take-up.” $Post1982_{it}$ is equal to one if an individual arrived after the cutoff date of January 1, 1982, and zero if an individual arrived before the cutoff date.

The difference-in-difference parameter β_1 measures the additional difference in educational attainment for the individuals who do not have access to amnesty through IRCA. The point estimate captures the effect of removing IRCA on educational attainment after controlling for variation in the following variables: gender, birthplace, race, household family size of the immigrant youth, family size squared, and age of arrival. These controls are similar to those used in Cortes’s (2013) study of the effect of IRCA on immigrant youth postsecondary enrollment. Cortes specifically controls for gender, race/ethnicity, household family size of the immigrant youth, family size squared,

and school-level age of arrival indicator variables. In my study of the effect of IRCA on immigrant youth educational attainment, I control for gender, birthplace, race, household family size of the immigrant youth, family size squared, and age of arrival.

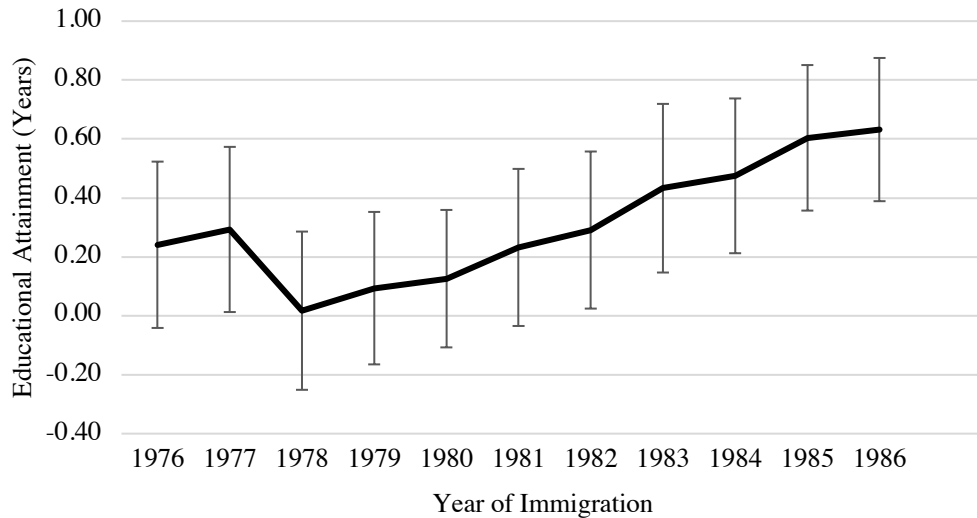
This empirical strategy is based on guidelines for the Legally Authorized Worker program, but about 34 percent of naturalizations took place through the Special Agricultural Workers program. The Special Agricultural Worker program could include those who arrived between 1984 and 1986. In this paper, I am looking specifically at childhood arrivals, and may not capture immigrants who applied through the SAW program.

CHAPTER FIVE

Results

In order to test the pre-trends and post-trends around the critical year of immigration of 1982, I run the following regression where each year interacts with the treatment group:

$$Y_{it} = \gamma_c + \eta_t + \sum_{t=1976}^{1986} \delta_t(\text{hightakeup} \cdot \text{year}_t) + \varepsilon_{it}$$



Note: Error bars show 95% confidence interval.

Figure 3. No Evidence for Differential Educational Attainment

From 1976 to 1981, with the exception of 1977, the difference in educational attainment between the treated and nontreated groups is statistically insignificant at the 5

percent level. This insignificant difference before the cut-off year of arrival of 1982 shows that there is little difference before 1982. Any effect of IRCA on educational attainment should be found within these years because immigrants needed to arrive before 1982 to be eligible for the policy. As shown in Figure 3, those who were not eligible for IRCA based on a year of immigration after 1982 experience higher levels of educational attainment.

The results of the difference-in-difference estimate are shown in Table 3. I estimate a positive and statistically significant β_1 . The point estimate on the DID estimator is 0.350 and is statistically significant at the 1 percent level. Unexpectedly, the effect of removing access to citizenship, as shown in Regression 2, is an increase in educational attainment of 0.35 years of education. I compare my results to the findings of Kuka, Shenhav, and Shih (2019), who find that DACA led to statistically significant increases in school attendance of 14–18-year-olds, with a 1.2 pp increase among all immigrants, which is equivalent to a 1.3 percent increase in school attendance. This 1.3 percent increase is approximately 0.156 years of education based on 12 total years of education. The increase in educational attainment for those not eligible for IRCA, and without access to citizenship, is large compared to the increases for those who were eligible for DACA.

The lack of access to citizenship has different effects on educational attainment for males and females. Males with a lack of access to citizenship experienced an increase of 0.448 years of educational attainment (Table 3, Regression 5). Females without access to citizenship through IRCA experience an increase in educational attainment of 0.273 years (Table 3, Regression 6). A lack of access to citizenship has a stronger effect on

male educational attainment versus female educational attainment by 0.175 years of education. On average, males who were not eligible to naturalize through IRCA’s amnesty program achieved higher levels of education than their female counterparts. Both the male and female DID estimators are large relative to the findings of Kuka, Shenhav, and Shih (2019) of approximately 0.156 years for all individuals.

Table 3. Effect of Lack of Access to Citizenship on Educational Attainment

| | | | Exclude | | Sex | |
|--------------------------|---------------------|---------------------|-------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | Mexico | 1980 | Male | Female |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post-1982 x High-Take up | 0.340*** (0.053) | 0.350*** (0.050) | 0.0472 (0.091) | 0.346*** (0.054) | 0.448*** (0.070) | 0.273*** (0.071) |
| Fixed Effects | | | | | | |
| Year of Immigration | | Yes | Yes | Yes | Yes | Yes |
| Birthplace | | Yes | Yes | Yes | Yes | Yes |
| Sex | | Yes | Yes | Yes | | |
| Race | | Yes | Yes | Yes | Yes | Yes |
| Family Size | | Yes | Yes | Yes | Yes | Yes |
| Age of arrival | | Yes | Yes | Yes | Yes | Yes |
| Observations | 137,656 | 137,656 | 90,646 | 116,862 | 71,585 | 66,071 |
| R ² | 0.22 | 0.33 | 0.19 | 0.33 | 0.34 | 0.32 |

Notes: Regression (3) excludes observations with the birthplace of Mexico. Regression (4) excludes observations with a year of arrival equal to 1980.

*** Significant at the 1 percent level.

Robustness

The difference-in-difference regression, using the five “high take-up” countries including Mexico, estimated a positive effect of lack of access to citizenship on educational attainment. Table 3 also shows the results of the difference-in-difference regression when Mexican-born immigrants are excluded from the sample and immigrants

who arrived in 1980 are excluded from the sample (Regression 3 and 4). I examine these results in greater depth to determine what effect Mexican-born immigrants and rounding to 1980 may have on the standard regression.

As shown in Figure 2, the year 1980 saw a sharp increase in reported year of immigration that is likely due to rounding. I drop observations with a year of arrival equal to 1980 and find a lack of access to citizenship is associated with an individual's educational attainment of 0.346 years, which is statistically significant at the 0.01 level. (Table 3, Regression 4). I conclude that the difference between the increases in years of education is negligible and that any rounding to 1980 does not adversely affect my results.

Mexican-born individuals account for approximately 34 percent of the observations in the data and 80 percent of the observations in the high take-up group. Therefore, the estimated effect of lacking access to citizenship may include a Mexico-specific effect rather than the effect for the average country. When individuals from Mexico are excluded from the sample, lack of access to citizenship is associated with a statistically insignificant effect on educational attainment (Table 3, Regression 3). For these reasons, I consider Mexico on a standalone basis by repeating my previous regression and including only Mexican-born individuals in the treated group.

Table 4 shows a stronger effect of lacking access to citizenship on educational attainment for Mexican-born immigrant children. Lacking access to citizenship is associated with an increase in educational attainment of 0.428 years, which is statistically significant at the 0.01 level (Table 4, Regression 2). The Mexico-specific effect of 0.428 years of education is large relative to the overall effect of 0.350 years of education. I

conclude that there is a Mexico-specific effect that is influencing the overall effect. Specifically, the effect is strong among Mexican-born men who lack access to citizenship relative to their female counterparts.

Table 4. The impact for immigrants born in Mexico

| | | | Exclude | Sex | |
|--------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | 1980 | Male | Female |
| | | | (3) | (4) | (5) |
| Post-1982 x High Take-Up | 0.329*** (0.056) | 0.428*** (0.054) | 0.426*** (0.058) | 0.552*** (0.075) | 0.319*** (0.078) |
| Fixed Effects | | Yes | Yes | Yes | Yes |
| Year of Immigration | | Yes | Yes | Yes | Yes |
| Sex | | Yes | Yes | | |
| Race | | Yes | Yes | Yes | Yes |
| Family Size | | Yes | Yes | Yes | Yes |
| Age of arrival | | Yes | Yes | Yes | Yes |
| Observations | 125,967 | 125,967 | 107,117 | 65,717 | 60,250 |
| R ² | 0.25 | 0.34 | 0.35 | 0.36 | 0.33 |

Notes: Regression (3) excludes observations with a year of immigration equal to 1980.
*** Significant at the 1 percent level.

Table 5. The impact for immigrants born in Haiti, El Salvador, Colombia, and Guatemala

| | | | Exclude | Sex | |
|--------------------------|---------------------|--------------------|--------------------|--------------------|------------------|
| | (1) | (2) | 1980 | Male | Female |
| | | | (3) | (4) | (5) |
| Post-1982 x High Take-Up | -0.0254 (0.0976) | 0.0472 (0.0909) | 0.0079 (0.0988) | -0.0211 (0.131) | 0.126 (0.125) |
| Fixed Effects | | Yes | Yes | Yes | Yes |
| Year of Immigration | | Yes | Yes | Yes | Yes |
| Sex | | Yes | Yes | | |
| Race | | Yes | Yes | Yes | Yes |
| Family Size | | Yes | Yes | Yes | Yes |
| Age of arrival | | Yes | Yes | Yes | Yes |
| Observations | 90,646 | 90,646 | 77,137 | 44,924 | 45,722 |
| R ² | 0.051 | 0.190 | 0.191 | 0.197 | 0.199 |

Notes: Regression (3) excludes observations with a year of immigration equal to 1980.
*** Significant at the 1 percent level.

I run an additional regression to test whether non-Mexican individuals had increases in education (see Table 5). Table 5 shows that immigrants born in Haiti, El Salvador, Colombia, and Guatemala face a statistically insignificant change in education in response to a lack of access to citizenship. Because my treated group consists of these four countries and Mexico, I conclude that Mexico is driving the effect I find in the results. Equivalently, the total effect is a weighted average of the results for Mexico and the results for Haiti, El Salvador Colombia, and Guatemala. The number of Mexican-born immigrants and the powerful Mexico-specific effect on educational attainment contribute to this finding.

For each subgroup of immigrants from high take-up countries, I also test for an adverse effect of an individual rounding their year of arrival to 1980. I drop observations with a reported year of arrival of 1980 and conclude that any rounding effect for year of immigration equal to 1980 does not adversely affect my findings.

My findings indicate that there is no evidence of a positive effect of IRCA on educational attainment. Equivalently, those who lacked access to citizenship experienced more years of education. However, the average effect on total years of education may mask the effect on degree completion. To test the level of education that is driving the effect of the increase in years of education, I consider how lacking access to citizenship affects high school and college completion. Table 6 summarizes these results and also includes a similar test for the ability to speak English.

In contrast to my expectations, the effect of lacking access to citizenship on educational attainment in these years is statistically insignificant except English-speaking

abilities for high take-up countries. High school and college completion levels are not noticeably affected by a lack of access to citizenship.

Combining my results from Table 3 and Table 6, I consider a possible explanation for my findings. Individuals who dropped out of high school and arrived after 1982 may have attended high school longer but still did not complete the degree. My model does not capture changes in local policy that may have positively or negatively influenced high school completion rates.

Table 6. Impact on High School, College Completion and Ability to Speak English

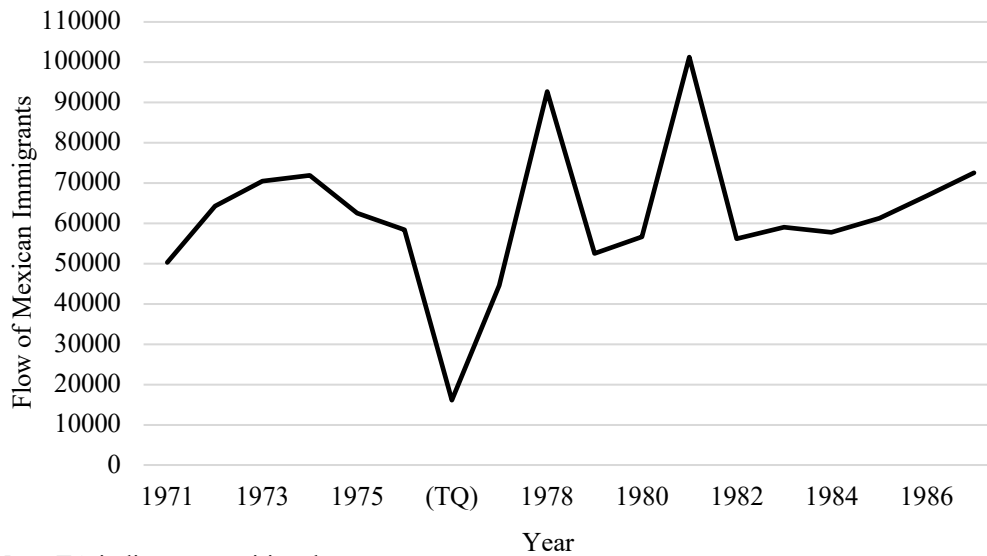
| | High Take-Up Countries | | | Excluding Mexico | | |
|--------------------------|------------------------|-------------------|-----------------------|--------------------|--------------------|-------------------|
| | High School | College | Speak English | High School | College | Speak English |
| Post-1982 x High Take-Up | -0.0004 (0.006) | 0.0027 (0.004) | -0.0182*** (0.005) | -0.0119 (0.011) | -0.0031 (0.006) | 0.0108 (0.009) |
| Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Year of Immigration | Yes | Yes | Yes | Yes | Yes | Yes |
| Birthplace | Yes | Yes | Yes | Yes | Yes | Yes |
| Sex | Yes | Yes | Yes | Yes | Yes | Yes |
| Race | Yes | Yes | Yes | Yes | Yes | Yes |
| Family Size | Yes | Yes | Yes | Yes | Yes | Yes |
| Age of arrival | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 137,656 | 137,656 | 137,656 | 90,646 | 90,646 | 90,646 |
| R ² | 0.29 | 0.12 | 0.17 | 0.53 | 0.08 | 0.09 |

*** Significant at the 1 percent level.

Importantly, individuals who lack access to citizenship from “high take-up” countries are approximately 2 percentage points less likely to speak English. The effect is insignificant when Mexican-born immigrants are excluded from the model. Because the effect appears to be driven by individuals from Mexico, I consider a potential explanation for my findings. One possible explanation is that individuals are self-reporting their

English-speaking abilities. This means that the variable is measured with error, and the effect I find is related to changes in reporting.

Overall, these results were largely unexpected due to the theoretical implications of large amnesty programs. In theory, access to citizenship through an amnesty program decreases the risk of deportation for previously undocumented immigrants. This decreased risk of deportation would incentivize newly documented individuals to invest in United States-specific human capital. The most common example of this capital is education such as high school or college degrees. Given the unexpected results and the pronounced effect related to Mexico, I consider the immigration trends from Mexico to the United States during the 1970s and 1980s. In the late 1970s, Mexico was burdened by international debt and enforced economic measures that hurt the poorest members of Mexican society. These conditions ultimately led to a recession as Mexico experienced high interest rates, falling oil prices, and inflation. These conditions led to high levels of unemployment and lower real wages for Mexican citizens. As a result, thousands of Mexican citizens traveled north to the United States, primarily to California, to lead their families to a better life. This increase in flows from Mexico is shown in Figure 4.



Note: TQ indicates transitional quarter.

Source: Carter, Susan B., et al. "Immigration by Country of Last Residence—North America": *Historical Statistics of the United States: Earliest Times to the Present*, New York: Cambridge University Press, 2006.

Figure 4. Flow of Mexican Immigrants increases in 1978 and 1981

The trend of Mexican flows into the United States shows a large spike in volume in both 1978 and 1981 with increasing volumes throughout the 1980s. The spikes suggest that the poorest members of Mexican society, with presumably low levels of education, may be contributing to the lower education levels for immigrants who arrived before 1982. Additionally, as more individuals left Mexico in years after 1982, higher levels of education would appear as an increase in educational attainment for those without access to citizenship.

CHAPTER SIX

Conclusion

In this paper, I quantify the education response of immigrant youth from high take-up countries to a lack of access to United States citizenship. I find that there is a positive effect on educational attainment for those who arrived after 1982 and did not have access to citizenship through IRCA. Equivalently, I find no evidence of a positive effect on educational attainment for those who were “treated” by the policy. My difference-in-difference model also shows that the effect is predominantly a Mexican effect and the effect was more pronounced among Mexican males.

Specifically, the lack of access to citizenship increased educational attainment by an average of 0.350 years. Given that Mexico accounts for approximately 34 percent of individuals in the data, I exclude Mexico and repeat my difference-in-difference regression. Excluding Mexico reduces the overall effect to a statistically insignificant level. Not surprisingly, the effect of Mexico is influencing my results. The Mexico specific effect of lacking access to citizenship is 0.428 years of education. For Mexican males, the effect of a lack of access to citizenship is an increase of 0.552 years of education.

My findings suggest that there is no evidence that IRCA had a positive effect on educational attainment. I find that the increases among those who lack access to citizenship are marginal, but they do not significantly affect high school completion rates

or college graduation rates. My final analysis shows that a lack of access to citizenship decreases the probability that Mexican-born immigrants can speak English 2 percent.

Overall, I find no evidence of a positive effect of IRCA on educational attainment. My findings do not confirm whether or not policies that decrease the risk of deportation for youth lead to a more educated population. However, studies of modern programs like DACA suggest that immigration policies that include incentives for education and reduce uncertainty can lead to improved outcomes for immigrant youth. Without such programs, undocumented immigrants face an unprecedented level of deportation risk in the United States from the U.S. Immigration and Customs Enforcement. Overall, the results suggest that one should not assume that amnesty would lead to a large increase in educational outcomes for the undocumented population.

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